|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0001** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0001=John Hopfield** “I’ve never been part of the gang. I was a one man band playing little tunes.” Meet physics laureate John Hopfield in a podcast recorded at his cottage in Selborne, England. Together with host Adam Smith, he reflects on the value of interdisciplinary work and how chemists and physicists might collaborate more closely.  They also discuss the future of AI and Hopfield’s greatest fears about it.  This conversation was published on 10 July, 2025. Podcast host Adam Smith is joined by Karin Svensson.  Below you find a transcript of the podcast interview. The transcript was created using speech recognition software. While it has been reviewed by human transcribers, it may contain errors. “I’ve never been part of the gang. I was a one man band playing little tunes.” Meet physics laureate John Hopfield in a podcast recorded at his cottage in Selborne, England. Together with host Adam Smith, he reflects on the value of interdisciplinary work and how chemists and physicists might collaborate more closely.  They also discuss the future of AI and Hopfield’s greatest fears about it.  This conversation was published on 10 July, 2025. Podcast host Adam Smith is joined by Karin Svensson.  Below you find a transcript of the podcast interview. The transcript was created using speech recognition software. While it has been reviewed by human transcribers, it may contain errors.  Karin Svensson: This is Nobel Prize Conversations and our guest is John Hopfield who received the 2024 Nobel Prize in Physics. He was awarded for foundational discoveries and inventions that enable machine learning with artificial neural networks and share the prize with [Geoffrey Hinton](https://www.nobelprize.org/prizes/physics/2024/hinton/facts/). Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces. John Hopfield is the Howard A. Pryor professor in the Life Sciences Emeritus and Professor of Molecular Biology Emeritus at Princeton University. Adam spoke to John Hopfield at his home in picturesque Selborne, England, where they discussed how interdisciplinary cooperation is the glue to build whole new fields when scouring neighbouring subjects yielded the perfect research problem and what scares him about his Nobel Prize awarded discovery. This time only we geek out on AI in a special discussion after the credits, so don’t miss it. But first, a few tips on what physicists can do when times are tough.  Smith: I wanted to start by asking you about your upbringing because your parents were physicists and you were brought up in a physics environment. I gather that that gave you a peculiar sense of the world through the lens of physics.  Hopfield: I would presume that it must have. Certainly I looked at my friends when I was a child. Their background was very different from mine. Mine was how does the world work? How do you find academic a job? I was born in -33. What was going on there at the time was the century of progress, the centennial of the city of Chicago, and there was a physics exposition. My father had been hired to set it up, set up the demonstration building as it were. This was deep in the depression. There were very few jobs. So a job which you wouldn’t otherwise thought of as being temporary was good enough at the moment for my father, it had to be. That’s how I wound up starting life in Chicago.  Smith: I see. With so much else to worry about it must have been a, in a way, a strange time to be focusing so much on physics and progress with the depression all around you.  Hopfield: Basically in 1930 there were no academic jobs that year period. Because universities always had young faculty and you paid them out of what money you had around. But the country was broke, so no universities were prospering. If you go forward and look at who was actually in the Los Alamos project, it was so heavy on Europeans because relative to the population, they came from a fairly steady academic income environment. There were more jobs per capita in Europe at the time, and the job base in science was per capita higher in the US so it was easy to get people, particularly on a threat of war, to come to the US at that point. If you look at what the president of Los Alamos in 1943, it’s strongly biased toward actually European scientists. That was a revolution. Ten years earlier Roosevelt came in and basically changed the economics of the country and in the long run it changed the job market and it’s very hard to see in this transient what’s going to be temporary, what’s the nature of the countries. But yes, it was an unusual time.  Smith: Was there ever any question that you were not going to be a physicist? Was it sort of expected of you and did you yourself feel that this was your path?  Hopfield: No, neither of them had come from science and it wouldn’t have been obvious I would to get from a child who a scientist in the US either. They simply didn’t push, but they didn’t push on a lot of things. They didn’t push on religion, for example, though I wound up with whatever I had and they had lived for about a year in Germany before I was born, in fact. But they were sort of European connected, but not as immigrants. It was what I had. They had always had the world around them as something that they could enthusiastically talk about.  Smith: So you grew up with enthusiasm or seeing enthusiasm for things and for knowledge for finding out?  Hopfield: Yes, I don’t think they would’ve understood the extent to which as the child grows, what you do reduces the environmental vacuum in which the child signs itself all directions possible.  Svensson: Hi Adam. John Hopfield was born in Chicago, but your conversation takes place in Selborne, a small town in the south of England. What kind of place is it?  Smith: It’s a beautiful place, beautiful and quiet. It’s nestled in rather sharp escarpments, wooded escarpments that form part of the south downs of England. It’s famous for having been the home of an 18th century parson called Gilbert White, who you may well have heard of. He wrote a book called ‘The Natural History of Cell Born’, which became very famous and detailed his close observation of all sorts of things in nature that a lot of people back in the 18th century felt were a bit irrelevant to look at like worms. His observational paths in particular I think were picked up on by people like Darwin later on as being important. Yes, it feels a bit stuck in time, a quiet English village.  Svensson: How did John Hop field end up there?  Smith: I believe it was his wife Mary Waltham’s home before they met. They kept it on and now they spend six months a year there and they spend the other half of the time in Princeton, which I guess you can do when you are in an emeritus professor.  Svensson: Sounds like a good life.  Smith: Yes, I think it is.  Svensson: Why was John Hopfield awarded the Nobel Prize?  Smith: He developed really this first neural network that has been the basis for so much all this machine learning. He was interested really in modeling how the brain works. He built a network of, if you like, artificial neurons with a feedback mechanism that could learn. That was the very beginning of people’s forays into modeling neural systems and building these learning networks. His network was called a Hopfield network and was the start of it all.  Svensson: He is a physicist, but his work is not confined to physics, is it?  Smith: No, not at all. He was trained as a physicist and he was in the physics department at Princeton in the 60s and 70s, but then also became part of the neuroscience department and the molecular biology department and really has used physics as a key to open up some of the mysteries of life. He explores all sorts of different aspects of the world.  Svensson: That seems quite intuitive but also quite unusual, isn’t it?  Smith: Many physicists have made the foray into life sciences, for instance. I think what he’s particularly good at is identifying just what the tools he has at his disposal allow him to ask and answer. For instance, he looked at a particular problem with hemoglobin called Alistair, and then he looked at electron transfer reactions and then he looked at neural networks. These are all problems that he could see that, what he had at his disposal allowed him to address interestingly. That’s all about the art of getting it right, having a big enough question and the tools to answer it.  Svensson: But why is it quite unusual then this kind of cross-disciplinary work?  Smith: I suppose one gets a bit confined in one’s rut. He might argue that there’s a tendency for people to move towards the sort of gravitational center of a field. If you’ve got the field of condensed metaphysics, you really want to be where the action is at in the middle of the field out at the periphery, you’re a bit alone. But that was his hunting territory. Places at the edge of things where interesting things were happening at the intersection between fields and maybe it’s riskier and also maybe your colleagues don’t recognise the achievement as much because it’s unknown territory, don’t quite know what you’ve done really that’s relevant to this field. Of course all scientists are probing the unknown, but are they probing the unknown that everyone’s expecting to be probed or are they going into places where few people dare to roam?  Svensson: So it’s not that biologists and physicists are sort of different species.  Smith: Wonder what Gilbert White would’ve said about that. I’ve often heard it said that, physics describes everything and so it is all physics essentially. Then the chemist might make the same claim and biologists might say they’re studying real complexity. I don’t know, I think the personality of scientists are so various that you really couldn’t draw any conclusions.  Svensson: How does he speak about this journey from physics to biology?  Smith: Well, I asked him about his transition from condensed metaphysics to neuroscience and let’s listen to what he said.  Hopfield: I worked in a particular narrow area very successfully and that area was running out of the kind of problems I knew how to solve. So I was at that point realising I had to find another source of problems because the good ones had been taken out, the small ones were small and you should work on the all problems. This was when I was roughly 35 and I was looking for a big problem. The area of physics I was associated with didn’t have anything appropriate that I could find. I went to the Nils Bohr Institute when they said they would run a seminar series on how does physics and biology get together and why don’t they get together? I had the wonderful position of being able to basically to pick the seminar speakers out of the best scientists available in Europe. They gave marvelous, interesting talks on their science. Nobody thought that the physicists would be of any help to them. This is true that of everybody, the physicists who might have done things weren’t exposed to the kind of biology that would’ve been much help. If you hear your thing about physics at the time is that if you asked the physicist what’s the biggest problem in biology, you might well have said the origin of life. Now, in many ways, that’s a wonderful answer. It’s true, but the time is not right. As I say, anybody who is going to seriously pursue that problem is starting in 1930 would get historically nowhere.  Smith: Exactly. The question is what actually is a good, big problem, not just a big problem.  Hopfield: I knew I had to be actively looking for a problem. I didn’t know how to do it. You had to be able to see that the problem was somehow related to things that I knew how to solve. That wasn’t a very big number. But when neurobiology, a neurobiologist came to talk, as did Nils Bohr, he would make it clear that his neurobiology had something to do with the ground problems of behaviour.  Smith: It seems to me that a lot of physicists let it lie with a sort of feeling that physics would solve it all eventually. But we don’t need to get involved yet. Let the biologists sort out some of the processes first and then then we can step in later. Is that fair?  Hopfield: There would’ve been many Hampshire who said not yet. It was never going to be the right time. The easy problems would be solved before very many in the community had waked up to the fact that you can take this physics over and drop it on a piece of science over here. There is some degree of mix of natural handshake because you need some mathematics actually to solve this problem over here. If you think about it for more than 10 seconds, you realise that most of what the neural system does is to solve problems that you write down traditional equations for. You may not know what the equation should be yet, but there was going to be equations of that structure and internal biology had to do with getting the signals in which start the system off and it computes for a while, gives you an answer. That process is computational. On the other hand, it’s dynamics. We want to know where the planets are next year. You see where they are now and you calculate using dynamics. So you see possibly there’s a connection between dynamical systems and understanding how neurobiology accurately processes this thing.  Smith: So you saw something like the question of associative memory and how it functioned as basically a calculation problem.  Hopfield: Yes. It was important that you saw the physics problem, that you saw the biology problem as the computational problem, that you knew that the computational stuff you already do some other edge of physics could be brought over and they look more like each other than you might have expected.  Smith: One of the things that you had the great insight to do was to take a problem, which is huge, but reduce it to something that you could get somewhere with. There have been many physicists who’ve attempted to tackle huge problems such as consciousness one thinks of Francis Crick or Donald Glaser, but those problems were just too big. You couldn’t get anywhere really.  Hopfield: Yes. They found that the problem how the nervous system generates motion. Motion is so complex, how many muscles do you have to drive to get and so on. People coming from biology, at least in the United States, had neglected mathematics ever since they were freshmen in college.  Smith: Yes. I suppose that is key, that dividing point makes a big difference.  Hopfield: Yes. You have to realise you don’t need all of the mathematics. The corners of mathematics, you better have them. Even with that basic idea, it took an act to put me in contact with a lot of good being done from different quarters on what looked to me like a common problem. I realised that even the people who were brought us in outlook just didn’t seem to have the tools themselves to work on the problem, which was the interesting problem, which is how the integration takes place. I say the interesting problem, you have to say it was a little naive. It’s a little luck.  Smith: Yes. It turned out to be a very interesting solution, which has spawned so much. It’s led to such enormous consequences. But given your success working in these interface areas, how do you encourage others to follow you? Because we still have a structure where physicists are physicists and biologists are biologists mainly and a few pioneers jump across and see what they can do. But is there more you can do than just wait for those pioneering individuals to make the links? Because it’s obviously very fertile territory if you can get it right.  Hopfield: I think it depends really only on the history of intellectual fields. We divide science up into chemistry, organic chemistry, physical chemistry, and with physics we do the same thing with physics labels. Every discipline keeps splitting itself as they get more knowledge within their discipline and they need some kind of contact with that larger piece of knowledge to work on the problems, which they’ve always to be getting bigger and more complex. There are pressures on the field to split and then to bring together those which are pretty close together anyway, to give yourself a little more intellectual base and into which you all are allowed can fit. You have to make space for them in your curriculum. Alright, these guys are marginal anyway, let’s push them off somewhere else. There’s the spontaneous inclination for large disciplines to split.  Smith: You have your put in many different Princeton departments, which you’ve helped build the neuroscience department, the molecular biology department, the physics department. So you span all of this. But for instance, with the teaching of neuroscience in colleges sometimes I always felt that it was full of possibilities and undergraduates coming through those courses felt they had a huge breadth of knowledge and it could see how to do many things, but they didn’t have any core discipline that they could bring to bear on the problems. They could see the connections, but they didn’t have the grounded center. I don’t know if that is how you see these things.  Hopfield: That was the problem. They were taught all about wood, but they never were told about glue with held things together and enabled you to do larger projects and understand some of the bigger issues which got you hooked. It’s a youngster.  Smith: People often talk about not seeing the wood for the trees, but in this case you’re seeing the wood but missing the glue.  Smith: What a beautiful place this is. It’s amazing the forested hill rising above.  Hopfield: Yes. And that’s the cell borne hanger.  Smith: Cell borne hanger.  Hopfield: Yeah. The geological formation of the south coast area is a series of ridges. They get higher higher and they get lower and lower ridge north inland and the hangers are dominated by beach and some kinds of oak. They were steep enough land that they weren’t suitable farmland for a long period of time. Pasture woodlands. But they were relatively undisturbed. It’s now what comprises a big part of the area of the South Cols National Park.  Smith: A lovely playground for young cell born resident to go exploring in now and back in the 18th century when Gilbert White was digging around in there.  Hopfield: The 18th century Gilbert White was still trying to find some of the basic behavioural questions of what do startlings do for the winter.  Smith: Yes. Having the observational powers to recognise that everything was interconnected. Even the small and despicable worm had a role to play.  Hopfield: Yes. What a wonderful discovery that was. Some people might divide science world into lumpers and splitters. The lumpers are all for putting the observed categories, putting them together in different ways. They’re a smaller number of things. Now they understand the structure of what the lumps should be. The splitters on the other hand say, look at this characteristic of the things that you say are lumps, they’re not lumps at all. Physics has far more glitters than many of the subsidies. There was a dividing line somewhere between is matter made up of atoms or is matter a continuous stol and your atoms are a whimsical creation of the particular way you did experiments.  Smith: That’s a nice question.  Smith: I wanted to ask you also about the consequences of that early work which have turned into machine learning and are dominating so much of what we have around us and promise to be more influential still. I suppose the question that everybody wants to know is what are your fears about it? There are many ways to think about that. It can be just the fears of the implementation of machine learning AI to replace things that humans are used to doing and feel that they don’t want to let go of. It can be doomsday scenarios of AI becoming conscious or already being conscious if you were talked to Geoffrey Hinton taking control where it’s not supposed to take control. Where do your fears lie if you have any?  Hopfield: The thing that bothers me most about AI is the inability of those people who work in the field to tell you why it works. There are enough things that you can do with it, which are extremely disruptive because you don’t know how far these things can be done intellectually or what might be done viciously. If you have no limits on what might happen, the idea that you should then advertise and sell this stuff as a viable product in a civilised world, if the product didn’t have its flaws, it wouldn’t be such an issue. But little like doing medicine, when you have a process which makes new medicines for you and see it works 70% of the time, and if one of those doesn’t work, suddenly it could wipe everything out. If you had a product which might make a chain reaction and destroy all of whatever was supposed to be good for. If you had that possibility, you’d worry.  Smith: It certainly forms the basis of sci-fi scenarios that where somebody creates something that has some cataclysmic effect unintended. Sort of Kurt Vonnegut scenario or something like that.  Hopfield: Yes, in 1973 the basis for modern genetic engineering was discovered and there was a lot of discussion in all biology departments of a variety of different flavours about whether this was something which was safe or whether it was so unsafe that you really had to tightly regulate laboratories, which you’re going to try to do it. There was active debate in major biology departments about how much recombinant DNA technology should be going on in universities. This is the kind of discussion which there ought to be in the AI world and you just do not see it going on.  Smith: So why is that? Because that, that example of the way that people talked about the potential of the genetic revolution in the seventies is held up as a paragon of how you should do things, the assima conferences etc. There if you liked the understanding of what was happening was greater than is true for AI now. Why are we left in this situation where the conversation isn’t happening to such an extent?  Hopfield: There are a lot of things pushing. One is the very large amounts of money, which can be rapidly made by ignoring regulation. People knew that there were all kinds of things you’d like to try in recombinant DNA. Nobody had much of any idea of how many of those things were real, how many could you actually do or was this a nice in principle thing. But the cost of doing one experiment just exhaust a university and AI is peculiar in the sense that its materials caused are very small and it suffers from all the things that people worried about in becoming in DNA. But the AI had the advantage that there was no long series of steps that you had to do to get the whole thing working, to make one operation on one child succeed. I can’t think of another case really involving physics where the same worry though there must have been a same, there must have been physically experiments with people of particular religion might believe that God would strike down so on if the experiment were done. That set of people never won in the physics. That you didn’t have a set of people who was going to be out of their livelihood if they didn’t win the battle of is dysregulated or not.  Smith: I wanted to ask what it’s like receiving the Nobel Prize. You’re sort of retired but must be very hard to retire when so many people want your attention. What’s it like adding yet another award to your collection of awards and having yet more attention at this age?  Hopfield: I’ve never been part of the gang. I was a one man band playing little tunes every once in a while. Somebody picked one up and found it was interesting more often than they picked it up and found it was not interesting.  Smith: Thank you very much indeed. What a pleasure to come down here and meet you.  Hopfield: Do you know where you’re going here locally?  Smith: I do, yes. I’m fine. Thank you very much.  Svensson: You just heard Nobel Prize Conversations. If you’d like to learn more about John Hopfield, you can go to nobelprize.org where you’ll find a wealth of information about the prizes and the people behind the discoveries. Nobel Prize Conversations is a podcast series with Adam Smith, a co-production of Filt and Nobel Prize Outreach. The producer for this episode was me, Karin Svensson. The editorial team also includes Andrew Hart and Olivia Lundqvist. Music by Epidemic Sound. If you’d like to hear from another researcher who mined gold between established scientific disciplines, check out our earlier episode with 2019 physics laureate [Didier Queloz](https://www.nobelprize.org/prizes/physics/2019/queloz/facts/). You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening.  Svensson: Adam, I really wanted this to fit into the episode, but it was just a bit too far off to the side.  Smith: While it is a bit speculative, bit abstract, exploratory if you like, but I think you’re right. I think it’s worth including we’ve never done that before. We’ve never bolted on an extra piece. But I think in this case you’re right. Although it doesn’t fit neatly into the rest of the conversation, it does matter.  Svensson: Okay. Then give us some setup on what we’re about to hear.  Smith: On first listen it might sound like it’s just a technical back and forth about AI neural networks, the brain, but really he’s discussing the limits of artificial intelligence, what current models of artificial intelligence might deliver. The big question of how much we can do with artificial intelligence without understanding more about biology  Svensson: Why is this so important?  Smith: I suppose he’s challenging the kind of popular narrative that we just need bigger AI models to solve everything. He’s reminding us that complexity, the kind of messy complexity of biology, gives us something that machines perhaps can never give us. We have to incorporate some of that messiness into our models if we’re ever going to be able to get machines to think like we do if that’s what we want them to do.  Svensson: Alright. Any other key points to look at for?  Smith: Yes, it’s interesting to listen to how Hopfield talks about the fact that things change with scale. That just adding more neurons or more nodes or more complexity doesn’t necessarily get you to just more of the same but can change things that you get new physics arising from differently sized systems or you should expect perhaps new physics to arise in different systems. That’s one of his arguments about the brain, that you cannot possibly understand the brain by understanding small scale neural systems. Even if you understand all the biology because something new is going to arise when it gets much bigger and much more complicated. Making it bigger will perhaps confound your understanding and new physics will pop up and he has demonstrated that and believes that that is really what we should be on the lookout for.  Svensson: Okay. Here it is an epilogue of sorts to this episode with John Hopfield.  Smith: One of the things that your work has shown or rather help make more apparent is that it’s very hard to predict how a system will behave as it gets bigger, as it gets more complex.  Hopfield: Most problems in physics and math have bridges by saying that there’s some measure along the mathematical line and there’s a transition to. Where that transition is dependent on what problems you have, but that’s just a coefficient. If it’s just a coefficient in any particular problem, you can ultimately figure out what’s going on. That problem can’t have been non-regular because you can’t find that out. But if going on from there you go.  Smith: Then let me finish with a question that follows on from that perhaps, which is, do you think that the coefficients will be found for the brain? Do you think we’ll be able to sort out in the end how it works? Which was the broadly the question that brought you into the computing field?  Hopfield: I think we’re going to see ever bigger biology is very complex, it will reduce biology to these two variables and solve the rest of it in some average way. Then that doesn’t work to solve all the problems you’d like to solve. You’re doing perception, it works for two years with one frequency or what have you, and somebody comes along and that, oh, but I can make a six neuron system and they can and down can solve all the problems. Well, not all, there’s a certain fraction that you can’t get, but there’s always this fracture that you can’t get and progress will be seen and there’ll be a step. The AI types of these, we’ve learned all these you needed to tell us about biology. We really are tired of hearing biology lectures. They will take their things off and their toys and play with them and they’ll get the toys will polish them really much better than when they were handed the toys. But they don’t solve all the problems. There’s another problem. Then there’s a set of problems. Let’s see. I think there’s a series of revolutions which actually goes on. One of the first big resolutions was discoveries would’ve been that you could take a one neuron system feed back on itself and make a two state system. How many different neurotransmitters do you need to have to make such a system go? One neuron doesn’t have much computing power. Minsky proves that for the perceptron wrote a whole book really driving the mathematical theory of the perceptron and why the inadequate description of computation. If you have layers of cells, the perception is one layer, one cell. You say, I know how to beat that. The multiple neuron, multiple cell problem can’t be solved if you don’t do something additional about it. If you take one neuron, one cell solves the problems, two neurons, four cells maybe solves a fourth problem. But it’s finite. You keep pushing it out but that’s all you could do. Every time you push something out, you’ll say, well, let’s have more cells. Biology has more cells. But if you make cells that simple, you don’t have as many possibilities is if you make them more complicated like their biological ones. So when these trade offs cells will get more biological, there will be more of them. It’ll take more data to set all those parameters to the system. Then you could never actually do this beyond such a point if biology didn’t have this additional thing. There is a model in which step by step you include more and more biology and always stick to something which you know you could make biologically. I think that would be a very hard road push very far.  Smith: It needs somebody to come along with some theoretical revelation. Modeling will get you so far and as you say, you can improve the biological integrity of your model system. But it would be a very long road. There are perhaps that many people working on theory of neuroscience. I don’t know.  Hopfield: What you say is I think true in all corners except for the question of computation and quantum mechanics.  Smith: Yes. There it is.  Hopfield: There are an astonishingly large number of different quantum systems. People are playing with hoping that they have the holy grail for moving forward. I think that’s the unknown. |
| **Telephone**  **interview** | **0001 = JH** “I’m still somewhat in shock” “You have to build up from the bottom.” In this interview shortly after the announcement, John J. Hopfield talks about how he found out about the prize when he was going through his e-mails. ”It didn’t sink in until I got to the fourth e-mail!” From his cottage in the village of Selborne in England, John J. Hopfield reflects on how to tackle big questions, such as how the mind works. He discusses how to choose good problems, his fears for the future of machine learning and the need for interdisciplinary approaches. Interview transcript John J. Hopfield: Hello?  Adam Smith: Oh, hello. Is this John Hopfield?  JH: This is John Hopfield speaking. Yes.  AS: Oh, hello, my name is Adam Smith. I’m calling from the website of the Nobel Prize, and Mary very kindly set up this time to talk to you. Is that okay?  JH: Yes.  AS: Are you on speaker phone?  JH: How is this at your end?  AS: That’s absolutely perfect. That’s great. Thank you very much indeed. First of all, congratulations on the award of the Nobel Prize.  JH: Oh, thank you. Thank you.  AS: Mary tells me, you find yourself in Hampshire today.  JH: That’s right.  AS: It’s quite a good place to hear the news of the Nobel Prize because you’re slightly hidden.  JH: We’re off on our own, as it were, in a tiny town of less than a thousand people.  AS: It gives you some solitude on such a busy day.  JH: I don’t think there’s another physicist in the town of Selborne, so that things slowly leak out over the news. But there’s no marching in the street here.  AS: How did you actually learn the news that you’d been awarded the Nobel Prize?  JH: I had been out doing things with my wife, flu shot, a cup of coffee somewhere, came back here and there was this enormous list of emails on my computer, which I did not expect at all. And reading into the first two or three of them, you realized there must be a Nobel Prize there. And it was just astounding. My first reaction was they’ve announced the Nobel Prize because he described it without actually managing to connect me and the Nobel Prize in the same sentence. And so I thought it was sort of an email to me about the Nobel Prize to somebody. And it wasn’t until I got down to about the third one that I realised, no, it was to me, that the leading ones on top were just ticklers. I didn’t sink in until I got down to about the fourth email.  AS: I like the idea of those teasers. The prize is given for enabling machine learning and artificial neural networks. But I think I’m right in saying that you didn’t embark on this work in order to create the tools, but rather to understand how mind arises from the wiring in the brain.  JH: That’s right. I, my motivation was really coming from seeing that something does work, the brain, and understanding more about how the brain works would be necessary to understand thought consciousness or what have you. And that it somehow was related to collective phenomena in networks. And I slowly wove my way from an interest in how the brain functioned to a question of how could hardware or software, or whatever you want to call it, wetware, produce such a thing. And the centre of gravity of my knowledge and understanding moved slowly from much more physics oriented to the neurobiological one. And somewhere along the line, this connection between AI, networks, neural networks and physics developed.  AS: You’ve looked at a number of different questions in biology over the years using the lens of physics. I wondered what, what tempts you, what makes a good problem for you as a physicist?  JH: Yes. In a good physics problem, you have a system which is well defined and where you can understand something about how collectively it may work in a way which is more robust than the individual little bits and pieces. You don’t leap into a problem overall saying, I want to understand how mind works. You have to build up from the bottom. If you were doing weather, you would say, well, I want to understand what storms are without going back to interacting air nitrogen molecules.  You have to have the right level of question. And it isn’t obvious what the level of question should be. You get your hands rather dirty in trying to work on several things which don’t pan out.  AS: Yes. I suppose there’s a long history of physicists turning their attention to the brain, to consciousness. People like [Francis Crick](https://www.nobelprize.org/prizes/medicine/1962/crick/facts/) or [Don Glaser](https://www.nobelprize.org/prizes/physics/1960/glaser/facts/), and it is all about getting the level of the question right, isn’t it?  JH: I’d read some of the things that Don Glaser wrote, for example, and they’re imaginative physics, they’re not quite such good biology. There was a consensus that said you had to be able to reach out from physics and get to some of these things you’d like to, but then you have to know enough about the biology that the whole thing makes sense. And you really have to present things in such a way that a community develops. I didn’t realize that at the time, but certainly one of the important things of what I did had to do with enabling people who came from physics, or who came from biology, become a community, working on not just one little problem or piece, but somehow collectively working together toward trying to get an understanding.  AS: Yes. It catalysed the community and the Hopfield network was a huge advance for people that they could latch onto and develop. Let me ask you one other thing that your co-laureate, Geoffrey Hinton, is very vocal in speaking about his fears about machine learning and its potential. Do you share his worries?  JH: Yes, I share his worries. You always worry when things look very, very powerful and you don’t understand why they are, which is to say you don’t understand how to control them, or if control is an issue, or what their potential is. If you don’t really understand and can’t explain how they work without saying, if you go deeply enough in the mathematics they’ll work. That’s not a satisfactory answer. I would like to have more understanding of how the microscopic physics gives rise to the interesting properties of the larger system.  AS: Do you hope that this Nobel Prize will send some message? It’s the first prize in artificial intelligence, if you like.  JH: I think that the prize is recognizing, in part, the fact that understanding the deep problems of things like mind is not going to come forth in some simple way like Newtonian physics. It really requires much more understanding of the relationship between structure and properties, and structure dynamics and properties. And that’s a mixture of some corners of physics, some corners of chemistry, some corners of biology, coming together to understand and create an area of study.  AS: Thank you. Very nicely put. Let me just finish by commenting that I realize you are hearing this news in Selborne, which was the subject of Gilbert White’s The Natural History of Selborne.  JH: Oh, you’ve discovered Gilbert White! Good for you.  AS: But it’s nice for Selborne that it gets to have a Nobel Prize announced in its midst, given that it has such a deep, ancient association with natural science.  JH: Well, Gilbert White was an astute observer.  AS: Yes. It’s been an enormous pleasure speaking to you. Thank you very, very much. And let me again add our congratulations on today’s news.  JH: Thank you. I know it’s not simple to try to interview me when I’m still somewhat in shock.  AS: Very understandable. It’s been fascinating, and I look forward to a longer conversation when all the dust settles in the future. Thank you.  JH: Right, bye, bye.  AS: Bye, bye. |
| **Interview** |  |
| Q | How big a role did physics play in your childhood? |
|  |  |
| Q | Is constant screen access hurting today’s kids? |
|  |  |
| Q | How do you stay curious as an adult? |
|  |  |
| Q | What advice would you give to a young researcher? |
|  |  |
| Q | Why is cross-disciplinary research important in science? |
|  |  |
| Q | What are the greatest opportunities and risks presented by AI? |
|  |  |
| Q | Reflecting on your career, do you have any life advice? |
|  |  |
| Q | How did you convey your passion for learning to your three daughters? |
|  |  |
| Q | What parenting advice would you share with others? |
|  |  |
| Q |  |
|  |  |
| Q |  |
|  |  |
| Q |  |
|  |  |
| Q |  |
|  |  |
|  |  |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0002** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0002=Geoffrey Hinton** “When we remember, what we’re doing is just making up a story that sounds plausible to us. That’s what memories are.” Join podcast host Adam Smith as he speaks to physicist Geoffrey Hinton, often called the godfather of AI. They discuss Hinton’s childhood memories and how his family legacy of successful scientists put pressure on Hinton to follow in their footsteps. Throughout the conversation it is clear that Hinton has always had a fascination with understanding how the human brain works.  Together with Smith, Hinton discusses the development of AI, how humans can best work with it, as well as his fears of how the technology will continue to develop. Will our world be taken over by AI? Find out in this podcast conversation with the 2024 physics laureate.  This conversation was published on 15 May, 2025. Podcast host Adam Smith is joined by Karin Svensson.  Below you find a transcript of the podcast interview. The transcript was created using speech recognition software. While it has been reviewed by human transcribers, it may contain errors.  Geoffrey Hinton: If you look at my academic history at Cambridge, I switched subjects every year and it didn’t make any sense at all. When you look back on it, it was all very useful, but at the time it was just a crazy random walk.  Adam Smith: I can relate to that. Isn’t it marvelous how indecision in what might be called one’s career path can in the end with the passage of time look like requiring sensible background? And in Geoffrey Hinton’s case, boy did he flourish once, he did eventually find the question that he was really interested in addressing and it led him to become one of the founding fathers of artificial intelligence. So join me now as we listen to him. Recount how he explored some of those blind avenues and then eventually found the light at the end of the tunnel light, which for him has uh, over recent years turned a bit darker.  Karin Svensson: This is Nobel Prize Conversations and our guest is Geoffrey Hinton, recipient of the 2024 Nobel Prize in Physics. He was awarded for foundational discoveries and inventions that enable machine learning with artificial neural networks. He shared the prize with [John Hopfield](https://www.nobelprize.org/prizes/physics/2024/hopfield/facts/). Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces. Geoffrey Hinton is co-founder and chief scientific advisor of the Vector Institute in Toronto. He talks to Adam about how to best cool your coffee and why we say bite down instead of up. But ultimately they reveal how these mundane musings capture the core of the question, what makes us human. But first, Geoffrey Hinton talks about growing up with a father who favoured bugs over his children.  Smith: You come from a very scientific background, though a lot of scientists in your forebears and your father was a great entomologist. Did that influence you to become a scientist? Did you feel that it was in the blood?  Hinton: Yes. I felt a lot of pressure to become a scientist. I was expected to become a scientist, but I always enjoyed science. So that was okay. When I was little, I wanted to be an entomologist because my father was an entomologist.  Smith: He shared an inordinate fondness for beetles.  Hinton: Oh, you read that right?  Smith: Yes, it was beetles he specialised in.  Hinton: And lots of other insects too. But beetles were his favourite. Among beetles it was the family Elmidae that was his real favourite.  Smith: Did you have to compete with beetles for his favourite?  Hinton: My sister often said that he would’ve loved us more if we had six legs. He had an office at the university that was very high. The ceiling must have been about 16 feet high and the walls were all lined with shelves and he had a stepladder so he could get at them. The walls were covered with hundreds of boxes and each box would have papers reprints. This was before Xerox machines, right? It would have reprints of papers from journals and on the box there would be the name of the genus of insect that the reprint was about or maybe the family. There were hundreds of these boxes. When he died, we sold them to the University of Florida for 10,000 pounds. Among those boxes there was one box, it was a bit smaller and was next to the door on a lower shelf, on that box it said not insects. That was the rest of his life. That had things like letters from his children.  Smith: At least you were differentiated from the insects. You were something special.  Hinton: Yes. Slightly smaller.  Smith: But if you were expected to be a scientist, did that necessarily mean that you wanted to follow expectations or did you want to break away and be something different?  Hinton: Not until I was late in my teens did I want to break away and do something different. When I first went to Cambridge, I was doing science and it took a lot of time to do it properly. You went to lectures in the morning, did experiments in the afternoon, and then in the evening I would rewrite the notes I’ve made during the lectures to make them neat. So that I went over it once more and then it would be time to go to bed. It was like 12 hour days I was doing. After a month I just got totally fed up with it and I left Cambridge. That was the first time I made a decision on my own.  Smith: Golly, that must have gone down badly.  Hinton: It went down quite badly, yes. I went back a year later.  Smith: What did you do with the time that you had between first start and second start?  Hinton: I went to London and got several different menial jobs in order to pay the rent on an apartment. I read a lot of depressing novels by people like Dostoevsky.  Smith: Sounds like independence.  Hinton: Yes, I decided I wanted to be an architect. Fortunately before I actually went back the next fall, I spent the summer working in an architect’s office and discover what architects actually do, which isn’t as romantic as what you think they do as sketch out airy buildings. What they actually do is decide are you going to have cheap door handles or cheap flooring? Because there’s no way you’re going to meet the budget without doing one of those two. After a day of doing architecture, I went and talked to my tutor and switched back to doing science. But I switched back to doing physics, chemistry and physiology. The first time I’ve been there I did physics and chemistry in crystalline state, which was a new subject they were teaching because of the success of x-ray crystallography in getting structure of DNA. X-ray crystallography was a big thing then. When I went back I didn’t do that. I instead did physiology and that was the first time I’d done any biology and I found it fascinating.  Smith: Had you found your path once you made the change to physiology?  Hinton: No. At the end of the physiology course there was a section which I was really looking forward to about the central nervous system and I thought they would tell us how it worked. I was very interested in how it worked and instead they told us how the axons of neurons conduct action potentials, how a wave of depolarisation goes down the axon. But that didn’t exactly tell us how it worked and I thought they were gonna tell us how it worked. So I was got very fed up and I switched to doing philosophy.  Smith: Which is definitely studying how it works.  Hinton: I thought I’d learn more about the mind. I didn’t. I did a year of philosophy and basically developed antibodies to philosophy.  Smith: That seems to be a bit of a theme here.  Hinton: If you look at my academic history at Cambridge, I switched subjects every year and it didn’t make any sense at all. When you look back on it, it was all very useful, but at the time it was just a crazy random walk.  Smith: Often in life that’s true. You can make sense of it in reverse, but yes. Well presumably you did quite well in all these things so they hang on to you and said, okay, fine.  Hinton: No. In my first year I did well in physics but I knew I couldn’t carry on in physics because my math wasn’t good enough. Advanced mathematics I found very difficult. I did quite well in physiology and okay in chemistry. Then when I did philosophy I did just okay and then I switched to psychology and I didn’t really like that at all.  Smith: Again, were you looking for a way to get into the mind?  Hinton: Yes. I thought psychologists would tell us how people worked and instead it was rats in mazes. There was some stuff that retrospectively is interesting, signal detection theory, how distinguishing noise and very faint signals and there’s some interesting mathematics there. But it wasn’t exactly what I wanted to know. I wanted to know how people worked.  Svensson: So Adam, did Geoffrey Hinton find out how people work?  Smith: Don’t think anybody’s found out how people work yet. I think the brain is just too complicated for us just at the moment.  Svensson: He talks about two things in the conversation that I need to understand better and that’s neural networks and large language models. Can you enlighten me a little bit?  Smith: Well, yes, I’ll give it a go. The neural network is a step towards understanding how people work in that it’s arranging processes in a way that seeks to mimic what happens in the brain. In the brain you have neurons arranged broadly in layers and hugely interconnected and in a neural network you have processes arranged in layers and there’s an input layer where information comes in and there’s an output layer where information goes out. Then in between there are these hidden layers. In the brain our understanding is that neurons reinforce their connections with each other when they send electrical signals to each other. So standardly people say neurons that fire together wire together. The same is happening in a neural network where nodes with inputs that are leading to the right answers further down the stream are reinforced and you program a neural network with a set of rules and then it turns out that they are so-called adaptive and that they can develop new rules for themselves, allowing them to get close to the right answer.  Svensson: Building a synthetic brain then basically.  Smith: Yes, exactly. That’s what he was seeking to do. Very few people thought he was gonna succeed, but he did. That led him to become, as everyone likes to say, the godfather of AI. A large language model broadly describes the way that these infrastructures are used to input huge, complex data sets. Then the model is able to recognise and interpret that dataset and give you an output which makes sense as the name large language model implies. That refers initially to producing text as we all know from something like chatGPT. But it can also work with visual data or any sorts of complex data. In the case of one of the Nobel Prizes awarded last October, of course that data was protein folding data which Alphafold 2 was able to interpret.  Svensson: What specifically did Geoffrey Hinton get his Nobel Prize for?  Smith: For the development of that initial neural network which had introduced concepts of statistical mechanics which allowed his neural network to recognize characteristic elements in sets of data.  Svensson: Apart from sort of creating a digital brain, Geoffrey Hinton himself also seems to have a very interesting mind.  Smith: Doesn’t switch off much. Yes, he seems to be constantly on the lookout. Before we started recording, somehow we started talking about religion and I said let’s not go there. And he said, why not? So we went there, we chatted about it a little before the recording started and I think it was just indicative of the fact that not much slips by.  Svensson: What was his take on religion then?  Smith: Yes, he is not keen on it. He thinks it’s all fake news and that those who believe are deluded. But then again, there are others who very much believe in an evidence-based view of the world who have space for religion as well.  Svensson: He doesn’t seem to mind to disagree with people.  Smith: He certainly doesn’t. I think again, it’s the characteristic of so many laureates. Not that they necessarily want to hold contrary views, but that they are not phased by having people criticise their view of things. This conversation about religion is well worth having, but I don’t think it’s a conversation that can be had by scientists alone.  Svensson: Indeed.  Smith: Anyway. That mind is constantly operating even in the dentist chair it turns out – let’s listen.  Hinton: At least in Canada and Britain, I don’t know about elsewhere in the world, when you go to the dentist and they want to see if your lower teeth fit your upper teeth properly, they say bite down. You don’t actually bite down, you bite up. So why do they say bite down and they all say it. I’ve asked dentists, why do you say bite down instead of bite up? They don’t have an answer. They spend their whole life saying bite down. It’s never occurred to them that you actually bite up. Why do you say bite down?  Smith: And what do they say?  Hinton: Do you want to know why they say it?  Smith: Yes please.  Hinton: Okay. I was once eating some bony fish. This really is an explanation of why they say bite down. I was once eating some bony fish in a cafeteria with a friend and we were trying to play blindfold chess. Now we weren’t that good at chess and we kept sort of moving bishops through pawns and things. But we were trying to play blindfold chess and I realised you can’t play blindfold chess while eating bony fish. The reason is the sort of spatial processes you use for dealing with where things are in space and how they’re relating to each other. It’s the same spatial processor you use both for playing the blindfold chess and for finding the bones in the fish. It’s not like you have a separate piece of apparatus for dealing with what’s going on inside your mouth and what’s going on in this chess game. It’s the same spatial processor you are using. So you get a lot of interference. You can’t do both at once. It’s like trying to hold two conversations at once. The question is, when you use that spatial process for dealing with things inside the mouth, how does that relate to how you use it for dealing with things outside the mouth? If for example, I’m reaching for objects, there’s a kind of me and I’m doing the reaching for the objects. If I’m thinking about what’s going on inside my mouth, what’s me and where’s the sort of center of my frame of reference? So inside your mouth, your tongue is you, the tongue’s the bit you can move. You can move the jaw too, but the tongue is you and you sort of feel around with the tip of your tongue. If you’re looking for bones in the fish, you sort of feel for them with the tip of your tongue. So the tongue is you. Now the tongue is attached to the lower jaw. So if you ask what moves relative to the tongue, well what moves relative to the tongue is your upper teeth. When I contract the jaw muscle, so my jaw closes, the tongue doesn’t move relative to the lower teeth. There’s no danger that you bite your tongue with your lower teeth because they’re not moving relative to the tongue. The thing that bites your tongue is your upper teeth. Of course what you need to worry about inside your mouth is biting your tongue. That’s the most important thing not to do. You are your tongue and you don’t want to get bitten and these upper teeth are coming down on your tongue. So of course you think of it in terms of biting down.  Smith: It’s all about frames of reference.  Hinton: It’s all about frames of reference, which I’m very interested in and that’s my best explanation of why dentists say bite down. But the point of this whole story is there is an explanation of why they say it that I think is pretty good. But nobody ever asks a question. Nobody ever says ‘Wait a minute, the teeth come up so why do they say bite down?’ There’s huge numbers of questions that nobody ever asks. My son when he was very little, asked a question that very few kids ask, he said, ‘Daddy, why do bridges stay up?’ The point is there’s nothing underneath the bridge. He was just beginning to understand that it is odd that bridges stay up.  Smith: It’s a lovely good question. All this asking of questions and finding solutions that can slow you up. I suppose most people are rushing through things and stopping to ask is time consuming? As well as being intellectually taxing.  Hinton: I’ve always envied people who can just read a lot of stuff. If I start reading a scientific paper, I keep getting sidetracked. It takes me a whole day to get through a scientific paper because I’ll read a little bit, then think, wait a minute or it’ll remind me of something else. So I get sidetracked a lot by asking questions and I actually like making up questions just for the fun of trying to figure out the answers. So for example, if you take a coffee cup and you put the coffee in, then you have to go and do something. You’re coming back in five minutes and you want the coffee to be as cool as possible in five minutes. Should you put the milk in when you’ve just put the coffee in or should you leave it and put the milk in when you come back? The answer is roughly speaking that you should leave it and put the milk in when you come back because the coffee will be hotter if you haven’t put the milk in. So it will cool faster. So you’ll be losing heat faster if the coffee doesn’t have the milk in, then you put the milk in when you come back and it cools it down some more. But it’ll end up coolest if you put the milk in later. But suppose that the coffee cup is conical shaped because it’s losing heat from the surface of the coffee. So if you’ve got the milk in, you get a bigger surface. Although the coffee overall, if the milk mixes with the coffee, the overall thing’s a bit cooler, it’s got a bigger surface. That must mean the some shape of coffee mug where it doesn’t matter whether you put the milk in first or later it’ll call the same amount. It depends a bit on sort of exactly how much coffee you put in and how much milk you put in. But I think most people don’t get sidetracked by wondering things like if we had the right shape coffee mug, we wouldn’t have to worry about whether you put in the milk first or second.  Smith: Absolutely not. I think that sounds like a wonderful physics project for some undergraduate to work out what the shape ideal shape of cup is. But yes, it sounds like the sort of question which really normally elicits the answer, ‘Oh dad, honestly’.  Hinton: My daughter was used to me lecturing her on scientific theories and one morning when she was a teenager I came down to breakfast early, she used to come down early because she had to get to school. She said, ‘You are down early, dad’. I said ‘Yes, I think I figured out how the brain works!’ And my daughter said, ‘Oh no dad, not again’.  Smith: All this reminds me of a nice thing Garrison Keillor said about old people. He said the reason old people walk so slowly is that everything they see reminds them of something and so they have to stop and think about it.  Hinton: I think it’s because their bodies don’t work so well.  Smith: Oh how prosaic! And how factual and true. Actually that brings me on to your own upbringing. How did that experience translate into the way you brought up your own kids? I think we’ve already seen insight into that.  Hinton: Yes, my kids, my daughter in particular, doesn’t want to be a scientist. She never did want to be a scientist. She would occasionally get very cross with me for trying to explain things scientifically when she wasn’t interested.  Smith: I suppose I was getting to the expectation piece, given that there were obviously huge expectations placed on you.  Hinton: My children are both adopted and that changes the imposition of expectations somewhat. I think that made a difference.  Smith: You’ve had a very interesting path through academia and business. I wanted to ask what it was that really attracts you to an environment. Where do you feel happy working? What defines somewhere you’re happy?  Hinton: I guess I need two things. I need smart other people to talk to who know things I don’t know. One of the best collaborators I’ve ever had was Terry Sejnowski who knows a lot of physics and a lot of biology that I don’t know. He also reads a lot so he knows all the literature, which I don’t. That’s been a very good collaboration. The other thing I need in environment is smart graduate students. At universities, if you have a PhD student, you are stuck with them and they’re stuck with you for about five years. That’s long enough so that after they’ve got used to you and you’ve got used to them, you still have four years left. They can try doing things that fail and they can spend six months trying to do something that fails and that’s not the end of the world. That’s great training for them and it’s a great resource for the advisor to get them to try things and try them really hard. The one disadvantage I found at Google is that the junior research scientists at Google work with more senior scientists on projects. They’ll work on a project for a while because it sounds interesting. But when the going gets tough, if after a month it hasn’t produced anything, they’ll go off and work with some other scientist. They have too much freedom. Now these are typically people who’ve already done PhDs so they deserve some freedom. But that’s something you have at universities that you don’t have at the big companies, not in the same way. I think a lot of original research will continue to be done at universities. Because I think this kind of apprenticeship system where you have an advisor and a student and the students’ apprentice to the advisor and stays as an apprentice for several years, I think that works very well for exploring new ideas. It’s an advantage that the students don’t yet know that much. They don’t have all sorts of opinions of their own or if they do have an opinion of their own, it’s sort of fresh and they can see things from new angles. I think actually for fundamental original research, universities are better than the big companies. But for resources the big companies are much better.  Smith: In your particular field of deep learning and neural networks is there now such a need for resources that the balance of power, if you like, in research is shifting a bit towards the companies?  Hinton: Yes, it’s a real mess. For these large models, you need a lot of resources and that sort of puts universities as a big disadvantage. That’s one big disadvantage. The other big disadvantage, it may change in the near future I don’t know. The companies just pay a lot more. For a good researcher who just got a PhD in machine learning, a few years ago they could go to one of the big companies and get paid $300,000 a year. If they went to a university, they’d get paid $150,000 a year if it was a relatively rich department. That just gives the big companies a huge edge.  Hinton: If you work for a big tobacco company and you want to tell people that tobacco causes cancer, you may start telling them when you work for the company. But really what you should do is quit the company and tell people tobacco causes cancer. Google treated me very well and the people I dealt with at Google were very nice people. So I felt wrong about criticising Google and other companies for not paying enough attention to safety while working for them. They actually, when I said I was leaving, they said ‘Well, you could stay here and say whatever you like and work on AI safety’. It just didn’t feel right. You’re just much cleaner if you don’t work for the company. The people who are uninhibited in saying what they think about AI safety are generally people who don’t work for a company or people unlike some of the people at OpenAI who are about to leave the company.  Smith: Your contention is that we are closer to a dangerous intelligent form of deep learning than people think.  Hinton: Yes. There’s two kinds of dangers of AI. There’s the kind of AI we have now being misused by bad people. Being misused for example to target voters to get them to stay home rather than voting for Kamala Harris. I’m sure quite a bit of that went on. If you know a lot about a person, you can know what presses their buttons and send them things that’ll manipulate their behaviour. Those hundreds of millions of dollars that Musk put into sporting trunk probably went into things like that. I wouldn’t be surprised if they did. Then there’s obviously cyber criminals using it for cyber attacks, which is very scary because these things are getting better and better at cyber attacks. There’s people using it to make nasty viruses, this lethal autonomous weapons which are gonna be very nasty and are coming very soon. There’s all those things that just depend on bad actors and many people are aware of those things. Then there’s things that are quite different that depend on AI itself trying to take over. A lot of people, particularly people concerned with the other things like discrimination and bias in AI say ‘This AI taking over, it’s just science fiction, it’s nonsense’. That tends to go together with the belief that AI doesn’t really understand anything. What I now think of as old fashioned linguists who are followers of Chomsky think this stuff doesn’t understand anything. It’s just a statistical trick using correlations. That belief gets a bit thin when you have arguments with it and it starts beating you at the arguments. I have a little game I play, which I enjoy a lot, which is you take some of the statements about how these large language models don’t really understand what they’re saying and you give those statements to a large language model and ask you to explain what’s wrong with the reasoning of the people who made those statements. It gives very coherent explanations of what they’re getting wrong. The nice thing about this game is it requires no effort on your part. All you have to do is type in the statements from these people. You don’t even have to type them in, just grab them and put them into GPT. It’ll tell you what’s wrong with and they say ‘What’s wrong with this?’ It’ll tell you exactly what’s wrong with it. It’s quite satisfying to get the large language models explaining to the critics of large language models why they’re wrong. At that point it seems just crazy to say they don’t understand anything.  Smith: People point very much to the mistakes they make, the stupid mistakes that large language models make as an evidence that they’re not really understanding that it is just correlation.  Hinton: Yes. I have various things to say about that, two things in particular. If you take someone with low intellectual ability with an IQ of 80 or something like that, they will sometimes make mistakes. They’ll get some common sense things wrong. We don’t say that means they didn’t understand anything. What we say is that means they didn’t understand that. There’s complicated things they don’t understand. But we don’t say that means they’re not understanding at all, that there’s no understanding there just because they made a mistake. That would be kind of crazy. The second thing is people make mistakes like this. Even people of average intelligence make mistakes like this all the time. Now people are generally better, at least until recently, are recognising when they got things wrong and sort of filtering it and doing a bit of reasoning that can’t be right. But they make mistakes all the time. It’s called hallucinations, but it ought to be called confabulation where it’s just a language model. There’s no vision involved. It ought to be called confabulation. Psychologists have studied that since the 1930s and shown that just people confabulate all the time. It’s hard to prove that. When we were remembering things that happened a long time ago, we make up a story that sounds plausible to us. We’ll have some relation to the truth but we’ll have lots of the details wrong. It’s normally hard to prove that because you need to know the precise details of something that happened a long time ago. But there are cases where you do know what happened. The best case I know of is there’s a nice paper by Ulrich Neisser on ‘John Dean’s memory’. John Dean was Nixon’s lawyer and he testified under oath about conversations that had happened in the Oval Office. At the time that he testified, he was unaware that there were tape recordings of those conversations. Basically what he produced in testimony was plausible kinds of meetings that might have happened given what was going on. Those meetings never actually happened. It wasn’t those precise people in the meeting. It wasn’t that person said something, it was somebody else said the same thing. It wasn’t quite that thing anyway. But it’s clear he was trying to tell the truth. But when we remember stuff, what we’re doing is just making up a story that sounds plausible to us. That’s what memories are. If the events happened very recently, what sounds plausible to us is what actually happened. As the events happened longer ago, what sounds plausible to us is something like what happened but influenced by things we’ve learned in the meantime. Human memory is full of confabulations and the fact that these large language models just make stuff up shows they’re more like us, it doesn’t show they’re less like us.  Smith: Would you say that those models are the closest we’ve got to exposing how the brain is actually processing information?  Hinton: Yes. They’re by far the closest we have to explaining what’s going on in the brain when we’re understanding language. I think in broad terms, we understand language in the same way as these large models understand language. They’re like us in that respect. The people who say they’re completely unlike us don’t have a workable theory of how we understand language. The best theory we’ve got is these large language models.  Smith: If there is a very serious danger that they are going to become far more intelligent than us quite soon, what do you think should be happening now in order to protect against potentially bad effects of that?  Hinton: Nearly all the leading researchers I know believe they will get more intelligent than us. Maybe not in everything all at once. They’ll get better at different things at different times. They’re already much better, for example, at playing go or playing chess or figuring out how proteins will fold. They’ll get better at different things at different times. If I ask you to write a sonnet where half the words begin with B, you could probably do that, but it’ll take you a whole day. If you ask GPT-4 to do that, it’ll just spit out the sonnet. They’re much better at things like that. They’re already not very good experts at everything. They’re getting better fairly rapidly. Most researchers think they will get better than us, smarter than us. It’s just a question of when. Some people seem to think it’ll be in the next few years. I think that’s optimistic, if you think it’s a good thing. I think it may take up to 20 years, I’d say probably within the next 20 years they get smarter than us. They’ll be agents too. They’re already making them into agents. They can do things, they can talk to other ones and they can cooperate to achieve things.  Smith: They can create their own goals.  Hinton: So they can create their own sub goals at least. We may put in their top level goals, but they’ll generate sub goals in order to achieve those. They’ll probably generate the sub goal of getting more control to make it easier to get things done. The real question is not will they get more intelligent than us, but if they’re more intelligent than us, will we have a way of making sure they don’t want to take over? We just don’t know. We don’t know whether that’s possible. But given that you’re about to make things more intelligent than you, it would seem wise to just put a lot of resources into figuring out if you’re going to be able to keep control. We are not going to stop AI. I think saying we should stop now that might be the rational policy, but that’s not going to happen. There’s too many profits to be made. Governments want it too much for weapons and so on. They will say for defending themselves against cyber attacks, but at least half of them must be using it for inventing the cyber attacks.  Smith: It was a race of escalation.  Hinton: So they’ll want it for that. It’s not going to be stopped. We should be putting a lot of resources into figuring out; can we generate these hyper intelligent agents in a way that allows us to stay in control? Now some people believe we can. Yann le Cun (who’s a friend of mine and was my postdoc) believes we can. I think it’s improbable, but we should put a lot of effort into seeing whether it’s possible.  Smith: What form does that effort take? Is it regulation? It’s research really, isn’t it?  Hinton: It’s research but only the big companies have the resources to do this research because it’s research on the large cutting edge models. My belief is that government’s the only people who are powerful enough to deal with these large companies and even they may not be. My belief is the government ought to mandate that they spend a certain fraction of their computing resources on safety research. Now it would be great if that happened. The Biden administration was moving very timidly towards a little bit of regulation. In California they were a bit more ambitious and they said that you’re going to require the large companies before they release these things to do a lot of safety tests and tell you the results. That got vetoed by the governor. It got passed by the assemblies, but vetoed by the governor. That was the first bill with real teeth. In Europe, the Europeans would like to have some regulation of AI, although they explicitly say we are not going to regulate military uses of AI because so many European companies want to use it for weapons. But recently the UK and the US have said they’re not going to sign on to the Europeans declaration about AI safety. Basically they explicitly say we’d rather have the profits than the safety. They say that by saying too much regulation will interfere with innovation. But you can rephrase that as when it’s profits versus safety, profits win. In Britain, for example, the prime ministers are being advised by someone who holds lots of shares in AI startups.  Smith: It does seem a case of the industry policing itself partly because as you say, nobody has else has access to the resources. But also because it’s such a fast moving area that people are finding it very hard to keep up with what’s happening.  Hinton: I find it very hard to keep up with what’s happening. There’s new models coming out every day and there’s new techniques being invented every day because there’s a very large number of very smart people working on it now. I find that scary. So it will be hard to regulate. But if you say something like spend a third of your computing resources on AI safety research, that’s sort of more generic and easier to do.  Smith: I can see why it’s to be wished for. In getting people to focus on that question and the provision for potentially controlling what could become dangerously intelligent in the future, is there a danger that you neglect the current problems that AI presents? Alongside all the benefits in diagnostics and all sorts of things that it can do for education etc potentially, there is the threat for automation and large scale unemployment as well as all the bad actors that you’ve already mentioned and that also needs to have attention paid to it. We need to, as a society, think about what we want now from AI, not just the danger of the future. There’s so much to think about. There’s so much to worry about and potentially control and direct. It’s too big.  Hinton: I completely agree. There are these many different dangers. There’s many short-term dangers and it’s not that we shouldn’t think about those. I mainly speak about the long-term existential threat of these things getting more intelligent than us and taking over because many people say that’s just science fiction. I feel I know enough about how these things work and how we work to say it’s not science fiction. But that doesn’t mean I’m not also very concerned about all the short-term things. It’s just my particular expertise means I’m best placed to talk about these longer-term existential threats. But I do try and emphasise when I talk about those, there are also many short-term threats like the social disruption if these things replace all mundane intellectual labour with AI.  Smith: It all comes back in a way to the question that you’ve been asking all these years about how we work and what it is to be human.  Hinton: That’s becoming very central what it is to be human. Because the debate about whether these things will want to take over is all about do they have desires and intentions. Many people think, for example, there’s something that’ll protect us, which is they’re not conscious and we are conscious. We’ve got something special that they ain’t got and they will never have. I think that’s just gibberish. I’m a materialist. Consciousness emerges in sufficiently complicated systems. Perhaps systems complicated enough to be able to model themselves. There’s no reason why these things won’t be conscious.  Smith: So we’re going to have to learn how to live with them as well.  Hinton: Hopefully we can learn to live with them. That’s the good scenario.  Smith: How do you think we should think of them in the future as friends or aliens?  Hinton: Okay, so Yann who thinks we’re going to be safe, thinks we should think of them as servants, good old fashioned servants who do what you tell them to. If they don’t, you fire them. I’m just worried by the fact that there’s very few cases of more intelligent things being controlled by less intelligent things. Once they’re a lot smarter than us, I don’t think they’ll put up with that. That’s what worries me at least. Now there’s one line of argument that’s more promising, which is a lot of the nasty characteristics of people have come from evolution. We evolved with small warring bands of chimpanzees or our common ancestor with chimpanzees. That led to this intense loyalty to your own group and intense competition with other groups being willing to kill members of other groups. That’s sort of shows up in our politics quite a lot right now. These things didn’t evolve so maybe we can avoid a lot of that nastiness in things that didn’t evolve.  Smith: It’s a nice thought that we could learn how to behave from them.  Hinton: Yes. In fact, AI mediators are now quite good at getting people with opposing views to come to see each other’s view. There’s a lot of good can be done with AI and if we can keep it safe, it’s gonna be a wonderful thing.  Smith: Hopeful note to end on. It’s been an absolute pleasure speaking. Thank you very much indeed.  Hinton: Bye for now.  Svensson: You just heard Nobel Prize Conversations. If you’d like to learn more about Geoffrey Hinton, you can go to nobelprize.org where you’ll find a wealth of information about the prizes and the people behind the discoveries. Nobel Prize Conversations is a podcast series with Adam Smith, a co-production of Filt and Nobel Prize Outreach. The producer for this episode was me, Karin Svensson. The editorial team also includes Andrew Hart and Olivia Lundqvist. Music by Epidemic Sound. If you are into big ideas, lateral thinking and in-depth explanations, why not check out our episode with 2020 physics laureate[Roger Penrose](https://www.nobelprize.org/prizes/physics/2020/penrose/facts/). You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| **Telephone**  **interview** | **0002 = GH**  Geoffrey Hinton: Hello?  Adam Smith: Hello, am I speaking with Geoffrey Hinton?  GH: You are.  AS: This is Adam Smith calling from the website of the Nobel Prize.  GH: Okay. I know who you are, because a long time ago I noticed that they have somebody who calls up to get people’s reactions.  AS: Exactly. So could we talk for just a few minutes?  GH: Yes.  AS: Thank you very much indeed. First of all, of course. Many congratulations.  GH: Thank you.  AS: And, where are you? Where did the news reach you?  GH: I’m in a cheap hotel in California, without an internet connection, and with a not very good phone line, phone connection. I was planning to get an MRI scan today, but I guess I’ll have to cancel that. I had no idea I’d even been nominated for the Nobel Prize in Physics. I was extremely surprised.  AS: It sounds like, quite a sensible place to receive the news in a way. Because you’re a little bit isolated. You can collect your thoughts before, before the deluge of the day.  GH: Yes. On the other hand, it’s two o’clock in the morning.  AS: Oh, goodness. Yes. I’m sorry. Yes. Oh dear. I don’t know if you’ve got …  GH: I think it’s three o’clock by now.  AS: … I don’t know if you’ve got the sang froid to go back to bed or whether you just have to accept that the day is going to be a long one.  GH: Yeah, I don’t think I’ve got that much sang froid.  AS: Well, an utter surprise. What were your first thoughts?  GH: My very first thought was how could I be sure it wasn’t a spoof call.  AS: And? How could you?  GH: It was coming from Sweden and the person had a strong Swedish accent and there were several of them.  AS: Yes. So it would have to be a posse of impersonators, which is unlikely, I suppose.  GH: Yes.  AS: How would you describe yourself? Would you say you were a computer scientist or would you say you were a physicist trying to understand biology when you were doing this work?  GH: I would say I am someone who doesn’t really know what field he’s in but would like to understand how the brain works. And in my attempts to understand how the brain works, I’ve helped to create a technology that works surprisingly well.  AS: It’s notable, I suppose that you’ve very publicly expressed fears about what the technology can bring. What do you think needs to be done in order to allay the fears that you and others are expressing?  GH: I think it’s rather different from climate change. With climate change, everybody knows what needs to be done. We need to stop burning carbon. It’s just a question of the political will to do that. And large companies making big profits not being willing to do that. But it’s clear what you need to do. Here we’re dealing with something where we have much less idea of what’s going to happen and what to do about it. I wish I had a sort of simple recipe that if you do this, everything’s going to be okay. But I don’t. In particular with respect to the existential threat of these things getting out of control and taking over, I think we’re a kind of bifurcation point in history where in the next few years we need to figure out if there’s a way to deal with that threat. I think it’s very important right now for people to be working on the issue of how will we keep control? We need to put a lot of research effort into it. I think one thing governments can do is force the big companies to spend a lot more of their resources on safety research. So that, for example, companies like OpenAI can’t just put safety research on the back burner.  AS: Is there a parallel with the biotechnology revolution when, the bio technologies themselves got together in those Asilomar conferences and sat down and said, you know, there is potential danger here and we need to be on it ourselves?  GH: Yes. I think there are similarities with that, and I think what they did was very good. Unfortunately there’s many more practical applications of AI than for the things like cloning that the biologists were trying to keep under control. And so, I think it’s going to be a lot harder. But I think the biologists, what they did is, a good model to look at. It’s impressive that they managed to achieve agreement, and the scientists did it.  AS: So, for instance with the large language models, the thing that I suppose contributes to your fear is you feel that these models are much closer to understanding than a lot of people say. When it comes to the impact of the Nobel Prize in this area, do you think it will make a difference?  GH: Yes, I think it will make a difference. Hopefully it’ll make me more credible when I say these things really do understand what they’re saying.  AS: Do you worry that people don’t take you seriously?  GH: So there is a whole school of linguistics that comes from Chomsky that thinks that it’s complete nonsense to say these things understand, that they don’t process language at all in the same way as we do. I think that school is wrong. I think it’s clear now that neural nets are much better at processing language than anything ever produced by the Chomsky School of Linguistics. But there’s still a lot of debate about that, particularly among linguists.  AS: I just wanted to come back though, to the circumstances of you receiving this news, in your hotel room, in the middle of the night. In some ways, a rather lonely place to hear the news. No one to turn to, to sort of hug and celebrate.  GH: Well, I’m here with my partner. I’m here with my partner and she’s quite excited.  AS: Okay. Yes, indeed. But for now, many, many congratulations.  GH: Thank you. Okay.  AS: Bye.  GH: Bye. |
| **Interview** |  |
| Q | How did you first learn about your Nobel Prize? |
|  |  |
| Q | What was it like growing up in a family of famous researchers? |
|  |  |
| Q | What made you interested in studying AI? |
|  |  |
| Q | What are the greatest risks posed by AI? |
|  |  |
| Q | How much time do we have before AI outsmarts us? |
|  |  |
| Q | What personal qualities are important in succeeding as a scientist? |
|  |  |
| Q | What’s your advice to young researchers? |
|  |  |
| Q | What responsibilities do scientists have in society? |
|  |  |
| Q | Looking back at your career, what could you have done differently? |
|  |  |
| Q | What are your plans for the prize money? |
|  |  |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0003** |
| **Biographical** | I was born in Tunisia in July 1941, during World War II. The war did not leave me many memories, as I was a bit young even at the 1945 armistice. In Tunis I was educated at the College Alaoui and Lycée Carnot until 1957 when I was transferred to the Prytanée Militaire à La Flèche (France) where I spent three years completing a Baccalaureat (high-school final exam), and Mathématiques Supérieures. This prestigious school, originally founded by Henri IV and turned into a military school by Napoleon, has an excellent alumni association, the existence of which I had forgotten. But apparently it had not forgotten me, as its president reached out for me after the Nobel to congratulate me, sixty-three years after my three years there. At the end of the Math Sup class I left the military life and entered the University in Marseille, France, where my parents had finally settled. I had the idea of doing Math studies, but I was discouraged from doing so and eventually took up a Physics major, more by obligation than a particular taste for the subject. The physics curriculum included mathematics for physicists, classical mechanics, thermodynamics, electricity, optics, electronics, and chemistry and took me three years to complete. Then I continued to a Master in Optics and eventually to a thesis (Doctorat d’Université) in Optics with Professor Henri Chantrel as my advisor. He was a spectroscopist and I worked under him to produce thin films to make high reflectivity mirrors in the UV, a technical subject, highly à la mode at Marseille in those days. It happened at that time a new department was being created at Saclay National Lab (CEA) to start studies in “Multiphoton Ionization”, a subject which was started a couple of years before in Moscow. The new department had generous funding and needed personnel. Yves Gontier, then a beginner in theory and recent hire in the department, was sent scouting around for finishing students. I visited Saclay and was offered the job a few months before defending my thesis. This is, for today’s students, the sign of a vanished time. The Physics curriculum did not have much Quantum Mechanics, except for a course on Mecanique Ondulatoire for the master’s degree. When I asked my advisor what he thought about multiphoton ionization, as a subject, he said “Ah yes, the nonlinear Maxwell’s equations! Don’t do it!”. Nevertheless, I took the job, (I had married the year before, and I guess I needed money) and moved from Marseille to Paris, for my first contact with research in Physics. Our group leader, François Bonnal, left the following year to join a group on nuclear fusion, and Gerard Mainfray was to be group leader for several decades. I am not sure how I got interested in Physics. It was not my natural choice, but circumstances that brought me there. But research was the thing that kept me in. I guess it could have been math or chemistry as well. I was soon convinced that experiment is the key in Physics, and even though it was slow, required a lot of money, had all kind of pitfalls, it was the way to go. The CEA-Saclay lab offered optimal conditions for experimental investigations and a career without worries. Over the years, it happened several times that atomic/molecular interaction with intense lasers came to what we felt was the end of the subject. Time and time again, though, new developments appeared – either technological or theoretical − which opened new roads. ATI, harmonic generation and sub-cycle physics have been stages on a road to the attoseconds.  “Multiphoton ionization” or MPI, required power lasers. I had no experience with lasers, naturally (except perhaps a He-Ne laser with mW power seen in a demo in the electronics course), and everything was new. We started with a Q-Switch Ruby laser and rapidly switched to a Nd-Glass equipment. This was composed of an oscillator triggered by a rotating prism (firing every three minutes, preceded by the siren sound of the prism until the bang of the shot) and three amplifiers, I seem to remember, with rods of increasing diameters to reduce the risk of glass breakdown (it did break down anyway, which provided us with a wonderful collection of used rods). The flash lamps which pumped the laser were augmented by giant capacitor banks which literally filled the room. The energy par pulse was around 50 Joules, and the pulses were a few tens of nanoseconds (peak power 1 GW). For focusing, we used special aspherical lenses with a big hole at the center to avoid breakdown from the reflection on the output face. The peak intensity of the order of 1012 w.cm–2 would appear rather weak in considering the volume and the noise of the installation. The connection with the theory was difficult, especially for me with my lack of preparation in Quantum Mechanics. The only parameter we could measure reliably was the slope of the ionization signal vs laser intensity in log-log coordinates. This slope was supposed to measure the number of photons needed to ionize the atom (from basic perturbation theory) but it was systematically lower by several units (Fig. 1), a mystery which remained for a number of years. Perturbation Theory was next to impossible except for atomic hydrogen. The Saclay theoretical team (Michel Trahin and Yves Gontier) were for that reason limited to hydrogen, but still thought in perturbation terms. Leonid Keldysh’s theory of ionization was known (tunneling), but it took 20 years, until an experiment by Chin’s group at Laval University (Quebec) with a CO2 laser started convincing people that tunneling was real.  I remember that many people were hostile to the multiphoton idea, (perhaps the experiments were not good enough due to the laser characteristics, fluctuations, perhaps for other reasons). It took quite some time for this hostility to recede. Few groups in the world had the ability to carry out such studies because of the price of the laser and the cost of maintenance. For a few years, the Moscow group was practically the only competitor, if we except a first and short-lived study at Ohio State by Damon and Tomlinson as early as 1963. Picosecond pulses and ATI  The heroic times of the Q-switch lasers terminated with the advent of mode-locking and, almost immediately, the Pockels-Cell technique. We now had a much higher peak power (and intensity) with less energy and less emissions of surfaces, and Gerard Mainfray was able to start systematic studies of MPI, which would occupy us for many years, with more confidence. The frequency of the laser shots was still low, but it was going to climb to 10Hz in the 1980s. It was in the late 1970s that I made my first long visit to the US (at the physics department at the University of Southern California, Los Angeles) where I worked for a year for Mark Levinson, a young professor after a PhD with Nicolaas Bloembergen (1981 Physics Nobel). See Fig. 4. Peter Lambropoulos was also a professor at USC at that time. Peter was also perturbation theory oriented, but with a taste for core atoms more complex than hydrogen. We had a collaboration which lasted for at least two decades. One of the highlights was a series of meetings in Crete in the 90s. Peter is the person I am indebted for knowing Lou DiMauro, with whom I had a long collaboration from the beginning of the 90s, and to whom I owe to be at Ohio-State. I guess I am jumping ahead of myself.  I would like to end the 70s part of my life by recalling the course led by Claude Cohen-Tannoudji at the Collège de France that several of us from Saclay followed with great interest for several years. A large part of what I know about photon/atom interaction, I owe to this course.  After my “postdoc” at USC in 1978–1979 we started looking at photoelectrons from MPI, and we were lucky enough to find the (fragile) first evidence of above-threshold ionization, or ATI (Fig. 5). I remember that after this first result it took us, with Guillaume Petite and Françoise Fabre, months to reproduce it. The 10 Hz rep rate laser helped a lot for the reproducibility. ATI is nothing but MPI, except that one or more photons are absorbed in the continuum. (Later it was shown by several groups that tens of photons can be absorbed, making the first result a bit skinny!). It is the ATI process which underlies the measurement of the harmonic phases, at the heart of the Attosecond Pulse Train. It was also in the 80s that Anne L’Huillier began the exploration of the harmonics which was the origin of the attosecond science almost forty years later. The 80s were the time when the Chirped Pulse Amplification (CPA) invention of Strickland and Mourou (2018 Physics Nobel) changed the appearance of laser labs around the world by drastically shrinking the size of the lasers and making the femtosecond the standard pulse duration for high intensity systems.  The 1990s were a great decade for me. I started collaborations with Lou DiMauro’s group at Brookhaven and with the Laboratoire d’Optique Appliquée in Palaiseau, near Saclay. The LOA, which had several femtosecond lasers, was a bit ahead of Saclay. That was the time when the Dutch Foundation for Fundamental Research on Matter (FOM) in Amsterdam also began to be interested in femtosecond technology and sent Harm Greet Muller to LOA to learn it. Another physicist from Amsterdam, Mihai Gavrilă, of Romanian origin and with connections to the University in Bucharest, initiated bringing PhD students from Romania to FOM. At the same time Saclay was becoming part of a university, and it was easier to get graduate students working toward PhDs. Both Harm and I had our students working together at LOA with femtosecond lasers while Lou DiMauro was visiting me once or twice a year and I was visiting him on a NATO grant. The great thing in laser/atom interaction was the “knee” in the double ionization signal (Fig. 6) and the “stabilization” concept: an idea of Mihai Gavrilă according to which the ionization rate of an atom does not increase indefinitely with intensity but saturates (not in the usual sense) or stabilizes. The experimental evidence was extremely difficult to get and, as far as I know, only the group of Harm Muller in Amsterdam managed to confirm the idea. Mihai Gavrilă played still another role, together with Professor Charles Joachain, from Brussels. They used funding from the European Union to create a network of European labs including Lund, Vienna etc., setting the stage for the attosecond breakthrough of the millennium. We did several experiments with Harm’s Amsterdam group. The one I recall with most pleasure, I guess, is the one on Auger decay. One general idea (promoted widely by Rick Freeman, from Bell Labs) was that plasma radiation would be intense enough to generate nonlinear effects. So many groups were trying to irradiate metal surfaces with intense lasers with the hope of producing enough X-rays for that purpose. It was necessary to change the position of the laser impact on the target at each laser shot to maintain the sharp focusing required for high intensity. This was not easy technically with Khz lasers. Harm Muller had another of his (usually) bright ideas: use as a metal plasma source a liquid whose surface would naturally recover after each laser shot. Gallium, liquid at low temperature, was to provide the plasma and the X-ray radiation. The experiment worked, although the liquid mirror had several drawbacks (it splashed around when the intensity was a bit too high). Because of the high energy of the photoelectrons, a special version of the magnetic bottle spectrometer had to be built by Juleon Schins, then a postdoc at Saclay. I was very pleased, when I visited the Laser Lab at Saclay in November 2023, to see the spectrometer still in use!  With the 2000s my last years at Saclay were coming fast. The last PhD student I supervised (Pierre-Mary Paul, now Vice President at Amplitude Laser Group) was working for his PhD. Elena Toma was working for hers, under Harm Muller’s supervision. The results were at first very disappointing. Instead of the regular oscillation we expected, the data looked completely random. It took us several months to realize that the laser intensity fluctuations were responsible for this unfortunate situation and to find the cure: normalization to the total counts. Finally, the oscillation was there, and we could measure the harmonic phases, which demonstrated the attosecond pulses. We knew about the competition from Anne L’Huillier in Lund and Ferenc Krausz in Vienna. I remember we had one of the EU network meetings in Paris and Pierre-Mary Paul was very proud to show the results. The *Science* paper was published quickly (June 2001). A few years later (2004) a journalist, working for a popular science journal, guessed that attoseconds would lead to a Nobel Prize and speculated on the winners. He had a very good judgement, considering he was almost 20 years ahead of his time.  At that time, the CEA had a mechanical rule for retirement, based on number of years worked and age. Everybody was subject to it. I recall that Claude Manus, the head of the department and Gerard Mainfray, for many decades the group leader, had to leave under that rule a few years before me. I was no exception and I had to go in 2002. Fortunately, I had friends around the world who helped me in those circumstances. See-Leang Chin, professor at Laval University (Quebec), gave me support for enduring the Quebec winter (September 2002 to April 2003). Then we moved to Amsterdam for six months with the support of the FOM. Harm Muller was already leaving the intense-laser topic and was interested in other things (protein folding), but he took time to educate me in numerical integration of the Schrödinger Equation and I later exported his program to Ohio State. Then I got the Humboldt grant to spend a year at the Max Born Institute in Berlin thanks to Wilhelm Becker, Wolfgang Sandner and Horst Rottke. I had a great time there and I am indebted to all. Howard Reiss, then retired from American University, was there too (Fig. 9) as a permanent guest. I had known him for decades but then we had lot of time together and I had many opportunities to learn about strong field theory with him. He did not like the idea of tunneling and did not spare his critics. I guess his opinion slowly percolated to me, but I must admit that the Ammosov, Delone, Krainov (ADK for short) formula is very attractive for its analytical simplicity. I had the opportunity to meet Leonid Keldysh in Sicily in the late 90s, I believe, but I did not seriously discuss the point. I think that part of his work was far from his thoughts. Finally in 2005, Lou DiMauro, who had moved there from Brookhaven as a Hagenlocker Professor of Physics, introduced me to the Department of Physics. I am very grateful to him and the College of Arts and Sciences for the help they have supplied in my life. The decision of the Nobel Committee for Physics to award the 2023 prize to the founding experiments in attosecond science (see Fig. 10) was of course a wonderful surprise. Time is an eternal subject of philosophical questions, but for us physicists, it’s a matter of technology and careful measurements. For the time being attosecond pulses have been mostly used to solve fundamental questions in atomic/molecular sub-cycle physics. Some new applications in medicine by the groups of Ferenc Krausz are naturally very exciting and promising. |
| **Autobiography** |  |
| **Podcast** | **0003=Agostini**  Pierre Agostini: When I had the first announcement, I felt bad. There must be something wrong but with the time I got used to that. Anyway, I don’t think I can do anything about it.  Adam Smith: There’s always something especially magical about listening to Nobel Prize laureates. Describe how truly surprised some of them are to receive the call from Stockholm. In this conversation with Pierre Agostini, I got a real sense of his playfulness of spirit as well as an introduction to some pretty deep physics. There’s a lovely moment, for instance, where he describes the panic that set in when he wondered whether he could actually reproduce his groundbreaking result. Then we get to talking about the fact that time is not perhaps infinitely divisible, which is a pretty mind bending concept. Like many of these conversations, it leaves me wondering whether there is any age limit to scientific creativity, which opens up the whole difficult question of how you distribute the jobs between the young and the older. That’s something we go on to discuss. I leave you in the hands of Pierre Agostini.  Clare Brilliant: This is Nobel Prize Conversations. Our guest is Pierre Agostini, the 2023 physics laureate. He was awarded the prize for experimental methods that generate attosecond pulses of light for the study of electron dynamics in matter. He shared the prize with [Anne L’Huillier](https://www.nobelprize.org/prizes/physics/2023/lhuillier/facts/) and [Ferenc Krausz](https://www.nobelprize.org/prizes/physics/2023/krausz/facts/). Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces. Pierre Agostini is emeritus professor at the Ohio State University in Columbus, Ohio. He was a researcher at CEA Saclay in Paris from 1968 to 2002. He speaks to Adam about contradicting [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/), how forced retirement can sideline scientists at the top of their games and the strange things that happen as you approach the very smallest units of time. But first green cards, transatlantic flights, and feeling at home in two places at once.  Smith: You live in two cultures. You live in Paris, but also travel to Ohio State University.  Agostini: Yes, I just spent half of a month half in Iowa and I’m just back now.  Smith: How do you find this co-existence of living in two countries at once?  Agostini: That’s a little bit difficult sometimes, especially because I am on the green card program. With the green card we are supposed to spend at least six months in the States and I don’t do that. Each time I go back I have to explain that to the officer, but otherwise it’s okay. I am very used to both places and I feel at home both in Ohio and in Paris.  Smith: It’s nice to have two homes from a scientific culture point of view. Do you find that there is a different approach at all to science in Paris and in Columbus, Ohio? Obviously science is universal, but the differences are interesting.  Agostini: Especially in this circumstance of the Nobel Prize. They had organised this month in Ohio state full of ceremonies. It was really very different from the atmosphere from Paris. The people at Saclay are okay, but they’re not doing the same thing as those guys.  Smith: Maybe the French are more used to having Nobel Prize laureates in optics.  Agostini: Perhaps, yes. That’s the case. In Ohio they told me that I am the first one. So they celebrated a lot.  Smith: I’d like to talk about your scientific upbringing, how you started? You were born in North Africa, but you went to a military school in France. When did you discover that you were scientifically minded?  Agostini: When I moved from Tunisia to military school in Marsailles, I had to choose the scientific option and there was no other solution that this school. I had to turn to science. Although the year before in Tunisia, I was rather classical. I was very much for classical studies like Latin, French and stuff like that. I had to do it and it was okay. I liked it, but my previous life was something else. I turned to science because I had to and it was really a discovery. I really liked it. I liked especially the math program and when I went to Marsailles to start my university classes. I wanted to do math, but I was discourage having people say, oh no, it’s too difficult. You will not make it in math. I took physics and probably physics was just like any other matter. It was only when I started doing research that I discovered the fun of physics.  Smith: It was a very happy circumstance that you were pushed in this direction. It wasn’t so much a choice.  Agostini: Yes, I agree.  Smith: But you never gave up on your interest in the humanities, did you? You read a lot and it’s always been a big part of life.  Agostini: Yes, I still do that when I can. My schedule is packed. I don’t have time to read a lot, but I try to.  Smith: That’s a bit sad that the Nobel Prize stops your reading.  Agostini: Yes, it does after all.  Smith: Can you describe what the excitement was of discovering the joy of physics through research when you were first exposed to it?  Agostini: First of all, there was some excitement because the subject I started working in multiphoton ionisation and our interaction strong laser fields with matter was nowhere. There was almost nothing on that in the end of the 60s and my PhD advisor, I told him, okay, I’m going to work on multiphoton ionisation. He said don’t do that. He did not believe in this field. The intensity was the parameter that doesn’t matter in those studies, that was the first thing. Then there was the excitement of discovering things that nobody else has seen before.  Smith: Why did you choose this field that you were advised not to go into?  Agostini: That’s an interesting question. There was someone from Saclay, actually my old friend Yves Contie, who was a theorist at Saclay at the time. He sort of scouted physicists in Marsailles where I had graduated two years before. He was trying to find people to work on the program. I had a job offer before finishing my PhD and I just took the offer.  Smith: Pragmatic, yes.  Agostini: Yes.  Smith: This work is of course all about the interaction, as you say, of light and matter. It is extraordinary to reflect on the fact that you discovered this phenomenon that was completely unknown above threshold ionisation.  Agostini: Yes.  Brilliant: I might need a bit of help here, Adam. You talk about multi photon ionisation and above threshold ionisation, and I just wondered if you could explain what those are.  Smith: Yes, I’ll try. The ionisation is referring to the idea of ejecting an electron from an atom, creating an ion. In order to do that, you have to give the electrons sufficient energy to escape from the atom. There’s a certain threshold of energy it needs. Above threshold ionisation just refers to the idea that you are chucking an electron out of an atom with more energy than it actually needs to make that journey from being part of the atom to being expelled. Then multi photon ionisation is one way that can happen, that an electron can absorb the energy not just of one photon, one particle of light, but several on its journey out of the atom. That way it receives more kicks of energy from these photons and is ejected with more than the threshold energy that it needed to get out.  Brilliant: What’s the consequence of it having more energy?  Smith: It has a different energy spectrum so that the ejected electron then occupies a different set of energy levels to what it would have if it had received less energy. That has all sorts of consequences that have opened up possibilities such as the creation of these very short pulses of laser light.  Brilliant: That’s very helpful, thanks. I do feel like I understand it a lot better now.  Smith: I think it’s very superficial understanding if you’re listening to me, I’m afraid.  Brilliant: I’m going to test you again now because he mentioned strong field physics and I wondered if you could explain what that is as well.  Smith: Yes, that’s just the term that physicists use to refer to this field of study of the interactions of laser light and matter. It’s a field that studies ultra-fast processes where light is interacting with matter.  Brilliant: I think this research extends the work that Einstein did on the photoelectric effect. How does it do this?  Smith: Einstein proposed an explanation for the observed photoelectric effect, which is that if you shine light at a sheet of metal in a particular way, you can get electrons ejected from it. He proposed that the energy that was necessary to make that happen was being transferred to the electrons through individual photons, which had quanta of energy. They were giving their packets of energy to the electrons. It was his explanation of the photoelectric effect, which was actually the thing that the Nobel Prize Committee identified when they awarded him the prize in 1921. That is the basis of what is going on here. But with multi photon ionisation, it’s not just one photon that is transferring the energy to electron is more than one. It’s absolutely an extension of Einstein. I asked Agostini about following Einstein’s footsteps, let’s listen to what he had to say.  Smith: There’s such a tradition here, there’s such a legacy. It must have been so thrilling to feel oneself part of that.  Agostini: Yes, we were contradicting Einstein a bit with multi photon ionisation. Because then it take several photons to ionise not just one. So the Einstein voice has to be modified a little bit. ATI was one step further. It was interesting because when we had the first result, I remember it was during the summer and then we wrote the paper and it was quickly published. It worked for a long time. We were really worried because we could not reproduce that result. It took really long time because we changed the data, we changed the electron spectrometer and so on. The experience was completely different. It took us some time to realise what we should do to see the ATI again. It was really the worst time in my life because the paper was published, we could not reproduce it.  Smith: It would’ve been extremely embarrassing to have contradicted Einstein and then been shown to be wrong.  Agostini: Yes, absolutely.  Smith: Gosh, I can imagine you remember the sense of relief when the second result came.  Agostini: Yes. We could sleep at night again.  Smith: Presumably the environment you found working at Saclay was a very productive and supportive one. I’m interested that eventually you had to leave Saclay. What prompted the move from Saclay?  Agostini: Oh, you mean in 2002! That was just the rule for retirement. If you were 60 and above, you had to retire. There was no choice. The head of the department had to retire this way. The head of my group had to retire this way. There was no choice. The rule doesn’t exist anymore and people can work until 70 or something. But back then, it was the rule and I couldn’t avoid it.  Smith: It seems a terrific waste of talent to let people go so young. Of course, as you say, it’s changed. What do you think about that? What do you think about the balance between people who have long track records and the new people coming in? How do you get that right?  Agostini: I’m not sure what’s the best solution, but certainly being sort of retired at 60 or 61 for me was probably the worst thing possible. I suppose there is always more ways to hire young people without retiring old people.  Smith: Sure. In your case, you’d only quite recently done the work, which is cited by the Nobel Prize Committee in their award producing at a second pulse trains and defining them.  Agostini: We did this kind of experiments. There’s always a little bit of luck I think, involved because you never know what will happen.  Smith: Were you very surprised that your experiments yielded that pulse train?  Agostini: We’re happy. The surprise was first because we didn’t find anything at all in the results. After we analysed, we divided by the total crowns and then the oscillation, almost as a miracle, appeared at the time. We were happy, we were expecting that. It was not a surprise. The surprise was because at the beginning we didn’t see the oscillation.  Smith: Right. Do you remember what you did when you realised that you were seeing the oscillation?  Agostini: I think we wrote the paper very quickly.  Brilliant: Adam, I know we’ve talked about attosecond in previous episodes, but I think I could do with a little bit of reminder. What is an attosecond?  Smith: An attosecond is 10 to the minus 18 seconds. That’s a zero followed by a decimal point and 17 zeroes in a row before you get to a one. It’s a very short space of time.  Brilliant: I can remember telling my kids about it because I just thought it sounded so cool. What was the specific research that Pierre Agostini did a round attosecond and that he was awarded the prize for?  Smith: He got the prize for producing the first pulse of light that was in the attosecond time domain. He produced a pulse of laser light. In fact, we had a little sequence of pulses, but the little sequence of pulses lasted just I think 250 attoseconds. He also was able to demonstrate that that was the case, which was far from trivial. But this was the beginning of the race to get shorter and shorter trains of pulses of laser light. Now they’re down to a few tens of attoseconds in length.  Brilliant: Amazing.  Smith: Basically the production of incredibly short pulses of light,  Brilliant: Which at the time that must have been quite an amazing thing to sort of realise the ability to do that.  Smith: Yes. I think what was opening out was a whole world of processes which last roughly that long. Suddenly you can isolate them and see them as never before.  Brilliant: We’re about to dive into some fairly complicated topics that I haven’t heard of before. I wondered if you could help explain some of them.  Smith: Sounds scary, yes.  Brilliant: This is not a test. The first is a term positronium.  Smith: Yes, positronium is like hydrogen, which is an electron orbiting a proton. But in the case of positronium, proton is replaced by another positively charged entity, which is the positron, the anti metaparticle to the electron. Since these two annihilate each other when they meet, the positronium is very short-lived. Fascinating, but only briefly there.  Brilliant: I see. We’ve talked about attosecond, but what are zepto and yocto seconds.  Smith: Things tend to go down in threes. If you go down a thousand folds from an attosecond, you get to a zeptosecond, which is 10 to the minus 21 seconds and another thousand fold down will take you to the yoctosecond 10 to the minus 24 seconds. I believe a zeptosecond is the shortest period of time ever measured so far. A yoctosecond is approximately at the time it takes light travel across the distance of the atomic nucleus. We’re down to really, really, really small things. Tiny amounts of time.  Brilliant: I can’t get my head around. It’s such a short amount of time.  Smith: Physicists have it all. They get to study the shortest things and also the biggest things like the cosmos. As you said, it’s all pretty cool.  Brilliant: Yes, very. Does the Planck time help us to understand this better? That’s another term I’m not so familiar with.  Smith: Think that Planck time takes us into dimensions of beyond our understanding, but the Planck time is the shortest time that anybody can talk about. It’s defined as the time it takes light to travel the Planck length, which doesn’t help you very much. But the Planck length is one of those fundamental constants defined by [Max Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) over a hundred years ago when he was seeking to establish a set of constants that were based not on things that we measure around us as humans, the meter and the second, but rather units based on the fundamental constants of the universe, like the speed of light. The Planck time equates to 10, to the minus 43 seconds. Physicists tell us that there’s no point in talking about anything shorter but this was something that I was interested to explore with Agostini, whether that’s the end of time or whether you can go still lower. Let’s listen to that conversation.  Smith: At the beginning of your Nobel Prize lecture you ask a question, which I suppose is one that physicists debate often, but is not something that most people think about, which is; is time infinitely divisible? The attosecond is as people keep saying very, very short, but can you just go shorter and shorter and shorter? Is there a limit to how much you can divide time up? Could you talk about that a little because it’s such an exciting question.  Agostini: Okay. After 2001 and the attosecond demonstration, there were a few papers, five or six papers from a lab group in Heidelberg trying to figure out how we could make even shorter pulses than attoseconds, yoctosecond and even later zeptosecond were in the picture. The solution they proposed is to use positronium, or stuff like that, to replace the atoms in the harmonics. It’s not completely obvious how could do that practically, but attoseconds is certainly not the limit and at least up to or down to zepto or yocto it’s certainly not physically impossible. On the other hand, there is a limit. The limit is the Planck time and conceptually you can’t go beyond that limit. That limited is 10 minus 44. We are still very far from it.  Smith: I know that the Planck time is the time that light takes to travel the Planck distance. You can define the speed of light by that. But please, why is that the limit?  Agostini: The Planck time is the limit as far as I understand it, because if you look at the spectrum corresponding to this short time. The spectrum that is the energy. The spectrum and the frequency and the energy is the same. This energy is of all the universe so there’s no way you can go beyond that. Then the time is 10 minus 44 seconds.  Smith: I guess most people don’t think of time and energy being related, but this shows very clearly that they are.  Agostini: Yes.  Smith: It’s just mind expanding to think of it even though it becomes too complicated to think about very quickly.  *MUSIC*  Smith: Let me change direction a little bit, please onto something non-scientific. Just how it has been since the announcement of the Nobel Prize. Because I am sure that it’s increased the amount of demands on your time and you must be receiving invitations to be all over the world all the time. How do you cope with the increased attention?  Agostini: First, let me tell you something. When I had the first announcement I felt there must be something wrong there. I couldn’t feel at all like being on the list of the Nobel Prize laureates, people like Serge Haroche. I felt that there must be something wrong somewhere. But with time I got used to that and I was always trying to figure out that, okay, there must be other cases where other people who didn’t feel like they really deserved the Nobel Prize. After that, especially after my trip to Ohio State and all the ceremonies and the things that we had there and meeting the president of the university, I felt a little bit more confident. Anyway, I don’t think we can do anything about it.  Smith: That’s true. It reminds me of a comment made by a laureate I was traveling with last summer. An audience member asked him, does receiving the Nobel Prize make imposter syndrome better or worse? He answered yes and no. I guess most new Nobel Prize laureates wonder how they fit into the general picture of the list of people who’ve received it. At the same time, as you say, it also maybe has the effect of building confidence.  Agostini: Yes. For some people it’s almost natural. If I think of Serge Haroche, I mean their life has been working in one direction and compared to them, what I did was not comparable at all. However, if you think that the prize was given not just for attosecond but for things that go back to the 70s then I have some reason to believe that it was a reasonable choice.  Smith: It’s the development of an idea. It’s the realisation of a potential, isn’t it?  Agostini: Yes. I’d say it’s an accumulation of affect time in a certain field.  Smith: I understand. Coming to terms with this, it was one aspect of it, but then just the purely practical point of how many people want you and want your time, how do you handle that?  Agostini: I am trying to fight for my time. I have to help leaders from Ohio and we are trying to get things in the reasonable limit. For instance, I’m declining, not most, but at least a lot of those invitations and keeping just a few. That’s one way of doing it.  Smith: How does the family feel about your prize?  Agostini: They are very happy. My daughter will give me the news that I got the Nobel Prize. She had heard about it on the internet or something. The others were happy and everybody was happy to see my grandchildren in Stockholm, especially my grandson.  Smith: How old was he when he went to Stockholm?  Agostini: He is turning 22 this year and she’s 24.  Smith: It’s really fascinating to hear your reflections on it because, like it or not, it is a life changing event.  Agostini: Yes, for a moment it has been a life changing event, and it’ll be like that until at least the end of this year. After that I hope it at least slowly return to normal. It’s difficult to manage. We still have problems with referees that with writing papers in Ohio, and I need five.  Smith: It doesn’t make any difference to that side of life at all. Maybe it makes life even harder with referees now. I don’t know. It’s been so very nice to talk with you. I’ve enjoyed it very much. Thank you.  Agostini: Thank you. |
| **Telephone**  **interview** | **0003 = PA**  Dawn Larzelere: Adam, are you there?  Adam Smith: I am.  DL: Pierre, are you there?  Pierre Agostini: I am!  DL: Awesome, I connected you two, this is great. All right.  PA: Ok!  AS: How lovely, many congratulations on the award of the Nobel Prize.  PA: Thank you, thank you so much.  AS: And you are in France, so, I think that they tried, but they could not reach you to tell you the news. How did you hear the news that you had been awarded the prize?  PA: Well, simple, my daughter called me asking “Is that true, I see it on google?” So yes, I didn’t know what to expect really. I thought it was some kind of mistake but it’s not apparently.  AS: It seems not, not at all.  PA: I’m glad to hear that.  AS: Most official. But what did you do when you found out that this was the case?  PA: I thought of going away, far from any telephone. [Laughs]  AS: [Laughs]  PA: But I guess I cannot do that completely one way or another.  AS: It’s nice for us that you don’t, but I understand the thought, because you are much in demand now. Apart from wanting to hide, what does it make you think?  PA: It makes me think that the reasons of the Nobel Committee are obscure, and why they chose to award this kind of research now is sort of a mystery. But, why not, after all. It’s a long time for me. It’s about twenty years since we did that experiment that started the attosecond stuff, but ok, better late than never.  AS: In 2001 when you produced that first train of attosecond light pulses, did you know that this was something that could perhaps one day receive a Nobel Prize?  PA: Well, ok, there was sort of a competition between the group at Saclay and our coworker Harm-Geert Muller from Amsterdam. There was a competition between us and the other two. Those two guys, Ferenc Krausz and Anne L’Huillier, so we were all in a conference, a sort of European conference and we were really happy to be the first one to announce the thing.  AS: And now the race continues, now the race is to get shorter and shorter pulses.  PA: Yes, at that time we were, I think, the first measurement was something like 400 attoseconds and now they are at 50.  AS: For those who don’t live in the world of attosecond physics, can you help people grasp just how short an attosecond is?  PA: Yes, I was once in a conference, and the guy was talking about femtoseconds, and he was comparing a nickel to the deficit of the United States. An attosecond is a one thousandth of a femtosecond, it’s very short.  AS: I like the idea of a thousandth of a nickel in comparison to the deficit of the United States, that works! It helps, you know it’s hard to get your head around it. It’s a strange question to ask, but do you think you are going to enjoy being a Nobel laureate?  PA: Not sure about that. I am a very, by the way, I am very happy for Ferenc and Anne, and please congratulate them if you talk to them on the phone.  AS: Indeed, I certainly shall. How will you celebrate the rest of today, or how will you enjoy the rest of today?  PA: That’s a good question. I will try to call my grand daughter and grand children who are in Paris at the moment and so we’ll try to get together, and sort of celebrate in the family.  AS: That sounds lovely. I wish a lovely rest of day and I hope that somehow you are able to escape at least some of the calls that come your way.  PA: Thank you, thank you. I will try.  DL: Thank you both of you, I really appreciate the time.  AS: I’ll let you two get on, thank you very much indeed Dawn for organising this. Thank you, thank you Pierre, bye bye.  DL: Of course, thank you Adam. |
| **Interview** |  |
| Q3 | **Where did your passion for science come from?** |
|  | Pierre Agostini: I think research is the thing that attracted me to science because before that I didn’t have such an interest. Experiments; doing experiments and trying to find something that was not predicted, I think that’s the most interesting thing in science. |
| Q2 | **How do you maintain your curiosity?** |
|  | Pierre Agostini: Reading and keeping eyes open, I guess. Curiosity is something that is built in. I think I always was curious, and I will be always. |
| Q5 | **Was there someone who inspired you?** |
|  | Pierre Agostini: Probably [Cohen-Tannoudji](https://www.nobelprize.org/prizes/physics/1997/cohen-tannoudji/facts/). In the seventies, we went to the College de France to his lectures, and he really excited me a lot. I think he was extremely rigorous and at the same time extremely open. The two qualities I think really inspired me. |
| Q4 | **How can science fight myths?** |
|  | Pierre Agostini: By spreading right ideas and, I think it’s difficult, but that’s the job. |
| Q2 | **How do you deal with failure?** |
|  | Pierre Agostini: Try and try again, I guess. If there is a reason, then we have to find the reason, so we have to think about it and then try again. Maybe you did something wrong, in the experiment it’s very easy to do things wrong. I think, just try again because it’s very rare that things work the first time. I have one experiment back in -79 that worked the first time, but this is usually not the case. We have to try again. |
| Q2 | **What would you say to a student who has experienced a big failure?** |
|  | Pierre Agostini: Big failure, OK. This happens all the time. A big failure as an experimentalist is something very easy to achieve. The important thing is to understand why this happens and just try to fix the problem. That’s the way I react at least. Most of the time you find two or three things which could have gone wrong, and you fix one after the other and usually that works. |
| Q6 | **Do you ever get imposter syndrome?** |
|  | Pierre Agostini: I try not to, but sometimes I guess it happens, not perhaps the last few years because I was really established in this attosecond business. I feel this is hope – but in the past, perhaps it happened. The way professional research works, you really do things that you’re supposed to do and, I think I have always felt at home with that kind of work. When you are not exactly sure what experiments to do and you try one and it’s not that great, then you really think ‘what am I doing here’ maybe. ‘This is not where I should be’. I remember when, I guess in the seventies, I went for a post-doc, or sort of post-doc in Los Angeles. But before that, I was doing experiments that were not working very well, and I was wondering if this is really the work I should do. Fortunately, this changed with time. |
| Q1 | **What good advice have you received?** |
|  | Pierre Agostini: Try to understand what you do, at least in as far as possible. Try to do theory also with experiment, and not just experiment. |
| Q7 | **What qualities does a successful scientist need?** |
|  | Pierre Agostini: I have known a number of people who are wonderful scientists and physicists, and they have qualities that I’m afraid I don’t have. I think about, Harm Muller, my old colleague from Amsterdam and from the FOM-Institute who has absolutely wonderful qualities – both an extraordinary theoretician and fantastic in the lab too. He is the example I would cite for anyone wants to do science. |
| Q7 | **What is your best quality?** |
|  | Pierre Agostini: It’s difficult to define in a single word, but I would say precision and being aware of things. I remember once we had a glass setup in the lab. It had to be heated to a gas to purify the atmosphere inside. Once we were heating that thing, and I forgot about it and we went for lunch, and when we came back, that thing was melting. It was horrible! I was there, you know, it was all my fault. This is not being aware of things. From that time, I think I’ve tried to be aware of things. |
| Q8 | **How do you spend your free time?** |
|  | Pierre Agostini: Now I have some free time. I read most of the time, that’s what I do. I mean literature – American, novels, mostly fiction, for example, Glück, [Louise Glück](https://www.nobelprize.org/prizes/literature/2020/gluck/facts/). |
| Q8 | **Does reading make you a better scientist?** |
|  | Pierre Agostini: Probably, because there are a lot of things in the literature that could make me a better person. I don’t think it has made me a better scientist, though. |
| Q4 | **Is keeping up to date with scientific literature essential?** |
|  | Pierre Agostini: Yes, because it’s something necessary. You cannot avoid that. That’s part of the job. |
|  |  |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0004** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0004=Krausz**  Ferenc Krausz:  Wow. These particles were discovered more than a hundred years before. It took an utter century to develop the tools to actually capture them in motion. It was an indescribable moment.  Adam Smith:  Listening to Ferenc Krausz, it’s really obvious how incredibly excited he is by the science, and that’s lovely. That excitement extends to not just his experiments and his discoveries, but then applying those discoveries or getting engaged in humanitarian work. He seems to be somebody who just gets on and gets things done, doesn’t faff about. He also feels very strongly the need to be an internationalist. I suppose it’s partly a result of him being the product of more than one country, scientifically. But he is intent on using science to forge international connections. That’s a very important message in this day and age, particularly, I suppose, when many people are becoming more nationalist in their outlook. There are many reasons to engage with Ferenc Krausz, and what he has to say. I hope you enjoy this conversation.  Clare Brilliant:  This is Nobel Prize Conversations. Our guest is Ferenc Krausz, the 2023 physics laureate. He was awarded the prize for experimental methods that generate attosecond pulses of light for the study of electron dynamics in matter. He shared the prize with Anne L’Huillier and Pierre Agostini. Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces.  Ferenc Krausz is director of the Max Plank Institute of Quantum Optics in Garing, Germany, and a professor at Ludwig Maximillians University in Munich. He speaks to Adam about being molded by the education systems of three different countries, donating his prize money and how a potentially life-saving test for early stage cancer and Alzheimer’s started with basic curiosity in the lab. But first, imagine taking a tour of the Max Plank Institute from a brand new Nobel Prize laureate.  Smith:  When we spoke on 3 October, just a few minutes after you’d learnt that you’d been awarded the prize, I got the impression then that you are a very conscientious person because you were planning that afternoon to do a public tour of your institute. I think you went ahead and did it even though you’d just received the news and there was so much demand on your time, you still fulfilled your duties and did the public tour. Is that right?  Krausz:  Just right. But I wouldn’t say that I considered this as a duty. I would say, this was a great deal of pleasure for me. Of course, this announcement made it obviously very special when I arrived at the institute, actually this was just a couple of minutes after their announcement. When I arrived here, people obviously knew already and they were smiling at me and taking photographs. Very quickly, obviously the news spread here on the campus. This was kind of an open day also at other institutes, research institutes here at the campus. Obviously the news did attract quite some number of interested people. I had the chance to actually give a talk to them. This was what I was preparing at home when I got the call. It was a very special experience. I didn’t consider this as a duty, but it was really a very special experience, certainly once for a lifetime experience.  Smith:  Something else we spoke about on that very brief call was the fact that just the day before Katalin Karikó had been awarded the Nobel Prize. You were the second Hungarian in two days, and you didn’t know each other before, but you said then you were looking forward to meeting and talking. I imagine that happened in Stockholm when you were together there in December.  Krausz:  This happened already before, both of us were invited by the President of Hungary to a reception a couple of weeks after the announcement. We had a chance to meet each other already there. I can always say that Katalin is, as a human being, as unique as she’s as a scientist. It was a fantastic experience for me to meet her, such an incredibly modest person after what she achieved. With her invention she saved a really uncountable number of lives over just the last couple of years. I have the highest possible regards for her. This is another hearty benefit from receiving such a prize that I had the pleasure to meet her and to get to know her so closely.  Smith:  Yes. I imagine you must have met all sorts of people who you never thought you would encounter before. Is there one who stands out, one person who you’ve bumped into because of the prize that has stuck in your memory?  Krausz:  There are too many to actually, I think it would be almost unfair to mention one, because there have been so many whom I probably would’ve never met in that way or never met in any way otherwise, including ministers, outstanding world-renowned scientists from other areas of science. One example was of course the chair of the Swedish parliament, whom I had the pleasure to meet actually in the reception of the Hungarian embassy in Stockholm. The biggest highlight was to meet the queen and the king, no question. It was an incredible experience to actually even have the chance to talk to them, even quite extensively. It was a fantastic experience.  Smith:  Of course, on the theme of being a Hungarian scientist. Of course, there have been many Hungarians awarded the Nobel Prize. It’s obviously very important to you to be a Hungarian scientist, and it’s important to Hungary. I’m interested how you think about that in the context of the international nature of science and how really science is, as you yourself, emphasise a very international effort.  Krausz:  Actually, I feel very privileged that this still unbelievable recognition of my work by the Nobel Prize is something that I can give back to three countries, not just one, to Hungary, to Austria and to Germany. Each time, whenever I’m asked about this, I emphasise that I, from the deepest of my heart, feel that all three countries deserve this to the same extent. From Hungary, I received my whole education and eventually even the first impulses to move in this direction, to get interested in lasers, ultra short pulse, physics, and get interested even in science and in research at all. In Austria, I had fantastic years, where I had freedom to do what I was interested in. Eventually we managed with a very small team to actually perform the experiments that obviously were considered to be worthy of such a recognition. The third phase of my professional career so far, and has been meanwhile, about 20 years in Germany, where we had just unparalleled opportunities and possibilities to expand the field and to help the field proliferate because I think most discoveries, however exciting they might appear, would not deserve such a recognition, if not very many others in the world get interested and utilise the new opportunities that particular discovery opened up. I think, here at the Max Planck Institute of Quantum Optics and the Ludwig-Maximilian University, we did have the chance to actually build up this family, the atto work family, like hundred people with whom we not just have the chance to perform fantastic experiments here under ideal conditions. But we also had the chance to actually build collaborations towards all directions, literally all directions, collaborations with groups from the US, from other European countries, from Japan, from Korea, from China. So we also forced the proliferation of the technology. Actually, we have played a very active role to create the very first attosecond laboratory of the Arabic world in Riyadh in Saudi Arabia, which we opened in 2013. I have to say, I’m very proud of this because in that area of our planet, this field just was not present at all and we created it. In this laboratory, really great results were achieved, which were published in the best journals of the world. This was very important.  Smith:  I think it’s really important to discuss this because again, the sort of snapshot view of you is a Hungarian scientist and great for Hungary, but the way you draw that out into a scientist who works across borders within the heart of Europe and see things in such an international sense, and it’s all about coming together, is the absolute opposite of the sort of rise of nationalism that one sees in so many places, rather worryingly now, there’s a very important message there.  Krausz:  Absolutely. I think science, like sports and culture, can play a really unique role to bring together different nations, bring together different cultures, disassemble boundaries and borders between countries, between different cultures. I think science can make a huge contribution there. Great contribution.  Smith:  In fact, on that subject, you and your colleagues in the attosecond lab have begun this charity called Science for People, which helps, I think mostly young people in Ukraine get access to education when education has been disrupted. Is that right?  Krausz:  That is correct. The story goes back basically to the early days after the war in Ukraine broke out. I used to be in Budapest frequently in the framework of a big project that we are running there. It was a completely different perspective, the war, just in a neighboring country, it’s kind of amazing how differently it feels, even just slightly farther away. Germany is not that dramatically farther away, but far enough to feel this completely differently if one is there in that very country, which is directly neighboring the country where there is war. Just directly from that country, 10,000 people crossed the border day by day. One is watching that on television, then the feeling is coming, wow, one has to do something. I mean, do we have to help these people?  My first thought was, okay, I go just to the border and also volunteer some help. But I recognised very quickly that my countryman did a fantastic job there, and I could do only an infinite improvement if I would be even needed. It looked like on television, there was not even a need because there were many there. I continued thinking, what is I could do? So the idea came up. While I do have a scientific network, which is already quite broad, I do know many scientists all over the world, far away from Germany, far away from Ukraine, where probably if the difference between looking at this war in Ukraine is so big between being in Hungary and being in Germany, then how big the difference can be if someone in the United States, in Canada, in Australia, wherever. I mean this is just like, wow, this is not my problem.  Maybe occasionally one gets some message about that in the news, but that’s it. I thought, well, I have to send a national network so I could utilise this to actually sensitise, these people that something is going on here, which is very bad. Of course, it’s bad because people are being killed, day by day, but it’s bad for a completely different reason, which may not be our first thought in the context of a war, namely, what about the children, right? One can think about what is the responsibility of the adults to have elected a certain president in one country or the other, and therefore they somehow are to be blamed to some extent. But for sure, this certainly cannot apply to the children. I mean, they are completely innocent and they suffer most. If this is going on, then they are just deprived of their future. Schools are closed down, they cannot go to school. They just are enforced to stay at home. They often don’t have even connections to their fellow students. Of course, they cannot develop as a normal child is supposed to at that age. This was a huge concern for me, what we could possibly do for them. I thought, well, I think this is a topic that is probably suitable to sensitise the scientific community with it, and that’s what I try to do.  Smith:  How do you actually do that? How do you bring the scientific community into the picture?  Krausz:  Yeah, first of all, of course, we have created our organisation. It was important to have that kind of legal basis. This was one thing and quite a few colleagues here at attoworld volunteered to support me. It’s really a fantastic team here. Up to someone, like a dozen people who are involved in this organisation and do basically almost it on a daily basis to work for it, of course, without getting paid for that at all. Simultaneously I just contacted several hundred scientists all over the world, via email explaining them what we are doing, what to try to do, and that we need their help and any donation, even 10 euro or 10 dollars would help us any small amount, because it is so incredibly valuable in a country like Ukraine at the moment.  That’s how it started. Then I happened to win one or the other prize even before the Nobel Prize, the Wolf Prize and the Frontiers of Knowledge Award, both also accompanied by quite a significant amount of monetary part, which I more or less completely channeled into this organisation in both cases. This then created a basis on which we could start operating. We teamed up with an organisation in Ukraine because we are not there and we cannot define the best possible way, the need, where is the biggest need and in cooperation with this organisation, then we have undertaken quite a number of actions.  Smith:  We should just dwell on that point, that not only having the energy and desire to set this up, but also donating your prize monies to, it’s a major help. I would imagine it’s quite something to have done.  Krausz:  It is particularly motivating to see how little funding, how big difference can make under the current circumstances. Now, just think of our current program, which we started just a few months ago, where we encouraged the teachers from different schools to actually participate in a tutoring program in the framework of which they would deal with either children who are kind of lagging behind. There are many, not just in Ukraine, even in Europe, of course there is many. There are also children who are excellent and who would like to learn even more than they can do at school. Basically both types. And very quickly we started two schools, couple of those in teachers. We pay the teachers for providing this private tutoring. This is a great help for the teacher because they are very ill paid.  This is certainly an additional big incentive for them to actually not start considering maybe moving away from the country, but staying there, dealing with the children. I think this is one big benefit. The other thing is, of course, this auxiliary additional education is free for the children, and there was a very positive response. Step by step, we now extend this. Currently four schools are involved, one of them actually close to the frontline, close to the Dnepr river. We are now about to actually extend it to more and more schools, depending on the influx of donations.  Smith:  Goodness. Given your Wolf Prize money and your BBVA prize money is already a huge undertaking. Am I right in thinking that you also gave some of your Nobel Prize money?  Krausz:  This is correct significant section of that is also not now in one step, but it’s kind of being transferred step by step in the next couple of years, right?  Smith:  That’s extremely generous. Is this something that can be extended to other countries? Because I imagine now that the network of scientists, as we were saying earlier, it extends internationally. Of course there are so many, sadly, crisis situations where the children especially are suffering. Can you see this becoming an organisation which steps in to try and provide assistance with partners in, for instance, Gaza or Sudan or wherever?  Krausz:  Absolutely. I do fully agree with you that unfortunately Ukraine is not the only region in the world where such a help, such a support is badly needed due to our very limited resources. I think we cannot diversify towards more crisis regions at the moment, without taking the risk that we are becoming under-critical. For sure it would be most desirable to actually scale this organisation towards really international scale, with several locations where it’s operating. We’ll have to see. In so many cases, it depends on individuals, right? Whether there are individuals out there who are not only willing to occasionally donate a little bit of money, which is already a huge help. We are incredibly grateful to all of them, but do much more. It is much more, of course, to actually say, okay, now I will also take action. Of course, that requires a big discussion for yourself. Are you willing to actually spend a significant fraction of your efforts, time and energy on such a course?  Smith:  Yes. It strikes me that you really have tapped into something very exciting and powerful, which is this network of scientists who already talk to each other, who already work together, who already know that they can have very robust conversations and get things done. Often working through difficult circumstances. I can’t imagine that it was particularly easy to set up an at second lab in Riyadh, but people work together and they get through the problems. With that resource behind you, I can see that that’s something to treasure and to nurture and more could be done.  Krausz:  Absolutely.  Smith:  It’s a little difficult territory. Can I ask where the responsibilities as scientists you think sort of lie and stop, because this is the provision of help to those who are in desperate need of help, in this case, in desperate need of education. How far would you go as scientists in feeling that it was your responsibility to get involved? For instance, in politics, we were discussing a little earlier the rise of nationalism. How much is it a scientist’s job, do you think to step into that arena? Not specifically that question, but the political arena?  Krausz:  This is a very good question and a very difficult question. At the same time, I personally feel that not just scientists, but we as a society do have the responsibility to speak out if we have the feeling that something develops in the wrong direction. I wouldn’t say that this is now a specific responsibility of scientists. I think lawyers, medical doctors, engineers, and all others you name, are actually challenged to undertake something, if necessary, go to the street and protest and make sure that their voice is being heard. In the particular case of science, I would feel that the particular responsibility of scientists lie in the context of challenges that humankind, these days for quite a while where science can do a lot, science can explore the reality to make sure that politicians are in a position to make important decisions about the future, which can affect the future of humankind, and not on the basis of assumptions, but on the basis of facts, scientific facts in the context of the degradation of biodiversity in the context of the climate change, to name just two major examples, right?  Unfortunately, we do hear almost on a daily basis that politicians with greatest influence, question the role of humans in the climate change. These are the things where I do see scientists to have an incredible responsibility. Here, we have to try to find all possible channels and all possible ways to actually make clear that this is nonsense. This is nonsense to question the role of humanity in the current situation.  Smith:  Of course, the problem is that nobody likes to be told they’re speaking nonsense. It’s a question of finding ways to engage people to become more evidence-based in their thinking. To become better at app praising evidence. It’s a long task.  Smith: Can you take us back to that moment and tell us what it felt like in the lab to be the very first to achieve this goal, which was at that point, I believe, just a curiosity-driven goal. You just wanted to do it because it was something you were fascinated by.  Krausz:  You have put it so beautifully that I almost don’t like to comment even on it, because you actually said what I wanted to say. I and my colleagues were driven purely by curiosity, be able to reach out to somewhere where humans couldn’t reach out before, to see how these electrons actually move within a molecule. In that moment when one sees this for the first time, first thoughts are like, wow, these particles, these elementary particles, the electrons were discovered more than a hundred years before. It took another century to develop the tools to actually capture them in motion. It was an indescribable moment, when one feels that there is just no effort, which would not be worth this. This is actually also my main conclusion for young scientists. I’ve been often asked over the past few weeks and months, what is my advice and what is my message to young people who are possibly considering to move into science and to become a scientist, a researcher? And I used to say, well, you should just ask yourself what the feeling could possibly be that you are seeing something which no one has seen before. We have here another eight billion people living on this planet. You just see something when you know, okay, I’m the first one who is seeing this and maybe start thinking what this could be good for. This is just indescribable. And this provides you with such a motivation to really overcome all obstacles.  Smith:  Getting to the point in 2001 where you achieved the goal is a very long haul, and there must have been so many setbacks along the way. What is it that drives you? Is it the little observations along the way, the answers to the smaller questions you ask that just keeps you going and keeps it building. If you like, where did it start and how did it not die? That curiosity?  Krausz:  A very good question. I think humankind deserves this. Actually the scientific community and our culture in science as it grew over the decades and even centuries, I think deserves a big credit here, very clever construction, that even if you set yourself a goal, which may be a very bold one, a very big one, so that it takes you 10 years or more to reach it, if you can reach it at all. Even then there is of course many small incremental steps towards that goal where it’s absolutely your good, right? It’s absolutely expected and accepted that you also share these incremental steps forward with the scientific community in form of scientific publications. Perhaps it is even underestimated but this of course, plays an extremely important role to actually keep all the people who are supposed to contribute to such a feat to be eventually achieved, keep them motivated all the time, right?  Because we human beings are not very different in certain properties from a dog. A dog also likes to serve you if you reward it, right? That’s how you train it actually. That’s how you make it do things. You would like it to do that you always reward it. We, human beings, also like to be rewarded with all our efforts and these publications on the way are our rewards. Which gives us, again, a lot of positive strength and energy to try to take the next step. I think it’s quite easy. It’s so trivial that we don’t even think of it, right? It’s just works, right?  Smith:  I love that the scientific method just works.  Brilliant:  Adam, what is an attosecond light pulse? How short is it?  Smith:  How short is an attosecond? Okay. It is naught 0.0000000000000001 seconds.  Brilliant:  How on earth can we picture in our heads what that means?  Smith:  It’s the time that it takes light to travel three angstroms, which is about the width of an atom. Apparently there are about as many attosecond in a second as there have been seconds in the age of the universe.  Brilliant:  That’s incredible. The mind boggles at something that’s so short.  Smith:  Yes. That’s only an approximation, not precise. Basically I think the answer is it’s pretty much impossible to picture something so short. It’s just really short.  Brilliant:  How does an attosecond relate to Ferenc Krausz’ work?  Smith:  He and his lab were working to create the shortest possible light pulses, and it’s intellectually and technically incredibly difficult to do this, but his lab was the first to make an attosecond light pulse, which lasted hundreds of attosecond, and now they produce light pulses also in the region of tens of attosecond. It’s the ability to actually produce an attosecond light pulse that was behind the award of Ferenc Krausz’ Nobel Prize.  Brilliant:  You talked about that euphoric moment when they realised they could do this. Why is it so important?  Smith:  There was this race to, if you like, a race to the bottom, to the shortest to see who could do it, and they did it. It’s important because it allows you to see things that could not be seen clearly before. Have you ever seen those pictures of moments frozen in time that were very popular a while ago? I don’t know whether people still look at them. There’s a particular image of a bullet traveling through an apple.  Brilliant:  Yes. I think I know what you mean. Yes.  Smith:  You see the bursting apple and the bullet frozen in time.  Brilliant: Yes.  Smith: That image, and many like it were made by a man at MIT called Harold Edgerton, who had developed new methods of photography using strobe lighting and things. The attosecond light pulse allows you to do a very similar thing at just a much shorter timescale. It allows you to look at things occurring in the attosecond timeframe, which include, for instance, the transfer of electrons between energy levels or the transfer of electrons between atoms in a chemical reaction. You can picture all sorts of things that were just invisible before.  Brilliant:  Presumably it’s helping us to understand a lot more about molecular physics. What other applications does it have?  Smith:  It’s important for understanding probably pretty much everything because everything boils down to the movement of subatomic particles, which can now be seen. But it’s had particular applications in electronic circuitry where you can see the propagation of information in electronic circuits. Also these days it’s being applied to medicine.  Brilliant:  That seems quite a leap to me. How can you go from this to medical applications?  Smith:  I asked Ferenc Krausz how you would apply these light pulses to the study of, for instance, the early stages of disease. Let’s listen.  Smith: The pulses that you have been able to produce allow us to visualise, allow scientists to visualise the world of the electron, but you have now taken it a step further and you are using it as a diagnostic tool. These are real world applications. It’s really the stuff of science fiction, if you like, that you can use them to discover molecular fingerprints of disease. Do tell us please how it works.  Krausz:  Before I would try to tell you, let me just emphasise something which I think very important and goes way beyond the actual method that you asked me about. It is the impossibility to actually predict what a discovery in basic science will eventually be good for. We would have had no chance to, even if someone instructs us now to sit down and think as long as you need to think, to come up with the idea that attosecond technology one day will offer a method for extracting sizable information from a human blood sample about the health state of the individual. No way. This is what happened 15 years later. I think this shows how big the responsibility of governments is to provide a decent funding for basic science without asking scientists too many questions about what will be this good for. Of course, if they will be asked and enforced to say something, they will always say something, but later on they will recognise that they were completely wrong because what they have discovered will be good for something completely different, which they could not foresee. I think my big advice to the political leadership of all those countries who can afford, who are in a fortunate position to be able to afford funding science at a decent level should never underestimate the importance of basic science and should always provide a very decent fraction of the total amount of the money available for funding science for basic science. This sooner or later will be reported, and I think here we can deliver a great example. 15 years later, the application came up, which just could not be foreseen. The application is as follows. We take a very brief single cycle infrared laser pulse consisting of one single oscillation cycle of the electric and expose a human blood plasma sample to this pulse.  This incredibly fast excitation brings all essentially all the molecules in this blood sample, all the billions of molecules or trillions, we don’t need to know how many molecules into vibration. These vibrating molecules also do emit coherent light in the wake of the excitation. This ultra short infrared pulse is irradiated in the sample in a well coated field. Behind this very short flesh of infrared light, actually a radiation is emitted by the sample by the excited blood sample, which we can capture and sample with attosecond technology. The oscillating electric field of the signal coming from the molecules, if you like, the sound of molecules we can capture with attosecond technology. The sound of molecules tells us a lot about the molecules themselves, because different molecules emit different sounds at different frequencies. That’s an incredible amount of information which we now can capture with attosecond technology.  As opposed to other techniques like mass spectrometry, we can’t identify the individual molecules in such a complex sample containing an unknown number of different types of molecules. Certainly more than 100,000 different types of molecules that we can’t do. But what we can do is we can capture the overall information coming, basically the voices of all these molecules and we can look whether this overall signal, which we call the infrared fingerprint of this blood sample, whether this shows any correlation to unfolding diseases. First of all, we have to establish how this signal is supposed to look like for healthy individuals, and our organism is complex enough that even as a healthy person, we are different and therefore our infrared fingerprints are also different. Just like our real fingerprints are, each and every individual has a different infrared fingerprint. This is what we have to measure initially.  Ideally, whilst we are healthy, this is our healthy baseline. From then on, we just need to follow up the individuals and see when actually the infra fingerprint starts deviating from their own healthy baseline, the individual baseline, and then ask what this may be due to. To figure out what this may be due to. We can do so-called cross-sectional studies case cultural studies where we select people with a certain physiological condition like lung cancer and health individuals to figure out whether lung cancer causes a certain definite pattern in this infrared fingerprint that we can assign to this disease. This we can do, of course, in other case control studies for other diseases, all types of different cancer. We have done this already for eight different types of cancer, but we can do this also for cardiovascular diseases, for diabetes and so on. In all cases that we have looked at so far, we have found a specific characteristic inherent pattern that is specific to that disease.  Smith:  Gosh. So different diseases affect, if you like, the pitch of the voices of the molecules in different ways. They sing a slightly different song.  Krausz:  Not the voice of the molecules. They affect different ways, but they change the molecule composition of human blood. This may be still the same molecules, but certain types of molecules have become more abundant. Other types become less abundant if some disease starts unfolding. This of course will change the overall signal.  Smith:  Your spotting changes in the molecular composition of the plasma sample.  Krausz:  Precisely.  Smith:  The subtlety is that you can detect molecular changes in the composition that are tied to specific diseases.  Krausz:  Exactly. This is a paradigm change in biomarker research.  Smith:  Absolutely.  Krausz:  Biomarker research so far has been about trying to identify the needle in the haystack, right? To pick out those individual molecules, which out of the 100,000 different molecules in human blood, which sensitively respond to the appearance of a certain abnormality. But not only that, this is the easier part, the more difficult part is to pick out those molecules which respond only to this disease. Unfortunately, the main reason for the failure of many biomarker projects is, um, due to the second part. It’s relatively easy to identify molecules which respond to a certain physiological condition in the body. It is extremely difficult to find some which responds only to that very condition and doesn’t respond to other ones.  Smith:  Absolutely. The power and implications of what you are doing are enormous. Imagine if you could, for instance, produce an early fingerprint for the onset of dementia for Alzheimer’s disease or something like that, it would be of course hugely beneficial to medical research, but it would also have profound implications for the insurance industry and people’s general wellbeing.  Krausz: Absolutely. You mentioned an excellent example. We just had a very exciting scientific discussion with a colleague from another German university just yesterday, who is doing research on dementia and Alzheimer. He identified that indeed infrared spectroscopy may be the tool to actually identify this disease way before current medical diagnostics can do so. This would be very important because now first medication is becoming available, but this is efficient only if what can use this at the earliest possible stage. He just showed yesterday in his talk that actually there is evidence that the disease starts developing at least 15 years before current technology can actually safely diagnose it. There is quite some room for improvements.  Smith:  Yes. Goodness. This does bring with it profound ethical implications. I guess that part of the challenge is to have a discussion about the ethical use of such technology. Should they arise in parallel with the development of the technology, because it’s not straightforward what one should do with information that tells people, for instance, that they are due in 15 years to get dementia.  Krausz:  Of course this would be completely counterproductive if there would be no therapies that can do something about it provided the unfolding disease is diagnosed early enough. But fortunately now as opposed to just a couple of years earlier, now we are in a position to be able to say that first medication is already on the markets. It is also obvious that this medication is all the more efficient, the earlier it is being applied to the disease, at the earliest stages where it can be applied. I think in all those cases where there is some sort of medication, and fortunately this actually applies to most diseases, right? Cancer does not only cause so many deaths because there are no therapies around, but much more because unfortunately just recognised far too late. When you have symptoms and with this time you go and see a doctor and the doctor has to come up with a diagnosis, you have stage four pancreatic cancer, you have another couple of months to live, right? That’s the problem. Not that there are no therapies around, but the therapies would be efficient if the disease would be recognised at a much earlier stage. There is a huge unmet medical need in terms of early detection.  Smith:  Isn’t it extraordinary that you find yourself speaking about this and having this conversation, who knows where research will take one.  Krausz:  Exactly. Wow. What a beautiful point that you make here. This is also something that you can’t possibly foresee, right? Even at a time when we started thinking, wow, let’s try to do something for medicine. I would’ve never thought that one day I will say, well, that’s what I want to focus on and nothing else. Because the challenge is still huge. The difference that we can make is so big that there is nothing that would compare an importance for me to this. Why don’t I focus myself fully on this and try to infect with my enthusiasm and my dedication to disclose all the people in my environment? Because alone, I can’t do anything, right? I need a strong team and I need to convince all the others in my environment that this is something that is worthy of being pursued with all possible dedication and commitment.  Smith:  Thank you very much indeed. For me, it’s been an utter joy listening to your enthusiasm. It’s been a wonderful conversation. I’m very grateful to you.  Krausz:  Thank you very much, and thanks for the great questions. I enjoyed it very much too.  Brilliant: You just heard Nobel Prize Conversations. If you’d like to learn more about Ferenc Krausz, you can go to nobelprize.org where you’ll find a wealth of information about the prizes and the people behind the discoveries. |
| **Telephone**  **interview** | **0004 = FK**  Ferenc Krausz: Hello? Krausz.  Adam Smith: Hello, this is Adam Smith calling from the website of the Nobel Foundation, nobelprize.org.  FK: Oh, hello!  AS: Hello! Many congratulations on the award of the Nobel Prize.  FK: Thank you!  AS: How did the news reach you?  FK: Well, I just was not sure whether I am dreaming or whether it’s reality. So it’s still a question that I have to clarify with myself.  AS: So, the call reached you at work from Stockholm?  FK: Right, I am working at this very moment still at home, just preparing myself for an open day at our institute which is supposed to start in about an hour, where I will do a few lab tours for interested visitors. It remains to be seen whether this is going to work out but this is at least the plan.  AS: I think, yes, the day seems to have taken an unexpected turn, but what are your first thoughts on hearing this news?  FK: My first thought is how many friends, colleagues, coworkers, collaborators have directly or indirectly contributed to this. My very first thoughts are with them, and I feel a great deal of gratitude to all of them. Without their contributions and without really concerted research efforts throughout my career first in Vienna, a very important period, and later on here in Garching, Munich this just wouldn’t have been possible.  AS: You are the second Hungarian to be awarded the prize this week so far, what does that mean?  FK: Wow! I was thrilled to bits yesterday when I heard the news about Katalin Karikó, unfortunately I don’t know her in person yet, but of course I know her by her name very well, and I am a very great admirer of her. Not just for her achievements, but also for the way she actually achieved what she achieved. When almost nobody else has believed in this, she just went on and carried on and under very poor circumstances with little funding, but she never gave up. I think this message is almost as important as the actual achievement.  AS: Exactly, it’s such a hopeful message that perseverance pays off, and yesterday she was saying how important it is not to be distracted by all the things you can’t change, but just to focus on what you can do.  FK: Absolutely, this is a really great, great saying, and I couldn’t agree more with it.  AS: No doubt the two of you will meet very soon, so isn’t it nice that the Nobel Prize becomes a way to make new friends.  FK: I am very much looking forward to that, of course I very much look forward to that for many reasons, but in this particular case, also for this particular reason to be allowed to meet her in person.  AS: Just very briefly, these attosecond pulses allow us to visualise chemistry in action, they allow us to see things at the timescale of the electron. What are you most excited about being able to see?  FK: I think it’s always exciting to see something that no one could see before, and I still really remember the excitement that we felt in a very particular morning in the basement laboratory of our institute in Vienna, back in Vienna in 2001 when we first indeed could resolve electron dynamics that evolved within the oscillation period of visible light. This was just an unbelievable moment which I will never forget. Just this very capability of actually being able to observe these motions which are really the fastest outside the atomic core that occur in nature, but actually with attosecond science we have now a tool which basically allows to develop models that simplify the description of these complex systems, and these simplified models might be handleable even for today’s computers and they can be validated by comparing with the experiment that can now be performed in real time.  AS: So exciting to see these things come to life that the theorists write about in books and now here you are revealing it. It has been a great pleasure to talk to you and congratulations again.  FK: Thank you very much, thank you! Bye bye!  AS: Bye bye. |
| **Interview** |  |
| Q3 | **Where does your passion for science come from?** |
|  | Ferenc Krausz: It always fascinated me to enter into a world which is not known to humans, and where we have the chance to discover new things. In general and in particular I have always been fascinated by the microcosm, by the very small things where we can look into and can get access to only by some very special tools like microscopes in space, and like very short pulses in time. I wouldn’t say there was a very decisive moment, a special moment, it was more due to a really excellent teacher in primary school who taught physics and who actually managed to present not just formulas, but also the whole concept of physics, how it works, and how it strives for explaining a whole range of phenomena with a theory as simply as possible. Kind of reduce everything down to the simplest level and try to develop concepts and theories with which we can explain and understand the broadest range of phenomena. This very concept was very fascinating to me, and probably that’s why eventually I decided to devote my life to this area. |
| Q5 | **Do you think you would be where you are without that teacher?** |
|  | Ferenc Krausz: Probably not. I think he really made a very decisive influence on my path. He directed my attention to a field where I had the feeling that I have the chance to just have joy when doing work and where I can go day by day to my workplace and do things which I don’t feel like work, but which feel like passion. That’s, I think to a large extent I have to thank to him. |
| Q9 | **How did you find out that you had been awarded the prize?** |
|  | Ferenc Krausz: I was indeed working at home just doing the last touches on some slides. We had this kind of open-door day at our institute. This was a bank holiday in Germany, the third of October, and we opened our institute to the public. I offered to volunteer a few lab tours for interested people, and just thought that, well, just to introduce this lab tour it’s always nice to say a few words about what we are doing and what those complicated instruments are being used for. I was just about to do the final touches on these slides. It was very interesting because then I was just done, and I thought, oh, okay, I still have a few more minutes, and I can maybe take a look at the interview of [Katalin Karikó](https://www.nobelprize.org/prizes/medicine/2023/kariko/facts/), who was announced just the day before to be one of the winners of the Nobel Prize for Physiology and Medicine. She comes from the same country as myself, so I just started watching that very brief video when the call came in. |
| Q9 | **What was your response to the prize?** |
|  | Ferenc Krausz: Wow, it was overwhelming. I arrived there a couple of minutes after the prize was announced, and it was amazing how quickly people figured out and when I was arriving in the institute, obviously, they just knew about it and just got to learn about this. They were already smiling and were taking photographs; it was a very special feeling. It was an overwhelming feeling to celebrate these first moments together with people interested in science, not necessarily my own colleagues. Some colleagues came in shortly thereafter as well, which was also very special, of course, to celebrate with them. But it was also beautiful to celebrate just with the public, and then they see how excited they are and wanted to take pictures together with me. It was really a great feeling. |
| Q2 | **Why is collaboration so important in science?** |
|  | Ferenc Krausz: I think these days technology altogether becomes so complicated, so sophisticated that to make complicated experiments work, we have to bring together a lot of bits and pieces from different areas, of not only physics, but also electrical engineering, information technologies, sometimes also life sciences. This of course relies on knowledge from many different fields which cannot possibly be brought in by just one or two persons. That’s why it was very important also in our field, also in our projects, to bring together scientists, often from different laboratories, to contribute basically their knowledge and their know-how to the project. This was also the case when we managed to generate and measure the first and the second pulse in Vienna. This was also the result of a truly international collaboration where scientists from Germany, from Hungary, Austria, of course, because this was in Austria, and Canada, were participating and were making contributions. |
| Q18 | **Why is it important that science is an international discipline?** |
|  | Ferenc Krausz: I think science is just inherently international. There is, at least in natural sciences, there is certainly no national science. I mean, the laws of nature are not restricted to a country, not even to a continent, but they are inherently international. I think that’s one of the truly beautiful things about science. That’s also what I think makes science so special and so valuable. Not just for spawning new knowledge, but also to connect people, connect people from different areas coming from different cultures, have them talk to each other, get to learn each other, get to learn each other’s culture and different habits. I think science can actually contribute a lot to a peaceful world. |
| Q10 | **What inspired you to support scientists displaced by the war in Ukraine?** |
|  | Ferenc Krausz: I have been spending significant fraction of my time in Hungary over the past few years, thanks to a new project which we started there about four years ago. This was also the case, I was also regularly in Budapest at that time when the war broke out. Hungary is a direct neighbour of Ukraine, we have a common border. In the first weeks, month of the war 10-15,000 people crossed the border day by day. I’ve seen the pictures of it, it was televised of course, there were daily reports about what’s going on there. I felt very, very sorry for these people, and I wanted to do something, and I thought, wow, I should go there and volunteer kind of some help there, but I quickly figured out that my countrymen do a great job there, so probably I could do just an infinitesimal contribution. What else could I possibly do, which could be more than just an infinitesimal contribution? That’s how the idea about founding an organisation named Science for People came about and utilise basically my worldwide network and connections to other organisations and colleagues working at other research institutions to sensitise them for what’s happening here. Because I found that the farther away one is from a conflict the less is one affected somehow emotionally because the farther one is away the less news can be heard about even a serious conflict.  I just wanted to set up this network to sensitise people and collect donations for allowing us to help not the refugees that left the country, because they were taking care of really very generously in different European countries, but those very many who stayed in Ukraine, and particularly the children who stayed there, who have been suffering ever since the beginning of this war. Many of them are deprived of their schools, meanwhile, either because the school is destroyed or because they due to continuous shelling – it’s very dangerous to just go even out and go to school. Many have the only chance to learn by online schooling. We thought we should try to provide some help, and that’s why we teamed up with an organisation in Western Ukraine, and together with them we provide support to children and young people in their education and also for their free time activities. |
| Q10 | **How have the countries in which you have lived influenced your life?** |
|  | Ferenc Krausz: I do feel myself in the very first place as Hungarian, for sure. I was born there and I got my education there, so I owe a lot to my teachers in Hungary who directed my attention to this field, which I have been devoting my life to, and therefore for sure Hungary has a very big importance in my life. On the other hand, I have to say that I owe a lot also to Austria, where I had the chance to work with a great deal of degrees of freedom on problems and on challenges that I personally found extremely exciting, and where I had the chance to actually decide, yes, this is the direction I want to go. Particularly thanks to one person, to my mentor, Arnold Schmidt, who allowed me to really build a great team at the technical University of Vienna. Eventually, I have to thank a lot to Germany, where I had the chance to bring the kind of tools that we developed in Vienna to fruition and really utilise these tools for tackling a number of problems and demonstrate the kind of power of these tools and technologies of attosecond science. In this way also a bit fertilises other groups and “infect” other people in the world with this passion for this field. |
| Q1 | **What advice would you give to young researchers?** |
|  | Ferenc Krausz: I think what is very important for everyone is to find his or her right questions to find those questions where the search for the answer makes someone feel passionate about what he or she’s doing. It may be often quite difficult to find the right question. I think this is something that everyone, particularly those who decide to become a researcher, become a scientist, should spend sufficient time on to find their right questions, because that’s from which one can actually derive the goals and that’s what defines the tasks to be tackled on a short run – next few weeks, month, years, and beyond. |
| Q2 | **How do you deal with failure?** |
|  | Ferenc Krausz: Science, indeed research, particularly research at the forefront consists to a large extent of failures, probably more failures than successful steps forward. Of course, in the first moment, a failure never feels nice, but in the second moment, we do realise that there is a lot to learn from those failures. In quite a few cases, we can learn more from failures than from successive steps forward. I think we always have to see how we can extract the maximum amount of new information we can possibly get from that failure and equipped with that new information think what is the right step to be taken next? It is not a particular advice to be given in the case of a failure, I think it’s more how to deal with a failure. I think that’s what is most important: to make a failure feel not being a failure, but an experience from which we can gain new knowledge, and with this new knowledge, we have a better chance to succeed now, in the next step. |
| Q7 | **What qualities are necessary to be a successful scientist?** |
|  | Ferenc Krausz: Curiosity in the first place, almost equally importantly, perseverance, and almost equally importantly interest in finding something which no one else has found before. I think these three things together probably can make anyone a good scientist and can give anyone the feeling that work is not really work but joy and passion. |
| Q2 | **What role does hard work play in research?** |
|  | Ferenc Krausz: I think science is not special in that respect, probably in most other areas of life if you meet a challenge, a serious challenge, and want to achieve something by overcoming that challenge, by overcoming all the hurdles, then there is no alternative to working hard. That’s not special to science, I think, that’s an absolutely necessary ingredient. But just as I mentioned before, if the other three qualities are in place, then this so-called hard work doesn’t feel like work. That’s how I think about this. |
| Q2 | **Is creativity necessary to be a scientist?** |
|  | Ferenc Krausz: I would say it’s probably very helpful. It’s very helpful. But this is of course something that cannot really be learned at school, that has to come together in a problem or has to develop in a more sophisticated way by being interested in new things and being curious and being eager to figure out things which other do not know yet. |
| Q10 | **What environments best encourage creativity?** |
|  | Ferenc Krausz: There are environments, I could mention as an example our environment in Munich. It’s a truly fantastic environment where many scientific fields are around, really on a world class level. I think it is a great privilege to be allowed to work in an environment where no matter what kind of question is coming up there is an expert almost next door to be asked. Definitely, there are places where many disciplines are present and are being represented at a world class level. These are, I think, the best environments for creating new knowledge. |
| Q11 | **Why is diversity in science important?** |
|  | Ferenc Krausz: I think science is the inherent platform which provides space for diversity and not just provide space but actually inherently encourages, because science doesn’t select, wow, this new knowledge that we need here, or new idea that will bring us forward, whether it comes from women or men or a person from one area of the world or the other area, it doesn’t make any difference, right? The main thing is that this idea, this new thought, is coming in and makes things move forward. I think science just inherently encourages people of any kind, irrespective of religion of culture – just come in and make a contribution. |
| Q8 | **How do you like to spend your free time outside of science?** |
|  | Ferenc Krausz: What is that? [Laughs] No, this was a joke. I try to use my free time to switch off because I feel that switching off what one is doing most of the time is important to be eager to return next day and do it with full power and full motivation. Switching off can happen by being together with my children previously since a while with my grandchildren meanwhile. It can also be by sports. I do run a lot, I feel that is not only important for maintaining physical fitness, but also for mental fitness is very useful. I very much like reading, not necessarily scientific literature. I do read scientific literature as well, but in my free time I like reading completely different things. |
| Q2 | **Do you think these hobbies make you a better scientist?** |
|  | Ferenc Krausz: I think we shouldn’t have hobbies to make us a better scientist, but probably if we can afford to have some hobbies and in this way we can recreate our creativity and our passion for science and our eagerness to create new knowledge, then probably this does help to make us a better scientist as well. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0005** |
| **Biographical** | I was born in Paris in 1958 and lived the first twenty years of my life in the 13th and 14th arrondissements. My late mother, Yvonne, was first a schoolteacher and then stayed at home to take care of her three children: myself, my sister Françoise, and my brother Jean-Marie. My mother suffered from diabetes from a very early age. As a child, she was told that she could not have children, would become blind, and eventually be amputated. Although she suffered from the consequences of this life-long disease, none of the predictions came true, thanks to progress in science and medicine. This left me with a strong belief in the power of research and science. Her father, Lucien Chrétien, was an engineer and teacher in radio technology. He wrote many books and helped the resistance movement during World War II with radio communications. I don’t remember him, since he died when I was 4, but this family history certainly influenced me as a child, reinforcing this feeling of the importance of science for society. My mother’s family lived in Provins, a small medieval town in Seine-et-Marne, 100 km east of Paris. When my grandfather died, my parents inherited an old smithy in a village called Saint-Hilliers, close to Provins. I have spent almost all my summer vacations there ever since. This small place, among the wheatfields of the Brie region, has remained to this day a much-loved spot.  My late father, Bernard, was an engineer in informatics at a time when computers were huge, rare, expensive, and slow machines. Employed at the CNES (Centre National d’Études Spatiales), he participated in the creation of a computer center devoted to the launch of satellites. He certainly inspired me to pursue a career in sciences, being always a strong supporter. Unfortunately, he passed away four months before the announcement of the Nobel Prize. Many in my family were (or are) talented musicians. I played the flute when I was a teenager and a student. My youngest son is studying to become a professional jazz musician. It is not by accident that I made as many analogies to music as possible during my [Nobel Prize lecture](https://www.nobelprize.org/prizes/physics/2023/lhuillier/lecture/) when he was present. Finally, I have always enjoyed practicing sports, like skiing, swimming, and playing tennis, wonderful activities which I still do today.  In July 1969, then on vacation in the French Basque region, my grandmother woke me up to watch the first landing on the moon. This was incredibly exciting for a 10-year-old girl with an interest in science. I was amazed by the technology and science that had gone into enabling this endeavor. The fact that it was broadcast live on television to hundreds of millions of people on Earth was also fantastic!  I had the privilege to go to school in the Quartier Latin in Paris, Ecole Monge, Lycée Montaigne then Lycée Fenelon. These were great places providing a great education! I remember in particular a fantastic teacher in mathematics, who encouraged me to continue in this direction. After the “baccalauréat”, the natural choice for me was to start the “classes préparatoires” in mathematics and physics. After two years of tough work, I was accepted at the Ecole Normale Supérieure (ENS) of Fontenay-aux-Roses in 1977 (the ENS is now in Lyon). The focus of the school fitted very well with one of my goals which was to become a teacher.  At the ENS, I could continue both mathematics and physics and obtained a Master’s degree in both subjects in 1979. The third year at the ENS was the preparation for a national teaching degree called “agrégation”. This was a tough year, especially since the number of positions in mathematics that year (1980) was very low! I was extremely happy to be admitted since it assured me of a teaching position in mathematics. During my fourth and last year at the ENS, I chose to do a DEA (Diplome d’Etudes Approfondies) in quantum physics. This was a fantastic year, with great teachers, like [Claude Cohen-Tannoudji](https://www.nobelprize.org/prizes/physics/1997/cohen-tannoudji/facts/) and [Serge Haroche](https://www.nobelprize.org/prizes/physics/2012/haroche/facts/), who would be awarded the Nobel Prize in Physics in 1997 and 2012 respectively. They made me fall in love with quantum mechanics and atomic physics. Part of the DEA was a two-month internship which I chose to do at the Commissariat à l’Energie Atomique (CEA) in Saclay, in the Service de Physique des Atomes et des Surfaces (SPAS), headed by Claude Manus, in the research group of Gérard Mainfray. The subject was a little exotic, it was about atoms exposed to a strong laser field. Unfortunately, the laser was out of order and the project was reduced to a literature study. However, after this internship, Claude Manus offered me a PhD position, starting in the fall of 1980, which I gladly accepted.  The area of research was atoms in strong laser fields, and the subject of my thesis was the multiple ionization of rare gases by multiphoton absorption. This was my first real encounter with experimental research, and I liked it a lot. I learned, among many things, how to align a laser beam, make an oscillator lase, and safely manipulate oil diffusion vacuum pumps. I worked together with a skilled experimentalist, Louis-André Lompré, and we were helped by dedicated technicians in mechanics and electronics. Data were acquired by taking a photograph of the signal on an oscilloscope screen with a polaroid camera, measuring the signal with a ruler, and writing down the results using logarithmic paper. The thesis work started very well, with results within the first few months and the first article published already in 1982. In the second year of my thesis, Louis-André left for a sabbatical year at Harvard University. I did not get as many results during this second year, but being alone, and responsible for the laboratory, was very educational. When needed I got help from colleagues in the group headed by Gérard Mainfray, called the multiphoton group: Didier Normand and Jacques Morellec working on resonant multiphoton ionization, and Pierre Agostini and Guillaume Petite, working on electron spectroscopy. The main result of my thesis was the identification of two mechanisms for the double ionization of atoms exposed to a strong laser field, sequential and non-sequential. In 1983, in search of a theoretical interpretation of the results of my thesis, I contacted a Swedish theorist, Göran Wendin, a specialist in the description of the photoionization of multielectron atomic systems, who was spending a sabbatical year at the University of Orsay. This resulted in a fruitful collaboration, three theoretical chapters in my thesis (out of six!), several research visits to Gothenburg, Sweden, and a lifelong friendship. I defended my thesis in January 1986. My thesis was one of the last “thèses d’état”. It took me five years, but it immediately gave me the habilitation to supervise theses.  After my thesis, I spent six months in Gothenburg working on applying diagrammatic many-body perturbation theory and the random phase approximation with exchange to multiphoton ionization. I would spend another brief postdoctoral visit, during the winter of 1988-1989, at the University of Southern California, Los Angeles, in the group of Peter Lambropoulos, using other theoretical approaches for the description of multiphoton processes. In addition to mastering the English language, these two short postdoctoral visits gave me a good foundation in atomic theory, which I have found very useful during my career.  I got a permanent position at the CEA in October 1986. In the summer of 1987, we performed an experiment that had a profound impact on my scientific career. Studying the photon emission from a gas target irradiated by an intense Nd-YAG laser field, we observed odd harmonics of the laser, up to order 33 in argon, and with comparable intensities from the fifth order. I was immediately fascinated by this new process and focused my research on this, both theoretically and experimentally. The theoretical description of high-order harmonic generation (HHG) required solving both the [Schrödinger](https://www.nobelprize.org/prizes/physics/1933/schrodinger/facts/) equation, describing the response of an atom to a strong radiation field, and a propagation wave equation. I developed a computer program for solving the second part and collaborated with Ken Kulander and Ken Schafer at the Lawrence Livermore National Laboratory, who were experts in the numerical solution of the time-dependent Schrödinger equation. Again, a fruitful collaboration, with many articles, and lifelong friendships.  In 1989, HHG was not a prioritized subject at CEA. Laser development was on the agenda, thanks to the new Chirped Pulse Amplification technique. I was allowed, however, to continue to work on HHG. I helped supervise a Chinese student, Xiao-Feng Li, designed a new dedicated instrument (see the photograph), and worked on simulations. This was the time when personal computers started to be part of experimental work and I spent a lot of time programming a data acquisition code, which allowed real-time analysis and visualization. In 1990, I had the pleasure of working with a Master’s student, Pascal Monot, and shortly after, with my first PhD student, Philippe Balcou. My previous colleagues Louis-André Lompré and Michel Ferray, who had joined us in 1987, were concentrating on laser development, while I led the research in HHG. To get someone like Philippe Balcou, a bright student from Ecole Polytechnique, as my first PhD student, was a fantastic opportunity! Together, we studied HHG using all available lasers and worked on improving our understanding of the phase matching of the high-order harmonics. Using a Nd:Glass laser newly upgraded with the Chirped Pulse Amplification technique, we could see harmonics with more than 100 eV energy! However, the repetition rate of this laser was 0.1 Hz, and it took many hours to acquire the data.  In the fall of 1992, I was invited together with Philippe, who was then finishing his thesis, and Pascal Salières, who had just started as a PhD student, to perform experiments at Lund University with our dedicated instrument (see above) in collaboration with an enthusiastic Swedish team. Sune Svanberg, Anders Persson, and Claes-Göran Wahlström had just bought the first amplified titanium sapphire laser in Europe, with a 10 Hz repetition rate, and were extremely interested in testing their new laser and facility. Just before we left for Sweden, Maciej Lewenstein began as my first postdoc, working on the theory of high-order harmonic generation. I remember being so embarrassed when welcoming him at the airport, announcing to him that I would leave for Lund for an experimental collaboration. The titanium sapphire laser was perfect for the study of HHG and the experiments in Lund were very successful. Based on our results, Maciej started to develop the Strong Field Approximation that would have a significant scientific impact in the field.  During the same period, I received an invitation to spend half a year at the Lawrence Livermore National Laboratory in 1993, in the group of Michael Perry, which I accepted. Together with Pascal, we spent half a year in California. We worked together with Kim Budil and Todd Ditmire, who were then PhD students of Michael Perry. We were using our instrument, which was flown from Sweden, and a LiSAF laser.  In 1994, I moved to Lund to share my life with Claes-Göran Wahlström. We got married in July 1994. I am very thankful to the CEA, and especially Didier Normand, for their help in the transition period that followed. I did not have a position in Lund, and they allowed me to continue to be employed at Saclay while being there only one week every month. I am also thankful to Pascal Salières, who was writing his thesis at the time, and to Bertrand Carré, who took over the leadership of the HHG activity, for their understanding and friendship. Sune Svanberg, then head of the Atomic Physics division at Lund University, helped a lot during all these years and beyond.  I got a three-year lectureship financed by the Swedish Research Council in 1995. One year later, the Swedish government decided to finance professorships for women to promote gender equality. These were called the Tham professorships, from the name (Carl Tham) of the minister of education and research at the time. In search of a permanent position in Sweden, I applied for a professorship in atomic physics at Gothenburg University, after the retirement of Ingvar Lindgren, and was ranked first. This made it easier for Lund University to open one of these Tham professorships in my research area, and for me, who chose to work in Lund rather than commute to Gothenburg, not to feel too much “positively” discriminated. I became professor in atomic physics at Lund University in 1997.  During my first years in Lund, I had the chance to work with a bright student from the University of Copenhagen, Mette Gaarde. It was a real pleasure to work with a woman for the first time in my career, and we are still in friendly contact. We had regular European collaborations through the access program of what is now called Laserlab-Europe, supported by the European Union. We worked together with Michael Meyer and Mathieu Gisselbrecht on the spectroscopy of excited states of helium using HHG. We had an interesting collaboration with Marco Bellini from the European Laboratory for Nonlinear Spectroscopy (LENS) in Florence, Italy, and [Theodor Hänsch](https://www.nobelprize.org/prizes/physics/2005/hansch/facts/) from the Max Planck Institute for Quantum Optics in Garching, Germany. We studied the coherence of high-order harmonics and observed experimentally the contributions of the two trajectories mainly responsible for harmonic generation.  I enjoyed working together with my husband Claes-Göran Wahlström for many years until we decided that it was better to be professionally independent: Claes-Göran moved to another research topic, physics at very high laser intensity, while I concentrated on high-order harmonic generation and its applications. Our two sons, Oscar and Victor, were born in January 1999 and April 2000 respectively. It was time for me to have a career break, which I thoroughly enjoyed. I took parental leave, full-time until they reached the age of six months, and then part-time with a decreasing percentage as they got older.  At the beginning of the millennium, I coordinated the European ATTO [Marie Curie](https://www.nobelprize.org/prizes/physics/1903/marie-curie/facts/) network. The idea of this network was to demonstrate experimentally the existence of attosecond pulses. Thanks to the experimental results of my co-laureates Pierre Agostini and Ferenc Krausz, the objectives of the network were reached during the first year. The network included all of the European laboratories working on the subject. It was great to exchange ideas and results regularly with all of our colleagues. I am convinced that European instruments such as the Marie Curie networks and training sites contributed a lot to promoting attosecond science in Europe.  I had the great pleasure of being elected to the Royal Swedish Academy of Sciences as a foreign member in 2004, and three years later I was asked to serve on the Nobel Committee for Physics. This was a lot of work and responsibility, during nine years. I learned a lot, and not only in physics.  During the first ten years of the millennium, my emphasis was on increased control of attosecond (as) pulses in a train of pulses. We demonstrated pulses as short as 130 as and explored the additional control provided by a two-color fundamental field. I had great postdocs during this time, like Rodrigo Lopéz-Martens, Thierry Ruchon, and Katalin Varjù, who have since pursued brilliant careers in academia. In 2010, Mathieu Gisselbrecht and I started to explore the idea of measuring the phase across a resonance using attosecond pulse trains, and in 2011, we stumbled on the subject of photoionization time delays, following seminal results by the group of Ferenc Krausz. I supervised excellent students on this subject, for example, Kathrin Klünder, Marcus Dahlström, and David Busto, and initiated long-lasting collaboration with theorists Alfred Maquet from the Université Pierre et Marie Curie in Paris, Eva Lindroth from Stockholm University and Fernando Martín from the University of Madrid, as well as with experimentalists, for example, the group of Raimund Feifel in Gothenborg. We have subsequently explored resonant and non-resonant photoionization in many systems using attosecond interferometric techniques.  In 2008, I obtained my first grant from the European Research Council. It was a great moment! It gave a boost to my research career, making it easier to get other funding. In 2013, I got a grant from the Swedish Research Council for distinguished professors, which meant good funding for 10 years. It was incredible! It helped a lot in reducing the continuous stress of attracting enough external funding to maintain a reasonably sized research group, allowing me to explore new research avenues. During all of my research years in Sweden, I have been supported by the Knut and Alice Wallenberg Foundation with equipment at the Lund High-Power Laser Facility, a Wallenberg Scholar award in 2009, several project contributions, and recently through my participation in the Wallenberg Center for Quantum Technology. From the 2010s, the research group working on attosecond physics in Lund grew from a few people to more than 20. The number of senior scientists working on this topic in Lund increased. Besides myself, Johan Mauritsson and Per Eng-Johnsson became professors. Mathieu Gisselbrecht and Cord Arnold became senior lecturers. Recently Anne-Lise Viotti was recruited as an assistant professor. We work together as independent scientists, each with a different specialty, but try to act as a coherent “attosecond group” toward the outside world.  When I moved to Sweden, I also discovered the pleasure of teaching, which provides a nice balance to research. I have taught optics, laser physics, light-matter interaction, and atomic physics, mostly at the Master’s level. A few years ago, I was asked to teach atomic physics to third-year students in physics engineering. I truly enjoy this course, and it is students from the 2023 intake of this course who were in the lecture room when I received the call from Stockholm: They will always remain special in my memory.  I have received several scientific prizes for my research, and I have been elected to academies as a foreign member in Sweden, the US, Austria, Italy, and France. I will just name a few awards here: The first prize I got was the “Prix Aimé Cotton” in 1990, a prize for young scientists from the French Physical Society. I am convinced that it played a big role in getting my first position in Sweden a few years later. The 1998 Göran Gustafsson Prize, a Swedish prize providing both recognition in Sweden and generous research funding, was important at the beginning of my career in Sweden. The 2011 Unesco-L’Oréal Prize for Women in Science was extraordinary in many respects. The team of journalists that spent a week in Lund to produce a 3-minute video and a few photographs was the most professional I had ever met. I was only moderately happy, however, to see huge photographs of myself at Paris airports, some of them where I was hiding behind laser googles. Who knows? Maybe they encouraged some young women. Recently, the Wolf Prize, the BBVA Award and the Leibinger Zukunftspreis led to unforgettable experiences and memories. To be elected to the French Académie des Sciences, even though I left France thirty years before, touched me a lot and made me very proud! However, nothing prepared me for the incredible recognition brought by the Nobel Prize! |
| **Autobiography** |  |
| **Podcast** | **0005=L’Huillier**  Anne L’Huillier: Sometimes I have questions like do I have the right to have fun in my lab doing this research? But on the other hand, you have teaching. You see that directly in the eyes of the student when they understand something. I’m doing something for society.  Adam Smith: I suppose it’s a question for all of us, whether to have fun or attend to your responsibilities. It’s clear listening to Anne L’Huillier, that she takes her teaching duties very seriously. Not, I think though, that she doesn’t also have fun doing. The duties attendant on being a Nobel Prize laureate are also ones that she takes seriously, although one might think of her as a reluctant Nobel Prize laureate, not perhaps asking to be thrust into the limelight like this. But now that it’s there, she intends to do a good job. Do join me for this most thoughtful conversation wither.  Clare Brilliant: This is Nobel Prize Conversations. Our guest is Anne L’Huillier, one of the recipients of the 2023 Nobel Prize in physics. She was awarded for experimental methods that generate attosecond pulses of light for the study of electron dynamics in matter. She shared the prize with [Pierre Agostini](https://www.nobelprize.org/prizes/physics/2023/agostini/facts/) and [Ferenc Krausz](https://www.nobelprize.org/prizes/physics/2023/krausz/facts/). Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces. Anne L’Huillier has been professor of atomic physics at Lund University since 1997. In this conversation, you’ll hear her talk about her new role as a role model, how she doubled her prize by winning it for both France and Sweden, and about learning a new language by using it to teach advanced physics. But first, Adam wonders what it’s like when your innate curiosity opens the door to a whole new field in physics.  Smith: One can’t really imagine what it’s like to be at that forefront where you are really exposing things you think are there. You imagine how they’ll be, but then you see them for the first time, which is what you do with your light pulses.  L’Huillier: Yes. Except we are not alone. It’s not like climbing the top of a mountain for very first time. Because we are still many in the world doing similar things, and then you never know what you see as is really interesting or not. You need some time to understand, analyse and really be able to judge if this is the first time.  Smith: It must be an interesting balance between, as you say, being part of this worldwide international effort, which is itself so exciting. Also obviously wanting just the very human thing of wanting to be the first, wanting to see things for yourself. How does that balance between the individual and the team work in your life?  L’Huillier: First I think you are talking a little bit too much about competition. It’s really not like that. It is absolutely a team effort, especially experimental physics and what we are doing, it’s a team effort. The reward is to find new things and to have beautiful results. That they’re beautiful. If there are no noise, if you see a clear variation or something, so this almost immediately that you have good results or not. I think the society should not see that as a sport where you, you win a gold medal. It’s not that. The Nobel Prize is an award or two a discovery, and it’s not personal. It’s a discovery. That’s the important focus.  Smith: Yes, indeed. In 1987, you were part of this team and generated these high order harmonics by hitting inert atoms with high energy lasers. Were you immediately aware that there was something of deep interest there?  L’Huillier: Yes, actually I was, and I remember this moment very clearly. It sat in my brain, but I can’t say I could see that this was important. The only thing I can say is that it was very exciting, very fascinating. What I felt is that, oh, I want to know more and I want to understand better. It was more an intuition than a clear realisation of the importance.  Smith: Often again and again in these conversations, we come to those moments where intuition plays such a strong part. Somehow you’ve developed in the right way to be able to spot what is interesting. I know teaching is incredibly important to you, and it’s must be one of the things that you try to instill in young people the feeling for what is important.  L’Huillier: Yes. Of course, it’s very difficult. I can only talk through my experience and through the way I am functioning as a scientist. I’m not saying this is the right way and this is the way, or it’s how I function. I very often follow some kind of feeling, intuition. Maybe it’s more on the artistic side that on a rigorous scientific side but probably it’s a little bit of both. But yes.  Brilliant: Adam, Anne L’Huillier was awarded the prize for the production of attosecond pulses. What was her contribution?  Smith: She started the field. It was her observation of these overtones in her experiments that began the whole thing.  Brilliant: If I understand correctly, I think her experiments involved transmitting laser light through a noble gas. Is that correct?  Smith: Exactly right. She used high intensity lasers to irradiate xenon gas and inert gas, and the oscillating electric field of the laser excited the electrons in the xenon atoms. In fact, it gave so much energy and altered their motions so much that at times they have enough energy to escape the atom. But then the electric field, which is oscillating changes, and when it changes, they get recaptured by the atoms. So they’re sort of going up and down, and as they do so, they emit energy in the form of photons, particles of light, and it’s those photon emissions that she observed.  Brilliant: That must have been a rather remarkable moment when she made this observation.  Smith: Yes, I think it was. What I think was unexpected was that she saw multiple photon energies, all integer multiples of the original laser frequency. In the same way that a musical instrument generates often, not just one note, but overtones above that note. She was seeing a whole range of different ionisation frequencies we’re used to the idea of harmonics in music. This was harmonics in light.  Brilliant: That’s really interesting. But why are harmonics in light important? What’s the application or use of that?  Smith: It was noticed that these harmonics were produced in little trains of pulses, and what you got was maybe one to 200 at a second’s worth of light pulses. And so it suggested that, you know, you suddenly had a way of generating very short flashes of light. But there was a very long way to go between that initial observation and the actual production of something that could be reproducibly, and definitively said to be a useful at a second pulse. L’Huillier herself talked about how that process took a long time.   L’Huillier: This is a journey that took time and it was not clear at all that this would be possible. There’s still a long way to go. By the way, this journey is really far from being finished. First of all, the idea that possibly this hi harmonics could in the time domain could be a sequence of very short night parts. This was proposed, I would say a couple of years after the discovery of this hi harmonics plateau that we did in 87. It was a natural idea for people dealing with lasers, with mode locking, because you have a similar process in lasers, but it was not clear at all. Actually I was one of the skeptical persons because at that time we were doing simulations and we could actually check numerically. The result at that time was, no, doesn’t work, but this idea was too good not to continue and investigate can in certain condition, we can have this attosecond pulses. So it took us, I think 14 years to really settle this question. This is also why the measurements by the two called laureates, Pierre Agostini and Ferenc Krausz cross were so important, really show experimentally that yes, one could produce attosecond pulses of light was terribly important.  Smith: It’s interesting to think of that timescale, because when one tells the story of it at all, it becomes compressed and it’s one thing leads to another. I suppose it’s unusual in the world outside things like science that anybody follows a train for so long, 14 years is a long time in most people’s lives. To follow one thought, if you like, but I suppose what drives you throughout that is the curiosity and the phenomenon, so that it’s not just that you are following that, but you are deeply interested, as you said, in the nature of the harmonics and the nature of the physics behind them.  L’Huillier: Yes, exactly. I’m still, it’s not 14 years, but 37 years, we are still working on the basic process itself and try to understand it even better. Especially to control it even more.  Smith: It’s wonderful that you find a problem that sustains you for 37 years.  L’Huillier: Yes, it’s incredible, isn’t it? I’m amazed myself, I’ve been so lucky.  Smith: Do you ever wake up of a morning and think maybe there’s another problem?  L’Huillier: No. But I can think about still very fundamental thing around this during night or something.  Smith: Obviously, of course, everybody has their problems along the way, but it sounds like a sort of dream journey to a certain extent. When we meet young scientists around the world, they’re aware of the dream, but they’re also these days terribly burdened by all the sort of things around science, the paraphernalia, the need to get published, get jobs, all of this, and it seems to worry them greatly. There’s a contrast between that concern and looking at your path and thinking, gosh, how wonderful.  L’Huillier: Yes. I also had problems on the way, obviously things don’t work all the time. I changed country in the middle of that. It was not easy at all to find a new job and to be able to continue. I have had years without much funding. I have had years where experiment continue not to work during so I have had my share of difficulties. But I guess I am very obstinate person and also passionate. The interest of the subject probably weighted more than the difficulties and the problems you can meet. Plus also things suddenly work. You have an application that goes through, you have a good article that also goes through. But that’s a little bit like everything else in life. Some things work, some things don’t work. The important is to continue going and to enjoy the good things that are happening.  Smith: Am I right that when you decided to move from France to Sweden, your dedication and if you like, your obstinacy came out in the fact that you managed to learn Swedish well enough within a year to be teaching in Swedish?  L’Huillier: Yes, I had no choice really, because I was going from a system in France, which was a research institute, no teaching to a university system in Sweden, where it’s important to have teaching merits. To be able to come in the system in the right way, I really had to teach and to accept all the opportunities that were given to me. Unfortunately, what happened is that one of our colleagues here in Lund passed away abruptly. It was teaching laser physics. I was given the opportunity to take this course in Swedish. This was maybe not one year, but two years after I had moved. This was a challenge. I don’t know how my teaching was, but my Swedish really improved during that period.  Smith: Do you feel yourself Swedish or French or a mixture?  L’Huillier: I really feel both. I have a foot in both countries. I have lived half of my life in France, almost same number of years now. I have two sons. One is living in Sweden, one is living in France. I really feel both, and this is one great thing what is happening with the Nobel Prize. Now, this prize is the two countries, say they have gotten a Nobel Prize laureate. And I’m happy that it’s doubled in two different countries. I would say it’s not shared. It is really doubled. That’s nice. This increased the number of invitations. But it’s also very nice, and I’m very touched by this. The attention from Sweden is kind of obvious, but the attention from France was not obvious to me that this would happen. But it does happen very much, and I’m very touched by that, and I am trying to accept those invitations from France.  Smith: It’s nice that again, not competition, but teamwork between countries.  L’Huillier: Exactly. The right term. It’s very nice.  Smith: We’ve mentioned before the lovely international nature of science. Do you think it makes any difference at all where people come from? Do you see different scientific traditions sort of being manifested in the way that people in the lab behave and think? I suppose what I’m asking really is: is it a great positive that you have science coming from different countries where people think a little bit differently? Or is it just one amalgamated way of thinking?  L’Huillier: I like to work with a group where people come from different countries and where they’re both women and men because I do think there are differences. Of course, there are differences between people. You can’t make the difference, he always thinks this way because he’s Spanish, or it’s mostly because this person is this person. But still the fact that the research group is diverse. I think it’s very nice and it’s also very pleasant. This is something I like very much. I also remember that one motivation, with research was actually this meeting people from all the countries and traveling. This is part of the attraction.  Smith: How about language? I remember Leo Zeki saying that he feared a little bit that if science was all done in English, that you’d lose something from a, at least from a Japanese perspective, that there was a Japanese way of thinking that was reflected in the way that the language worked, that somehow translated into the way people thought about science and that you’d lose something if people weren’t thinking scientifically in Japanese. I don’t know whether there’s any truth in that, but it was an interesting perspective.  L’Huillier: Yes, it is. The different languages, they have their own culture, if I can speak like that. For example, I think the French is very precise. You can express yourself, especially feelings much better than in English. But this is the way it is. We have an international language, which is English, and we write in English, and I think it’s great. If this was not the case, would be very complicated. I like very much that we there is no barrier because of the language.  Smith: I can’t resist asking you whether your knowledge of Swedish helps you think differently.  L’Huillier: That’s a good question. I don’t really know. I think I pass on this question, and for me, Swedish is the language of everyday life now, and English is the language of science. I’m using every day the three languages that I can tell you, and especially now with the Nobel Prize. I’m doing lectures in Swedish, in French and English, almost the same amount. This is a very interesting process. I’m talking about the research actually in all three languages.  Smith: First of all, how brilliant! But which do you actually think in, or again, do you think in all three, if you are just reflecting on your own without anybody demanding that you speak this or that language, do you reside in one language primarily?  L’Huillier: Probably French. This is my mother language, but I don’t really know because I am switching so much. It’s not clear when you are in a country something, which language I’m thinking, but in my case, it’s so much switching between the three languages that I don’t even think about it. That’s just automatic.  Smith: It must be a huge advantage. I wonder, do you worry about the fact that the technological tools are making it so easy to translate that young people are not going to bother to learn languages anymore because their phones will do it for them?  L’Huillier: My opinion on that is that this kind of question arose periodically in the human history. What happens when a computer arrive? Does it mean that we can’t calculate anymore with use your head? What happens when we had this transition between what is the name writing machine and the computer? I mean, this was also a fear that this would degrade the ability of human being to do some task probably does, but it is also a progress. Somehow I think the human society is going to of course to make use of this new techniques and translation and artificial intelligence. There will be a learning process, probably we will at least some of us will lose some ability, but we will learn other abilities. I think this is a transition that somehow we will go through, and I think hopefully this will be positive transition.  Smith: It was fascinating that your English, which is utterly fluent, you didn’t quite have the word for typewriter there, because a typewriter is totally obsolescent. Nobody has the need to say the word typewriter at all anymore. It’s something that you put in a museum, and yet it was omnipresent not long ago.  L’Huillier: This is what happened with my English. My English of the everyday life is kind of gone unfortunately.  Smith: It seems to be very much there, not gone. But anyway, let’s talk about the importance of students a little bit. What does the teaching add to the research, if you like?  L’Huillier: For me, it adds a lot in different ways. First, something I want to say about research and teaching as well. For me, research is also the interaction with other people that is so important. This I liked very much. This is something you do with other people and that is terribly important. Now about teaching, to me it’s a balance in my work, because I like what I’m doing research on this higher harmonics at the second parts. But sometime I have questions, what is the interest? Will this be ever interesting for the society? We are using for the most part, taxpayers, money, do I have the right to have fun in my lab doing this research? There is a feedback, but it takes years. I think now we are getting towards a little bit application. I start to see a little bit the feedback that this research can give to the society. Not only increase knowledge, but also application. It takes years and it’s not clear. You have that, but on the other hand, you have teaching where what you do is giving you direct feedback. You see that directly in the eyes of the student when they understand something. Yes, I’m doing something for society. This balance for me, that is very important.  Smith: I can see absolutely how it sustains one. I suppose it perhaps takes us to the point of diversity in science. It’s an extraordinary and awful thing that you are only the fifth woman out of, what is it, 224 different people who’ve been awarded the physics prize and it’s in existence. That indicates that there’s an awfully long way to go without one snapshot. How do you think about promoting and the importance of diversity in physics?  L’Huillier: I think it is important, it is important for women, of course, and it is also important for physics not to miss half of humanity. I feel this very strongly that I have a role that my older men colleagues don’t have, which is to try to inspire the young women, the girls. This is something I’m doing a lot these days to speak in schools, universities, of course, but also in schools about the Nobel Prize with the idea to maybe inspire young women and to tell them that it’s possible to do science, to do research.  Smith: What do you think is, it’s obviously the $64,000 question, what is the barrier that has so far made it so difficult? Obviously there are many barriers, but what would you highlight?  L’Huillier: If I knew the answer to this question, I would be very happy. I don’t really know. I think one is the lack of models, probably another reason is the stereotypes to do the same as the others in my category, something like this, which is similar actually, not quite, but similar idea with model. Then of course, you have more practical issues like, it’s a difficult career, especially at the age of around 30 where you should get permanent position. And this is the age where normally you can get children. This is a bit of a problem. This can be, of course, in some countries really help, like I think in Sweden, there is a very good system for that. The society and the school and kindergarten systems. This is more a kind of practical issues, of course, but which of course play a role.  Smith: All of that has to be put into place. The childcare, the support systems, the idea of basically, I suppose that you don’t have to dedicate a hundred percent of your life to being there doing the science at that period in time. How do you also get over the perceptual barrier that it’s a male dominated world?  L’Huillier: Having more role models? This is what I said in my speech at the Nobel Banquet. I think things are changing. I can see during the 40 years of my career that things are different. That there are more women now in the research groups, that there are more women taking physics, university studies. This is a change that will take time. I think we should not be too impatient. We should not force quotas. It has to be a little bit natural process, of course, help by awareness, maybe by some programs. I don’t know. It’ll take some time, but I think it is ongoing, at least in what I know Europe and United States. I can’t say too much about Asia and Africa.  Smith: I suppose you were given a very broad and interesting perspective on worldwide physics from being on the Nobel Committee, which were for a number of years, was it from 2007 to 15. That must give you an extraordinary insight because you see all the nominations, you see all the physics that people out there thinking is worthy of this particular recognition, and it must expose you to a bewildering and wonderful variety of people and ideas.  L’Huillier: Yes, it was a very interesting task to serve on the committee, but I can’t tell you obviously.  Smith: No, you can’t tell me anything about it. I’m fully aware of that. But it is notable, for instance, that for that period that you served, there were no female laureates in physics. I just wonder whether, I suppose you can’t even talk about that. It’s completely off the table. But it’s interesting to know whether you see whether that backs up this idea that there’s a change going on, that there are more females coming through.  L’Huillier: Yes, one thing I can say is that the Nobel Prize is looking back in time a little bit by definition. That’s because this requirement by Alfred Nobel that this has to be for the benefit of humankind. Often when you have a discovery like hio harmonic generation, it takes 36 years to get to a point where, yes, you can see that this is useful for humankind or can be useful for humans. Because of that, the committees is, sometime this is not the case when you have, for example, the gravitational waves, this goes very fast, but, most of the time it’s really looking back how the situation was 40 years ago. I think the situation has really changed the last 40 years. I’m saying that, not as a previous committee member, but really as a researcher simply.  Smith: Do you feel that the situation for science in general is looking bright?  L’Huillier: I really don’t know the answer to this question. I think it is, but my personality is optimistic, and I also a strong believer of science. I think science can solve many problems.  Smith: Your own research is a beautiful example of a totally unexpected finding leading to application. I suppose that you can’t necessarily go after the solution because the solution may come from totally unexpected places. You have to go after science in general and be opportunistic and aware, and have the right intuition to spot what’s interesting.  L’Huillier: Yes, and this is why it’s very important that the basic science can continue, because I think the great discoveries in science almost always come from unexpected areas. This is why it’s terribly important too, that there is a continuation in funding basic science by the politics.  Smith: Which places an enormous amount of trust in science. People really have to believe yes, that it’s going to work. That’s why it’s so important that people can understand that and have the patience and optimism that scientists themselves have. I love this word, optimism. You keep coming back to what, as an optimistic person, do you think optimism is a very important personal characteristic for a scientist?  L’Huillier: Maybe not so much optimism by obstinacy. I think that that is very important.  Smith: Would your family call you obstinate as well?  L’Huillier: Yes.  Smith: It strikes me that you are not somebody who particularly likes talking about yourself.  L’Huillier: No and this Nobel Prize is putting me in, what do you say in English?  Smith: The spotlight.  L’Huillier: In the spotlight, exactly. And this is a little bit against my personality. I’m forced to be someone that maybe I am not naturally, which I accept, let’s put it this way. But hopefully it’s not during too much time.  Smith: Absolutely. No, I can see that it interferes. Being reluctantly famous is difficult.  L’Huillier: I’m not complaining. As I said, I accept it and I see the, not the advantage, but the consequence. This is why I’m taking very seriously this, not role model, but as an inspiration to the younger generation. But it’s not what something I would’ve applied to, if you see what I mean. I rather like to do my teaching and to do my research, write a book. This is what I want to do, and now I’m doing something else, which is this kind of outreach activities because of all of this attention because of the Nobel Prize.  Smith: I like that since you are cast into this role of being a role model, it’s very apparent that the role does not involve seeking for fame. It’s a very serious role. You approach this with a great degree, as you say, of obstinacy and seriousness. That’s what you have to be to get things done. What’s the book going to be about when people like me leave you alone and you have time to write it?  L’Huillier: I want to write a textbook for beginning students, PhD students. It’s still a scientific book, but my idea is to write a textbook because I think I’ve learned so much about hi harmonics and add to second parts. We have developed simple models, in many aspects of it. I would like to write this down. So hopefully when all of this Nobel wave is over, I can find time to write this book.  Smith: I very much hope you do. What language do you write in when you write?  L’Huillier: In English.  Smith: Thank you very much for allowing me to steal this period of time with you. Anyway, it’s been a joy to talk to you. Thank you.  L’Huillier: Thank you. Bye-Bye.  *MUSIC*  Brilliant: You just heard Nobel Prize Conversations. If you’d like to learn more about Anne L’Huillier, you can go to nobelprize.org where you’ll find a wealth of information about the prizes and the people behind the discoveries. |
| **Telephone**  **interview** | **0005 = AL**  Anne L’Huillier: Hej?  Adam Smith: Hello, this is Adam Smith calling from nobelprize.org, am I speaking with Anne L’Huillier?  AL: Yes, but I am a little bit busy now.  AS: I can well imagine, and I promise not to keep you on the phone for more than two or three minutes.  AL: Ok.  AS: Thank you very, very much! Of course, many congratulations.  AL: Thank you very much!  AS: I gather that you were teaching when you heard, and I just wanted to know what your students thought of this news?  AL: I think they were very happy,  AS: Of course.  AL: It was really fun, let’s put it this way!  AS: It must have changed the lesson.  AL: Yes, but I tried to, I continued my lecture.  AS: That shows tremendous presence of mind. I gather that it must all be a fairly overwhelming thing to happen, and you’re deluged. But I suppose, I mean, I recall that you were on the Nobel Committee until what, 2015, so you know all the behind the scenes stuff, you know what led to this. Does that, in some way, make it even more special that you know how…  AL: Yes, yes, it definitely does, and I know what it is to get a Nobel Prize, it’s extremely difficult, and I know the work behind, which is done by the committee, so I am very, very grateful.  AS: Of course, may I just ask you about the, you know, just about the thrill of studying these sort of secret sides of light, the overtone you’re able to create. It was almost forty years ago, I guess, that you were switched on to this. What is it that makes it just so thrilling to study?  AL: This is a basic research, I don’t know, this was new, it was not expected, and not predicted. The understanding took some time, took several years, so it was very interesting to study and to try to understand more and then later on, many years after, look for applications and explore new things with it.  AS: It seems such a wonderful combination of the head and the hands, if you like, the complexity of the theory and the practicality of producing these exquisitely sensitive pieces of apparatus.  AL: Yes, I mean this is what makes the field so, so exciting, and even now, thirty years afterwards, we are still learning new things, we are still trying to improve the process in order for some applications, so its complex physics but that makes it very, very interesting.  AS: Of course, thank you very much indeed, and thank so much for speaking to me in the midst of all this. Congratulations again.  AL: Thank you!  AS: Bye bye!  AL: Bye! |
| **Interview** |  |
| Q3 | **What inspired your passion for science?** |
|  | Anne L’Huillier: I don’t know exactly, it’s a very long time ago, but I was very much inspired by my grandfather on my mother’s side, who was a researcher and teacher in radio. During the World War II, he was working with the resistance movement, and he was helping them with communication. I don’t remember him very much because he died when I was four, but his story has inspired me very much, and he wrote many books that were at home. It inspires me. Then there is also something that I remember very clearly that was important for me, to watch the people walking on the moon. This was middle of the night, I was 10-year-old and it was very impressive. This I remember, and it kind of motivated me to go towards science and technique. |
| Q3 | **What do you enjoy about science?** |
|  | Anne L’Huillier: I enjoy doing research because you learn new things all the time. This I like very much. I also enjoy teaching science, teaching young people. These both aspects, learning new things and teaching others, that motivates me very much. |
| Q5 | **Was there a teacher or a mentor that really influenced your scientific career?** |
|  | Anne L’Huillier: Yes, several teachers have definitely influenced my scientific career, a mathematics professor at high school and then a little bit later, when I studied at university, very good teachers in physics, in especially atomic physics, inspired me very much, so I think that’s very important. I am thinking of [Claude Cohen-Tannoudji](https://www.nobelprize.org/prizes/physics/1997/cohen-tannoudji/facts/) and [Serge Haroche](https://www.nobelprize.org/prizes/physics/2012/haroche/facts/) and both got Nobel Prizes afterwards. They inspired me in their way of teaching, which is quite rigorous, but very pedagogical, and also to do both maybe experiment and theory. This little bit French school inspired me in my way to be a researcher and teacher. |
| Q1 | **How do you want your students to see you as a teacher?** |
|  | Anne L’Huillier: This I don’t know. I just would like them to become enthusiastic with what I am teaching them, with the physics. This is maybe my motivation. Of course, they should learn, but they should also become like what they hear and become interested and enthusiastic. This is my aim. |
| Q9 | **How did you feel when you found out about the award?** |
|  | Anne L’Huillier: When I received the telephone call not very much because at that time, I was struggling between what should I do, because I had my class waiting for me after the paus, and the Nobel Committee and the Nobel organisation were asking me to stay on the phone. I was a little bit struggling, what should I do? Then I said, No, I want to go back to my class. I agreed with them that I would finish the class 15 minutes earlier so that I could be part of the press conference. This is what I did, but they agreed to that. I think the whole thing with students became very, very nice and it just happened like that. That is nothing we planned obviously. This was very nice because somehow these students – and it was many students, about a hundred – became part of my Nobel Prize, if you see what I mean. The day afterwards, I also had a lecture. I started lecturing as usual, but then during the pause, again they came with an enormous flower … What is that in English? Flower bouquet, and they said, You have gotten many flowers, but we hope this is the largest. And it actually was. It was so nice from them, it was very, very touching, so for me the student response to that is going to stay in my memory forever. |
| Q18 | **Do you think it’s important that science is an international discipline?** |
|  | Anne L’Huillier: Yes, it is. Absolutely and it’s remarkable that the oldest three physics laureates, we have all moved. For me, it’s from France to Sweden, and my colleague Pierre Agostini from France to the United States, Ferenc Krausz, it’s Hungary, Austria, Germany. This really shows that science is international and open, and I think this is extremely important. I think it influenced my life very much. I cannot really say what is the difference compared to if I had stayed, but to move country in general is making your life kind of richer. For me, it was a difficult decision on the professional point of view when I moved. But in the end, it became so good in looking at the facit. It was a very good decision, even on professional point of view, to move. I would encourage the young people not to be afraid to do that. |
| Q2 | **When failure strikes, what do you do to move forward?** |
|  | Anne L’Huillier: When you fail, it’s about being obstinate and to continue. This is a quality that you need to have if you want to do research, to be obstinate and to never give up. That’s essential. |
| Q2 | **What advice do you give to your students about failure?** |
|  | Anne L’Huillier: I’m just trying to tell them that the normal thing is failure. It’s not, success it is failure. I’m trying to show them examples and I guess this is how it is, but at the same time, I’m trying to motivate them and to say, Okay, but maybe this did not completely fail, you have this result that still is the coming through. There is nothing black and white, so even if something’s not exactly what you want, but maybe this is also interesting. That’s what I’m trying to do. |
| Q10 | **How do you support the students in your lab?** |
|  | Anne L’Huillier: First of all, the way we do research in my group, it’s never one student on a project. It’s always a group of students. The basic idea is that the little bit senior PhD student is teaching the junior PhD students. I’m trying to … so that everyone does not feel inadequate. For example, the first year you are a PhD student, I’m telling the student I don’t have any expectation from you, just learn, try to learn how it works. I don’t expect any result, just learn how things work. I’m trying to reduce the pressure on all of the students so that they feel that they learn and they come up to speed and try to make them feel good. |
| Q1 | **What advice would you give to a student or young researcher?** |
|  | Anne L’Huillier: This is a difficult question. I’m going to answer a little bit differently. What I’m trying to get from my students is actually a motivation from them. I think that a successful scientist, whether it’s in academia or industry, should have the motivation by himself or herself. This is what I’m trying to get from the students, that they are motivated for what they’re doing. This needs a little bit of time and care, so I’m not, I’m never pressing my students for example to work more, it should come from themselves. So that’s the idea. |
| Q7 | **What qualities make a good scientist?** |
|  | Anne L’Huillier: I think you, again, you need to be motivated and to be a little bit passionate, I think that’s important. I think you need to be obstinate, it is also very important, not to give up because something fails or because you don’t know where it’s going. Then I think for me science is not only working with machines and gas or lasers, it’s working with people. To build up a good research group and to have a good relation with your colleagues and the students. This is also very important. Finally, I believe that in communication, teaching and research goes hand in hand. I think it’s important that the researcher can teach what they are doing and this communication with students is important. |
| Q7 | **What skill has been particularly important to your career?** |
|  | Anne L’Huillier: This is the thing I like to use, and that’s the word intuition. To be a scientist is about following my intuition. I think for me, to follow my intuition has been something I have done the whole life without having real reasons or a scientific reason, maybe from when I was a child. Why did I do science? Maybe I was motivated, but I followed my intuition. This is maybe a little bit with this obstination, why I was obstinate to do that. I followed my intuition. Why did I continue to look at harmonic generation although my boss at the time was telling me, Oh, you should do something else, this is not leading anywhere. It’s not that I had a vision that, Oh, it’ll lead to attosecond pulses, or, This is very interesting. It’s not, because I was not finished with doing research, and I was following my intuition. This is something I would like to tell the young people: follow your intuition in life. |
| Q17 | **Do you prefer research or teaching?** |
|  | Anne L’Huillier: I like both, it depends a little bit. I like research very much, but I like teaching very much, I could not do only one. Moving from France to Sweden also meant that I changed from a research institute with no teaching to a university with teaching, and this was a discovery. It was really wow, so fun it is to teach. I think it’s about communication, to communicate your enthusiasm to students, and it could be also to other researchers. This is something I like to do very much. Research of course requires all the skill, which is to go to the depths of something, to be able to analyze new experiments in the right way, to have ideas – maybe when you see something a bit new, different, to realize that this might be important. Research of course requires different skills. |
| Q11 | **Why is diversity in science important?** |
|  | Anne L’Huillier: I think it’s very important. First of all I would like to stress that very often new ideas don’t come. Of course, you can have your own ideas that pop up, but very often it’s a group, it’s within a group discussion. You have people discussing, and then someone says something and another one get an idea and say something, a third person is kind of putting two things together. Often, and I have seen that many times, it’s a group discussion, and the more diverse this group is, the better. I’m coming to diversity here. I think here, it’s nice if you have people with different backgrounds that contribute to this process, with different educations and different backgrounds. Then when you do research, to have a good atmosphere in the group is extremely important. I think it is always better if you have a diverse group with people coming from different countries, different genders. This really helps to have a more dynamical group where people are happy to be in that group and to contribute and to do great research. So there I think diversity is important. |
| Q8 | **How do you like to spend your free time?** |
|  | Anne L’Huillier: First, I work quite a lot, I can say that. What I’m doing in my free time is I am doing some tennis. I’m trying to get active. This is now, but a few years ago, I was at the family, my sons, at home, To have a family and, and taking care of the family was a big free time activity. I think to be a good teacher and scientist, you need to have a good balanced life. I don’t really believe in scientists just working all the time or sitting in the attic and just working. I think this is an image you find in books. I think it’s good to have a balanced life with a family, with a little bit of free time activities. |
| Q10 | **Are there certain environments that you think encourage creative thinking?** |
|  | Anne L’Huillier: Yes, absolutely. This is what I’ve been trying to describe. You need to have a good social atmosphere in the research group and good discussions where people dare to express what they think, even if it’s maybe stupid, but then they express what they say, and no one is telling them, No, no, this is wrong. You need to have this freedom in the research group, and this is where creativity can come. |
| Q2 | **How do you maintain your own creativity?** |
|  | Anne L’Huillier: This is a good question. For me, creativity is associated to interest and passion. If what we are doing is do we have new results that we need to understand, and it seems really interesting and fun to understand these results – this is where creativity is coming. For me, as long as I feel that what I’m doing is fascinating, interesting, I think I will still have a little bit of creativity to do research. I think this is the case for everyone. It’s not like you are creative, it’s like, Oh, this is very interesting, fascinating, I want to understand. This is where creativity is coming. That’s my interpretation. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0006** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0006 = Aspect**  Alain Aspect: People say, “Why are you wasting your time in doing that?” And I say, ”Do you give me 10 minutes or 15 minutes?” Yes. And in 15 minutes, I could convince a normal physicist that it was an interesting problem.  Adam Smith: I can’t guarantee that listening to this conversation with Alan Aspect is going to bring you a total understanding of the strange quantum mechanical world that he’s been investigating. When you use normal language to describe it, it’s just so crazy. The idea that two particles so widely separated in space that they can’t possibly be communicating with each other but are somehow linked just sounds like some kind of magical force from elsewhere. The system that he’s talking about is one that is only understood within the mathematics, the formalism of quantum mechanics. There’s always a disconnect between those who do understand quantum mechanics and those who don’t in such a conversation. But I do think Alan Aspect is so good at explaining what he does and such a good teacher that listening to him does give you a very good flavour of what it’s like to do research at the cutting edge of quantum mechanics.  Clare Brilliant: This is Nobel Prize Conversations, and our guest is Alain Aspect, recipient of the 2022 Nobel Prize in Physics. He was awarded for his experiments with entangled photons, establishing the violation of Bell inequalities and pioneering quantum information science. He shared the prize with John Clauser and Anton Zeilinger.  Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces.  Alain Aspect is a professor at the Institut d’Optique Graduate School and the École Polytechnique at Université Paris-Saclay, as well as a research professor emeritus at CNRS, The French National Centre for Scientific Research.  In the conversation you’ll hear him talk about his love of teaching, the joy of pioneering a field which many physicists used to dismiss at ”crackpot science”, and how to cook the perfect egg.  But we start, with the end.  Smith: I wanted to start by exploring this amazing journey that for you began with an idea in 1974. There was a dramatically successful experiment in 1982. Then 40 years later, you were in Stockholm receiving a Nobel Prize. If we just jump right to the end and think about how it felt to be standing in the City Hall in Stockholm, giving your banquet speech to the assembled 1300 people.  *CLIP from the presentation of the speech*  Smith: What did it feel like getting to the end of a very long journey?  Aspect: I think the feeling was the same as when I had to stand up to receive the medal from the hands of the king, having a thought to the young Alain Aspect opening a file in which there was the paper by John Bell. Being absolutely amazed by that paper. My friend Nicolas Gisin uses ”love at first sight”, the expression love at first sight. I think it’s exactly what happened. After one hour, I knew I wanted to work on that subject.  Smith: How lovely to find your true love. So young. So early. Let’s begin to explore this so that the file that was handed to you. This was while you were trying to sort out what you might be doing for your PhD.  Aspect: My situation was quite nice because I had a teaching position. I could do my research in any lab. I went from one lab to the next one asking for an interesting subject. At Institut d’Optique, there had been an experiment on so called one photon and difference experiment. I knew that that the professor who became my advisor, had been involved in this experiment. I asked him, would you have something of that kind? He gave me that file in which the first paper was John Bell’s paper. There was the thesis of Stuart Friedman and the thesis of Dick Holt. I first read the paper of Bell, I found that it was absolutely the subject I wanted to work on. There was a debate between [Bohr](https://www.nobelprize.org/prizes/physics/1922/bohr/facts/) and [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) in 1935. Einstein, with his colleagues Podolsky and Rosen, considered the situation with two particles and realising that the formalism, the mathematical formalism of quantum physics allows for this so-called entangled state. From this, he concluded that quantum mechanics should be completed. Bohr disagreed on that. There was a debate between them until their death.  Brilliant: Adam, I’d like to ask you about this debate, but first, is there a simple way to describe quantum mechanics?  Smith: The American physicist [Richard Feynman](https://www.nobelprize.org/prizes/physics/1965/feynman/facts/) famously said in 1965, “I think I can safely say that nobody understands quantum mechanics”. That’s a worrying start, but yes, quantum mechanics is the branch of physics that describes the interaction and behaviour of particles on the atomic and subatomic scale. So classical physics, classical mechanics as developed by Newton, for instance, describes very well the behaviour of things on the scale in which we live. Motions of planets and the way things behave when you drop them in gravity. It breaks down when you try and use it to describe the way that really small things interact like electrons and protons. For that, you need this new branch of physics, new at the beginning of the 20th century and still being developed – quantum mechanics.  Brilliant: The Nobel Prize was related to quantum entanglement. What is quantum entanglement, Adam?  Smith: The sad truth is probably that you can’t really talk about quantum entanglement unless you properly understand quantum mechanics. In the terms that I understand, at least, quantum entanglement is a phenomenon derived from quantum mechanics in which two particles can be separated by any amount of space, vast distances, and yet their properties are still linked so that any change in the properties of one will produce an instantaneous change in the property of the other. That is contrary to all we know about the universe because we understand that nothing can travel faster than the speed of light. Yet such apparent communication between two particles instantaneously would necessitate the transfer of information in a way that must travel faster than the speed of light. It seems paradoxical that this can happen, but from a quantum mechanical point of view, it’s perfectly fine. People like to say that quantum mechanics doesn’t care about space and time. This is a prime example of that.  Brilliant: But Einstein wasn’t happy with that. He felt that there must be something missing from quantum mechanics. Is that right?  Smith: Yes. For Einstein, this was a step too far. He, together with colleagues Podolsky and Rosen wrote a paper in 1935 in which he thought that what he’s termed this “spooky action at a distance” was not sufficiently explained by quantum mechanics and that quantum mechanics was somehow incomplete because it didn’t explain the phenomenon of entanglement. Others disagreed with him. Bohr, for instance, thought that quantum mechanics was perfectly sufficient to explain quantum entanglement.  Brilliant: Bohr and Einstein disagreed on this quite fundamental point related to quantum mechanics. Then a scientist called Bell came along a few years later and wrote a very important paper. What was Bell’s paper about?  Smith: That’s right. The debate happened in the mid 30s and then sort of just lay there in the record. In 1965, Bell published the first of his inequalities, which were mathematical proofs designed to test the thought experiment that Einstein had laid out in his 1935 paper.  Brilliant: A few years later, the young Alain Aspect came along and read Bell’s paper. What did he do as a consequence of that?  Smith: He and others, such as notably John Clauser, one of his co-laureates, saw Bell’s inequalities and realised that they laid a path towards experiments which could actually test whether quantum entanglement was a real phenomenon. They were tremendously excited by this. They could see that an experiment was possible. They set out on a path of experimentation, which after much development intellectually and technically, led, eventually, to Alain Aspect’s definitive experiment proving the existence of quantum entanglement.  Brilliant: This helped to settle the debate?  Smith: Yes, these experiments basically put the debate to rest. It’s very interesting to listen to Alain Aspect talk about the significance of the debate. Let’s listen to him.  Aspect: Mostly nobody cared for several good reasons, I think. The most important reason is that it was only about interpretation. If you are a young physicist, active, you use quantum mechanics, it works. It allows you to explain experiments or to discover new theoretical features. Why would you care about an epistemological debate between these two old glories. The real role of Bell is to have discovered that it is not only a matter of interpretation, not only a matter of epistemology, but that you can settle it. This is absolutely fantastic. I don’t know any other instance in the evolution of ideas, where a debate looking like a philosophical debate can be settled by an experiment.  Smith: I begin to understand why you fell in love with this possibility.  Aspect: I was absolutely struck. Now you have to realise that I was mostly impressed by Einstein. In my in my head, I am an Einsteinian. On the other hand, I understood the quantum mechanical formalism. For me, it was a totally open question, because I knew of the success of quantum mechanics. I could not imagine how Einstein could be wrong.  Smith: When you say you understood quantum mechanical formalism, that understanding, I remember you mentioning when we spoke on the day that you’d been it was announced you received the prize, that understanding actually came in a strange way that you worked on it while you were a teacher in Cameroon.  Aspect: Exactly. The point is that I am old enough that during my studies, I have an excellent classical physics education. But I must say that my education in quantum mechanics was extremely poor. It was not clear at all to me, what was the relationship between what they were teaching me and physics. I was frustrated of that. I knew that it was important. I was frustrated not to know it well. When the book of [Claude Cohen-Tannoudji](https://www.nobelprize.org/prizes/physics/1997/cohen-tannoudji/facts/), Bernard Diu and Franck Laloë was published, I was indeed in Cameroon teaching, I got the book immediately. I really studied the book from page one to page 1300 or something like that. There is a big advantage of doing that. The book is totally neutral regarding interpretation. I’m sure that if you have a professor saying, well, this is not possible. Shut up and calculate. Shut up and learn. Then we will discuss later. But there was nothing like Shut up and accepted. It was just this formalism, and use it, and nothing else. I think it was very important. I have nobody to wash my brain, telling me that it was not interesting to think about this stuff.  Smith: It obviously gave you great clarity of thought.  Aspect: The book is fantastic. It’s known in the world, and it deserves it. I had a good mathematical education. It means that no mathematical formalism in the book was difficult for me.  Smith: It’s quite rare to take time on your own to study in that way. It’s obviously incredibly beneficial in your case. But it’s hard to achieve that such space to think through something like that. You have to be quite disciplined under normal circumstances to do it. Certainly then when you were teaching in Cameroon, you had enough to be doing I’m sure.  Aspect: Yes, but on the other hand, when you were teaching overseas, as I was, you are cut off almost everywhere. There was no TV, nothing else to do than reading. No family to go with during vacation. In fact, there was plenty of time of free time. Moreover between 12 and 4pm it was quite hot. It was better to stay home to wait for the heat to decrease. It was a moment when I was studying.  Smith: Yes, some people might have just taken a little nap, a siesta.  Aspect: Yes, but I was young at that time. I did not need any nap.  Smith: Anyway, marvelous preparation for encountering Bell’s paper in 1974. Of course, it was a risky thing to undertake. It was a risky project. You told the story beautifully at the Nobel banquet of your first encounter with Bell at CERN.  *CLIP from the banquet speech*  Aspect: I said, why are you asking this question? Then he tells me, oh, you know, doing this kind of subject, people are going to think that you are a crackpot. I did not know the word. I asked him to explain the word to me. He said, you know, most people think that the subject is not interesting. That one has to be crazy to embark into that. I said, but I have a permanent position, they cannot fire me. I have a job, I am teaching. I can spend my time of research as I want. He said, Okay, let us do science. Then he encouraged me. A few months later, I published the idea. It was with the recommendation of John Bell. When I visited John Bell, I asked him about publishing the idea. He told me that I should absolutely publish it. It was a very good advice, because nobody has any doubt that it was my idea. I am the only author of the idea.  Smith: That’s very clever. You weren’t at all worried about anybody else going off and trying to do the experiment?  Aspect: No, no, I was not worried, I was quite young. I was eager to receive advice. When I asked John Bell, should I publish it? He was very clear. He told me, yes, in fundamental science, you have to publish everything. Don’t count upon me to keep it secret. You tell me your idea, I will speak. It’s a good idea. I will speak about it. You better publish it.  Smith: John Bell must have been thrilled to encounter a young enthusiastic, talented person who wanted to take this on.  Aspect: John Bell was a cool person, not showing much feelings or it’s not to say that he was not a nice person, but he was not warm. He was very serious. Was he thrilled? No idea. What I can say is that we met quite often until the result of the experiment. He was part of the committee of my PhD thesis when I defended my PhD thesis. He always showed that he had a high respect for experimentalists. He clearly stated that he was unhappy of the result, but he did not discuss, the result is what it is. He would have preferred another result, but he had absolutely no nit-picking about details of the experiment. He was totally trusting experimentalists.  Smith: Your PhD defense was in 1983. The 82 experiment, which is the experiment cited in the by the Nobel Prize committee – it was a part of your PhD and it’s quite unusual to do work during your PhD, which results in a Nobel Prize in such a ground-breaking discovery. It must have been quite a thesis defense, because of course, it was pretty apparent just how groundbreakingly important this work was.  Aspect: Yes, the point is that the wind had been changing direction. Until 1978, 79, there was still this feeling that, oh, this guy is a crackpot. This is a guy who is checking quantum mechanics. Everybody knows that quantum mechanics works. Everybody knows that Bohr replies satisfactorily to Einstein, etc. But I must say, and I think that I need to have some credit for that. I found a way to explain what the goal is of Bell’s inequality. Explain that not to experts in hidden variables, but to physicists totally naive with respect to this question. People say, “Why are you wasting your time in doing that?” And I say, “But do you know what is the question?” They say, “No”. ”Do you give me 10 minutes or 15 minutes?” Yes. In 15 minutes, I could convince a ”normal”  physicist that it was an interesting problem. You know what happens? You are invited for a seminar. There is a one-dozen-of-person. But if your seminar is good and clear, there are people in the room that will invite. It’s just an exponential growth. In 1980, there were many people who would realise that it was an interesting subject and were inviting me. When I defended my PhD thesis, yes, the big auditorium of Institut d’Optique could not accommodate all the people who wanted to enter. There was no video at that time. The people were out.  Smith: That was quite something to miss. It’s very interesting because it speaks to the value of being able to convey ideas.  Aspect: Absolutely. In a sense, I think that part of my Nobel Prize is due to my ability to convey and to explain things. I am a professor. I am fundamentally a professor. I love teaching and explaining difficult things.  Smith: That’s lovely to hear because so often people see teaching as being in conflict with their research. It gets in the way. It means they haven’t got time, but obviously not here.  Aspect: No, another Nobel Prize laureate close to my building, Albert Fert, said exactly the same thing. ”It is by when preparing my courses that sometime I got to think deeply to the subject”. It’s the same for me. When I prepare a course, I think about the most difficult question that the most clever student could ask me. What should I reply? This allows you to go in depth in the subjects.  Smith: What a lovely approach. It must be nice sometimes to get surprised by people who ask questions that are even cleverer than that.  Aspect: Yes.  Smith: Bell took the question out of the philosophical realm into the practical realm, if you like, if you take, go back into the philosophical realm, what you showed is going on does seem like magic. It’s that two things that are separated by vast amounts of space can be influenced apparently at the same time in the same way. It is hard to accept if you just think about the universe as we know it.  Aspect: We have to distinguish the scientific or logical conclusion that we can draw and the kind of interpretation and image that we give of the conclusion. The conclusion is that the local realist description does not work. There are two important notions, locality, the fact that everything you can measure on one object in a given region of space time, everything is determined by properties, parameters, which are in that volume. This is local realism. There is a fact that nothing can go faster than light. I think that what the experiments show is that we have to reject both realism and locality. Then deciding that you could reject one or the other one is a matter of personal decision, I would say. I accept the idea that something goes faster than light. But I know that this is nothing more than an image to support my intuition. What I found is that this image is extremely fruitful.  Brilliant: I think I might need some more help here, Adam. What does he mean when he says “rejecting one or the other is a matter of personal decision”?  Smith: I think what he means is that it’s very hard to give up on beliefs that you’ve held all your life. In order to accept quantum entanglement, you have to drop the idea of locality which is the phenomenon that things happen because of local influences. If I push you, you’ll fall over. That’s how the world works – we know that. Everything is dictated by what’s happening in the *local environment* and by realism I think he means the relativistic universe. In other words, a place where nothing can travel faster than the speed of light. In quantum entanglement two particles are apparently acting on each other – or interacting – over distances which mean that you’re totally outside the local environment. Those influences are happening in a very non-local way and they’re also happening at speeds which indicate that something is transferring between them at much faster than the speed of light. If that’s happening you then have to drop the idea of local influences and you have to drop the idea of a universe governed by relativity. Obviously as far as we know nothing can travel as fast than the speed of light and local influences still go on. It’s not that these things are really totally incompatible it’s just that in order to imagine quantum entanglement and to accept quantum entanglement you have to have a mental framework which rejects locality and, as he says, realism. Whether you can do that is up to you. In his case he can. He can inhabit these different worlds but I suppose that’s because he understands quantum mechanics and so it all makes sense to him. It’s a bit of a leap of faith I suppose on his part to say, “Yes, I accept quantum mechanics and I drop these other two principles for the moment”.  Brilliant: Clearly he’s not the only one who’s made that leap of faith because there are now many people trying to work out real-world applications that are a consequence of being able to accept quantum entanglement and therefore quantum mechanics.  Smith: Absolutely and I think people would probably be very cross with me for using the word leap of faith because it’s not a leap of faith, it’s a leap of fact I suppose.  Brilliant: Or acceptance.  Smith: It’s acceptance, yes. It’s a crossover between the science and the sort of the philosophy of the science. All of that comes down to absolute real-world applications like quantum cryptography which is only possible because you do have quantum entanglement because you can have information existing in two places at once instantaneously linked so that in theory you could have a particle code something at one location and decode it at another. There is no need for you, the operator, to be sending anything between the two and that is the basis of this new science of quantum cryptography.  Brilliant: Is this something that people are working on right now?  Smith: Working on very actively just like quantum computing. I don’t think it’s the case that people know exactly how it’s going to turn out, or how it’s going to happen, but they are certain it will be able to work. In that way, it’s a bit like Alain Aspect’s own reaction to seeing Bell’s inequalities that he was I think certain that you could do an experiment that would put quantum entanglement to the test. Didn’t really know how that was going to happen yet but was sure that if you set off on the path towards it you’d get there and that’s very much how the world is reacting to quantum computing that there will definitely be quantum computers but I don’t think anybody can tell you definitively yet how they’re going to work. It’s interesting to listen to Alain Aspect himself talk about one of the applications of quantum entanglement.  Aspect: I have an example, which is of a big interest in the famous quantum teleportation, which is a big issue, for instance, for linking one quantum computer with another one and transferring quantum information at a distance. When I think of the non-locality of entanglement, I immediately conclude that we absolutely need a good quantum memory if we want the scheme to work. Until the moment when we have a good quantum memory, the teleportation will just be true for principle, but not more than that. Once again, I can see that immediately by looking at the experiment and accepting the idea of a non-locality, then I am not a philosopher. I don’t know if the world is non-local or not. At least this image is useful for me.  Smith: It’s very pragmatic, if you like.  Aspect: Yes. I have no choice.  Smith: Yes. The way you think of it, and the way that physicists think of it, is not in conflict with  relativity. Nobody’s saying that you can transfer information, transfer anything faster than the speed of light.  Aspect: There is something quite subtle about that. When you try to use the scheme to transmit information faster than light, you discover that the impossibility is strongly linked to the fundamental randomness of quantum mechanics. In a sense, it comforts Bohr position, which was quantum mechanics as its self-consistency. If you add anything, then it loses its self-consistency. I would say it comforts the self-consistency of the formalism, which I am sad in a sense, because I love so much Einstein, but I have to accept the result.  Smith: You could live with a foot in both camps, obviously, it’s possible.  Aspect: The same year, I got the Einstein medal and the Niels Bohr medal.  Smith: Okay, that’s a very clear demonstration of being of both parties.  Aspect: Exactly.  Aspect: I think the real basis for my trust and love in science, and when I say science at that time, when I was a kid, for me, it was science and technology. For me, it was the same thing. I did not have any distinction between a fundamental science and technology. I think that this belief in the value of science and technology goes back to the elementary school teacher. Remember that it was sometime after World War II, I was born in ’47, and it was a time of progress, more comfort in houses, more vaccination and this teacher conveyed the feeling that thanks to medical doctors, engineers, our situation was improving all the time. Second point, this elementary school teacher would do some small experiments in the classroom, and I was always fascinated. When I think about it, I think that the profound reason why I was fascinated was the following: When you see this, it seems mysterious or strange or amazing, but then the teacher tells you an explanation, a rational explanation, and I think this is my way of liking science. I like to have an effect, which is surprising, and then I want to find an explanation for that effect. I think that the first people who influenced me were these elementary school teachers, and I must say that it was at the same moment that I was reading the book of Jules Verne, *L’île Misterieuse* and these of course were reinforcing me in the idea that with science and engineering, you can win everything, you can build a new world.  *CLIP with reading from L’Île Mystérieuse:*  Smith instructed his companions in everything, and especially explained to them the practical applications of science. The colonists had no library at their disposal, but the engineer was a book, always ready, always open at the wished-for page. A book which answered their every question, and one which they often read. Thus the time passed, and these brave man had no fear for the future.  Smith: This is a story of a group of engineers and scientists who plummet from a balloon down onto an uninhabited island, and through the use of science, they survive.  Aspect: There are some things which fascinated me. For instance, one of the find a grain of wheat. He says, “Oh, we are going to have a bread with that,” and the other one says, “Hey, you are crazy,” and then he explains the exponential growth. He says, “If this one gives us, then after two or three generations, we have enough wheat to produce our own bread.” There are plenty of notations like that based on the understanding, then you will be able to do something.  Smith: Absolutely. A recent example of that sort of application of science is this book and the film *The Martian*, which people have liked very much, where this astronaut finds himself marooned on Mars and survives through the application of science.  Aspect: Yes.  *CLIP from The Martian*  Aspect: I think that the result of that was that when I arrived in high school, I was eager to learn physics. Unfortunately, at that time, we had the first course on physics only at the age of 15. In the first year of the high school, we had only math and biology, geology, but not physics and chemistry. I was eager, I was waiting for it. I think it is as a result of the small experiment at the elementary school. I was lucky enough to have this fantastic professor that I named several times, Maurice Hirsch, who was absolutely exceptional. He gave me the basis of my culture in physics that I have kept all my life, which is when you want to describe things, you have to use enough mathematics to do it well, but not more. You must understand at which point you must make an approximation. It’s no longer necessary to be rigorous. This I learned at high school.  Smith: Your own experimentation is infinitely intricate and requires endless tweaking and a lot of playing around in the lab. How does that translate into quantum computers that we hope to see around us?  Aspect: There are several aspects to that question. The first thing I want to emphasise is that for me, I defended my PhD. I had settled a debate between Bohr and Einstein, which is not bad, and it was time to go to elsewhere, to another subject. Exactly at that moment, Claude Cohen-Tannoudji, the author of the book, who accepted to be in my PhD thesis committee.  Smith: It was a grand committee you had.  Aspect: Yes. He told me, “Look, I want to start a program on laser cooling of atoms. Would you join me? I have a position for you.” I accepted. I turned the page. This was ’85. In about ’90, a young student came during a conference, sat in front of me, and very politely told me, “Professor Aspect, with your entangled photons, we can do quantum cryptography.” I said, “What? Explain me that.” The young guy was Artur Ekert. Until this point, I had absolutely no idea that there could be any application to that. But from that time on, then I kept an eye on the progress in the so-called quantum technologies. Now, there is another way of considering your question. How could such a complicated experiment be translated into useful schemes? I think we have so many examples of that. My belief is that when there is a market, engineers are absolutely fantastic to making reliable experiments. I’m deeply convinced that when there is enough interest, the engineers are good at transforming a lab experiment into an easy to operate experiment. It is exactly the idea of several start-ups, which are developing quantum technologies. Just take one example in my institute. The start-up Pascal is trying to make an easy to operate experiment from the laboratory experiment of Antoine Browaeys, which is one of the most advanced quantum simulator in the world, but it demands a three PhD and two postdoc to operate. Pascal is trying to duplicate it in a way which is just push button and that you can put in a computing center. I’m very confident that engineers can do it if there is a market, For me, if there is no fundamental law telling you that it is impossible, it will happen. An example, gravitational wave detection. At the end of the 80s, I belonged to a committee which had to decide if France with Italy would invest or not in such detection scheme. At that time, it was clear that we have no idea of how long it would take, but personally, I voted for it under the same statement. There is no fundamental law saying that it is impossible. One day or another, it should happen. It did happen.  Smith: Indeed, it did. As you say, it took an awful lot of engineers and an awful lot of thinkers to make it possible.  Aspect: And ingenuity.  Smith: Ingenuity. Let’s finish with ingenuity on a totally different level. I hear that you are, maybe obsessed is too strong a word, but you are interested in the ability to cook the perfect egg.  Aspect: Who told you that?  Smith: I was told that you like to make a perfect egg.  Aspect: The perfect egg is a name invented by a cook. The point, because I experimented on it, is that if you cook an egg for one hour at 63 Celsius, then the consistency is absolutely unique. I like to do that. I bought a small equipment, which is like a lab equipment, but it’s done for cooking, it circulates the water and it keeps it at 63. I have checked that 62 is not the same texture and 64 is not the same texture.  Smith: What is the texture of a 63 degree for an hour egg?  Aspect: It is a texture in which the white is half solid but not solid and not liquid. It is in between. The yellow is soft. Then you need to accommodate it with interesting things. For instance, you can add some truffles, fois gras, and then the ensemble is delightful.  Smith: Finally, we spoke about magic or rather I spoke a little bit about magic in terms of quantum entanglement. I understand that you also like the practice of magic tricks.  Aspect: Yes. When I was 65, CNRS put me on mandatory retirement. I was unhappy with that. A good friend of mine, Thierry Giamarchi, who was professor in Geneva and a fantastic magician, told me, look Alain, you seem to be worried. As a birthday present, I can propose to start you in magic cards. I thought that maybe I would miss occupation, which is not the case. When I started, I was fascinated. I am not bad. But my specificity is the following. These magic tricks are very well documented when you belong to the community. The way it is described is standard. You do this, you do that. In order to be able to do the trick, you have to practice for weeks, hundreds of times because you need to have the finger just like a piano, you must have the fingers doing everything correctly. You have plenty of time to think about it. I think about the text relating to quantum mechanics. For instance, when there are cards jumping from the carpet to my hands, I pretend that it is a quantum tunnelling effect. When some cards are infected by other one, I say these bosonic simulation. I have also quantum teleportation in having a card going from the left to the right and of course, everybody understands that it is a joke. I think that it is pleasant that I do the same trick as a normal magician, but my text is different. I remember a conference on quantum cryptography, the big world conference on quantum cryptography in Paris a few years ago, with all the big shots from Peter Shor to Artur Ekert, Brassard etc. I was asked to do the after-dinner speech. Rather than doing a speech, I asked for a camera and I did the quantum tricks.  Smith: Were you nervous that it wouldn’t work in such in front of such an audience?  Aspect: No, I can tell you when I was most nervous in my life, it was in the Nobel banquet, when I asked to go up and speak to people who had been drinking and eating for three hours and a half. There I was really nervous, but it worked.  Smith: It worked beautifully. I remember the applause. It was lovely. Thank you so much.  Aspect: Thank you.  Brilliant: You just heard Nobel Prize Conversations. If you’d like to learn more about Alain Aspect, you can go to nobel prize dot org, where you’ll find a wealth of information about the prizes and the people behind the discoveries.  Nobel Prize Conversations is a podcast series with Adam Smith, a co-production of Filt and Nobel Prize Outreach. The producer for this episode was Karin Svensson. The editorial team also includes Andrew Hart, Olivia Lundqvist, and me, Clare Brilliant. Music by Epidemic Sound.  You can find previous seasons and conversations on Acast, or wherever you listen to podcasts.  Thanks for listening. |
| **Telephone** | **0006=AA**  Adam Smith: May I speak with Alain Aspect please?  Alain Aspect: It’s me, hello.  AS: Oh hello, hello. My name is Adam Smith, I’m calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  AA: Yes.  AS: Many congratulations.  AA: Thank you, it’s […] news.  AS: Quite lovely news. How did you receive it?  AA: Well, it’s of course a surprise because we know that there are so many outstanding physicists who deserve it and…  AS: It comes at the end of… I mean, not at the end, sorry… it comes as part of a great lineage of prizes in quantum mechanics…  AA: Yes.  AS: …starting, goodness, ninety years ago with the prize in 1932.  AA: Sure.  AS: That’s an extraordinary thought, isn’t it?  AA: Absolutely, and you know I think even two earlier times, 1922, yes, you are right. ‘22 is [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) and [Bohr](https://www.nobelprize.org/prizes/physics/1922/bohr/facts/), right? The same year.  AS: Yes, and then… or ’21, I can’t remember.  AA: Yes.  AS: But then [Heisenberg](https://www.nobelprize.org/prizes/physics/1932/heisenberg/facts/) in 1932 for the birth of quantum mechanics.  AA: Yes, and then Heisenberg, and of course all these great names. Of course I am very impressed because I’m certainly not at the same level as these people who have really totally changed the physics. But then I am proud to be of the same league of course.  AS: Indeed. And it really speaks to the robustness of quantum mechanics.  AA: Yes. You know when I did these experiments testing Bell’s inequalities, at the end the conclusion is yes, quantum mechanics resists all possible attacks! In a sense my experiment was trying to find a limit of quantum mechanics, and we didn’t find it.  AS: Isn’t that extraordinary, what a creation by collective humanity.  AA: Yes, yes.  AS: And also the international nature of this prize to Vienna, and to you in France, and John Clauser in the US, speaks to the international effort that goes into all of this.  AA: Okay, it’s important that scientists keep their international community at a time when the world is not so nice, and where nationalism is taking over in many countries. So we have to do all efforts to keep scientists making international communities, there is no doubt.  AS: It’s a very important point. The phenomenon you studied, this quantum entanglement, is so weird.  AA: Yes. Absolutely, absolutely. And it’s so weird that as I presented in the Nobel symposium a few weeks ago, the fact that I am accepting in my mental images something which is totally crazy, which is nonlocality. Of course I know that nonlocality does not allow you to send a useful message faster than light, but in my mental images I have accepted nonlocality because otherwise I cannot even think of entanglement, except in the equations of course. But if I want to have an image, I put nonlocality in my image. Nonlocality is the fact that there is a kind of instantaneous relation between two objects, of course something that Einstein could not accept. But he had realised that entanglement meant that.  AS: Yes, this connection over unimaginably vast distances that is possible. Extraordinary.  AA: Yes.  AS: And of course your work settled the debates between Einstein and Bohr.  AA: Yes.  AS: So, again the historical significance of your work is amazing.  AA: Okay, I am glad you say that, but there is one point I want to make clear. When people say ‘okay, the debate between Einstein and Bohr was settled in favour of Bohr’, I like to say that Einstein owes a great, great merit in raising the question. And if nowadays we know so many things about entanglement and we want to use it for quantum technology etc we must give the credit to Einstein to have raised the question. So, for instance, I got the same year Niels Bohr Medal and an Albert Einstein Medal, and I think that it’s fair. There is not one who wins and the other one who loses. Bohr wins from a certain point of view, but Einstein wins because he spotted something extraordinary.  AS: And it’s always important to challenge.  AA: Yes, exactly.  AS: Thank you very much indeed. I notice something from your CV, which is that just before you did your PhD you spent three years doing Voluntary Service Overseas in Cameroon.  AA: Absolutely, absolutely. And you should know that it was essential because it is a place where I studied quantum mechanics in its modern formalism, in the book of [Claude Cohen-Tannoudji](https://www.nobelprize.org/prizes/physics/1997/cohen-tannoudji/facts/), who got the Nobel Prize in 1997, and I studied by myself during this Cameroon period the book of Claude Cohen-Tannoudji, Franck Laloë and Bernard Diu, and when I came back to France I was ready to understand the various papers, Bell’s paper. Of course, John Bell, it’s a pity that he’s no longer there, but John Bell is a very important figure in that equation.  AS: Indeed. Isn’t that interesting, that life propels you onwards, and sometimes you have to just stop and take time for reflection.  AA: Yes, it was exactly… when I was in Cameroon I was teaching, but I had plenty of free time, and I used that time to… I knew that my education in quantum mechanics was not good at all. You know, it was the old quantum mechanics, solving partial differential equations. And the book of Cohen-Tannoudji, Laloë and Diu taught me where is the physics in quantum mechanics, and I was then ready to understand the discussion by John Bell etc.  AS: It’s a very important lesson for people listening I think.  AA: Absolutely.  AS: Yeah. Goodness, how do you feel about the prospect of all this attention that is focussed on you now?  AA: Aah! I will try to survive it. You know, my phone is ringing all the time, so I have set it down. This is why we have to go to a different phone in an office.  AS: Well, I’m very lucky that you took the time to do that. Thank you very much indeed.  AA: And I’m so happy, thank you very much.  AS: That’s good to hear, lovely.  AA: Thank you.  AS: Bye bye. |
| **Interview** |  |
| Q2 | **Where does your passion for science come from?** |
|  | Alain Aspect: When I was a kid, I lived in a small village in France. At that time it was very rural, so there were not so many technical objects, but I was fascinated by any tool from the carpenter, from people like that. I don’t know why, but I was really always fascinated by technical objects. Then I was a reader of Jules Verne. Jules Verne is an inspiration or more, it was confirming what we were learning. Remember it was in the fifties and the sixties, it was after the war, and there were the ideas of progress. There is one book of Jules Verne *L’Île mystérieuse*, ‘The mysterious island’, where engineers and scientists arrive on an isolated island, and because they have the knowledge of science and technology, they will be able to develop a comfortable civilisation. That was a belief that science is good for humankind. |
| Q8 | **Are you still a reader?** |
|  | Alain Aspect: Oh, yes. Sometimes, for the pleasure, I go into one of these old books. |
| Q3 | **What made you decide to pursue science?** |
|  | Alain Aspect: I think really since elementary school, I was attracted towards scientific “things”, because at elementary school it’s very simple. Anytime the teacher was doing a little “experiment”, I was always interested by that. Then I went to high school, and it turned out that at that time, for four years in high school, there was math, and natural science, biology, but there was nothing about physics and chemistry for four years, and I felt frustrated. I was waiting for the moment when I would learn physics and chemistry, so I think I was always fascinated by it. What happened then? In the last year of high school, I had a fantastic teacher at the high school who really determined my life, because he taught me what is the approach of physics. That is to say a blend between pragmatism, but also solid mathematical model, and at each time finding the simplest possible model, but anyway, it must be sophisticated enough to render an account of the phenomena. This is really a high school teacher who taught me that. |
| Q3 | **When did you know you wanted to pursue quantum mechanics?** |
|  | Alain Aspect: Well, I smiled because the story is interesting. I had excellent studies, excellent courses at university except for quantum physics. Seeing it from now, clearly it was a very bad teaching. The professor did not understand what was really quantum physics. I had that frustration, I knew that it was important, and I knew that I did not know it well. What happened that while I was in Africa, in Cameroon, for military service – okay, I was teaching rather than going to the army – I had the opportunity to buy a book by [Claude Cohen-Tannoudji](https://www.nobelprize.org/prizes/physics/1997/cohen-tannoudji/facts/), Nobel Laureate in 1997, Bernard Diu and Frank Laloe, and I learned quantum physics into that book, and then I understood what it was about. When I came back to France and found a paper by John Bell, which determines our experiments, I was ready to appreciate the paper by John Bell. |
| Q10 | **What does it take to proceed in science without support from others?** |
|  | Alain Aspect: When you are teaching, it’s a different world, so for me, the week was cut in two parts. There was the part dedicated to building that experiment and making tests and progressing in the development of the experiment that I was doing. On the other hand, there was teaching. Teaching is very rewarding because if you feel that the students understand what you are explaining, that they’re making progress, really it makes you happy. I think the fact that I was teaching was crucial for me. Teachers are very important. I mean, the fact that we have a future scientist depends on teacher and elementary school teachers, high school teachers. |
| Q1 | **What advice would you give to a student interested in science?** |
|  | Alain Aspect: More generally, my advice would be follow your taste. Don’t listen to fashion, because maybe nowadays they tell you that this is a fashionable subject, but five years from now, it’ll be another one, so I think you should really work in what you are interested. It may be basic science, it may be technology. It must be application – it just depends on the person. Each person is able to know exactly what is his or her passion, and this is what they should follow. |
| Q8 | **How do you like to spend your free time?** |
|  | Alain Aspect: What can I say? It depends on the moment in my life. When I was younger, I was spending a lot of time in sport, hiking, playing tennis, etc. When you get old you are not as fitted for doing so much sport, so what do I do? Cooking, I like cooking. I also like rehearsing for magic tricks. Since I got mandatory retirement I thought I had some free time, and I started learning to do magic with cards. I’m not an excellent magician, I know some classical tricks, but when I do these tricks, rather than saying the usual words of a magician, I pretend that it is quantum. So, when a car jumps from here to my hand, I say, Oh, this quantum tunneling effect. I spend some time doing that, and of course, spending time with the family, my wife, grandchildren, my children. |
| Q4 | **Are there any similarities between quantum physics and magic tricks?** |
|  | Alain Aspect: Not at all. Because magic tricks, really if you want to do well a magic trick, it has to become automatic, like riding a bicycle. Your fingers work independently of you, and then you can think about a speech, which has nothing to do with what you are doing. You can pretend that it is quantum physics because your fingers work without thinking of them. It’s certainly not like an experiment, because in an experiment, each time you turn a knob, you have to think about what you are doing, and observe and react. |
| Q10 | **What environments help with creativity?** |
|  | Alain Aspect: I need to have apparatus and knobs to turn. So, for instance when I cook, there is something which is very special, *l’oeuf parfait*, perfect egg. It has to be cooked at 64 ºC, not 62, not 66. I like to have a thermometer and to tune the heating, etc. Probably it reminds me of doing an experiment, adjusting everything in real time – observing, reacting, tuning. |
| Q13 | **Can you tell us about the object that you are donating to the Nobel Prize Museum?** |
|  | Alain Aspect: The original thing in my experiment is the fact that the experiment is about two photons traveling to detectors. The idea is to change the setting of the detector while the photons are traveling. This takes only one or two dozen of nanoseconds, so it has to be very fast, and you cannot rotate an object in a few nanoseconds. My idea was to have a switch, and the switch would send the light either towards a first apparatus or towards a second apparatus. If the switch was fast enough it would be equivalent to a single apparatus operators rotating from one position to the other one, etc. These was my initial idea, I had published it from the beginning, and a company had promised to deliver the switch. In fact, the switch was my idea. It was based on the interaction of light with an acoustic standing wave. A company had promised to deliver it, but I wanted it big enough. After some time the company said, We cannot deliver it because so big we cannot we cannot make it, it’s too big. The reason was that when there were sticking a transducer on a crystal, it has to be done at high temperature, and then because a dilation is different between the material, it was breaking all the time. The problem was a contact between the material and the transducer. I was in despair because I needed that. I began to think, and when you are despaired, you become creative. Suddenly I realised that I did not need a crystal, water turns out to be a good material for that. If I have transducers in water, no problem of different dilation, there will be no breaking, so we built homemade – and the object I come is really homemade – it’s like, I don’t know how you say, in French a *bricolage*, one thing which is not a nice object, but it worked! |
| Q14 | **What about the future of quantum mechanics excites you?** |
|  | Alain Aspect: In fact, what is exciting in the last, say decade or decades, is the fact that smart people discovered that entanglement can be used for applications and this whole world of quantum technologies. The idea that maybe we have a quantum computer or quantum simulator, which is able to solve problems that we don’t know how to solve with our usual computers. I think this is very exciting because when you have been working in fundamental science and suddenly somebody comes and say, You know, your pair of entangled photons can be useful for something, I like this idea. This idea of quantum computer, I think that they can be fantastic. We have problems of optimisation, for instance, now we have wind electricity, solar electricity, and the equilibration of the electric grid is a very complicated problem. It’s so complicated that you cannot find the solution with a standard computer because it’s called a difficult problem, a problem for which the time of computing increases exponentially with a number of elements to adjust. It may well be that quantum computers can help solve this kind of problem. Not only this one of course, but this kind of problem. To be honest, I would be excited to see that. |
| Q15 | **Do you ever think about your professional legacy?** |
|  | Alain Aspect: Oh no, but I can think about it. I think that if there is something I would like to teach is curiosity. When something interests you, just go into it and try to understand better and better. Don’t listen to people who tell you it’s not interesting. If you find it interesting, just continue in it. |
| Q15 | **What do you think about people saying that you proved Einstein wrong?** |
|  | Alain Aspect: Often people say that my experiment showed [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) wrong. I think it’s not fair at all, because what happened is that Einstein raised a problem and his vision of the world was such that he came to the conclusion that one must complete quantum theory and Niels Bohr disagreed with him. The experiments we have been doing in test of Bell’s inequality show that quantum mechanics cannot be completed along the lines that probably Einstein had in mind. From this point of view, it’s true that he was not fully right, but he did not know about our experiments. In his time he could think that his interpretation was compatible with standard physics, so we don’t know what he would’ve said. Moreover, I told you that entanglement is a very important property who put the finger on entanglement and said, Ah, this is really amazing. It is Einstein. So, when people say, Oh, you showed Einstein wrong, I say, Come on, I showed Einstein was great, and if he had known what we have today, nobody knows what he would’ve said. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0007** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0007 = JC**  Adam Smith: John Clauser, please.  Bobbi Tosse: He’s Zooming right now with England.  AS: Okay, I understand that.  BT: But he should be off in a second. Hang on.  AS: Thank you.  BT: Or call back in a few minutes, maybe that would work.  AS: The only thing I worry about is if I hang up then I might never be able to get back in touch again, you know, because your phone’s going to be ringing off the hook.  BT: It has been since 2:50. It’s a very exciting day.  John Clauser: … Swedish news media, European and American news media …  BT: And this is Sweden on the phone.  JC: Oh hang on, they’re on the phone right now.  John Clauser: Hello?  AS: Hello, my name is Adam Smith, and I’m calling from the website of the Nobel Prize.  JC: Ah-ha! I’ve been talking all day with various news, I have yet to hear anything from the Swedish Academy.  AS: Well, gosh, my goodness. Well, in fact I’m not the Swedish Academy, but we have this tradition of recording extremely short interviews with new laureates, just…  JC: Okay.  AS: So if you can… I know you’re on another Zoom call at the moment, but if you were able to talk to me for three or four minutes that would just be wonderful. What do you think?  JC: Okay, hang on just a second.  AS: Yep.  JC: Can I talk to the guys from the Swedish Nobel Committee? If you can pause for a second.  [Unknown interviewer]: Yes absolutely.  JC: Sure, go ahead.  AS: First of all many, many congratulations on the award.  JC: Thank you.  AS: I guess you have already been, as you said, on calls all morning.  JC: Ah yes. Took me a long time before I even got a cup of coffee. I got waked up at three in the morning.  AS: My goodness, what a start to a very long day.  JC: So far it took me over an hour to even get my pants on, there were so many phone calls.  AS: That’s slow progress with the regular things in life but nice, nice distractions. This work that’s been awarded, I mean, you were the person who thought that it might be possible to test Bell’s theorems in the laboratory, and people didn’t believe…  JC: Yes, I had the idea independently with, from Abner Shimony and Mike Horne, and then we wrote a small… gave a talk, wrote an abstract and gave a talk, in an APS meeting, Physical Society meeting, and we got together at that meeting and decided to share our resources and we published what’s called the CHSH – Clauser, Horne, Shimony and Holt – paper in 1969, and that was, kind of, the first proposal for doing the experiment.  AS: But I gather…  JC: And then in 1972, I came… Or in ’69 actually, I got my degree at Columbia and I came to Berkeley, and actually then collaborated with Stuart Freedman, a graduate student. This became his PhD thesis at Cal, and we did the first experimental test of Bell’s theorem.  AS: I gather that many great physicists didn’t believe you, and you were turned away by people such as [Feynman](https://www.nobelprize.org/prizes/physics/1965/feynman/facts/).  JC: Oh indeed, yes. Everybody at the time, my whole faculty at Columbia… while I was doing the experiment I had a short conversation where Feynman kind of threw me out of his office. He was very offended that I should even be considering the possibility that quantum mechanics might not give the correct predictions. And only through the very kind efforts of [Charlie Townes](https://www.nobelprize.org/prizes/physics/1964/townes/facts/) and Howard Shugart here at Cal Berkeley was I able to do the experiments. And afterwards all of my faculty still in Columbia said ‘why, what a waste of time, you got the results that everybody expected – now start doing some real physics’.  AS: I love the thought of this bold young man hoping to topple quantum mechanics.  JC: Well, I was having fun. It was a challenging experiment. I thought it was important at the time, even though everybody told me I was crazy and was going to ruin my career by doing it. And to some extent I did – I’ve never been a professor. So… but I had a lot of fun doing some really challenging experimental physics. Didn’t have any money to do the work, and so Stu Freedman and I had to build everything from scratch. Spent a lot of time in the shop cutting metal and whatever. And then after he got his degree he left, went to Princeton, and I continued on doing three more experiments. And all of these had to build everything from scratch, so…  AS: I gather your…  JC: … there was very little money and so I was basically cobbling together old junk or scrap from the UC Physics department.  AS: Yeah, I heard you were famous for scavenging people out of the dumpster.  JC: Well, there was a lot of stuff unused in storerooms if you recognise what it is. Most people haven’t the faintest idea, and they just sort of say ‘well, it might be useful, we’ll put it in the storeroom’. So I would rummage around and say ‘oh hey, I can use this’.  AS: You know there was a famous… You know there was a medicine laureate called [Oliver Smithies](https://www.nobelprize.org/prizes/medicine/2007/smithies/facts/) who had exactly the same approach, and people used to write NBGBOKFO on the equipment that they put out in the corridor, and it stood for ‘no bloody good, but okay for Oliver’.  JC:  I was working in labs later on, of some very famous people – Oppenheimer and [Lawrence](https://www.nobelprize.org/prizes/physics/1939/lawrence/facts/) and whatever – and I’m told they were also scavengers.  AS: It’s a great tradition. And really a wonderful encouragement to people out there that you can be a Nobel Laureate and not be a professor.  JC: Well, whatever, so I just say we’re all right, in that respect it did ruin my career, that’s why nobody was interested in hiring me. I had a great difficulty finding a job, so I went off to Livermore Lab to do controlled fusion plasma physics experiments. So I proved that I was a decent experimentalist by doing these experiments.  AS: I think that much is proved, and it all turned out well in the end.  JC: Yes.  AS: Anyway, it’s an absolute joy to speak to you, and we… I look forward to…  JC: My pleasure.  AS: I look forward to speaking a great deal more in the future. We’ll record a long interview.  JC: I will too. I will finally get some dates and times of what I’m expected to do.  AS: Yes, you will, so over the coming days you’ll be sent lots of information by the Nobel Foundation.  JC: Great. Okay.  AS: And you’ll know everything. Thank you so much, and many, many congratulations.  JC: Okay, my pleasure.  AS: Okay.  JC: Okay, thanks a lot.  AS: Bye now.  JC: Bye bye. |
| **Interview** |  |
| Q3 | **Where does your passion for science come from?** |
|  | John Clauser: Oh, I think that’s very clear. My dad was a scientist and when I was a kid, he was a professor, in fact chairman, indeed the creator of the Aeronautics Department of Johns Hopkins University in Baltimore. I would add, all of my primary life leading up to this, every question I asked he would answer in detail very patiently with me, and I just soaked it all up. Then when I was in high school, I would take the bus up from Baltimore Polytechnic in Baltimore and walk across the campus to his office where I was supposed to be doing my homework waiting for when he would drive us home. Instead, I would just wander all over the campus into all various laboratories, into his laboratory and I would walk in and look at all these marvellous pieces of devices sitting around and say, Boy, when I grow up, I want to be a scientist. Look at the great toys you get to play with.  I’ve just been bathed in it since I was a very young kid. I loved science, my dad taught me lots of it. I was a high school whiz kid in electronics, built a whole bunch of computer science projects, even won a few national affairs, and I was just having fun. My parents left me as a free-range kid, I could do pretty much anything I wanted and did, wandered all over the city. One of the things that I enjoyed, I was a member of a high school radio club, went out and just built a lot of electronics and I was having a blast doing it all. |
| Q3 | **When did you know you wanted to pursue quantum mechanics?** |
|  | John Clauser: When I went to Caltech, originally I was thinking about electrical engineering, but very quickly realised, No, I think I want to do physics. Caltech was a marvellous place, some of the greatest physicists in the world wandered the halls there. Very quickly got enamoured with doing physics, and all of which I was just having fun. I enjoyed doing a lot of it. I was not as good a student as I probably could have been. I was the social chairman of our residence house, the Dabney house at Caltech. Threw some great parties and flunked out a bunch of freshmen who really weren’t going anywhere anyway, so probably the best thing for them. Somehow – I have no idea how – I got into graduate school at Columbia. There I was working on my thesis project, on the cosmic microwave background. Originally we were going to put a radio telescope in a U2 high-flying U2 apparatus, where I learned a little bit about the spectrum of water vapor in the atmosphere that could relative to climate change. We ended up not doing that, ended up looking at interstellar cyanide and making the third measurement of the microwave background. |
| Q16 | **Would you consider yourself to be a good student** |
|  | John Clauser: In order to I get your degree at Columbia, there were, I think, five important courses you had to get a b or better, otherwise you were considered having flunked. Quantum mechanics, I had flunked twice. I think finally my [Nobel Lecture](https://www.nobelprize.org/prizes/physics/2022/clauser/lecture/) will explain why I flunked – what was driving me nuts and why I still don’t understand quantum mechanics. |
| Q2 | **What was the reaction people had to your theories?** |
|  | John Clauser: Much to the distress of my thesis advisor, in fact, much of the distress of the whole Columbia physics faculty, everybody told me I was totally nuts in doing this. Everybody knew that [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) was wrong, that he was getting senile was frequently the claim, and that [Bohr](https://www.nobelprize.org/prizes/physics/1922/bohr/facts/) had it all right. I couldn’t understand Bohr at all. Then I ran across John Bell’s paper, and that started my whole career in studying quantum mechanics. That was while I was still a graduate student. Columbia had more physics Nobel Laureates than I think Caltech did. They all told me, You’re nuts doing this. When I was doing the experiment at Cal Berkeley my dad, who was then on the physics faculty, he was Dean of Applied Science and Engineering at Caltech, and I would go home to Pasadena for Christmas and his birthday and Thanksgiving and whatever, so I was down there for Christmas. Then he said, Oh, I have made an appointment for you to talk to [Richard Feynman](https://www.nobelprize.org/prizes/physics/1965/feynman/facts/). Oh, okay. I was doing the experiment, so I walked into Feynman’s office, and he threw me back out instantly. He was actually angry, as best I could tell that I was a challenge doing an experiment to test the predictions of quantum mechanics. Everybody knows predictions of quantum mechanics are perfect, don’t even need to look. I think his line was, if you find something wrong with the prediction of quantum mechanics, you can come back then and we’ll talk and figure out what your problem was, or what your problem is, and dismissed me immediately. He was rather abrupt. |
| Q2 | **Did you find people who supported your work? How did you do so?** |
|  | John Clauser: When I was at Cal Berkeley trying to do the experiments, the really only two people, or maybe three later on, that thought it was actually a good idea, [Charlie Townes](https://www.nobelprize.org/prizes/physics/1964/townes/facts/), he is a Nobel Laureate, of course, and Howard Shugart was a great atomic physicists there, fortunately allowed me to stay there for I guess from 1969–76, where I did like four different experiments. I had a great time. I was having fun even though I had destroyed my hero Einstein’s work. I was not very happy about that, but that’s what the data kept telling me. I got to report what I see, that’s what experimental science is all about. Literally you’re talking to God, and God has spoken. It was not easy getting into a position to do the experiment, absolutely not, I had to struggle to do that. It was only through the kindness and generosity of Townes and Shugart that I stayed on. |
| Q2 | **What was the hardest part of getting people to pay attention to your work?** |
|  | John Clauser: The hard part was getting around this, this was a religion among physicists, and in fact even in the Bohr-Einstein debates, and with [Schrödinger](https://www.nobelprize.org/prizes/physics/1933/schrodinger/facts/) also, the universal religion was quantum mechanics makes correct predictions, period, no, off the table. What Bell’s theorem and in particular our version of it, the Clauser-Horne-Shimony-Holt experimental prediction, showed that either that had to be wrong, nor Einstein’s whole legacy had to be wrong. I think at the time people who were critical of doing the experiment didn’t realise really what was at stake, that it was that cut and dried between two religious icons, if you will, the field of physics. Since I kind of was the inventor of the CHSH inequality and the CH inequality and all that, I knew exactly what it was all about. But it took a long time for that to filter in. |
| Q1 | **What advice would you give to a student or young researcher?** |
|  | John Clauser: If you’re not enjoying it, find something else that you are enjoying and then you can put your heart into it and really be good at it. Totally silly to waste your time if you’re not enjoying what you’re doing. |
| Q15 | **How do you see science? Why do you think science is so special?** |
|  | John Clauser: Okay, to start out with, I’m an atheist, for that point of view. But literally when you go into – at least I feel this way – when you go into a physics laboratory, you’re talking to God. It’s like going into a church, asking a question of God. It’s not often that people will walk into a church and say, All right, God, what’s the mass of an electron? But nonetheless, if you ask a question carefully, you’ll get a definite answer. It’s an unusual church from that point of view. If somebody else asks the same question and walks into another church he or she will get exactly the same answer. Find me another church for which that is true, that two people could walk into different churches, ask the same question and get the same answer. Enough said.  Nature is beautiful. There is great majesty in all of the natural patterns. You probably heard the comment about to look on Maxwell’s equations is to look on the work of God. There is great beauty in the symmetries that are built in, and if you’re the first guy to see something new, some new patterns in nature that nobody has noticed before, it’s an awesome experience, realising that you’re the only person in the whole world who knows this, and you have communed with nature as no one else has in the past. It’s a truly marvellous feeling, it gives you a great spiritual feeling. |
| Q20 | **What qualities do you need to be a successful scientist?** |
|  | John Clauser: One of the things that my dad did that helped me along was taught me to be sceptical of everything, especially other people’s interpretations of experiments. He would say, Okay, go back and look at the original data, if you possibly can. What was really done and be sceptical that they have drawn the right conclusions. And I do that to this day on everything. I am the world’s worst critic of pseudoscientific claims. |
| Q5 | **What has sailing taught you as a scientist?** |
|  | John Clauser: I’ve raced across the Pacific Ocean any number of times, and you learn a lot about clouds. I was using solar power for the instrument systems, and I was just sitting there in my berth watching, and I had an air meter on the solar panels and watched every time we go under a cloud, the total current charging their batteries would drop to one half. Why is that? Almost exactly one half every and in and out of clouds, flick, flick, flick, flick goes the needle. Amazing. It gave me a whole different opinion of how climate change works. I spent a lot of time trying to figure out what makes sailboats go fast, how to win races, built a lot of hardware. The present boat the rudder that came on the boat was a total joke. It was slow, you couldn’t steer the boat, it had a lot of drag so I just tore it out and built a whole new one in my backyard with starting rolls of carbon fibre cloth and buckets of high-tech epoxy and boars of polyurethane foam and put the whole thing together. It was fun. I didn’t know how to do it to start with, but I taught myself along the way. It’s not that, doing the stress analysis is pretty straight. If you’re a physicist, it’s piece of cake. It’s just straightforward handbook engineering. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0008** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0008=Zeilinger**  Anton Zeilinger: “When journalists asked me even in the 1990s what this is good for, my answer always was, I can tell you very honestly and proudly, this is good for nothing. I do it for curiosity. It helps to change our view of the world. What goes on in the head of people is quite important.”  Adam Smith: Anton Zeilinger is a joy to talk to. You might think that being, for instance, the person who was the first to realise teleportation in any sense, you might become a bit big-headed and pleased with yourself, and it really strikes me that he’s just not that. He’s clearly very confident, but he also seems very humble. Those two words might sometimes be seen as being in opposition, but maybe that’s the ideal combination: to be both confident and humble. He doesn’t seem so surprised by the strangeness of quantum mechanics – it’s such a normal reaction to quantum mechanics to say, wow, this is just far out there. He does say that. He does say, this is really odd, but that’s not so surprising. He sort of expects things to be odd, and that’s quite refreshing and probably a good way to approach the world, not to expect predictability, but rather to expect strangeness.  I always find these conversations fascinating, but perhaps more than usually, this one gave me an insight into who Anton Zeilinger is, and I very much hope that you too enjoy this conversation.  Clare Brilliant: This is Nobel Prize Conversations. Our guest is Anton Zeilinger, who received the 2022 Nobel Prize in Physics. He was awarded for experiments with entangles photons, establishing the violation of Bell inequalities and pioneering quantum information science.  He shared the prize with John Clauser and Alain Aspect.  Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces.  Anton Zeilinger is professor emeritus of physics at the University of Vienna and senior scientist at the Institute for Quantum Optics and Quantum Information of the Austrian Academy of Sciences.  He talks to Adam about embracing randomness, contemplating the changing world through his collection of old maps, and how his life-long curiosity was sparked when he, as a child, observed the world from the window of a castle.  But we begin, with the beauty of quantum mechanics.  Smith: There are, if you like, two stars of this podcast. There’s you, of course, and there’s quantum mechanics. We should start with quantum mechanics since that’s, for most of us, quite hard to get our heads around. Obviously, to those who understand it, it’s very beautiful. Could you describe what is so enormously attractive about quantum mechanics?  Zeilinger: One is that it’s mathematically extremely beautiful. When I first heard of it, I was impressed with how you can express a lot of complexity through symbols. How the mathematics kind of imposes novel phenomena, which nobody had thought about when they wrote down the original equations.  Smith: Yes, because in its simplest form, and although it’s impenetrable to most of us, if you look at [Schrödinger](https://www.nobelprize.org/prizes/physics/1933/schrodinger/facts/)‘s equation in its simplest form, there are just five symbols there. It encompasses so much. It’s extraordinary.  Zeilinger: Yes, the same holds for the [Heisenberg](https://www.nobelprize.org/prizes/physics/1932/heisenberg/facts/) formulation, which is just a commutation relation between two operators. How can such a thing be so important in describing the physical world?  Smith: There’s that aspect. Its beauty is in its power, if you like, from that description. Is there more about it that attracts you?  Zeilinger: I think the beauty is a little bit more than the power. It’s more like, when you look at arts, there’s one line of the development of arts, and this holds for many arts. It holds for jazz, for example. There’s a development that you work with as little as possible. You hit the drum fewer and fewer times, and so on, and you still get it. That’s part of the beauty. That is the same beauty I see in, actually, not only quantum physics, but also in fundamental science. Your question whether there is more, yes, there is more. Namely, the incredible precision to which the predictions of quantum mechanics have been confirmed in experiments. This is the record. No human theory has been better confirmed than just a few symbols of quantum mechanics.  Smith: I really like this comparison of quantum mechanics and jazz. As you said, the simplicity. Could you just talk a little bit more about that parallel? That is a very nice way to think about it.  Zeilinger: Yes, a friend of mine, he was a quantum physicist, Mike Horn, a famous guy, and he was also a jazz player, and he showed it to me. He showed me that the original tune is like, do-do, do-do, do-do, and then he left out basically every second or every third tone, and so on. You still get it. You say, oh, that is beautiful. That is really something. There seems to be something going on in our heads. There is something going on in our brain, or however we call this thing up there, where we add, very often, things which are not in the beginning there, but which are justified. That could be something like that in science, but that is very speculative, you know.  Brilliant: Adam, after your recent conversation with Alain Aspect, you and I had a conversation where we tried to make sense of some of the difficult concepts in quantum mechanics.  Smith: I wonder how we did, oh dear. Anyway, yes.  Brilliant: I mean things were feeling a bit clearer but I think I might need a bit more help now. In this conversation we’ve already had Schrödinger’s equation and the Heisenberg formulation mentioned. I feel like I’ve heard of both but don’t really know what they are so it’d be great if you could explain a bit about them please.  Smith: The Schrödinger equation is a differential equation which describes the behaviour of the wave function of a particle in quantum mechanics. Now in quantum mechanics every particle can be described by a probabilistic wave which shows its probability of being somewhere in space at a particular time. It’s like a sort of bell curve shape with a greater probability of it being in the middle and a lesser probability of it being elsewhere but immediately that takes us out of the realm of what we know. If you look at your sofa you don’t say it’s got a strong likelihood of being in the centre of the room and a lesser likelihood of sort of drifting off into the corners of the room. This isn’t the world we all live in but this is how you describe the behaviour of the smallest things in the universe in quantum mechanics. The concept perhaps to take forward from the Schrödinger equation is the description that all these tiny things from which we’re made up all have probabilities of existing in particular places but that shows that you don’t necessarily know exactly where they are. The Heisenberg formulation that Zeilinger referred to, commonly known as the Heisenberg uncertainty principle, states that for any particle in quantum mechanics you cannot be absolutely sure of its position and its speed, its velocity, at the same time. The more certain about its velocity you are the less certain about where it is or the more certain about where it is the less certain you are about where it’s heading. That relates to the Schrödinger equation because again it comes down to this probabilistic description of the nature of particles seen as waves.  Brilliant: It all sounds pretty random yet the way Zeilinger was describing it was the sort of the beauty and simplicity.  Smith: You’ve obviously got a deep innate understanding of quantum mechanics to leap to randomness because that is I think the underlying principle of it all that everything that is happening is governed by statistics, by the probability of something being in one state or another or in one place or another.  Brilliant: One of the things that I wondered is I recognise Schrödinger’s equation and I’ve also come across the term Schrödinger’s cat. Is that something that helps us to understand Schrödinger’s equation a bit better? Or not at all?  Smith: If you’re a brilliant quantum physicist it probably does. For the likes of me, Schrödinger’s cat is a thought experiment designed to illustrate the oddness of one quantum mechanical phenomenon which is superposition of states. The idea that in quantum mechanics particles can exist in combinations of two or more states at the same time. This idea of existing in two or more states at once is so contrary again to what we in the universe we kind of see around us know. Schrödinger’s cat was simply a thought experiment designed to show how crazy it could be and the idea behind that experiment is that you have a cat in a box and you have a mechanism that will kill the cat releasing a deadly gas if a particular radioactive particle decays. If it decays the gas is released, the cat dies. If it doesn’t decay the gas isn’t released, the cat does not die. In quantum mechanics it is possible to describe the radioactive particle as decayed and not decayed at the same time. If that was the case then the gas would be released or not released at the same time and the cat would be dead or alive at the same time which is obviously crazy. The point of that thought experiment was that as long as you keep the box sealed and the cat is in there it is possible in quantum mechanical terms to say the cat is both dead and alive at the same time. Obviously bonkers. What it really illustrates is the difficulty of describing quantum mechanics in terms of the real world that we know and love. Possibly an easier way to think of superposition is, it all comes down to maths of course, it all comes down to equations, is a very simple equation which is x squared equals four. That’s pretty easy to solve because you know that x in that equation can either be two or minus two. Both of those values of x suit that equation and you can say in that equation therefore that two and minus two are in a state of superposition perhaps. It’s a bit of a stretch but it’s a sort of start.  Brilliant: The complexity of quantum mechanics and the way that you have to sort of suspend your belief in some ways must attract people who have quite unique perspectives on the world.  Smith: Anton Zeilinger actually speaks interestingly about that, about how people with different worldviews come to him. Let’s listen to him talk about that.  Zeilinger: As you can imagine, I am quite often asked by people with esoteric viewpoints whether what they do is supported by quantum mechanics or they actually claim that. My usual answer is, you have no idea how strange quantum mechanics is. Really, get used to it, work with it. Then you will see that your concepts are extremely naive and simplistic. Randomness is, in my eyes, a very nice feature of the universe. If you think about the universe in the old terms, where it was basically thought of as a classical machine, every generation thought that the latest theory describes how the world works, how the brain works or whatever. Sometimes this is a classical machine, very much like the planets. Now, I wouldn’t want to live in a world where everything is completely predetermined. This is a horrible idea. I love it that quantum mechanics tells us there can be randomness. Randomness is not so irrelevant. For example, the quantum randomness most certainly also plays some role in random changes in the genetic code. It’s not something out there which is not relevant for us.  Smith: The message from a deep study of quantum mechanics is embrace strangeness and randomness.  Zeilinger: Embrace it and see it as a step forward to understand this universe and our whole in it.  Zeilinger: The case of this last Nobel Prize… All three people, Clauser, Aspect and myself, we had not the slightest idea of an application. Not the slightest one. Now there seems to be a big business, quantum computation, etc. When journalists asked me even in the 1990s what this is good for – and there had been the first ideas already out for quantum cryptography and quantum computation – my answer always was, I can tell you very honestly and proudly, this is good for nothing. I do it for curiosity, and I think that it has the same use as astronomy. It helps to change our view of the world, to widen our view of the world, to understand more what is going on in the deep sense. I would say this is also an application. It’s a different kind of application than technology, but what goes on in the head of people is quite important.  Smith: Indeed. There’s that lovely quote from Robert Wilson when he went in front of the review board for building the Fermi lab’s first accelerator, and they asked him how it would help with the defense of the country. He said something like, it won’t help at all with the defense of the country, but it will make the country worth defending.  Zeilinger: Yes, exactly right.  Smith: Your own curiosity and desire to explore, can you say where that came from? I’ve read that you were brought up for a while looking out of the window of a castle at the world around you and observing. Tell us about that.  Zeilinger: It was after ’45 and Austria was in a very bad economic shape. My father got a position at a small agricultural school in the countryside in Austria as a researcher. They had this castle available, which was empty, so we got to live in this castle. We were living up on the second floor by European count, third floor by American count. They tied me to this window with some harness so that I couldn’t fall out. I looked down for hours. I saw cattle coming by, and I saw the farmers coming, and the machines, and everything. The people in the village thought that this guy is a little strange. But I was always curious, as far as I can think back, I was always curious to look behind the immediately obvious. I was never interested in a technical way. I was interested in how things work technically, but I was never interested in building something. This was in a small village, and my friends were children of farmers and so we played out in the fields and everything. I was not isolated. But these hours of looking down were obviously crucial. I know that I often ask questions, also friends or whatever, some of the questions which I thought were deep, and they said, what a silly question are you asking. Interestingly, that did not turn me off.  Smith: That is interesting, because most people just probably drop it and think, oh well, I better not go down that line. I look foolish.  Zeilinger: It was clear that the guy saying that didn’t understand the question.  Smith: There’s an interesting confidence there in you then, that you knew that you had something that you were thinking that was interesting.  Zeilinger: This seems to be a feature of my personality. It has been remarked by other people, that I seem to have a very large confidence in what happens to come out of my head. I don’t know why. This probably was crucial for my success. There were situations where I changed some direction of research in my group, and my group was not happy with that decision. But I had confidence that it was the right way to do, and it happened. The point is really that,  to me, it’s absolutely clear that most of our thinking goes on subconsciously, unconsciously, and so on. Even in physics, nobody knows where the ideas come from. If you then write a paper and explain this is for that and that reason, that’s some explanation in hindsight. I learned one thing from my thesis supervisor, Helmut Rauch. He was a real leader in foundations of quantum mechanics, and I was absolutely lucky to work with him here in Vienna. I learned from him, just the way we interacted, that sometimes he had ideas, and he gave the reason why, and the reasoning was completely wrong, it was absurd. But the idea was right. That is fascinating, our brain is able to produce ideas which turn out to be right, when the reasoning is wrong. The opposite is also true, but that’s a different story, right?  Smith: Yes, it’s wonderful to think of that creativity going on inside, and ideas that one should take seriously popping out. Of course, it’s a bit dangerous as well, it can lead you in all sorts of directions.  Zeilinger: It can be dangerous and there are many cases where it was not good for the person. But, again, there’s a parallel to music. A very good friend of mine was a famous conductor, Nikolaus Harnoncourt. Nikolaus Harnoncourt was the guy who kind of brought back a lot of music by playing original instruments, and by trying to find out, by lots of study, what was really going on when these composers wrote this 200, 300 years ago, or whatsoever.  *CLIP with music:* ”Pièces pour la flûte traversière, 1er livre: VI. Menuet ”Le Beaulieu” by Nikolaus Harnoncourt and Concentus Musicus Wien.  Anton Zeilinger in the laboratory. © Jacqueline Godany (Press image from ÖAW)  Zeilinger: I asked him, how can you argue that this works? He says, in the end it’s intuition, in the end it’s creativity. In the end I have to trust that what I found is the right way, at least for me to do it. This is all over the place, this creativity is all over the place, even in daily life I would say.  Smith: This intuition that you had, and confidence, that allowed you to maintain the curiosity you had, have you any idea why you were able to do that? Most people are very nice and curious as children, and lots of people ask questions, but so many people then sort of get overwhelmed by a desire for a career or something like that, or not a desire perhaps, but just the feeling that they need to get serious and plan. Somehow much of the curiosity, not all of it, but much of it ebbs away. Do you have any feeling for why it didn’t happen to you, why you just stayed childlike, if you like?  Zeilinger: Yes, childlike is the right word. We have at the end of what we call gymnasium, in America it’s called high school, I don’t know how it is called in Sweden or in England, when you are 18 year old, you have a final exam, right? A final big exam with a committee, and it’s supposed to be very serious and so on, and you supposedly have to show a lot of respect. A friend of mine, I was so lucky, I had a friend in high school who was as curious as I was, and he was as childish as I was. Honestly, when the others started to talk about girls, we talked about the Big Bang and all this kind of thing. It’s really strange. So, this is one important ingredient. There was another one who I could talk to, so I was not completely isolated. The two of us were not completely isolated. Then came this final exam, and you are waiting in your classroom for the official verdict, okay, and you are supposed to be nervous, and the two of us were playing a childish game with coins on the table, and our class head teacher came in and said, Zeilinger, you will never grow up. I considered that a compliment. You have this curiosity, something like, I think I can say that I started some interesting subfields in quantum experiments. Not just the Bell stuff, which was started by others, but some other things. Whenever things like that became popular, and other groups did it, I made a turn. I said, let’s look for something new. Even now I am looking for some. I think I have some ideas that I will do in the next couple of years, which is a little bit different from what I did so far. You have to reinvent yourself every couple of years. It’s absolutely important.  Smith: Most people would think that was pretty brave to do so, but you don’t see it as brave, you see it as just necessary.  Zeilinger: It’s necessary for me to make my life interesting. Life is too short. It happened once that we took a turn and we buried something like 2 million euros or whatever. Because in that case, what we had planned was really too ambitious. We worked on it and we couldn’t, there was no way to understand what’s going on. Now this field is taking up again slowly. That can happen. Science has to have the possibility that sometimes the goals are too ambitious. But another thing I’d like to say is, if somebody writes down an application, writes down what he or she will do in 5 years, if after 5 years he or she does exactly what they said 5 years ago, then it was a waste of time and money. There should be something new coming out. There should be something exciting where people say, Oh my God, oh, why didn’t I think of it, and things like that.  Smith: Do you want to tell us what it is that you’re turning to now?  Zeilinger: No.  Smith: Fair enough.  Zeilinger: No, I would love to, but I have made the experience that when I say something like that, people can be excited and they will also do it. The ideas are very premature, maybe in a year I can say more.  Smith: Nice to have an insight into the kind of burgeoning new idea and how, as you say, you’re going for unoccupied territory.  Zeilinger: That’s right, yes. There are still so many white spots on the map. My general view about science is when people say something like that the theory of everything is just around the corner, then what they do is – maybe I’m too impolite here – in my eyes they just expose publicly the limits of their imagination. If you look at it, we do modern science, science as a mathematical science, which was started by people like Kepler, who was still somewhat esoteric, Kepler, Galilei, and finally Newton, who wrote down physics as a mathematical science. This is only a few hundred years old. To believe that we found a significant part of what can be found, I think that it must just be wrong.  Smith: Yes, isn’t that interesting? There is a huge tendency to think that we’ve kind of reached the epitome of what we are, that we’re the sort of pinnacle of everything, that we’ve thought through things, the current generation is the one that’s going to solve it – and no. It’s strange that, really, when you look at, because we also can just look out at the history of the world, how long it’s been here, and realise that this is just a moment in time, and things are progressing. Or regressing, but something’s happening anyway.  Zeilinger: I’m not so pessimistic. I don’t think that our time is significantly more regressing than other times were.  Smith: That’s good!  Zeilinger: I don’t share these negative viewpoints. I don’t know. I think the universe is just beautiful. I’m not pessimistic, I think.  Smith: I know that you collect maps, and you’re interested in the development of borders and countries. From that historical perspective, does that give you hope about the world?  Zeilinger: It tells you that we shouldn’t take today’s borders, particularly, but other situations, too seriously. Because things change a lot. If you want to learn something about how strange the world was, just look at the map around 1900, with all the colonies and so on. Today you would say that’s ridiculous. It’s impressive how that changed. Look at the development in Europe, specifically, or Eastern Europe. How many different large empires were where today Russia is. There was Lithuania, for example, a huge country. All this changes, all this is relative. One shouldn’t take the world as it is now, as given in a way that changes in itself are negative. One shouldn’t be too conservative about what is today. We can be surprised and we will be surprised. I once said I would be very surprised if the future wouldn’t surprise me. This is also a negative side, this is quite clear. This is not only the positive thing, who would have thought that Russia would make that huge mistake to attack Ukraine. It’s unbelievable. Who would have thought that? Some of my very good friends are historians who specialised in Eastern Europe. They were completely shocked and completely taken by what happens. Maybe my position is more, not a naive positive view, but more a question of humbleness. We have to accept how things develop. We have to ask ourselves what is our role here, what is going on and so on. But again, I’m a physicist, and probably very simple-minded.  Smith: You’re a physicist brought up in Austria and based in Vienna. How important do you think Vienna is and the culture of Vienna to your physics?  Zeilinger: I think I found out after some time that it was important for me. Because in Vienna you had this special culture around the turn from the, around the 1900s, even before and afterwards. In the sciences there was Ludwig Boltzmann. Boltzmann was interested in philosophy. He was the founder of thermodynamics as a molecular science. Schrödinger was about to become professor of philosophy in Czernowitz, which was the easternmost part of the Austro-Hungarian Empire, which is now Ukrainian. He said, we lost the war and therefore I turn to physics, something like that. This basic interest in Austria, in fundamental questions, be it in the sciences or be it in literature or painting or whatever, this still exists. That is an openness to foundations which I discovered that it was special when I came to America. When I came to America, I realised that this is not there. I’m pretty sure that that attitude was very important to my scientific development.  Smith: You have these conversations with the [Dalai Lama](https://www.nobelprize.org/prizes/peace/1989/lama/facts/), I understand. Are they revealing? Do your two worlds meld well?  Zeilinger: In a sense, yes, but in a different sense as many people in the West think. This discussion with him was not at all esoteric or mystical and so on. It was about hard facts, it was about science. For example, I asked him once, what is your evidence that karma exists? He said, that is a typical scientist question, I like it. The Dalai Lama is a very open mind.  Smith: Did he answer it, as well as liking it?  Zeilinger: No, he did not answer it. The point is that you have to go, and that is parallel to science, you go in your analysis deeper and deeper. That’s the same in Buddhism. At some point you have to say, that’s just the way it is. You cannot argue deeper. In science, it’s fundamental concepts. You cannot explain every fundamental concept. You have to start somewhere. To me, the big question in science now, is in physics, what are really the most fundamental concepts we have to use? Are we using the right concepts since Newton and so on? Or should we change somewhere? Maybe we find something and the next generation will knock on their heads and will say, oh my God, how could we have missed it? The facts of observation are not to be changed. But the fundamental concepts which we developed on the base of analysing the facts, that is flexible.  Zeilinger: A central book for me, which I recommend to everyone interested in physics, is a book called *Albert Einstein, Philosopher-Scientist*. It’s a collection of about 20 articles by leading minds in the 1950s, including mathematicians, and including also [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)‘s autobiography. In that book, there are still a number of questions today open, which were addressed there. That’s quite interesting. It’s very deep. I suppose when you write an article for a collection which is for Albert Einstein, then you write down your very best.  Smith: Yes, you do. Another book that might have had an influence on you is *The Hitchhiker’s Guide to the Galaxy.* In particular, the number 42. Let’s just play quickly this excerpt on 42.  *CLIP with reading from the book The Hitchhiker’s Guide to the Galaxy:* “The answer to the ultimate question of life, the universe and everything is… 42”, said Deep Thought, with infinite majesty and calm. ”42!”, yelled Loonquall. ”Is that all you’ve got to show for seven and a half million years’ work?” ”I checked it very thoroughly, said the computer, and that quite definitely is the answer. I think the problem, to be quite honest with you, is that you’ve never actually known what the question is.”  Smith: There’s a lot of depth in that particular quote. Tell me, tell us about the meaning of 42 for you.  Zeilinger: 42 is the name of my sailboat. My sailboat is the answer. Very simply.  Smith: How is your sailboat the answer?  Zeilinger: It’s the answer to life, the universe and everything. You go there and you sail and you are happy and everything is fine. Sailing is very special. I was thinking about why sailing is so special for me. It’s probably because when you sail, you are completely occupied by that activity. Both your mind and your body. You have no time to worry about anything else. You are completely immersed in something. That’s it.  Smith: The way you describe your work, it sounds so joyous. Do you find you actually need to escape from it to be on the sailboat?  Zeilinger: I don’t know. That’s a good question. I never ask myself that question. I don’t see sailing as an escape from it. I think it’s part of myself. When you work on science, there are also different stages. There are stages where you are also very deeply immersed. Everything in your head goes on and thinks about that and so on. That could be an analogue. I don’t know.  Zeilinger: I’m also in some sense a religious person, as you probably found out. Not in the sense of following the rituals of any given religion precisely. But in my life I always had the feeling that there is a God somewhere. I cannot argue that. You cannot argue that logically. You cannot give a reason, something like that. It’s the way it is.  Smith: Just a sense of that’s the way things are.  Zeilinger: That’s the way it is. Exactly.  Smith: To those listening who would say, and this of course is a debate, a discussion had all the time all over the world. To those who would say that the scientific concepts that you study and other scientists study are at odds with that belief. Do you have any repost?  Zeilinger: There is not everyone at odds with this belief. When you talk about religion versus science, there is no way to argue scientifically pro or con whether God exists. Either way is overstepping the limits of science. I have a feeling that that position is accepted by most people.  Smith: Yes. I’ve used up all my time with you, which is a shame because this has been absolutely lovely. I’ve really enjoyed talking to you.  Zeilinger: It’s better that we run out of time than the opposite.  Smith: Yes, isn’t that true? If we were just sitting here trying desperately to think of something else to say.  Zeilinger: Exactly, yes. Absolutely, yes.  Smith: Okay, good. Let’s leave it there then. I look forward very much to the next time.  Zeilinger: Thank you for the good discussion.  Brilliant: You just heard Nobel Prize Conversations. If you’d like to learn more about Anton Zeilinger, you can go to nobel prize dot org, where you’ll find a wealth of information about the prizes and the people behind the discoveries.  Nobel Prize Conversations is a podcast series with Adam Smith, a co-production of Filt and Nobel Prize Outreach. The producer for this episode was Karin Svensson. The editorial team also includes Andrew Hart, Olivia Lundqvist, and me, Clare Brilliant. Music by Epidemic Sound.  If you want to delve deeper into the mysteries of quantum mechanics, listen to our episode with Anton Zeilinger’s co-laureate Alain Aspect.  You can find previous seasons and conversations on Acast, or wherever you listen to podcasts.  Thanks for listening. |
| **Telephone**  **interview** | **0008 = AZ**  Adam Smith: May I speak with Anton Zeilinger please?  Anton Zeilinger: Yes, that’s me.  AS: Hello. My name is Adam Smith, calling from Nobelprize.org.  AZ: Can you just hold on for a second? Just a moment please.  [Music. ‘Please hold the line’]  AZ: Hello.  AS: As you may know, we have a tradition of recording extremely short interviews with new laureates.  [Phone rings]  AZ: Yes, absolutely, I saw yesterday the interview you did with [Svante Pääbo](https://www.nobelprize.org/prizes/medicine/2022/paabo/facts/), whom I know very well actually.  AS: Then you already know what questions might be coming.  AZ: [Laughs] Right.  AS: How did you hear the news?  AZ: Just when the secretary general called me, yes.  AS: Yes.  AZ: This was at 11 o’clock. I was sitting at home working on some paper, you know, and it came the phone call.  AS: What was your first reaction or action on hearing the news?  AZ: I was speechless! I’m still kind of … I don’t know what to say. I mean this is a fantastic appreciation. I would emphasise it’s also a huge appreciation for all the people who I worked with, starting from my teacher, Helmut Rauch, who started the foundations work in Vienna in late sixties, early seventies, and this was really a curiosity, but it was encouraging for me. Then I also talk of the 150 or even more students, who ever worked with me. I appreciate everyone. This was really something.  AS: It’s a very nice thing to say, because of course science is a very social thing, and I imagine that’s one of the key reasons that you’ve spent decades doing this, that it’s just such a lovely activity being with all these great people around you.  AZ: It’s lovely to work, to see the excitement in the eyes of young people when they realise how interesting things we are working on. For myself it was just curiosity. It was always curiosity, and still is curiosity. I make possible some work on applications in my group, but my interest is always curiosity. You know, life is short, and still I’m curious to see what will happen in the near future, or in the future as long as I am alive, I don’t know how I can follow this anymore.  AS: The concepts are very hard to get one’s head around. I mean, for ordinary people quantum mechanics is a mysterious world. But it’s a world that has been so robust that it’s held up to every challenge. It’s quite remarkable isn’t it.  AZ: That is actually really, really remarkable. There’s two things that are remarkable about quantum physics. One is that it is absolutely robust against all experimental challenges. The predictions which the theory makes for experiments are confirmed to incredible precision, and even in the most counterintuitive ways. On the other hand, the theory is also mathematically extremely beautiful. It’s probably one of the most beautiful theories ever invented by mankind. And these are two features which are so enormous. I try to convey this over to the general public, so I like to give talks about this to just regular people who have no background. I have a feeling that people can appreciate that.  AS: It’s really lovely to hear you talk about the joy to be gained from the beauty of a mathematical formula, even though of course most of us can’t quite see that beauty for ourselves, but to have it translated is a very special thing.  AZ: Yah, the beauty is … I don’t know how to define the mathematical beauty, but probably that with very few symbols, which are arranged in some kind of symmetric way, you can explain a whole lot of things from the smallest quantum particles up to the origin of the universe. That is beautiful. It’s amazing. It’s not a complicated thing in the basic, in the basic quantum mechanics, it’s still very simple, simple points.  AS: I can’t let you go with[out] asking about the phenomenon of teleportation, because they mention it in the press release, and I think people will be very excited by the mention of this word. I am.  AZ: We all know teleportation from StarTrek and so on, where somebody’s transported. Teleportation in quantum physics is somewhat different. It’s a transfer of information, and the reconstitution of this new matter, like, if you think, basically, the information is actually what defines everything. Like your body is defined by the information, how the atoms are arranged. It doesn’t matter whether it’s changed, for example the carbon molecules in your body, against some others. The matter’s not important, information is important. Using this quantum entanglement one can transfer the information from one object to another one without actually knowing the information. This is actually quite fascinating, quite interesting. But it’s extremely beautiful.  AS: It sounds both beautiful and potentially incredibly useful in the future.  AZ: The point is that this is one of the basics of how future quantum computers can talk to each other. They can send information from one quantum computer to another one.  AS: An amazing world is opening up. It’s been a huge pleasure speaking to you. We look forward to seeing you in Stockholm in December.  AZ: Yes, yes, thank you.  AS: Thank you. Bye bye.  AZ: Okay, bye bye. |
| Q3 | **Where does your passion for science come from?** |
|  | Anton Zeilinger: My passion for science goes back very, very far. Even as a child, I wanted to know how things work, and I was also curious what happens. We lived in the countryside in a small castle, it was not private, it was after war, so the castle was free to be used. My father was working there, and I’m told that I was sitting up there on the second floor of the castle and watching out all the time, as a child, three, four or five years old. The people in the village thought that I’m kind of crazy, which maybe I am, you never know. This is how it started, and as I said, I was interested in how things work. I was usually not interested in putting them together. Many physicists try to put things together. As soon as you know how it works, why should you put it back together again? You know it already, right? It seems to be curiosity, and it seems to be curiosity for everything. |
| Q3 | **Was there a single defining moment when you decided to pursue science?** |
|  | Anton Zeilinger: I don’t think that there was a single moment which made me this decide to go for science. There were various inputs, like my father had given me a microscope when I was 14 years old, and I played with the microscope. I had a fantastic teacher in physics, in gymnasium, as we call it, high school, and at the time when the others in my class, the other boys, were talking about girls and all this, I had a friend and we were talking for hours about cosmology and about the big bang and this and that. This was curiosity. |
| Q5 | **Was there a particular person who influenced you?** |
|  | Anton Zeilinger: My teacher in physics and mathematics, he was clearly excited about what he was telling us. That’s the most important ingredient in my eyes for a good teacher. He or she can make mistakes, and it doesn’t matter as long as the person is excited and you see that the whole soul is behind it. That’s enough. That’s it.  To be excited, you cannot fake. Young students, they know immediately what goes on in a teacher. They know it right away after 10 minutes. Let me make one point, another feature of a teacher is that the teacher has to take you as a person seriously. I had a teacher in another field who was very cynical and made jokes about us, but the fact that he made jokes meant that he took you seriously. That was a good teacher too. It is not fashionable today to say something like that, but you felt accepted and the teacher took you so serious that he even tried to make fun about you. At least that’s the way I saw it. The worst are teachers who don’t care. That happens unfortunately sometimes. |
| Q2 | **How do you cope with failure?** |
|  | Anton Zeilinger: There was only one big failure. There was one experiment, about 2000, where we tried to do something which was too ambitious at that time. We spent a lot of money and the results we got were so complicated that we didn’t understand it, so we gave up. This was the only really big failure in my academic life, having been too ambitious in terms of … and with my students. But otherwise, you meet challenges. There are things which you did not expect, new problems, and you don’t know how to solve it right away. But this can all be attacked by essentially stamina. Keep going what you want to do, and most important, in my field, in experiments, talk with your students, your postdocs all the time. Discuss, and certainly somebody has an idea, and off we go. |
| Q16 | **What was it like when you first started pursuing science?** |
|  | Anton Zeilinger: I was very lucky, which I found out later, for having been educated in Vienna. Because in Vienna, you still had or still have a spirit of openness to very fundamental questions. The idea that something has to be useful is secondary in Viennese culture, and that was extremely useful for me, as I discovered when I came to the US. The second point is that I had my PhD thesis supervisor was doing experiments on the foundations, fundamental experiments which were unique at that time, there were maybe two or three others in the world, I didn’t realise how unique this was. I was lucky to work on that thing. It’s an intellectual environment, which is extremely important. |
| Q4 | **How is science today different from when you first started? How can it be improved?** |
|  | Anton Zeilinger: Not that common, and it gets worse. Now we have a development that when you want to get money, you have to say what it can be used for. 30 years ago, when journalists asked me what can this be used for, I said, I can probably tell you this is not good for anything. We just do it out of curiosity. It is very important that you have the possibility to do something out of curiosity. In today’s funding schemes, you’re always asked, what is it good for? You’re also asked, what methods will you use to do this? In the beginning, I had no idea how to realise the things I wanted to do. This came slowly, it took years. What to me is my most important experiment is entanglement of more than two particles. We had the idea in the late 1980s, and the realisation was in the late 1990s. My appeal is really that funding agencies, universities, and so on, should be much more open to really curiosity driven research with no application in mind. I will fight for that as long as I’m alive. Not for me, but for the young people. The young people need to be encouraged to do this kind of thing. |
| Q7 | **What attitude do you need to be a successful scientist?** |
|  | Anton Zeilinger: This attitude is playfulness, personally, and that also means that like myself, I never worried about my career. I never thought about what will I do? Where will I get a job? This was just not a theme. I worked on this kind of thing because I was curious and I enjoyed it, and the rest came. I think today there’s too much worry about what will happen and so on. I tell young people, if you find something where you are curious about where you are excited about, do it. Don’t listen to what your supervisor tells you, what other people tell you. Because if you are excited about it, you will always be better than the others who are not excited about it. |
| Q8 | **How do you like to spend your free time?** |
|  | Anton Zeilinger: In my free time, I like to sail. I have a boat on the lake in Austria, and I love to just be on the boat. This is an old wooden boat, just to be on the boat and fix this or that. That’s already relaxing. Sailing, you stand the top. I also like to sail in the Mediterranean with the crew and so on. I think I know why sailing is so important for me. This is because when you are sailing, the complete mind is focused on just what happens on the boat and on the wind and on the waves and all this. There’s no possibility for your mind to go off and think about the usual daily problems or even the questions, what do you have to solve in science and so on. It takes your complete person, and that I think is a kind of recovery to get strengths and so on.  The second thing I like to do is to collect old maps and they collect old views, and with the maps, it’s a focus on political change, like a map of the Soviet Union is very interesting to look at today. A map of the British Empire is very interesting to look at. Even way back 100-200 years ago, the maps of Europe, like how big Sweden once was, for example. All these things are very instructive. It tells you in the end how unimportant some questions are. The name of my boat is 42, because this is taken from *The Hitchhiker’s guide to the Galaxy*. There was the question about life, the universe and everything, and the supercomputer produced the answer, which was 42. And my boat is the answer. |
| Q6 | **What was it like talking physics with the**[**Dalai Lama**](https://www.nobelprize.org/prizes/peace/1989/lama/facts/)**?** |
|  | Anton Zeilinger: I visited Dalai Lama twice, for a week in his residence in Northern India, and he went to my lab in Austria, and looked at the lab. He has a scientific mind, very clear scientific mind, and he asked the right questions. We talked about, on the one side, the basic statements of quantum physics, and on the other hand, he told us some of their philosophical findings. Not meditation and so on, but some of their philosophical findings, which are quite interesting. It was a very interesting discussion, it’s not written up yet, but at some day it has to be written up, what came out of it. What that actually tells me is there maybe are interesting parallels between Eastern philosophy and what we are doing, but I say maybe because some of these explanations are wisdom in hindsight. I told them, there were some leading Buddhist teachers and philosophers there also, and I told them when it is said that quantum mechanics has realised this or that, then I believe that under one challenge. Tell me one thing which we have not discovered yet, and I go to the lab and check it. This has not happened. |
| Q14 | **What about the future of quantum mechanics excites you?** |
|  | Anton Zeilinger: The most important question is why quantum mechanics. Quantum physics is probably the most beautiful mathematical theory humanity ever invented, and it’s also the best proven. It’s incredible how precise the predictions are realised. But as John Clauser said in an interview recently, he doesn’t know what goes on, why do we have this? This is in my eyes one of the most important questions, and I think it has to do with what is the role of information versus reality, and there is something where we can make a breakthrough, I think, and I hope this will happen. I have some ideas about that, and I want to spend the rest of my life working on these questions. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0009** |
| **Biographical** | Suki Manabe was born 21 September 1931. He is a Japanese-educated American meteorologist and climatologist who pioneered the use of computers to simulate global climate change and natural climate variations.  Suki was born in Shingu Village, Uma District, Ehime Prefecture, Japan. He enjoyed his childhood growing up in the Shikoku mountains where there were many beautiful cedar and cypress trees. As a young boy, he learned to fish for ayu (sweetfish) and amego (Japanese river trout) in the Dozan River.  Both his grandfather and his father were physicians, who operated the only clinic in the village. They traveled on horseback in the mountains caring for patients. Suki looked forward to dinners after his dad went hunting for rabbits and pheasants. Suki’s mom was very busy managing the financial matters for their clinic which was very challenging because many people in the village could not afford medical care. Because many people brought maple trees and other plants to pay the clinic for care, Suki’s family had a beautiful Japanese garden in front of their house.  A classmate recalled that, even in elementary school, he was already “interested in the weather, making comments such as ‘If Japan didn’t have typhoons, we wouldn’t have so much rain.’” Manabe attended Ehime Prefectural Mishima High School. When he was accepted into the University of Tokyo, his family expected him to study medicine, but he often complained that “whenever there’s an emergency, the blood rushes to my head, so I would not have made a good doctor.” Furthermore, “I had a horrible memory and I was clumsy with my hands. I thought that my only good trait was to gaze at the sky and get lost in my thoughts.” He joined the research team of Shigekata Shono (1911–1969) and majored in meteorology. Manabe received a BA degree in 1953, an MA degree in 1955, and a DSc degree in 1959, all from the University of Tokyo.  After finishing his doctorate in 1959, Manabe went to the United States to work at the General Circulation Research Section of the U.S. Weather Bureau, now the Geophysical Fluid Dynamics Laboratory of the National Oceanographic and Atmospheric Administration, where he continued to work until 1997. From 1997 to 2001, he worked at the Frontier Research System for Global Change in Japan, serving as Director of the Global Warming Research Division. In 2002 he returned to the United States as a visiting research collaborator at the Program in Atmospheric and Oceanic Science at Princeton University. He currently serves as senior meteorologist at the university. He also engaged as a specially invited professor at Nagoya University from December 2007 to March 2014.  **Family and personal life**  In the fall of 1960, Dr. Manabe met Nobuko (“Noko”) Nakamura in Tokyo, Japan. Suki and Noko were married in January of 1962. The couple had two children, Nagisa in September of 1963 and Yukari in June of 1965. Both girls attended Princeton Regional Schools and Yale University. Nagisa worked at Morgan Stanley and then went to Harvard Business School. Yukari went directly to Columbia Medical School. Both girls completed graduate school in 1991. Today, Nagisa owns and operates a certified organic farm in Princeton, New Jersey (River Stoan Farm) and is the Executive Director of the Northeast Organic Farming Association of New Jersey. Nagisa is married to Oscar Schofield, a distinguished professor at Rutgers University and Department Chair of Marine Sciences. Nagisa and Oscar have three children: Samantha, Allegra and Tobias. Yukari is a professor of medicine and the Associate Director of Global Health Research and Innovation at Johns Hopkins. Yukari is married to Dr. James Daniel Campbell, a Professor of Pediatrics in the Division of Infectious Diseases at the University of Maryland School of Medicine since 2001 in the Center for Vaccine Development. Yukari and James have five children: Catherine,Thomas, Matthew, John, and James.  After the girls left for college, Noko dedicated much of her time to the study of Japanese tea ceremony. She is the Philadelphia Special District Representative of the Executive Committee for the Omotesenke Domonkai Eastern Region USA. Omotesenke Domonkai is the association of the practitioners of *chanoyu* following the traditions of Omotesenke Fushin-an and was established with the objective of promoting it. Noko continues to study and teach tea ceremony in the Princeton area.  **Scientific accomplishments**  Working at NOAA’s GFDL first in Washington, DC and later in Princeton, New Jersey, Manabe worked with the inaugural director of GFDL, Joseph Smagorinsky, to develop three-dimensional models of the atmosphere. As the first step, Manabe and Wetherald (1967) developed a one-dimensional, single-column model of the atmosphere in radiative-convective equilibrium with a positive feedback effect of water vapor. Using the model, they found that, in response to the change in atmospheric concentration of carbon dioxide, the temperature increases at the Earth’s surface and in the troposphere, whereas it decreases in the stratosphere. The development of the radiative-convective model was a critically important step towards the development of a comprehensive general circulation model of the atmosphere (Manabe et al., 1965). They used the model to simulate for the first time the three-dimensional response of temperature and the hydrologic cycle to increased carbon dioxide (Manabe and Wetherald, 1975). In 1969 Manabe and Bryan published the first simulations of the climate by coupled ocean-atmosphere models, in which the general circulation model of the atmosphere is combined with that of the ocean.  Throughout the 1990s and early 2000s, Manabe’s research group published seminal papers using the coupled atmosphere-ocean models to investigate the time-dependent response of climate to changing greenhouse gas concentrations in the atmosphere (Stouffer et al., 1989; Manabe et al., 1991 & 1992).  They also applied the model to the study of past climate change, including the role of freshwater input to the North Atlantic Ocean as a potential cause of the so-called abrupt climate change evident in the paleoclimatic record (Manabe and Stouffer, 1995 & 2000).  His scientific journey is described in the book *Beyond Global Warming* written by Manabe and Broccoli and published by Princeton University Press in 2020.  **Awards and honors**  Manabe is a member of the United States National Academy of Science and a foreign member of the Japan Academy, the Academia Europea and the Royal Society of Canada.  In 1992, Manabe was the first recipient of the Blue Planet Prize of the Asahi Glass Foundation. In 1995, he received the Asahi Prize from Asahi News-Cultural Foundation. In 1997 Manabe was awarded the Volvo Environmental Prize from the Volvo Foundation. In 2015 he was awarded the Benjamin Franklin Medal of Franklin Institute.  Manabe has also been honored with the American Meteorological Society’s Carl-Gustaf Rossby Research Medal, the Second Half Century Award, and the Meisinger Award. In addition, he received the American Geophysical Union’s William Bowie Medal and Revelle Medal, and in 1998 he received the Milutin Milankovic Medal from the European Geophysical Society.  Manabe and Bryan’s work in the development of the first global climate model has been selected as one of the Top Ten Breakthroughs to have occurred in NOAA’s first 200 years. In honor of his retirement from NOAA’s GFDL, a three-day scientific meeting was held in Princeton, New Jersey in March 1998. It was titled “Understanding Climate Change: A Symposium in Honor of Syukuro Manabe”. The 2005 annual meeting of the American Meteorological Society included a special Suki Manabe Symposium.  Jointly, Manabe and climatologist James Hansen received the BBVA Foundation Frontiers of Knowledge Award in the Climate Change category in the ninth edition (2016) of the awards. The two were separately responsible for constructing the first computational models with the power to simulate climate behavior. Decades ago, they correctly predicted how much Earth’s temperature would rise due to increasing atmospheric CO2. The scores of models currently in use to chart climate evolution are heirs to those developed by Manabe and Hansen.  In 2018, Manabe received the Crafoord Prize in Geosciences jointly with Susan Solomon “for fundamental contributions to understanding the role of atmospheric trace gases in Earth’s climate system”.  In 2021, he was awarded half of the 2021 Nobel Prize in Physics, jointly with Klaus Hasselmann, for his contributions to the physical modelling of Earth’s climate, quantifying its variability, and reliably predicting global warming.  In 2021, he received the Order of Culture (文化勲章, *Bunka-kunshō*). It is a Japanese order, first established on February 11, 1937. The order has one class only, and may be awarded to men and women for contributions to Japan’s art, literature science, technology, or anything related to culture in general. The order is conferred by Japan on Culture Day, which is November 3 each year. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | Adam Smith: My name is Adam Smith from Nobelprize.org. Is it possible please to speak briefly with Professor Manabe?  Nobuko Manabe: Ah, just a second.  AS: Thank you.  Syukuro Manabe: Hello.  AS: Hello, yes, many, many congratulations on the award of the Nobel Prize.  SM: Thank you.  AS: There’s much excitement downstairs I can hear. Everybody’s very happy.  SM: Yeah, yeah, yeah. I never thought that I receive the Nobel Physics Prize for the work I have done long time, which I enjoyed very much, but I have been doing science of climate change. And when I look at all these former distinguished recipients, nobody received physics prize for my kind of work. And I really appreciate that the Swedish Academy of Science to choose this field – climate topics, climate change, which I enjoyed very much to do throughout my lengthy career which last more than 60 years, and so I am so surprised, but at the same time I dearly appreciate that Swedish Academy of Science choose my research for this honour of this year.  AS: I suppose they want to show how important a fundamental scientific understanding of the climate is.  SM: That’s right, yeah. I think one of the important things is understanding climate change. And what I did was, using climate model which I constructed, changing one thing at a time, just like a chemistry do a chemical, chemistry experiment, I made a numerical experiment using this model, changing one thing at a time, thereby understanding how climate system works, and how temperature change, how rainfall change, and why these changes result in the massive flooding which have been occurring near Beijing, and in Japan, and all this massive flooding. And how this very frequent drought has been occurring all over the world. I try to understand it using the climate model – why this is happening? I think, most important thing is understanding climate change. The prediction of climate change without accompanying understanding of it is no better than prediction of fortune teller.  AS: Such an important topic, such an important problem.  SM: Yeah.  AS: And almost everybody agrees about the importance of it, but tell me, why did you begin to be interested in meteorology and the climate in the first place? What drew you to the subject when you were young?  SM: Yeah, you know, it was, I went to graduate school of University of Tokyo. Then this graduate school for geophysics, I decided to focus on meteorology. At that time weather forecasting was more of an art, rather than science. You know, look at the past weather map and make a forecast based upon the sort of country and experience. But at that time at Institute of Advanced Study, Von Neumann, who is a pioneer of computers, among many other things, decided to do weather forecasting based upon laws of physics, which is the hydrodynamical equation, right? And so he started small groups and started studying weather forecasting. And this is the beginning of daily weather forecasting, which has become indispensable for our daily life.  AS: Over your career you’ve seen weather forecasting and climate prediction change from art to science. Amazing!  SM: Yeah.  AS: It’s been a pleasure to talk to you, and my congratulations.  Nobuko Manabe: Moshi moshi, Hi, hello?  AS: Hello, it’s Adam here.  NM: Okay, thank you.  AS: Thank you, and congratulations! What a day, what a day.  NM: Oh, thank you so much, thank you. Bye, bye.  AS: Bye. |
| Q3 | **I wanted to start off by asking where your passion for science comes from?** |
|  | Suki Manabe: I guess that when I was little, I really liked to look at the sky and daydream. I was interested in weather and the climate is sort of the average of weather. In a sense, I think my curiosity about weather and climate started when I was very little. |
| Q5 | **Would you say that there was a particular person, a role model, a parent, or a teacher that encouraged this curiosity in some way?** |
|  | I’m not sure, but I was a little bit of an unsociable kind of person. When I was in elementary school, I usually stayed at home, laid down and thought about something endlessly. I don’t think this is a good personal characteristic for some professions. But in my profession, it has been a very good personal trait, which I think has shaped my career as a research scientist. This personality, which is a shortcoming for most professions, has been good for me. |
| Q16 | **Would you say that daydreaming about science is something that you still do?** |
|  | That is right. I think about the same things day after day and I try to understand them but I can’t. I keep on day dreaming, thinking about the same things again and again and again. That has turned out to be a very important factor for my success as a scientist. |
| Q7 | **Is that something you would say is a quality that successful scientists need? What other qualities do you think you need to become a successful scientist?** |
|  | I guess that one of the things is curiosity. You are curious about something and then you think about why this is happening. That is what happened to me with climate change, which I have been working with for the last six decades. It’s a long time. This turned out to be a very fun thing to think about because, as you know, daily weather is very interesting. Why is the climate changing? Why is the climate changing the way it does? And what is controlling that change? I have always been interested in climate change such as global warming. I am also interested in why the ice age happened and why the climate was so warm when dinosaurs were living on this planet. It looks as if carbon dioxide is one of the important factors. I have been doing this driven by curiosity: why is our climate changing the way it does? I think this also helped me in my career. I’m curious about it. I was doing it not because climate change is very important for human beings. I never had that slightest idea climate was going to be so important when I was studying it in the 1960s. I never had the slightest idea that climate change was going to be such an important factor for human beings. What drove me was pure curiosity. |
| Q15 | **You’ve been working with the issue of climate change for six decades, what are your thoughts about climate change today? Do you have a message that you would like to share?** |
|  | When we talk about current climate change, which we are right in the middle of, we mainly think about temperature. But what is happening now is that the temperature change on this planet has affected the global water cycle – distribution of precipitation, distribution of evaporation from the continent. It has a profound effect on the water cycle of this planet. That is one of the reasons why we get droughts more and more frequently. For example in the sub-Saharan region in Africa, Sahel, I think that this change, which is ongoing, is starting to have a profound effect on our daily lives. This is why climate change has become such an important phenomenon. What is happening is that we are burning fossil fuels at a tremendous speed. We are burning coal, which has accumulated over a few hundred million years, in a few centuries instead of a few hundred million years. That is why the climate is changing so rapidly and posing profound problems for us.. |
| Q10 | **You are born in Japan but you have lived in the US for many years. How was that journey from Japan to the US?** |
|  | You know, the Japanese people always worry about each other – they don’t want to hurt each other’s feelings. So for example, Japanese people don’t want to say no. So even though your answer is no, you try very hard to moderate the impact of saying no to another person listening to you. In science this is not a very good thing, you have to say very clearly you disagree with each other. Then you think about how we disagree. Why do we disagree? In order to tell him I’m right and to prove that I’m right, you have to do additional research. What is that research? The other person also thinks about why we disagree with each other in order to prove he is right. What kind of result do you have to present to prove you are right? So in a sense, by disagreeing, both gain greatly and make good progress to understand the problem which we are tackling. When I came to the US, I found out that people can disagree openly in public. I thought this was a great thing. Of course, Japanese people are trying to think about each other, and that’s wonderful and that’s why they are successful. We live together harmoniously because of this trait. But, in a sense, I like the way things are done in the US. |
| Q11 | **How do you think that we can encourage more women and more diversity in science?** |
|  | I think this is very important. Women are half of our population, right? I think female scientists, even though they are very capable of doing a great job and getting married and having children, many of them stop doing research at that point. What is very important is to have more daycare centers so that they can put their children in daycare. I think more countries should invest more money in daycare, it is very important. You could give financial support for families who have more children. I would like to have every country in the world do that; to put more emphasis on these activities. |
| Q9 | **How did you celebrate the news of the prize? How did you react when you got the news?** |
|  | I have to say it was a big surprise because I never expected to receive it. It was a kind of shock to me and for five months after the announcement of the Nobel Prize, this news has gradually sunk in. Nobody doing the kind of work I was doing has got this prize. Almost everybody who got a prize got it for contributions for the advancement of modern physics, quantum mechanics and so forth. Nobody got this award for climate research before. But I am very grateful that the Swedish Academy of Sciences thought about giving me the prize. You know, I am 90 years old so at this age, I will die happily knowing that I got a Nobel Prize in Physics. It’s gradually sinking in and now finally I can relax and begin to think about reading a book which I never had the time to do before. |
| Q1 | **You turned 90 years old last year, what life advice would you give to a young person?** |
|  | Looking back at the last six decades of my career, I really enjoyed what I was doing and being curious. I liked what I was doing and digging deeper and deeper. Sometimes it was just a struggle to get through but looking back, it is a nice memory. I have enjoyed my life exploring the secret of climate change. The most important reason is; I was doing what I like most and probably what I was best suited to doing, as I explained at the beginning. There is a saying that what one likes one does well, so I would like to have young people do what they like in their life. I think when young people do what they like they usually do well. When you get more and more involved you enjoy it more and dig deeper. I really recommend that young people do things that they like and have a great life. |
| Q8 | **How do you like to spend your spare time?** |
|  | To be honest with you, I spent a major fraction of my life thinking about the same thing. I really didn’t do much else. Now at 90, I finally decided to stop doing active research. What I’m thinking now is to read a book about something which I never had time to read, I was too busy all the time. One of the topics I’m reading is how this planet evolved during the last four and half billion years and how life evolved starting from phytoplankton. Then more animals started developing and then us human beings. That is one topic I’m interested in reading about. At night, sometimes I want to relax more. I’m looking for some books. My wife has many books written in Japanese so I think I will start reading some of those. That’s how I want to spend my life. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0010** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0010 = KH**  Adam Smith: I was hoping to speak to Professor Hasselmann?  Frau Hasselmann: Yes, Klaus…  AS: Hello?  Klaus Hasselmann: Hello.  AS: Hello, is this Professor Hasselmann  KH: Yes, this is Klaus Hasselmann. Yes?  AS: How nice to speak to you. My name is Adam Smith. I’m calling from Nobelprize.org, the website of the Nobel Prize. Well first of all, many, many congratulations on the award of the Nobel Prize.  KH: Oh thank you. I’m completely surprised, I can’t quite understand it, but I get it. Okay, wonderful.  AS: [Laughs] How did the news reach you?  KH: I got a call about 10 minutes ago, which my wife took, and she explained that something was going to happen, which I didn’t quite understand, and apparently this was this Nobel Prize. So I was quite surprised.  AS: It sounds as if it’s come from a real bolt from the blue for you.  KH: Well it did, it’s entirely the bolt from the blue. I’m quite… I’m quite [unclear] I can’t understand this. I’ll wake up tomorrow morning and find out. Yeah. [Laughs]  AS: The Nobel committee for Physics emphasise that, you know, with this Prize they want to show that our knowledge about the climate is based on truly rigorous scientific foundations.  KH: Yes, it’s true.  AS: Yep. And of course, this has been the basis of your work.  KH: That’s right, yes. I can… I am a physicist, and I came to climate as a physicist so, this is true, yeah.  AS: And I suppose one of the things that, if there are sceptics out there, they find difficult, is that there is a difference between predictions about the climate, which could be made very accurately, and the weather we see about us every day, which changes.  KH: Yes, of course there’s a question of timescales. I mean, everything beyond a few years can be attributed to climate. And it’s only when you see it over a few years that you’re sure it is the climate change, and not just a weather impact.  AS: And your own work has very much identified the fingerprints of our activities on climate.  KH: Yes, yes that’s right.  AS: And what do you think is most urgently needed now?  KH: Most urgently needed is some action against climate change. I mean there are many things we can do to prevent climate change, and it’s a whole question of whether people will realise that something which will happen in 20 or 30 years is something which you have to respond to now, and that’s the main problem with the climate change. Ever since, I mean we’ve been warning about climate change for 50 years or so, and it’s just that people are not willing to accept the fact that they have to react now to something that will happen in a few years, and this is something we’ve been sort of battling against now for many years as climate scientists.  AS: Does it surprise you that people don’t just look back in history and see that the predictions are correct, and therefore, you know, it’s clear which way we’re headed?  KH: Well it’s not that clear, because there are so many natural variabilities super-imposed upon climate change, that it’s sometimes difficult to recognise that. I mean we have a change over say 10 years where things get warmer, you think it’s climate change, but it actually may just be a natural variability of weather, which occurs over… over several years. And so to distinguish between the long-term climate change and the shorter term of a few months or years that you see on weather changes is sometimes difficult to decide. And you also get many people that make all sorts of statements that appear in the press, which confuse people. So it’s difficult for somebody who’s not actually working in climate to recognise that we are actually changing climate until it’s become quite obvious.  AS: How do you feel about the conferment of the Nobel Prize and that fact that that will suddenly direct a great deal of attention to yourselves, and of course yet more attention to the problem on which there is already much attention?  KH: Well I’m very happy that they put the attention on the climate problem, which is very important. Whether they put the attention on myself I don’t know, we’ll see what happens. Probably not. I forget so many things that the journalists will probably give up pretty soon, interviewing me.  AS: I don’t think that’s going to happen! I think people will be banging on your door for some time to come. Well, it’s been a great pleasure speaking to you, thank you very much indeed, and once again many, many congratulations.  KH: Well, thank you very much, and I really have to wake up and see if this is all true, but it’s good to hear from you.  AS: Thank you. Bye, bye.  KH: Okay, bye, bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0011** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0011=Parisi**  **No script** |
| **Telephone**  **interview** | **0011 = GP**  Giorgio Parisi: Hello?  Adam Smith: Hello, am I speaking with Professor Parisi?  GP: Yes, I’m Giorgio Parisi.  AS: Hello, my name is Adam Smith. I’m calling from the website of the Nobel Prize. Many congratulations on the award of the Nobel Prize.  GP: Thank you, you are very kind.  AS: Of course there has been speculation that you might be in the running for a Nobel Prize for some years. How does it feel to be awarded the Prize?  GP: Well, let’s say that I’m very happy for, for … also because it’s a recognition of the importance of the field on which I’ve been working for so many time, and that I have developed, because I was one of the founders of the … of the study of complex systems in physics. So I am very happy that all this lifelong work has been recognised not only by other prizes but also by the Nobel Prize.  AS: Yes indeed. You work on such a variety of problems, from quantum chromodynamics to the movement of flocks of birds. What is it about a particular problem that attracts you?  GP: Well, my mentor Nicola Cabibbo was usually saying that we should work on a problem only if working on the problem is fun. So, I mean, fun is not very clear what it means, but it’s something that we find deeply interesting, and that we strongly believe that it is … I mean you won’t find fun in [unclear] because one gets a new idea of something unexpected and so on. So I tried to work on something that was interesting and which I believed that had some capacity to add something.  AS: Do you choose problems that are seemingly almost impossible? How hard should the problem be?  GP: Look, the problem may be relatively simple but maybe, I mean, not impossible but very hard … extremely hard to study, and we really don’t … we are not able for the moment to understand precisely. But I mean there are very, very simple problems that are very hard to understand.  AS: And this award really highlights the importance of the fundamental science behind understanding climate change.  GP: Yes, so that’s correct, because fundamental science is crucial. It’s crucial for understanding everything, and will not go too much towards, only in applied science, but it’s important that applied science and fundamental science go together. Because many times the application from fundamental science to applied science, applications that can be useful to humanity comes in some unexpected ways from your science.  AS: Do you think that in general people understand that, or does it need to be reinforced?  GP: I think that usually people do understand, but sometimes at the moment where a decision has been taken by the government, they’re sometimes … but some governments usually forget. I mean there are some countries like South Korea that spend a high percentage of money on fundamental science. Italy is very low level, but I hope and am confident that in the future this thing is going to change. But let’s see what happens.  AS: I suppose one thing that will change a little bit is your life for the next little while because the Nobel Prize brings a lot of attention and people call you all the time. I mean you had a lot of attention already, but this will just increase it. How do you feel about that, and the disruption?  GP: I think that … [laughs]. Well, I hope that they’re not going to change too much my life. Of course it will take me a few days to answer to all the phone calls that I had today, but it is important to have the people that are going to pay the bills of science to have some understanding of what science is going on.  AS: Yes, indeed. Well thank you very much indeed. It’s such a pleasure to speak to you. Once again, many, many congratulations.  GP: Thank you.  AS: Bye, bye. |
| Q12 | **Could you tell us a bit about your childhood. Were you always interested in science even as a child?** |
|  | Georgio Parisi: Yes, let’s say that. I do not have very precise memory but my mother tells me that when I was three years old, I was already able to read the numbers. The typical activity was that when we were waiting for the bus at the bus station I was able to read the number of the buses that were arriving. I think that is true – it’s what my mother tells me – and what I am certain of is that when I was at elementary school, I was very interested in astronomy. I was reading books for children on astronomy, popular books, I had an idea of the distance of the planets from the sun, more or less an idea of the diameters of the planets and so on. And at 10 years old I started to read science fiction. |
| Q3 | **Was there a particular moment when you decided to become a physicist?** |
|  | I think that during high school I was interested in mathematics and physics, but I was reading mostly non-professional books. I was reading something on the history of mathematics, the history of physics. There was a particular book on the history of mathematics that I liked very much.  When I finished high school, I started to think what I should do at university. My father wanted to me to do engineering. I found that engineering maybe was not interesting enough. I knew that I was very good at mathematics, but I had no particular idea of what mathematics was doing at that moment. I mean, this is my reconstruction, but in the end I decided on physics. |
| Q5 | **Was there a particular person, a teacher or role model, that influenced you in your early career?** |
|  | Not, not too much. I think that when I was at high school, from up to 18 going to university, I did not speak to people about physics and mathematics. I think that I was not proactive in searching for them because I was satisfied by what I was doing. Sometimes I went to the library and read essentially articles in the encyclopedia, but I did not have any particularly contact [with anyone about it]. I would say the only person that did have an influence on me but in quite an indirect way was a friend of my family that had a microscope. They gave me a small microscope where I could see some very small animals, of a fraction of a millimeter and so on, that could not be seen by the naked eye. I spent a lot of time collecting dirty water, putting it on the microscope and looking at them. So I think that this was a positive influence in the idea that nature is interesting, that it’s surprising, that you can discover things and so on, but that was something that was not at all related to physics or to mathematics. In that way I was an autodidact. |
| Q1 | **Do you have any advice that you would give to a young researcher or young scientist just starting out in the field?** |
|  | My advice is to try to understand his or her own abilities, and what interests them most. It’s clear that interests may change, abilities may be cultivated, but have some ideas of what they feel they can contribute. For example, for someone that is good in doing programming, maybe it’s not worthwhile doing lot of computation by hand. Or for someone that very much likes to do experiments, it’s better to do experiments instead of theory. Try to understand their own qualities, try to cultivate their own qualities and also be confident, because sometimes confidence is very important. If you are confident that you can do something you will do it.  Let me tell you a small anecdote, if I may. There was one conjecture on spin glass that we were not able to prove. Then someone told me that they proved this conjecture and they explained the proof to me, which was relatively simple. So, I was speaking with a friend of mine and he started the conversation by saying, well, I think that we are never going to be able to prove that conjecture, that it is too difficult. I told him, “No, no, no, you are wrong. Those people proved the conjecture.” And he said, “Ah okay, I see the proof.” And he told me the proof! The point was that he had more or less the same background of these two other people. So when I told him that the other two people have solved the proof, knowing what the other two people knew, he realised that it should be possible to do the proof. In 10 seconds or something like that, he told me the proof. It was quite amazing.  So you see that if you have confidence, that in some sense anybody if they were told that they have the capacity to solve the problem would be able to solve it. Self-confidence is very important. Of course, it’s clear that you can’t exaggerate with self-confidence. But I think that self-confidence is an important ingredient. |
| Q4 | **What do you like best about your work?** |
|  | Well, there are many things. I think that doing science is like playing puzzles or reading a detective story and trying to understand who is guilty before the author tells you. I think that most people like to play with puzzles but as a scientist I think this is on a different scale because the type of puzzles that we are interested in are on a much bigger scale. And what is important is that we don’t play by ourselves, it’s all of the scientific community trying to solve this puzzle. So we work together and this is an intellectual pleasure that is important.  Also, the other thing that I think is important is that sometimes when you understand something, you and your team are the only ones that know this particular thing and you want to communicate to other people. I think that is something that also gives you lot of satisfaction when you discover something or come to a conclusion about some phenomenon. |
| Q8 | **And what do you enjoy doing outside of work?** |
|  | Well, it depends. These last two years have been somewhat difficult because, with COVID, a lot of activities were not possible. I always like to read books – novels, histories of science, science fiction and so on. For example, just in the last year, I read many things by Asimov, some of Asimov’s novels I’ve not read before. Also I read all of the Foundation Series in order, one after the other, which I always read in some random order and not contiguously. I very much like to go skiing, to the seaside, to do this kind of of activity outside. And, after 50, 55, I started to be strongly interested in traditional dance, for example, one that I like to dance is Greek dance. That is something that I like very much. I also do some couple dances, for example the one that I was doing before COVID was Forró, [a Brazilian dance].  Also I like walking. I am quite lucky that near to my house is a big villa, Villa Ada, which is I think two by three kilometers, something like that. So it’s large villa and I very much like to go there to walk. |
| Q2 | **Do you think it’s important to relax and have an outlet outside of work?** |
|  | It’s clear that it is very important to relax. There’s always some kind of activity that in some way helps you to establish some good connection with your body in some sense… I mean, when you’re dancing, you have to pay attention to your body. It’s also good that there’s something that is very different from the usual thinking, if you have to think of the angle that you put the foot on the ground, if you have to think that before moving the foot on the ground I should move backwards a few centimeters in advance, I mean, it’s not necessary to do, but if you want to do with style, you should add such small movements and so on, concentrating all these kind of things. And also try to follow the music because you have to be on time. So all these activities they absorb you completely, and there are some kinds of activity that are completely different from what you do in your work so it’s very good for relaxing. |
| Q9 | **And how have you found your time since you have been awarded the Nobel Prize?** |
|  | One of the things I found most amazing was that in Italy usually in this period one walks in the street with masks. But even with a mask sometimes I find that people stop me or just pass nearby me and say congratulations professor. It was something that was quite amazing to be recognised in the street by people I’ve never met. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0012** |
| **Biographical** | Roger Penrose was born on August 8th, 1931 in Colchester, Essex, England. His father Lionel Sharples Penrose FRS was a distinguished medically trained scientist, who worked primarily on human genetics, studying mainly the genetic origins of numerous mental conditions, particularly the issue of Downs Syndrome. He had earlier, in around 1920, studied psychoanalysis, travelling to Vienna to attend lectures by Sigmund Freud, then returning to England to obtain a medical degree in Cambridge. Lionel had considerable talents also as an artist and as a chess problemist. He enjoyed playing the piano and a spinet (small harpsichord). He frequently occupied himself making things out of wood such as puzzles of various kinds and, in later life, gadgets that would reproduce themselves if shaken together appropriately. Lionel’s father, James Doyle Penrose was a Quaker and very distinguished painter of portraits and religious topics, and Lionel’s mother, Elizabeth Josephine Peckover, came from a very well-to-do religious Quaker pacifist banking family. Lionel eventually donated his entire inheritance to various good causes.  Lionel was one of four brothers, one of whom, Sir Roland Algernon Penrose CBE, became one of Britain’s leading surrealists, and was a good friend of Pablo Picasso and many others of the surrealist community. Roland’s second marriage was to an American, Lee Miller, a well-known 2nd World War photographer. Both Lionel and Roland served in the 1st World War in the Friends’ Ambulance Unit.  Roger’s mother, Margaret (Leathes) had studied medicine at Newnham College Cambridge. Her father, John Beresford Leathes FRS, was a professor of physiology at the University of Sheffield. Margaret’s mother, Sonia Marie Natanson (though named “Sara Mara Natansohn” on her birth certificate) was Jewish – although completely secretive about her heritage, even keeping her maiden name a secret. She had lived with her strict Jewish family in in St Petersburg, Russia, though born in Latvia. She was a concert-level pianist, having known various Russian composers and classical music performers. She had apparently left her family to study in Switzerland when she met John Leathes, and returned to England with him, whom she subsequently married.  Margaret first met Lionel on a Swiss mountain trip (the Grossglockner). She was a highly intelligent, talented, and attractive young woman with a medical degree and a talent for amusing writing. She had been Head Girl at her school, Bedales, in southern England. However, after her marriage to Lionel, Margaret found that her circumstances with him made it particularly difficult for her to develop these talents, except in relation to her children.  After Lionel died in 1972, Margaret gained some freedom to express herself. She married Maxwell Herman Alexander Newman FRS, a prominent mathematician, who had been a close friend of both Lionel and Margaret in earlier days (Max having accompanied Lionel on his early trip to Vienna, Max’s purpose there to meet the Viennese mathematical logicians). Max Newman was born in 1897 and died in 1984. Margaret was born in 1901 and died in 1989.  Max Newman had done particularly important work at Bletchley Park, during World War 2, being in charge of the “Newmanry” which was responsible for decoding Hitler’s especially secret *Lorenz*code that he used for communication with his generals. It should be mentioned also, that there was a crucial component to their being able to crack the Lorenz code, coming from a brilliant contribution from the mathematician W.T. Tutte. This decoding is not so well known publicly as that, associated with the name of Alan Turing, who cracked the Nazi Enigma code using electronic devices referred to as “*Bombes*”. Newman concluded that cracking the Lorenz code required the construction of a more flexible computer assembly, termed “*Colossus*”. This was designed and constructed by the research telephone engineer Thomas H. Flowers, under Newman’s guidance. It first worked on 1 June 1944, just in time for the Normandy landings on D-Day, 6 June 1944. The existence of the Colossus was kept secret until the mid-1970s, Colossus is now being commonly regarded as being the world’s first programmable electronic digital computer.  Margaret and Lionel had four children, Roger being the second-born of their three sons, the fourth being a girl, Shirley, born in Canada much later. The eldest, Oliver, was born on June 6th, 1929 and the youngest of the sons, Jonathan, was born on October 7th, 1933, and in early 1939 the five of them sailed to the USA on the Aquitania, in view of the impending danger of the 2nd World War, Lionel taking up a job in Philadelphia, Pennsylvania. There, the three boys attended a Quaker school, the main thing that Roger could remember about it being the forced nap that pupils had to have in the afternoon during which he never once succeeded in sleeping. In the summer of 1939, the family travelled to London, Ontario, Canada, where Lionel had obtained a job at the Ontario Hospital, and the family lived at 1000 Wellington Street, where they stayed until the end of the war with Germany, in 1945, when the family had also been able to celebrate, with their 4th child – a lovely baby daughter, Shirley Victoria, born on February 22nd 1945.  As for the boys’ schooling, while at London, Ontario, Oliver demonstrated his considerable precocity by moving rapidly up, owing to his extremely high rating on an IQ test and consequent attendance at what was called an “advancement class”, thereby skipping elementary school altogether and transferring to *Central Collegiate*, where he was two years younger than anyone else in the same class, and nevertheless being first or second in each subject. Jonathan was precocious in a different way, showing natural skills in most games, but particularly in *chess*. Roger did not excel at school while in Canada, having a “bumpy ride” at best, though the main problem was spotted by one of his teachers, Mr Stenett, who, with considerable insight, realized that Roger was basically just *slow*, and when subsequently given as long as needed in mathematics tests, could consistently get marks in the 90s where without the extra time would obtain perhaps 40% to 50%. This helped Roger’s confidence in later years! At home, Roger liked making things with moving parts that he had designed, like a perpetual calendar and a moon clock, and also some “pop-up” books that described simple stories that he wrote. He also found a close relation with Lionel constructing various regular polyhedra. Roger also gained much from his older brother Oliver, who read science-related books to him and, on one occasion, showing him what could be achieved with the manipulations of simple algebra. The family returned to England by boat in the summer of 1949, after the war with Germany had ended.  On returning to England, Lionel took up a professorship at University College, London, named the *Galton Chair of Eugenics*. However, Lionel, being strongly against the notion of “eugenics”, regarding this as a distinctly unsavoury political movement, rather than a scientifically motivated pursuit, was determined to change the name of his chair, which he finally succeeded in doing, changing it to its current title “Galton Chair of Human Genetics”, though this change took many more years than Lionel had anticipated. As far as the schooling of the three of their boys was concerned, Oliver was already at the stage of going to University, so he embarked on a 3-year degree course in Physics at University College. However, it was perhaps the jolt of moving directly to University in a different country, at the young age of 16, had been a bit more than had been anticipated, and he ended up getting what he referred to as “a miserable 2nd”. However, it was good enough for him to get a place to do research in statistical mechanics at Cambridge in 1948, at the age of 19.  In contrast, Roger started at University College School (UCS), in Hampstead, north London at the age of 14 in Grade 3, in which he was one year *older*than the normal age of 13 for that grade, apparently due to his grossly insufficient knowledge of Latin, for which he had obtained a little private tuition in Canada.  Fortunately, he was able to be top of the class that year and was promoted up to Grade 5 for the next year. After that, he was able to impress his teachers with his natural grasp of mathematics – geometry in particular – once finding a nice geometrical result unknown to his teacher.  At one point, Roger told Lionel that his maths teacher had informed his class that on the following day he would explain about the ideas of *calculus*. Lionel, with an element of desperation, immediately took Roger to a table in the corner of the room and gave him a rapid account of the ideas and the beauty of calculus! This made a big impression on Roger, not necessarily that he understood everything that Lionel told him, but more that Lionel regarded calculus as being such a beautiful and powerful set of ideas that he couldn’t bear to have someone else have the privilege of being the first to reveal this magic to Roger! Later, Roger learnt from Oliver that Lionel had done the same thing with him, several years earlier!  One day in Roger’s second year at UCS, there was an event when each pupil in the class, in turn, had to go up to a table in the room where the headmaster (Mr Walton) would discuss which specialist subjects would be appropriate for that pupil, for his final two years of study. When Roger’s time came, as he walked up to the table, he was of the clear opinion that he would be the one to carry on the family tradition to study medicine. Both Lionel and Margaret were keen on this. For many years, they had regarded Roger as being the obvious one to carry on the family tradition. Clearly Oliver would not do, as he was dedicated to the study of physics. Moreover, Jonathan’s primary interest was chess, having never expressed any interest in science at all. As Roger walked up to the headmaster’s table, he also believed that he would become a doctor – or perhaps a *brain surgeon*as even at that time he had hopes that he might find something out about how that strange and wonderful organ actually worked! Accordingly, Roger sat down, facing the headmaster, firmly believing that he would be a doctor. When the headmaster asked Roger what subjects did he want to study in his final two years, Roger asserted: “biology, chemistry, and mathematics”. However, the headmaster immediately responded: “No, that combination is not possible. If you want to do mathematics you can’t do biology; if you want to do biology, you can’t do mathematics. Make your choice.” Without hesitation, Roger said “Mathematics, chemistry and physics”. At that moment his medical career evaporated! His love for mathematics had become too strong for him to leave that subject behind.  When Roger returned home, and explained to his parents what had happened, they were furious, thinking that Roger had been too influenced by a schoolfriend who wanted to study nuclear physics, which they regarded as a taboo subject because of the nuclear bomb. How could he give up his medical career so easily? In the end, however, they won their case. Not only did Roger’s sister Shirley become a doctor, but she married one: Humphrey Hodgson – two for the price of one!  The next such conflict came when Roger wanted to study for a BSc Mathematics degree at University College London. Again, Lionel was unhappy about this, arguing that just studying mathematics alone was too limiting, and a broader perspective on scientific life would be much preferable. After having some difficulties in persuading Roger, Lionel consulted one of his mathematical colleagues at UCL, Hyman Kestelman, who very generously constructed a collection of around 12 different somewhat unusual mathematical problems, giving Roger the rest of the day to see whether he could answer perhaps two or three of them. By the end of the day, Roger had answered all of them, apparently almost all correctly. This impressed Kestelman enough to persuade Lionel to allow Roger to study for his mathematics degree, which he completed, in 1952, after three years, obtaining 1st Class honours. It may be mentioned that he did not just concentrate on his degree work during this period, but also spent time developing other ideas with colleagues, particularly Ian Percival (subsequently FRS) and Peter Ungar. During his 2nd year, Roger gave a seminar at UCL (which Lionel attended), on a geometrical theorem that Roger had found, concerning 8 conics, with numerous remarkable specializations – currently still unpublished!  It is undoubtedly true that from his particular family background – especially from Lionel, but definitely also, on occasion, from Oliver – Roger had grown up with a deep appreciation for science, mathematics, games, puzzles, and geometrical patterns. His siblings all became distinguished intellectuals. His older brother Oliver became a highly respected professor of statistical mechanics and FRS, having done important work on liquid helium and Bose-[Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) condensates, partly collaborating with [Lars Onsager](https://www.nobelprize.org/prizes/chemistry/1968/onsager/facts/) (1968 Nobel Prize in Chemistry). Roger’s younger brother Jonathan was a chess prodigy, winning the British Chess Championship a record 10 times (7 consecutively), and once beating the reigning world champion Mikhail Tal in a chess game, becoming a grandmaster and world leader at correspondence chess. Moreover, Roger’s sister Shirley became a distinguished geneticist.  In such an intellectual environment Roger’s entry into a research career was hardly unexpected, and officially started when he was accepted for research at Cambridge, in pure mathematics, specifically in *algebraic geometry*under the distinguished Cambridge mathematician William V.D. Hodge. Roger had perhaps felt a little uneasy about his choice of specific topic to do research on, having been given a list of possible topics to choose from, only one of which he could really understand. This concerned what are called “Cayley forms” (or “Chow forms”), a Cayley form being an intriguing but unusual way of representing an “algebraic variety” of any particular dimension, where an *algebraic variety*is, in essence, a curved space defined by algebraic equations. The problem that Hodge had suggested was to find a formula for the Cayley form of the *intersection*of two algebraic varieties, in terms of the Cayley form of each of them individually. Although this seemed like a complicated problem, its nature was clear, not require an understanding of the more sophisticated abstract conceptual ideas that Roger had not yet come to terms with.  Perhaps he had suffered somewhat from the fact that practically all of the graduate students starting mathematical research at Cambridge would have done an extra year, following their undergraduate degrees, probably doing Cambridge “Part 3”, or possibly, for a student coming from outside Cambridge, some other qualification judged as effectively equivalent. But Roger had had no such prior preparation. After a few weeks, Hodge, perhaps sensing an unease in Roger’s particular choice of research topic, suggested that he might sit in on a supervision session of one of his other beginning graduate students, working on a different topic, to see whether that might be more to Roger’s liking.  At this point, it should be mentioned that Roger was one of four graduate students taken on by Hodge that year. As it eventually turned out, one of the students gave up, after several weeks, while another (Michael Hoskin) did his three years of research, and wrote a very decent thesis to obtain his PhD, but then gave up mathematics to become a philosopher and historian of science, becoming a pre-eminent historian of astronomy. It was the third student in the group whose research session Hodge suggested that Roger might sit in on. Roger did this, but felt totally bewildered, finding that he could not really understand a word of what was going on! He came away thinking “If they are all like this, what am I doing here?”  What Roger didn’t know was that this student, a “Mr Atiyah” was no ordinary student. He would subsequently become Sir Michael Francis Atiyah OM FRS, President of the Royal Society, First Director of the New Isaac Newton Institute in Cambridge, winner of the highly distinguished 1966 Fields Medal in mathematics and a very early winner of the new Abel Prize for 2004 (considered the effective equivalent of a Nobel Prize, in Mathematics) and, indeed, Britain’s leading mathematician. So, they were not “all like this”! Roger stuck with his choice of topic, and after various twists and turns taking him in strange directions, he did eventually write a thesis which grew out of this “Cayley form” topic, finally obtaining his PhD somewhat belatedly in 1957.  As a way of overcoming his uncertainty and unease, with such highpower mathematical colleagues, Roger brought with him to Cambridge a 6-piece assembly puzzle – which he had designed and constructed from Perspex acrylic (using a hacksaw and a broken file) some 6 months before coming to Cambridge. The pieces would have to be put together by means of a complicated locking mechanism involving confusing-looking angular parts, to make a regular tetrahedron when assembled. It went the rounds among the various mathematics graduate students (including Atiyah) taking each of them about 5 hours or more of puzzling time to find the solution. Roger found this to be a good way to gain respect from his mathematical colleagues, despite his lack of confidence with the high-power mathematical activity going on, which he felt unable to keep up with.  The research problem that Roger chose to work on was not really “mainstream”, and it led him in some unorthodox directions, such as devising an unusual diagrammatic notation for the algebra of complicated systems of tensors, these being regarded by Roger as abstract algebraic entities. Certain such “abstract tensors” would have to be taken to be *negative dimensiona*l and these provided a formulation of Roger’s concept of “spin-networks” which much later were picked up by others in attempts to provide a combinatorial basis for a “quantized space”, such as in the theory of *loop quantum gravity*(developed by Ashtekar, Rovelli and Smolin).  One of the more orthodox concepts arising from this work on Cayley forms was the concept of a “generalized inverse”, which exists uniquely for any matrix with complex elements, and was the topic of one of Roger’s very earliest papers. It turned out that this idea was not actually new, having been initially found by E.H. Moore, who was primarily a philosopher, and Moore’s announcement of this discovery apparently lay deep within one of his philosophical treatises. It seems that this “Moore-Penrose pseudo-inverse” finds application in certain statistical problems, arising from Roger’s follow-up paper on this topic.  Roger’s interest in this “generalized inverse” notion arose from its use in the Cayley form problem, in the special case when both the algebraic varieties involved are simply collections of hyperplanes. The required algebraic expression is then a generalized inverse. However, this expression would need to work also when the algebraic varieties are not just collections of hyperplanes, but then it turns out that the expression does not always work and consequently there cannot be a polynomial solution to this general Cayley form problem.  But why should we expect a polynomial solution in any case? This expectation arose from an observation that Roger made very early on, that if this Cayley form problem were expressed in terms of “dual variables”, rather than the original coordinates, then the problem looked much neater than in its original form, and when Roger explained this to Hodge, he was very encouraged, and it looked as though a polynomial solution to the whole problem was very likely. But when Roger subsequently informed Hodge of his negative conclusion for an overall polynomial solution, it appears that Hodge didn’t believe him, but was too polite to say so directly! Instead, at the end of Roger’s first year Hodge decided that John A. Todd would be better as a supervisor for Roger, apparently because he thought that Todd would be better than himself at dealing with the very complicated expressions that Roger had become involved with.  However, there were two misconceptions involved in this decision. The first was that Roger was happy dealing with complicated algebraic equations! This misconception arose because Roger was able to use his diagrammatic notation, whereby certain equations can look pretty simple, although when written out in conventional notation can appear extremely complicated. The other misconception seems to be that Hodge thought Roger must have made a mistake in his calculations, and that Todd should be able to sort him out. Roger eventually found out that Hodge held this view only when, later in Roger’s third year, Todd had repeated (in conventional notation) a particular critical case of Roger’s calculations, finding that Roger was correct about the failure of polynomial solutions, and suggested to Hodge that he, also, might repeat that calculation. Roger was very struck by the *delight*in Hodge’s expression when coming up to Roger to tell him he had been correct all along!  A couple of years later, in 1957, Roger wrote a much more general document for his application for a Research Fellowship at St John’s College Cambridge, in which he provided an argument to show that whereas polynomial solutions do not always exist for algebraic/geometrical problems of this general kind, nevertheless, there is always a solution in terms of *quotients*of polynomials or as *factors*of polynomial outer products. There was no indication, however, of what this could look like, in the case of the Cayley form problem.  When he had been a graduate student in Cambridge, still working for his PhD in pure mathematics, Roger developed a strong friendship with Dennis Sciama, who had been a graduate student of the great physicist [Paul Dirac](https://www.nobelprize.org/prizes/physics/1933/dirac/facts/). Dennis had been a colleague of Roger’s brother Oliver and had first met Roger in the Kingswood Restaurant in Cambridge, when Roger had come up from London and was visiting Oliver. Roger posed a query concerning Fred Hoyle’s very stimulating radio talks on cosmology at that time, but Oliver referred Roger to Dennis, who was sitting at another table. Dennis had no immediate answer but was impressed by Roger’s genuine interest in cosmology, so that when Roger later came to Cambridge as a graduate student, Dennis felt that it was worth developing Roger’s cosmological interests further, particularly in relation to the “steady state” model of cosmology, of which both Hoyle and Sciama were strong proponents. As it turned out, this friendship proved very valuable to Roger, as he learned a great deal of physics from Dennis. Not only did Dennis have a considerable knowledge of physics over a broad range, but he was an excellent expositor and had friends who were experts in several areas of physics, and often made efforts to bring such people together if he felt that it could be valuable in promoting research. Their friendship continued at a high level until Dennis died in 1999.  Despite Dennis’s important influence on Roger in opening his eyes to the wonders of physics, and how he might divert Roger’s talents in that direction, it should be mentioned that there were also other influences on Roger in that direction. In his early years as a graduate student, he attended three courses of lectures that could be said to have had greater influences on his future research than the pure-mathematical courses that were of direct importance to his official research project. These were an impressive course by Herman Bondi on general relativity (clearly of great relevance to Roger’s later work in that area), a course by Dirac on basic quantum mechanics (clearly also of later relevance), and a course by S.W.P. Steen on mathematical logic.  The importance of Steen’s course to Roger was that he described the notion of *computability*(Turing machines, etc.) and Gödel’s theorem(s), the latter being a revelation to Roger, providing the case that the quality of human *understanding*cannot be a computational process. Many years later, this insight led to two of Roger’s semi-popular books *“The Emperor’s New Mind”*and *“Shadows of the Mind”*(Oxford University Press 1989 and 1994), where Roger presented his case that the phenomenon of consciousness (specifically “conscious understanding”) could not arise from classical-physics processes, nor even from Schrödinger’s quantum evolution of the wave-function, but had to be an effect of the other part of quantum mechanics, namely the *“collapse of the wave-function”*(denoted **OR**= *objective reduction*) which Roger later provided arguments for it being an objective *gravitational*effect – a “gravitisation of quantum mechanics,” rather the more usual reverse endeavour of “quantized gravity”. When Stuart Hameroff, (University of Arizona) read *“The Emperor’s New Mind”*, he contacted Roger to suggest that neuronal microtubules (then unknown to Roger) might be promising locations for preserving quantum coherence long enough for this **OR**effect to be appropriately “orchestrated” thereby providing the macroscopic effects of consciously controlled actions. Though undoubtedly speculative in various respects, this “Orch-**OR**” proposal is now regarded, after some 20 years, as a serious contender amongst current theories of consciousness.  There were also other topics that Roger worked on from time to time. He constructed self-contradictory pictures, stimulated by the works of M.C Escher, called “impossible objects”. He also produced quasi-symmetric tiling patterns, which have close relations to the *quasicrystals*discovered by [Dan Shechtman](https://www.nobelprize.org/prizes/chemistry/2011/shechtman/facts/) (2011 Chemistry Nobel Prize).  Yet, Roger had a particular respect for Dennis Sciama, not only for his broad understanding and promotion of physical science, but also for his *scientific integrity.*For over a decade, Dennis had been a strong promoter of steady-state cosmology, according to which the universe had no beginning, its eternal expansion being sustained by the continual creation of hydrogen to compensate for the depletion of material due to the expansion. However, when in 1964 [Penzias](https://www.nobelprize.org/prizes/physics/1978/penzias/facts/) and [Wilson](https://www.nobelprize.org/prizes/physics/1978/wilson/facts/) (1978 Physics Nobel Prize) provided a convincing refutation of the steady-state model, with their discovery of the *microwave background*(CMB), Dennis made gave numerous powerful lectures refuting his earlier viewpoint, and now firmly supporting the idea of a “Big-Bang” origin for our universe and strongly encouraged research into the physical nature of this initial state.  This momentous shift in viewpoint strongly influenced Roger’s own thinking. How is one to deal with the physics of this initial state, where space-time curvatures appear to have to diverge to infinity, resulting in what is referred to as a “singularity” in the classical space-time structure. Large curvatures mean small radii of curvature, so one appears to be forced into considering the nature of physics at the ridiculously tiny “[Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/)-scale” lengths and times (~10–33 cm and ~10–43 s) which are enormously smaller than those encountered in ordinary particle physics. This is indeed taken to be the realm of *quantum gravity*, where it is supposed that the very nature of space-time must itself be treated in some kind of quantum-theory terms, whether or not the above-mentioned “gravitization” of quantum mechanics also plays a role in unifying these two great theories of 20th century physics.  Roger’s own concern with such “singular” space-time states occurred several years earlier, being basically initiated by a lecture given by David Finkelstein, in early 1959, in the second year of Roger’s Research Fellowship at St Johns College Cambridge. This talk was given at Kings College London, and Dennis drove he two of them there, having persuaded Roger that the talk would be of interest to him – as indeed it turned out to be! Finkelstein’s talk was to show how, using an appropriate choice of coordinates, one can eliminate what had been commonly referred to as the “Schwarzschild singularity” in Karl Schwarzschild’s famous solution of Einstein’s equations for the curved vacuum space-time for the gravitational field of a static spherically symmetrically symmetrical body. This “singularity” occurs at the radius *r = 2m*, in units where Newton’s gravitational constant G and the speed of light c are both taken to be unity: G = 1, c = 1.  At this radius, a term in Schwarzschild’s expression for the space-time metric becomes *infinite*, and this is why this radius was referred to as a “singularity”. However, this feature arises from a demand that the solution is *time-symmetrical*, and Finkelstein, in his talk, showed how an elegant time-*asymmetrical*change of coordinates can remove this singularity, providing a picture that we currently refer to as a “black hole”. This was the first time that Roger had seen this extension to within the *r = 2m*radius (though he later learned of other such ways of extending the Schwarzschild solution to within this radius, though not done with the insight or elegance that Finkelstein had demonstrated in his lecture.  After the talk, Roger told Finkelstein about spin-networks, which intrigued him. He later told Roger that on that day they had “swapped topics” since Roger subsequently worked on general relativity and he on combinatorial space-time, this being his approach to the issue of *quantum gravity.*  Roger began to wonder about the singularity at *r*= 0, that persists despite the coordinate change. It had seemed to him that this central singularity was much more robust than that at *r = 2m*, especially since the space-time curvature becomes *infinite*at *r*= 0. He began to wonder whether there might be some theorem which showed that even if we perturb the solution away from the spherical symmetry assumed by Schwarzschild, a space-time singularity would persist. He had not heard of such a theorem and began to wonder whether some different slant on the equations of general relativity might be helpful for this. His thoughts turned to the theory of *2-spinors*that provides a distinctive way of analysing space-time. This had been greatly clarified for him by Dirac’s 2nd-term lectures in early 1958. In Roger’s diagrammatic notation, the tensor lines in the diagram become double stranded 2-spinor lines, the formalism allowing manipulation of strands independently, thereby providing a way of examining general relativity in ways that are not so immediately addressed in the standard tensor formalism. Many years later, Roger collaborated with Wolfgang Rindler to provide a 2-volume account explaining these procedures in detail (including the “spin-coefficient techniques introduced by Roer and his close colleague Ezra T. Newman): “*Spinors and Space-Tine*” (Cambridge University Press, 1984 and 1986). Volume 2 also gave an extended account of the theory of *twistors*, that Roger had introduced in 1967, and which became a major part of Roger’s later research, involving many of his students.  The 2-spinor formalism has an advantage over the standard tensor formalism which makes it much clearer that the free gravitational degrees of freedom are described by the *Weyl conformal tensor,*closely analogously to the way that the electromagnetic degrees of freedom are described by the Maxwell field tensor. Here “conformal” refers to the structure given by the metric only up to proportionality. The conformal structure of spacetime is effectively its *causal structure*, i.e., it determines which points can be connected by time-like or null (light-like) curves, this being central to the analysis that Roger eventually used to show that singularities cannot be avoided in gravitational collapse, irrespective of the spherical symmetry of Schwarzschild’s space-time, thereby providing the *theorem*that Roger had wondered about in 1958, following Finkelstein’s lecture. Roger’s 1965 paper on this, earned his share of the 2020 Physics Nobel Prize. It had been motivated by the discovery of the violently energetic *quasars*, indicating the presence of very distant enormous gravitational collapse events – *black holes*!  The 2-spinor formalism had earlier been found useful by Roger in relation to work being carried out mainly in the 196os, concerning gravitationally radiating systems and the mass-energy carried away by the gravitational waves. Following initial contributions by Andrzej Trautman and Bondi, with collaborators, it had indeed become clear that in an asymptotically flat space-time, there was a clear-cut contribution to the *mass*in the waves, carried away from the sources.  In this work, an intriguing an effect, known as the “peeling-off” property of the *Weyl*tensor, was pointed out by Rainer Sachs. This was best understood in terms of 2-spinors, and Roger showed, in a 1965 Royal Society paper, that Sachs’s peeling property can be understood as the spin-2 massless field described by the Weyl tensor being *finite*at a conformally defined *boundary J,*attached smoothly to the space-time, *J* being referred to as the space-time’s “conformal infinity”.  Roger also studied cosmological models, using this same conformal technique for “bringing infinity in” to provide a smooth conformal boundary *J*. He obtained a clear role for Einstein’s cosmological constant Λ, finding that Λ>0 (the observed “dark energy” value) corresponds to *J* being *spacelike*, where in the case of asymptotically flat space-times, *J* is null.  This conformal “squashing down” of the future can be used also in the reverse sense of conformally “stretching out” the big-bang singularity of a cosmological model to obtain a (normally spacelike) initial boundary *B*. However, this situation is very different from the *time-reverse*of a realistic collapsing universe, which would involve the congealing of numerous black-hole singularities, very probably involving wildly complicated oscillating and diverging Weyl curvature, as suggested by the BKLM picture (Misner in 1969 and, separately, Beinskii, Khalatnikov and Lifshitz in 1970). One may regard BKLM as presenting an enormous entropy in the gravitational degrees of freedom, hugely exceeding the already enormous Bekenstein-Hawking entropy in the individual congealing black holes, the latter already vastly swamping all other forms of entropy in our observed universe. All this is consistent with the 2nd Law of Thermodynamics and with direct observations, where the *earliest*direct observations reveal a very uniform CMB, indicating that this enormous reservoir of gravitational degrees of freedom had still hardly been touched, 380,000 years after our Big Bang!  No conventional form of quantum gravity could provide such a *vast*time-asymmetry in the past/future singularity structure. Nor could the mere introduction of an “inflation field”, the assumed source of an inflationary very early stage of the universe, so we appear to be presented with a fundamental conundrum.  Accordingly, in 2006 – as described in his books *Cycles of Time*(Knopf 2010), and *Fashion, Faith, and Fantasy in the New Physics of the Universe*(Princeton University Press 2016) – Roger put forward his exotic “conformal cyclic cosmology” proposal (CCC), proposing that the Big Bang was in fact the *conformal continuation*of an earlier “aeon” whose conformally squashed remote future joins smoothly to our conformally stretched Big Bang of our own “aeon”, this continuing in both directions with an unending succession of such aeons, joined successively in this way. This incorporates a proposal due to Paul Tod that the conformal smoothness of our *B* suffices to characterize its extraordinary specialness. Though highly unconventional, this model not only resolves the aforementioned conundrum, but has also obtained some remarkable observational support, the strongest, published in 2020 in the *Monthly Notices of the Royal Astronomical*Society by Daniel An, Krzysztof Meissner, Pawel Nurowski, and Roger, confirming a predicted effect of CCC at a 99.98 % confidence level.  Roger Penrose had two marriages: in 1958 to Joan Isabel Wedge (divorced 1980; died 2019) and in 1988 to Vanessa Dee Thomas. There were four sons altogether: 1963 Christopher Shaun, 1964 Toby Nicholas, 1966 Eric Alexander; and then in 2000 Maxwell Sebastian. |
| **Autobiography** |  |
| **Podcast** | **0012=Penrose**  No script |
| **Telephone**  **interview** | **0012 = RP**  Roger Penrose: Hello?  Adam Smith: Hello, this is Adam Smith speaking.  RP: Yes, hello.  AS: Many, many congratulations on the award of the Nobel Prize.  RP: Thank you so much, it’s much appreciated.  AS: How did the news actually reach you?  RP: [Laughs] All a bit peculiar – I’m not sure I want to go into it all. No, I had a call from Petrona, who received a message from the Swedish Academy and she wasn’t sure what it was about, but I think she guessed what it was about, and she wasn’t allowed to speak to them, and they tried to contact me and then their phone went dead and then I hung up and then finally they called me back again to tell me about it.  AS: The news made it to you in the end, so …  RP: Yes!  AS: It’s yet again a nice demonstration of the interplay between theoretical and experimental physics, your discovery.  RP: Yes, that’s true, yes. It was, I mean, what I did was basically in 1964, so we’re going way back, in which … it was just a little while after the quasars had been observed and people had found this very puzzling. It was a paper in 1939 by Oppenheimer and Snyder with a theoretical model of a collapsed … of a dust cloud, and it was more or less the kind of situation we would now refer to as the collapse of a black hole. But the thing is they had first of all dust, and dust by definition is something with no pressure, so there’s nothing to stop it. And secondly it was completely symmetrical, so everything fell in towards the centre and so since there was nothing to stop it you got this singular point in the middle and actually a model which looks like a black hole. But not many people believed it, most particularly because of the symmetry. When the Russians, the two Russians, Lifshitz and Khalatnikov, and they had written a paper that more or less said that in the general case you would not get singularities. I looked at the paper and I sort of thought that the way they were doing it wasn’t terribly convincing, that I didn’t know whether to trust it, and so I started thinking about it on my own and thinking about this problem in a more geometrical way, not really solving equations because you know it’s too complicated, and not making simple assumptions about symmetry because that’s the point, you mustn’t have that, so I produced arguments. There’s a little bit of a story about how the idea came to me actually. I don’t know, do you want a story about that?  AS: I’d love a story, yes please.  RP: At that time I was at Birkbeck College, and a friend of mine, Ivor Robinson, who’s an Englishman but he was working in Dallas, Texas at the time, and he was talking to me … I forget what it was … he was a very … he had a wonderful way with words and so he was talking to me, and we got to this crossroad and as we crossed the road he stopped talking as we were watching out for traffic. We got to the other side and then he started talking again. And then when he left I had this strange feeling of elation and I couldn’t quite work out why I was feeling like that. I went through all the things that had happened to me during the day – you know, what I had for breakfast and goodness knows what – and finally it came to this point when I was crossing the street, and I realised that I had a certain idea, and this idea what the crucial characterisation of when a collapse had reached a point of no return, without assuming any symmetry or anything like that. So this is what I called a trapped surface. And this was the key thing, so I went back to my office and I sketched out a proof of the collapse theorem. The paper I wrote was not that long afterwards, which went to Physical Review Letters, and it was published in 1965 I think.  AS: And that was the paper. The crossroads, it’s quite extraordinary, that image of you having the idea at the crossroads. Where precisely was this crossroads?  RP: It’s actually … I’ve been there again and it’s kind of ruined now because the other end of the road is actually now buildings. Somebody wanted to take a photo at that point, and it was a bit disappointing. It’s a walkway now, I don’t think it’s a proper road at the moment. It was a proper road at the time, I think the main road … I could identify it.  AS: If you pinpoint it you’ll have theoreticians in droves crossing it for inspiration.  RP: I think … perhaps I’d better keep it quiet. [Laughs] I had no good idea going back there, so I can’t say it works every time.  AS: Amazing, How do you feel about being portrayed in things like *The Theory of Everything*, in film?  RP: A mite strange really because it’s not really me. I can’t identify with the character, who didn’t seem like me at all.  AS: The concepts we’re talking about, black holes, they’re hugely attractive in the sort of popular imagination. When you think about them, do you visualise them, do you think in terms of maths?  RP: Well, yes, no, I certainly visualise … it was really … I had to have a very good idea of the geometry – that was crucial. Spacetime geometry so it’s not three dimensions, you have to think of the whole four dimensional spacetime, and I get sort of used to thinking about four dimensions and using various tricks to get the picture properly. I do most of my thinking in visual terms, and I’m a very visual thinker rather than writing down equations. Where were we? There was something I wanted to say, I think you asked me something that I didn’t answer. No, black holes have become more and more important you see, also in ways that people don’t normally appreciate. They are the basis of the second law of thermodynamics, which is a quite strange thing. I mean I’ve always been puzzled by you know the second law, tells you entropy increases and therefore randomness increases and so on … oh, now the other phone’s going, oh dear … [Laughs]  AS: I long to know the … why they’re the basis for the second law of thermodynamics.  RP: Can you wait on just a second because I think … oh it’s my sister.  AS: Yes, of course I can.  RP: Sorry, I’m on two phones at once, as you might guess … no that was my sister calling me … where am I, yes, they are absolutely fundamental to second law, yes. They are in fact, you see the entropy in the universe, or the randomness if you like, increases with time, and you might ask where the greatest entropy is in the universe now. Well by far, by an absolutely enormous factor, it’s in black holes. And then where does it go? Well, Hawking tells us that in the remote future these black holes will evaporate away, which is … I certainly accept that. For the really biggest ones – you get absolutely enormous black holes, and that’s where most of the entropy is – and these black holes eventually, after about, let’s say, I think it, according to Don Page it’s about a thousand googol years. A googol is ten to the power 100, so ten to the power, which is one with 100 zeros, but now you have to put 100 and … 103 I think is his figure. So it’s roughly that number of years before the biggest black holes all disappear.  Now according to me, and this is my cosmology scheme, which I’m having trouble persuading the cosmologists about, is that when the universe is rid of pretty well all its matter, it in a certain sense forgets how big it is. Now that’s a crazy idea, but you see if you don’t have any mass around you don’t have any way of scaling the size of the universe, and so it in effect becomes the big bang of the next aeon as I’m calling it – A E O N. So according to my scheme, the universe as we currently understand it, which is from big bang, and then there’s this inflationary phase which I don’t believe in, which is supposed to take place very, very early on, and then the universe has this more sedate expansion, and then it has another exponential expansion, and that’s it. According to me, that’s not it. It morphs into the next big bang. And our big bang was the morphing, if you like, of the previous exponential expansion of the previous aeon. Now there would have been black holes in that previous aeon. Those black holes would have evaporated away in Hawking evaporation, and that’s where all the entropy would have gone into, or into the singularity, both ways, and that concentrates itself into a single point in our cosmic microwave back sky. But we don’t see that single point because nothing gets out until 380,000 years – this is all standard … that part is all standard cosmology. 380,000 years, and then the point which was the black hole coming through, or the remnants of it if you like, or radiation from it coming through, so the black hole would have evaporated away, but it’s all its energy comes through. And that comes through at one point but it spreads out through the 380,000 years to a region in the sky about eight times the diameter of the moon. So the claim is that we see these regions of heat, heated … slightly warmed up … not all that slightly, significantly warmed up regions in the sky about eight times the diameter of the moon. And in this paper which I wrote with a couple of Polish colleagues, Krzysztof Meissner and Pawel Nurowski, and a Korean American person who did the calculations, that’s Daniel An. And the paper was published a couple of months ago in the *Monthly Notices of the Royal Astronomical Society*, which is a very respectable journal. And we claimed that the signals we see, which we called the Hawking points, are the regions eight times the diameter of the moon, and the six most significant points, we see them in both the WMAP satellite data and in the Planck, the more recent Planck satellite data, in exactly the same places. So we believe there’s a strong piece of evidence that they are actually there, and the confidence level that we give for this, from the data, is 99.98% confidence that this is a real signal and not just random. So these are the remnants, if you like, of black holes in the aeon prior to ours, and all the entropy, pretty well, in the black holes was squashed into those points, and it really gets lost at that point.  AS: How utterly extraordinary. [phone rings] And the idea … this beautiful vision of the previous universe leaving its trace in our current universe, and then perhaps our universe leaving a trace in the next one, it’s a beautiful picture to paint. It sounds like you have another call coming, is that right?  RP: No, no, it’s Petrona doing my hair. I’m in the barber’s chair at the moment at home. Oh this is better, yes, sorry.  AS: All of us in our various lockdown states around the world have been in the barbers chair at some point.  RP: Yes, that’s right. Well I did it, last time I cut my own hair, it’s quite true. But I didn’t do as good a job as Petrona, no she’s an expert.  AS: It’s been a very great pleasure to speak to you, thank you, and best of luck with the …  RP: It’s a pleasure for me, thank you.  AS: … with the rest of the day. Okay. Speak again soon.  RP: Okay.  AS: Thank you.  RP: Bye for now.  AS: Bye. |
| **Interview** |  |
| Q12 | **Could you tell us a bit about when you were a child? Did you always want to go into science and be a scientist?** |
|  | Roger Penrose: I think science in some sense. Both my parents were medical and I was one of three brothers. I had a sister who came along a lot later, but for a long time I was just one of three. My parents had given up on my older brother. He was obviously going to be a physicist or something. My younger brother was obviously going to play chess. He became British chess champion a record ten times. So they decided I was going to be the one to carry on the medical profession. And I thought so too. We used to do tests and things and it came out that I was going to be a doctor.  I think it was when I was about 16 when we had to decide what we were going to do in the final two years at school. Each one had to go and talk to the headmaster. He said, “What do you want to do in your final two years?” And I think what I said was biology, chemistry, and mathematics. And he said, “No, you can’t do that combination. If you want to do mathematics, you can’t do biology. If you want to do biology, you can’t do mathematics, make your choice.”  At that stage, I had fallen in love with mathematics and so I said chemistry, physics, and mathematics. So that was my future completely changed from my medical career. When I went home, my parents were both very annoyed. They thought I had been keeping bad company. |
| Q12 | **So your passion was mathematics. I understand, however, that you struggled with maths as a very young child?** |
|  | I think that’s a slight misunderstanding. I was slow. I was very slow and I was not quick at doing arithmetic. I remember particularly, when I was in Canada during the war years when I was about eight. The teacher there, she had little mental tests and you had to do a mental arithmetic – add seven, multiply by three, subtract four. I would simply lose track very quickly. She thought I was very stupid and decided to move me down. Finally, she got rid of me by moving me into high grade three. I think she just didn’t get on with me.  I did have a very understanding teacher a year or so later. He realised that I was just being very slow. He insightfully decided that he would allow me as long as I liked to do the test. I can remember looking out of the window and seeing people in the playground and I was still struggling with my test.  Then I did very well. It was really just that I used to have to work. I didn’t remember my tables very well – I had to work them out each time. I really wanted to know what was going on rather than just parroting what I’d been taught. I just didn’t remember these things. Whereas when I was allowed to think about it, then I did much better. So I think to say I was not good at mathematics is misleading. |
| Q12 | **What was it about mathematics that particularly drew you to it?** |
|  | A lot of it was geometry. I remember in particular, again when I was in Canada, I don’t remember what age. There was, I think it was a sink top, which was tiled with regular hexagons. I asked my father: suppose this pattern went on and on and on and went all the way around the world, could it cover the whole world with that pattern? So he said, no, you can’t do that with hexagons, but you could do it with pentagons. And this was the dodecahedron. I spent a lot of time making models with my father. We used to make polyhedron or truncated ones and different kinds of polyhedra.  I think my father was a big influence on me. He was a scientist – he worked in human genetics, it wasn’t mathematic, but he definitely had a feeling for mathematics, particularly geometry. We used to talk a lot about that. |
| Q12 | **Do you feel like your family environment – maybe you felt a bit competitive with your brothers – influenced you to take the path that you ended up going down?** |
|  | Yes. I think it was not so much the competitive aspect – that was there but I don’t think that was what it was. It was more just the science for itself. I remember at one stage, my father, he had a telescope, which he liked to look at the sky and he showed me the rings of Saturn. And then, wow. I’d seen it in pictures but to see it’s real, out there, was something special. |
| Q2 | **Is that when your interest in the universe started?** |
|  | Yes. I think so. I think my father knew quite a lot of astronomy, but, of course there wasn’t much cosmology known at that time. I picked up a lot more of that later on. |
| Q2 | **How important do you think bouncing ideas off your father was to you?** |
|  | I think it was very important to me. It was strange because my brother certainly taught me a lot more specifically about physics. But it was particularly my relation with my father which developed these interests. He liked to play with things. There was no boundary between what he did for fun and what was his profession. He used to make puzzles for little children and things like that so he was very good in that way. It was partly also to do with his profession. I remember he made a very complicated slide rule, which had all sorts of conditions. You had to slide all these different rods together and then you went through to the bottom and you’ve got the diagnosis. It was very much playfulness on his part. So what with his serious profession and his playfulness there was no borderline between the two. I think that rubbed off on me very much. |
| Q7 | **You have had a very long and successful career in science. What do you think you need to have or do in order to be a successful scientist?** |
|  | Admit you’re wrong when you are wrong, is one. That is important and I think a lot of people aren’t prepared to do that.  Gosh it’s very difficult. People are so different and often say to me what do you recommend? And I say, well it depends on you – do what excites you. Of course you might be going up the wrong track, but this happens a lot.  I would say generally do what excites you, not just what you think you should be doing. That’s certainly the factor with me. It’s difficult because you’ve got to earn a living after all. And it may be that you have to do something which isn’t quite what your ambition is. I don’t know whether that’s a very good moral always, but it seemed to work with me. |
| Q2 | **I heard that you have a lot of blackboards around your house in case inspiration strikes. Are you always thinking and coming up with ideas?** |
|  | Always is not the correct statement, but I do have a blackboard. I can see it right where I’m sitting in the study. And I have a portable whiteboard, which I sometimes put behind me if I’m talking to somebody and they want to illustrate something on the board. And I have a whiteboard in the bedroom, which I should clean off actually, because it’s got something on there which keeps on distracting me when I look at it.  Sometimes I get an idea, which I forget you see. I remember the other day it wasn’t so long ago when I woke up in the night thinking I had an idea. I thought, ‘Oh, I’ll remember that idea for sure.’ I went to sleep, woke up the next morning and I thought ‘Didn’t I have an idea in the night which I was sure I would remember?’ So I had to go through very carefully to try and resurrect what that idea was. I think I did resurrect that one. I do keep a notebook within arm’s length when I’m in bed so I write something down. It doesn’t often happen. |
| Q8 | **What do you do in your downtime? Do you have any hobbies or interests outside of work?** |
|  | It’s difficult to find time for hobbies now because there’s so many things I’ve got to do. Particularly with a Nobel Prize there’s so many things that I was working on I’ve got to put on hold for a bit.  I don’t know which you call a hobby and which you’ve don’t… I do draw sometimes. I drew a Christmas card, which was developed from the tribar. It was an impossible object, but it was a different kind. So that was Christmas card I sent out to people.  There’s this thing called the campaign for drawing, which I haven’t done much for recently, but I used to. They wanted to have people who belong to it to draw pictures, which they would sell at auction. So I drew a diagram, it was close to the Christmas card one, which was not one I’d drawn before. And this was sold at the auction I think for nearly £2000. I was quite surprised. This was for a hospice, it was a charity.  I like doing things like that. My notebooks, particularly the old ones, are crowded with all sorts of weird looking drawings I used to do. I don’t know if surrealistic is that the right word, they’re all distorted forms of one kind or another. |
| Q15 | **You’ve written quite a few popular science books. Do you think it’s important to make your work accessible for other people? Do you find that you benefit from it as well?** |
|  | Yes. I think that’s true. Both ways. Yes. I certainly have felt that it’s worth trying to get express ideas also visually, because as you say that’s a lot of the way I think. Most of the books I’ve written, all the illustrations I’ve drawn myself. So, in fact, most of my artwork as anything that’s published is in those illustrations in books. |
| Q9 | **After a long scientific career, how does it feel to be a Nobel Laureate?** |
|  | That’s a tricky question. I used to think Nobel Laureates were other people. ‘The establishment’ kind of people. And that was not me. I did things outside the establishment. I hadn’t expected to get a Nobel Prize but a lot of people, my colleagues, friends of mine said, ‘Oh yes, no, you’re bound to get one.’ I didn’t think this. So it was a bit strange.  It’s difficult question because I certainly had attached the idea a bit more to the establishment and I think of myself not as part of the establishment, but the Nobel Prize makes one part of it in a sense, which is an interesting experience.  It is a bit surreal. |
| Q3 | **And finally, can we ask you what is your favourite thing about the universe?** |
|  | Everything. Oh gosh, now, that’s a tricky one. I think I like the way it fits in with general relativity. I would certainly say that. Quantum mechanics is amazing. The trouble is, as I keep saying, it’s not quite right. There will be something just as beautiful when we get it right which has to do with the collapse of the wave function. Quantum mechanics has this self-inconsistency about it. It’s what Schrödinger made this huge point about. That’s why he talked about his cat in the box. He just wanted to show the absurdity of his own equation that leads you to this absurdity.  General relativity I think I would have to say, but you see, there are a lot of observational things about the universe, which are amazing. It’s a very hard question to answer.  Let me put it more generally: the way the physical world ties in with mathematics. Not so much just general relativity. How you see when you get it right the mathematics fits with the physical world in such a beautiful way. Yes, that’s it. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0013** |
| **Biographical** | Iwas born in Bad Homburg, near Frankfurt, Germany in 1952. My parents and I lived in an apartment right above the physics laboratories of the University, where my father was an instructor. I also started elementary school there. In 1960 my parents moved to southwestern Germany, to the idyllic Black Forest town of Freiburg, bordering France (Alsace) and Switzerland. There I went to high school at a ‘humanistic gymnasium’, featuring 9 years of Latin and 6 years of Greek. Perhaps as a result I have enjoyed a lifetime interest in history and archeology.  My father was a well-known experimental solid state physicist and a gifted university teacher. I learned most of my early physics from him. I will always treasure how he showed me (aged 16) how to build a decent optical spectrometer from its basic optical components, which could resolve the sodium D lines. My mother had studied economics and then worked, together with her father, in managing a leather factory near Frankfurt. Once we had moved to Freiburg, she stopped her professional career and devoted herself to her husband and only child, as was typical of most German women of her generation. Only at the end of her life did she tell me that she regretted having given up her career.  During my high school years, I spent a lot of my time doing intense sports. To this day I am proud of having been one of Germany’s best young javelin throwers, as well as being on my school’s handball team. I even made it into the national German junior track and field team training for the 1972 Munich Olympics. An elbow injury and the increasing incompatibility with university studies brought my track and field career to a rapid end in the early 1970s. I still feel compelled to do some daily workout activity and until recently went frequently on mountain hikes.  After completing my undergraduate physics education at the Albert Ludwig University in Freiburg I moved to Bonn for my graduate education (1970–1974). At that time my father had also moved from Freiburg, to become one of the founding Directors of the new Max-Planck Institute for solid state research in Stuttgart. In a discussion about my future in physics, he advised me against nuclear and particle physics (and in any case his own field, solid state physics). He mentioned that the Max Planck Society (MPG) had just founded a new Institute for Radio Astronomy in Bonn (MPIfR), with a 100m single dish telescope in the Eifel mountains as its key new research instrument. Would I perhaps be interested in doing astronomy? And so, I followed this advice and began my astronomy career initially as a master (Diplom) student, and then as a PhD student (Doktorand) student at the MPIfR, under Peter Mezger, one of the Institute’s Directors.  Working with the then largest radio telescope in the world was a fantastic opportunity for a young student. I was particularly fascinated by the emerging field of molecular spectroscopy. Working in close collaboration with Dennis Downes, my PhD thesis work (1976–78) was on the phenomenon of interstellar water vapor masers, which had been discovered ten years previously by the group of [Charles Townes](https://www.nobelprize.org/prizes/physics/1964/townes/facts/) at the University of California, Berkeley (UCB). Townes, Nobel Laureate for the invention of the maser and laser, had switched to experimental astrophysics research in the late 1960s, and was at the forefront of the fledgling field of molecular astrophysics. My thesis work showed that the H2O masers originated in dense, dusty clouds in the process of forming massive stars. Working together with colleagues in the USA and the Soviet Union, we used the 100m telescope as part of a network of intercontinental “Very Long Baseline” Interferometry (VLBI), generated by wave interference milli-arcsecond angular resolution. We demonstrated that the masers were extremely dense little cloudlets, probably compressed and excited by supersonic shocks and the intense radiation of the newly formed stars.  My thesis work was completed in 1978 and led seamlessly to postdoctoral work with James Moran at the Harvard-Smithsonian Center for Astrophysics (CfA). In the next two years, during my time as a CfA Postdoc, we expanded further on the maser VLBI studies and could show that the maser phenomenon was triggered by rapid gas outflows during the protostellar stages.  In 1980 I was offered the unique opportunity to change fields and join the group of Charles Townes in Berkeley, California, initially as a Miller Postdoctoral Fellow and then one year later as Associate Professor in the Physics Department. My luck of becoming tenured faculty at Berkeley at age 29 was mainly the result of Charlie’s support and invaluable mentorship, but perhaps also because I had in the meantime received a competing offer for a professorship at Caltech. The scientific life with Charlie and his large group was an incredible experience. I finally did experimental physics in the lab, as in my youth, was polishing my own mirrors, and learned to work with cryogenic far-infrared detectors. The Townes group was carrying out ground-breaking experimental astrophysics work across the mid-infrared to submillimeter wave-bands, which fascinated me enormously. Life for the next six years at Berkeley was intense and exciting. We were flying an innovative high-resolution Fabry-Perot spectrometer on NASA’s Kuiper Airborne Observatory, to explore for the first time Galactic star formation regions, the Galactic Center, and nearby galaxies, in far-infrared fine-structure and molecular rotation lines. Interestingly the Fabry-Perot reflectors and other filters in the instrument were based on a stretched metal-mesh technology which my father had invented some years prior. I was trying to combine teaching physics, with intense research in infrared and submillimeter spectroscopy – developing new instruments, working with my first graduate students, and finding time for the family. During that time, I was very much helped by Dan Jaffe, an old friend from the CfA, who had joined my fledgling group as a Senior Fellow at the UCB Space Sciences Lab, and taking on many tasks I really disliked and was not good at, in particular hunting for grants. Another key person was my first student, Andy Harris, who has also remained a lifelong friend.  At that time, my family was also developing. In 1976, I had married Orsolya Boroviczény, whom I had got to know in my late high school days. She had studied medicine at the University in Bonn and at the Harvard Medical School in Boston, and then carried out her pediatrics residency at Children’s Hospital in Oakland. Our older daughter, Daria, was born in Boston in 1979 and the younger one, Lisa, in Berkeley in 1983. California and Berkeley became more than a temporary residence. Even after returning to Germany in 1987, the family considered Berkeley as our ‘Heimat’, that sentiment being an important factor in my taking on a commitment as part-time professor in the UCB Physics department again in 1999. The current status of the family, including two grandchildren is summarized in Figure 4.  In 1986 I was offered and accepted a directorship at the Max Planck Institute for Extraterrestrial Physics (MPE) in Garching, near Munich, in significant part due to the support of Gerhard Haerendel and Joachim Trümper. At the time, I was quite unsure whether returning to Germany was the best choice. Berkeley was wonderful, Europe looked pretty un-dynamic. However, I never regretted it. The older I get the more I am grateful to the Max Planck Society (MPG) for providing such an absolutely wonderful opportunity to pursue basic research at the top level with so few strings attached.  Building up a substantial infrared/submillimeter experimental astrophysics group at MPE has been a fantastic and all-consuming opportunity for me. We started from a small core of a dozen people around senior scientist Siegfried Drapatz, and grew to more than sixty over two decades, when we were engaged in two large space experiments with the European Space Agency (ESA). Our style was similar to what I had learned from Charlie in Berkeley. The most important aspects driving our research are the fundamental large science questions in Galactic and extragalactic star formation, physics of the interstellar medium, galaxy evolution, massive black holes (the Galactic Center being the most important goal) and their activity, and co-evolution over cosmic time of the activity of these massive black holes with their host galaxies (active galactic nuclei: AGN). As I have described in more detail in my [Nobel Lecture](https://www.nobelprize.org/prizes/physics/2020/genzel/lecture/) about our Galactic Center research, we next identified how we could tackle these questions with novel infrared to millimeter experiments. These longer wavelengths were not yet explored, but are of great interest astronomically for studying cold, dusty regions, or regions with large cosmological redshifts. Novel technologies (adaptive optics, integral field spectroscopy, spatial interferometry, photonics, new semi-conductor detectors etc.) promised very substantial progress. Typically, each project has a senior scientist project leader, and 2 to 7 additional scientists and postdocs, a few masters and PhD students, plus additional technicians and engineers ‘on delegation’ from the MPE central divisions. Our team culture calls for sharing as best as possible in the various challenges, from hardware/software design, building and testing, to observing, data analysis and interpretation to, finally, publications and conference presentations. As an umbrella of these individual projects groups, we have a circle of senior scientists (chaired for many years by Linda Tacconi and Dieter Lutz) who bring the individual parts back to an overall strategy.  Having obtained first science results with one of these new instruments, new ideas and questions came up and the cycle repeated. A key difference from Charlie’s approach has been the scale of the experiments, and of our teams, driven by the possibilities available at the MPG and at the MPE. Because of the long space research tradition of MPE since its inception in 1963, we dared to do bigger, more ambitious and sometimes riskier experiments, not only relying on students and postdocs, but also on professional physics, astronomy, engineering and IT staff in the group, and in the central divisions at MPE.  Initially we carried out projects without immediate connection to a specific telescope. Rather we developed an instrument based on our science questions and then made a proposal to a suitable observatory to bring the instrument to their telescope for a campaign typically lasting 1–2 months, sometimes several times. To conclude such deals typically meant that in return we made our instrument available to other scientists in the community of the observatory. The SHARP speckle camera for the European Southern Observatory’s New Technology Telescope (ESO NTT) was one example of such an instrument, ‘3D’, the world’s first infrared integral field spectrometer, a second, and ‘Receiver G’, a heterodyne spectrometer for submillimeter observations on the James Clerk Maxwell Telescope (JCMT) on Hawaii’s Mauna Kea, a third.  This style changed with the advent of the Very Large Telescope (VLT). ESO was unlikely to allow visitor instruments on its brand-new facility any time soon, so we changed our style to competing for instrumentation developments ESO wanted to have on their new 8m class telescopes, and bidding for a building contract. The adaptive optics camera CONICA (with the Max Planck Institute for Astronomy, MPIA Heidelberg) as part of the NACO adaptive optics system (with the Observatoire de Paris), the SPIFFI-SINFONI integral field spectrometer (with the Observatory of Munich (LMU), the PARSEC laser guide star facility, the KMOS multiplexed integral field spectrometer (with the UK Universities Durham and Oxford, with the UK ATC in Edinburgh, and the Observatory of Munich (LMU) and finally, largest of all, the GRAVITY interferometric beam combiner, were all examples of this approach. In this way we earned privileged (guaranteed) telescope time (GTO), which we could use for our top science. The same happened for our involvement in the European Space Agency (ESA) infrared space telescopes ISO (as co-PI Institute on the Short Wavelength Spectrometer (SWS), with Leo Haser as co-PI) and on the Herschel space telescope (as lead institute for the PACS instrument, with Albrecht Poglitsch as PI). In retrospect I am sure that most of our most important science results in the Galactic Center, in active galactic nucleus (AGN) studies and in galaxy evolution would not have been possible without this GTO mechanism, which permitted devoting significant telescope time over sustained periods to a single or a few core science projects.  In the millimeter range, we gave up our own developments fairly early on, and instead relied on and strongly supported the Institut de Radioastronomie Millimetrique IRAM), a German-French-Spanish joint institution in Grenoble, France. IRAM operates a 30m diameter single dish telescope on Pico Veleta (Spain), and an increasingly powerful interferometer on the Plateau de Bure in southern France and develops all necessary focal plane instrumentation for these world-class facilities. Our IRAM involvement was and continues to be a great success. With the recent upgrade to the Northern Extended Millimeter Array (NOEMA), the interferometer is by far the most capable millimeter interferometer in the northern hemisphere, and in several aspects comparable and competitive with the larger international Atacama Large Millimeter Array (ALMA) facility in Chile.  At the current count, we have built 25 instruments, most in a leadership or co-leadership role. Over my past 35 years as a Director here at MPE, the IR-submm group has had 70 PhD students, 85 postdocs and scientists, 15 senior scientists and 25 technical staff. I am particularly proud about the diversity of our group, both in terms of geographical distribution (>25 different countries) and gender (our female contingent has lately been varying between 30 and 36%, as compared to 12% in 1986).  I have to admit that my intense and continuing engagement in many aspects and areas of astrophysical research – trying to keep our group at the top level, and anticipating the next steps (e.g. with the remarkable breakthroughs our VLTI infrared interferometer activities led by Frank Eisenhauer, and our leadership of the first-light camera MICADO, P.I. Ric Davies, for the next-generation, 39m diameter giant extremely large telescope of ESO, the ESO-ELT), leaves little time for other things. My regular visits to Berkeley in winter and summer have become welcome interludes, with the possibility of reading books, writing papers in peace, hiking along the beautiful California coast and thinking about the next steps.  It has been a privilege for me to be able to have had such a wonderful, supportive family and a group of many outstanding and capable colleagues and friends, who were able to join and tolerate a sometimes grumpy or “single channel” guy. It has been (and continues to be) a lot of fun! Thank you, Linda, Natascha, Adriane, Susanne, Albrecht, Amiel, Dieter, Eckhard, Frank, Helmut, Sebastian, Stefan, Ric, Taro and Thomas. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0013 = RG**  Adam Smith: Hello, am I speaking with Reinhard Genzel?  Reinhard Genzel: Yes, speaking.  AS: First of all, many congratulations on the award of the Nobel Prize.  RG: Well, thank you. It was completely unexpected and I’m … wow … I’m on cloud 17.  AS: How lovely. How did the news reach you? Where were you?  RG: I was in my office, in fact, working, as we do these days, in a virtual conference on hiring of another Max Planck Institute when I was called, and … and the almost stereotypic telephone call took place which I never would have thought I ever get, which is ‘this is Stockholm’.  AS: So they actually say ‘this is Stockholm calling’?  RG: Yes, yes, yes.  AS: This prize is a lovely marriage of experiment and theory, which is so much the way that physics proceeds, isn’t it?  RG: Oh yes. No, I think that’s a very strong theme in the black hole research, and if you think about more broadly in terms of gravity, I mean, here you know we celebrated a few years ago the theory of general relativity. In fact I was in Berlin for the ceremony. A hundred years later, research on gravity and black holes is at the very core … forefront of physics, in several domains, gravitational waves and so forth. It’s extremely exciting.  AS: And it took an enormous amount of effort to discover this black hole at the centre of our galaxy.  RG: Yeah, well you see this goes back in my case to another Nobel Laureate whose second father, or second son I feel I am, that’s Charles Townes, who got the prize in ‘64 for the laser and the maser, and then turned astronomer. And he in fact, after the discovery of the quasars in the ‘60s, felt that, okay, well maybe one should look and the galactic centre is so close but you can’t look at it in the visible. So Charlie then developed the first instrumentation to look at basically the doppler shift of gas, and I joined him as a postdoc and then a colleague in Berkley and so we started working on this problem back in the 1980s, so that’s 40 years ago. And yes it took a you know, a lot, a lot of patience, luck and always trying hard to get better and better and better.  AS: It’s a lovely illustration of the way that research hands from person to person, that it grows and it’s such a personal thing really.  RG: Yep, yeah. No, absolutely.  AS: When you image the black hole at the centre of the galaxy, what do you see?  RG: Well, I mean, we are sensing … I mean, what we’re doing, we’re using electromagnetic waves and mostly infrared range with the telescopes of the European Southern Observatory in Chile, and initially we used one of the big 8m telescopes, then you have to combat the Earth’s atmosphere, and make sure that the blurring of the images is removed. That’s called adaptive optics. That was the first phase of innovation in the late ‘90s. But that’s not sufficient to come very close to the galactic centre, so our most recent innovation has been to combine four of these telescopes, two of them cause an interferometer, so there’s four 8m telescopes. And with that, we can then sense the motions of stars which are orbiting the black hole with exquisite precision. We also see actually gas in the … very close in the accretion zone around the black hole. That’s about as close as you can get because any closer all material has to disappear in the black hole. So in that sense we are seeing not it, but we are seeing so to speak, we are sensing its gravity, and we are seeing, you know, gas and stars moving around it, and just by how the gas moves, how the stars move we can then infer with high precision what it is and that there must be a black hole. And also in fact that general relativity holds even in this super strong regime of curvature.  AS: Yes, sensing its impact on the surroundings. And it’s extremely active just at the moment, isn’t it?  RG: It’s a little more active than it was, but on the scale of active black holes it is a dwarf, it doesn’t do much. I mean it’s under-luminous compared to what it could be. Let’s be glad that this is so because indeed if super-massive black holes get fed at the maximum rate they can destroy their own galaxy systems, and they do. They have, in particular in the past. So that’s why massive black holes turn out to be such an important regulator, if you like, in the ecosystems of galaxies.  AS: And I suppose one point to make is that this takes us back to Einstein and how extraordinary it was that general relativity so precisely defines so much around us.  RG: Absolutely, absolutely, it’s incredible. But, you know … All of physics of course suspects it must be wrong somewhere, you know certainly on the smaller scales – now those scales we cannot reach. But at least another option would have been that it is mass dependent, and that we have now tested, and so you know we can be sure that theory is right. Now to really be sure that what is called a parametric is correct to the innermost region of waves and trace matter we still don’t know that. And gravitational waves haven’t done that either. So the story will go on experimentally, I would say, for at least another one or two decades. I predict that the most likely final triumph will be a space mission, with gravitational waves seeing an in spiral a solar mass star into a black hole. That probably would clinch the case. Unfortunately I’m afraid I won’t be there. The mission is being studied by the European Space Agency for some time now and is very expensive; very, very difficult mission. There will be collaboration with NASA too, but I assume that the Europeans will lead, and the most likely slot for launch will be in the late ‘30s I would expect.  AS: It’s been a huge pleasure speaking to you. Thank you very much indeed. I guess you’re about to have a total onslaught of the press and well-wishers. How do you feel about that?  RG: Well, okay, we’ll see how we do on that. I know, I guess I’ll try to do my best, but it’s a great distinction. It’s just wonderful. |
| **Interview** |  |
| Q12 | **Could you tell us a bit about your childhood? Did you always want to be a scientist?** |
|  | Reinhard Genzel: Well, let’s see. I started with archeology. I went to, what you call in Germany, a humanistic gymnasium. So nine years of Latin and six years of ancient Greek. Archeology seemed like a fascinating thing. But it became clear, maybe when I was 13 or 14, that the classic period which you read in the books about was more or less done. That’s not true actually, but at least it seemed like that to me. My father is a physicist and was also a director of a Max Planck Institute. So in that sense, I was very close to physics from the beginning and also to experimental physics. As soon as my father discovered that I was interested in experimental stuff we built a laboratory at home. A little bit of chemistry but then later on in our house, up in the attic, I built up a fairly sophisticated optical spectrograph with high voltage sparks so that you could excite molecules and atoms and then measure the light and so forth. So, it was pretty clear that this was an exciting thing and that I would want to study physics. |
| Q2 | **Did you have any accidents with your experiments at home or were they always quite straightforward?** |
|  | That’s interesting that you ask that! Well, yes, actually. Of course if one is 15, one has friends and wants to show them these kinds of experiments. In one experiment we made bromium hydride. Bromium hydride at room temperature is just between a vapour and a liquid. So if you make the reaction to create that bromide hydride and then you cool it, you get a liquid which you can deal with. It’s extremely corrosive. What I had not realised at the end of the experiment was my cooling apparatus was full of the vapour. So at the end of the experiment, I brought this all into the bathroom to wash up. But I had not realised that I was basically spraying bromium hydride vapour against the walls. Two hours later, my mother came home and already at that time some of the tapestry came down!  So that was the chemistry disaster. The physics disaster – I told you that in order to basically generate a spectrum [from molecules or atoms] you would have a charge device. That’s called a Leiden bottle. So you make a device with an insulator and then you build up charge and then you let that charge go through the spark plug and that generates a 15 kilovolts spark. Of course the entire 15 kilovolts is already at the surface of the bottle. And one day as I was playing around with the spectrometer and then having my hands all over the place, boy, did I get the full 15 kilovolts! |
| Q3 | **But neither of those were enough to put you off of science?** |
|  | No, it was fine. Actually I should say most of my time at that age, I spent doing sports. It became very clear, very early on that I was good at throwing things. Initially I did handball and later on I went into track and field. In fact at age 16, which was the last two years of school, I really was very good. I was the best German javelin thrower and I managed to get into the ‘group B’ young future athlete group for the Munich Olympics. So we were training for the Munich Olympics. For the last few years in school, I spent typically three, four hours in every afternoon doing sports. Fortunately I was a good enough student so I could do that. But then, as I started studying physics at university, it was clear either you’re going to go this way or that way. |
| Q5 | **You mentioned your father but were there any other people that influenced you on the scientific path you took?** |
|  | Absolutely. In retrospect now, I would say even more than my father, my real mentor of later years was [Charlie Townes](https://www.nobelprize.org/prizes/physics/1964/townes/facts/).  Townes was Nobel Laureate in ’64 in physics – he got his prize for the invention of the maser and the laser. He was then a very influential advisor to the American government for many years and then became provost of MIT. And then all of a sudden he decided he wanted to go back into research and moved across the country to Berkeley, to California, and started doing astronomy. I joined his group as a post-doc and that was just absolutely fantastic. I would say, if you look at the kind of style of research which I’ve been pursuing since that time, I really learned that with Townes. |
| Q5 | **What was the major lesson that he taught you?** |
|  | Well, lesson number one is to focus yourself on important questions. Don’t get lost in the forest of arbitrary questions. Focus on one thing or a few things, work hard and see how far you get. And then if you have success and other people start coming along and do the same thing – which is very typical, if you’re successful and many people start imitating – then leave the field. He would always do that. He would always stay in a given field for only a certain length of time. The laser was one of the examples. He did the maser, he wrote a very famous paper about the laser, but never got into laser research much because that was done by industry immediately. Hundreds of people were descending on that field and he said, okay, goodbye, I’m going now. I’ve listened to that advice a number of times where after a while – it’s good enough, let’s go on, let’s do something else. Do challenging things and try hard. |
| Q7 | **What qualities do you think are needed to be a successful scientist?** |
|  | Well, I think I wouldn’t generalise. I would say it depends on the field. Surely everyone would agree that with the exception of very, very few discoveries, intensity is the most important thing. A burning desire, a concentration. I’m really concentrated on something and I just don’t notice the world anymore. That’s the kind of thing that you need to really focus.  Hard work. I mean, that may not be true for all. It’s possible that we’ll find some examples of extremely successful work, which was done in a more casual way, although Einstein’s general relativity which is supposed to be one of these great strikes of the mind really wasn’t if you look at it. He struggled ten years after the theory of special relativity to get to the general theory. He made many mistakes, his math wasn’t good enough. He had to learn maths from mathematicians and other people were telling him what he was doing wrong. So it’s by no means something that came overnight.  Now there are examples of things that came very quickly. I guess this last 2020 Chemistry Prize, CRISPR, that is more like it. From what I learned from [Emmanuelle [Charpentier]](https://www.nobelprize.org/prizes/chemistry/2020/charpentier/facts/) it was done in a relatively short period of time, and it was based on a good idea executed fairly rapidly. |
| Q1 | **What advice would you give to a young person starting in science?** |
|  | I would say if you really want to become a successful research scientist, you must really want to do it. You have to really be willing to sacrifice. If I look at my successful colleagues overall, then all of these are 150% people, so to speak. They’re really going all the way out to do it.  And of course, luck – you have to be lucky. |
| Q5 | **And what about mentors?** |
|  | Now the mentor business, that’s an interesting question. I would maintain that, as I did, having mentors is just absolutely fantastic because they can point you so easily in the right direction if you’re willing to learn.  In the US certainly I would say the idealistic style of research is one where you are independent. I was never independent. I was building independence over the years, but I’m not independent now – I depend on my team. I wouldn’t have done anything without my team.  So I have to really say, you can want to get into a position where you can realise your dreams, maybe that type of independence. But to say, ‘I want to control everything by myself and not depend on other people.’ No, no, no. |
| Q8 | **You are very passionate about science, but what do you do outside of work in your free time?** |
|  | I mentioned sports and, while I do not do professional sports, for a long time, until three years ago when I damaged my knee, I was intensely going into the mountains. Here in Europe in the Alps. And then in the United States, where I live two months a year in Berkeley, I go into Sierra Nevada. Mountains are great to hike in and relax for me. I’m not a beach person. My family is, they love to be at the beach. I don’t, I find that boring. But mountains, they’re great. And I mentioned archeology. Archeology you can still see when you come to our house. You’ll find hundreds and hundreds of books. I live in a library! |
| Q3 | **What is it about the universe and about your work that you enjoy?** |
|  | I tell people the universe and astronomy in particular is a bit like hiking into a new forest you’ve never seen. So you go into the forest, you hike and you enjoy, and you look around, you see the trees, see some flowers. So that’s the first phase is just to enjoy the richness of the universe. That’s what I would call astronomy. That’s not enough for me because, I now have learned [there are] trees and flowers, but I have not understood how this forest works. So the physicist in me, he now sees that the blue flowers are always on the left side of the trail and the red flowers are always on the right side of the trail.  And then of course you go ahead and you start doing experiments, you ask yourself, does it have anything to do with the direction, is it up or down, or how the water’s running? So you’re trying to learn about the universe through asking ever more questions and finding systematic features about something.  Now, the galactic center is a little different from this, but most of our work actually is in this way I just described to you. We are trying to understand how galaxies formed and evolved early in the universe. So that’s 12 billion years ago, about two or three billion years after the Big Bang.  So you’re using astronomy as a time machine because the speed of light is constant. And so as we look out in space with ever bigger telescopes and ever better equipment, we can look and see ever more details of the blue flowers and the red flowers and understand how they work. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0014** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** | **0014=Ghez**  **No script** |
| **Telephone**  **interview** | **0014 = AG**  Andrea Ghez: Hello.  Adam Smith: Oh hello, this is Adam Smith calling from Nobelprize.org, is that Andrea Ghez?  AG: Yes, speaking.  AS: Oh Hi, it’s nice to speak to you. Congratulations on the award of the Nobel Prize.  AG: Thank you. So thrilled! I still can’t quite believe it.  AS: And having a fairly crazy morning I imagine?  AG: Yes. The best kind though.  AS: That’s good. Predictable question, but how did you hear the news?  AG: I heard the news with a phone call at two in the morning, so I was fast asleep. And I think for the first few minutes thought I was dreaming.  AS: You are the fourth female physics laureate – what does that mean to you?  AG: Oh, gosh, it’s such an interesting question of what it means. To me it’s always been very important to encourage young women into the sciences, so to me it means an opportunity and a responsibility to encouraging the next generation of scientists who are passionate about this kind of work into the field.  AS: I mean I suppose you’re very much already a mentor for people and a role model, but this will really thrust you very much into the limelight I suppose.  AG: I think that’s so important because I think seeing people who look like you, or people who are different, succeeding shows you that there’s an opportunity there, that you can do it, that this is a field that is open to you. So I think that visibility is so important.  AS: Of course you work in teams, and what does diversity bring to a team in science do you think?  AG: Different ways of looking at things. Over the years of … As I’ve gotten older I’ve had a chance to think a little bit more about the question of diversity, and one of the things that I think can be an asset is not being part of the majority gives you an opportunity to do something that’s new and different. It’s often hard to do things that are different, and if you’re already different there’s I think, in some sense, there’s an opportunity as long as you have the confidence to do things that are indeed different.  AS: That’s an inspiring thought, thank you. And talking of inspiration, what inspired you to become an astrophysicist?  AG: I think it’s a passion for the universe. I think the questions of the universe just inspired me, so I … for me it was really following my passions, my curiosity about the universe.  AS: It’s beautifully expressed. I guess we should all have a passion for the universe really. Do you spend a lot of time pondering what’s happening inside your supermassive black hole?  AG: Oh, I think that’s what inspires me to pursue the work, is to really try to understand the physics of black holes and the astrophysical role that they play in our universe. There’s so much that we don’t understand, and from a scientist’s point of view it’s really … it’s most interesting to be working in the area that we … that frontier of our knowledge.  AS: Well, once again it’s an illustration of that beautiful interplay between theory and experiment in physics. They just go hand in hand.  AG: Indeed, and I think it … and the third piece of this is technology, the technology introduces an ability to see the universe in a way that is different.  AS: It is quite extraordinary that you can peer through the murk and see all the surroundings of this object. Does it still amaze you that you can do that?  AG: Yes! It amazes me every time we go to the telescope to think about ‘here is this light that we’re capturing that’s been on a journey for 26,000 years.’ And you know, if you think about 26,000 years ago when these photons left the vicinity around the black hole it’s just … it’s rather amazing to think we can do this as human beings.  AS: Yes, and to be talking about ‘things are kind of busy at this particular moment 26,000 years ago.’  AG: That’s right. [Laughs] It is a moment that we’re trying to capture, this vast timescale.  AS: With all the attention that this is going to focus on you, yet more attention, do you think you’ll have any time left for work?  AG: Oh gosh I hope so. You know, it’s the science that is … that keeps me going.  AS: Indeed, indeed. I just wanted to ask about your relationship with the other team and Reinhard Genzel. Do you work together at all, or is it completely competition?  AG: It’s, well competition … it’s independent. And I think in a project like this where it’s very difficult there’s a tremendous advantage to keeping the projects independent. There’s nothing like competition to keep you going, to propel you forwards. And to get it right – these measurements are hard. So there’s both the aspect of somebody else is going to figure out your mistakes, and also together you might think … independently you have the opportunity to bring different ways of thinking to the problem. I really appreciate the way in which the teams have worked together – together but independently over the last two decades.  AS: Yes, I can see that. It really must keep you on your toes. You’ve probably been on the phone ever since you got this call. What would you most like to be able to do that isn’t being on the phone talking to journalists? Perhaps you might want to celebrate at some point I imagine.  AG: Yes, Yes! I would indeed like to celebrate but of course now we’re living in such unusual times that celebrations have to take … we have to be creative about our celebrations. But I’m excited.  AS: Congratulations again.  AG: Thank you so much Adam. I really appreciate it. Bye. |
| **Interview** |  |
| Q3 | **Could you tell me a little bit about your childhood? Were you interested in science as a young child?** |
|  | Andrea Ghez: Hindsight is always 20/20 in terms of where our interest in what we do originates. But I think I can identify that point in the moon landing. I was four, but I think that was what got me thinking about space and the enormity of space. But I think it’s also important to point out that at the same time I was saying I wanted to be an astronaut I was also saying I wanted to be a ballerina. So it wasn’t as if I, as a child, really understood science is my passion.  I’d say what definitely emerged was that I love puzzles – jigsaw puzzles, crossword puzzles, Sudoku. I see being a scientist as really fundamentally being a puzzle solver. Putting together the pieces, trying to find the evidence, trying to see the bigger picture.  I understood that I was interested in math and science by the time I went to college, I wanted to go to MIT. So I was clearly driven in that direction, but I went to college as a math major. But I was definitely thinking a lot about space. It used to keep me up at night. The enormity of space, just how small we are, our space and time is so insignificant on the scale of the universe. It’s both fascinating and horrifying at the same time. And I think that both of those aspects really took hold of me when I was young. |
| Q5 | **Was there a particular person, like a teacher, a mentor, a role model that influenced you?** |
|  | I was so lucky. I had a lot of really wonderful role models in my life for lots of different aspects of it. First my parents were just great role models. My dad was a professor, so I was really exposed to the world of academia. What being a professor looked like, felt like, and he was always encouraging my curiosity. And then my mother was the director of a contemporary art gallery. So I had a really strong role model in terms of women going out and leading. I had an uncle who was a physicist. I have very clear memories of sitting around the kitchen table with my uncle and my dad talking about how the early Greeks learned, figured things out. And there was something so compelling about that – that process of the early understanding and the beauty of their discovery or their logic. So I think my uncle was a big figure early on.  And then there were teachers, I had an early chemistry teacher who was really encouraging. I was really fortunate in a sense that later in life I didn’t have many women science teachers. So she was an early encourager or advocate. I often tell my students that the most important decisions you make, especially later in life in grad school and even in college, is who your mentors are, who you work with because they just have such an important role in your professional development. You really want to work with people who have your back, who really want to encourage and nurture your development as a scientist. I was lucky as an undergrad to work with Hale Bradt and then as a grad student to work with Gerry Neugebauer, as well as Anneila Sargent who was sort of a secondary advisor to me. These were people who were just wonderfully supportive. So I’m really grateful. |
| Q5 | **Are there any particular lessons or life skills that you learned from them?** |
|  | Oh, gosh, I’ve learned so many things from them. Everybody has something to teach you. I think that’s one of the things that I’ve taken away along the way. That being able to interact with a large variety of people is important because each of those interactions has the potential to teach you about something. So my mother: ‘Just do it!’ was her, she had that down before Nancy Reagan did.  I think there were some really important lessons along the way about understanding how to deal with challenges, how to translate challenges into opportunities and Anneila Sargent, was particularly good – she provided me critical advice at a moment in graduate school where there was a hiccup and how to move a hiccup over into an opportunity, and figuring out how to stay focused on where you’re trying to go.  Similarly, and probably the most important lesson I think I ever learned from Gerry Neugebauer, as my PhD advisor, was pay attention to the data. That’s where the information is. And that the story may evolve around the data because more information will keep coming in, but you really have to pay attention to the data. |
| Q2 | **You talked about translating hiccups into an opportunity. How do you deal with challenges?** |
|  | It’s really important to realise that hiccups are a natural part of doing research. And that’s why I think that ability to deal with hiccups is so important. Research and discovery is about exploring our frontier of knowledge, where we don’t know, we don’t have everything worked out. It’s not neat and tidy.  I think it’s just an acceptance that things are hard and messy, things are complex. Your job as a scientist is to figure them out. You have to relish in the mess. It’s almost like the process of putting things together. Or if we come back to the puzzle analogy – you’re trying to figure out which goes with which, so some of them aren’t going to work and if you can view that as progress.  I think it’s just a viewpoint. I’ve now come to understand that’s something I take for granted, it isn’t necessarily how the world views things. I think it’s really interesting when things don’t work because that’s the path to discovery. Scientifically when you discover things that are inconsistent with how our current paradigm tells us things should look, it’s like, ‘Oh, well, this is interesting! There’s something more to be discovered here.’ In a sense, you could say that’s a failure of the theory, but it’s actually a huge opportunity for discovery and understanding. So if you really embrace that, if things don’t work, there’s something that can be learned. |
| Q1 | **Is there a particular piece of advice that you would give to a young scientist?** |
|  | These days, one of the things I like to talk about with young scientists in my group, or anywhere is the idea that you want to take stock every few years of three things:  One is what is it that you love to do, what gives you pleasure? What do you find intriguing?  Two is what is it that you want to explore? It’s important to keep exploring because you don’t know what you’re going to enjoy until you try it. I’ll come back to an example of that for me, which really taught me that lesson.  And then the last thing is, how do you give back, how do you open up that opportunity for others to figure out what they enjoy? You’ve benefited from mentors. So how can you give back?  So those three things I think are useful to continually take stock of, because as you evolve the answer to the first thing isn’t going to stay the same. It’s just going to keep changing. So check in with yourself every once in a while.  The example that I like to share about trying things that you don’t know, or you might be afraid of, for me was public speaking. Given that I do so much of it now, I was terrified of public speaking, so much so that where I chose to go to graduate school was based on whether or not I had to teach, When I got to graduate school, I gave my first research talk and I shook from my head to my toe. I was so nervous. And my PhD advisor, Gerry Neugebauer said, you have to teach, you’ve got to get over this. So I ended up teaching and I discovered that I really enjoyed it. I quickly decided that I really wanted to teach the intro physics classes because there were very few women. So I took something that I liked, which is trying to expand or encourage young girls along for the ride. And it was just a great experience. I think I discovered my interest and passion for teaching as well as research. |
| Q11 | **Is there any advice that you would give to young female scientists? What do you think we should do to encourage more women to do science and increase diversity in general?** |
|  | Well there’s two parts of that question, I think, which is how do you succeed? I think there’s some general advice, no matter who you are, which is the importance of community, having people that you can turn to to support you through all the bumps and wiggles that whatever path you’re on presents. That’s your support system. It’s your network, your professional family that no matter who you are you really need. I think the challenge when you’re a minority, no matter what your minority is, is making sure that that’s in place. There’s just less people to choose from that look like you, although, it’s true, that you can get support from people who don’t look like you. It’s just that you need a support system.  The other part of your question, is encouraging diversity. I think we learn so much when we bring different points of view to the table, I think that’s a really important part of the conversation. I personally think that the people with the biggest passions for doing the work are going to do the best work. So I think in some sense, the challenge, or the opportunity is, to feed that desire – that desire to succeed or the quest to want to know. To not suppress it, or inhibit those who have that desire to understand. As kids we’re all so curious, that’s just the way we learn about the world. I think for us really the challenge is to encourage that no matter who you are or what you look like. |
| Q8 | **You’re obviously very passionate about science and physics, but what do you like to do outside of work?** |
|  | I think it’s really important to have other things for many reasons – one it’s just healthy and two, sometimes you just need to look away from your problem, in order to sort it out, in some sense to give your brain a break or relax and see something from a different perspective.  I’ve always enjoyed doing something athletic. So today I love swimming but even that has been an interesting evolution. I started in high school as a runner and as a field hockey player. I went to college and I joined the cross country team, so I got into marathon running. Then I decided at the end I wanted to do a triathlon, but my swimming was really weak. So I joined the MIT swim team in my senior year for a bit. As a graduate student, I got into masters swimming and I’ve done masters swimming ever since.  For me, swimming or just being active has two really important components. One is that physically it’s really important to stay healthy. Often times I have to say the problems I have at work get sorted out in my head in the pool – it used to be running now it’s swimming. And then in terms of this idea of having a network of people, I love the masters swim team, it brings together people from all communities. So I have the social network, or place, also where I’m not a minority. At work there aren’t that many women, but it’s really nice to be part of a community where you don’t stand out. It also just gives you sort of fortitude, that’s really important.  I should say my other thing I do a lot of outside of work is I have two kids, so I take care of them. I mean they’re much older now, but being a mom has been a great addition to life. I’m really glad I got that opportunity. I think I’ve learned a lot from being a mom. |
| Q7 | **What do you like best about being a scientist?** |
|  | Gosh so many things. Maybe the best thing is I have a job I love. That’s the ultimate privilege in life, to love what you do. I feel very fortunate.  That’s not quite what you asked though. About being a scientist: I think it is being able to pursue questions that you define for yourself. Sometimes there’s a lot of freedom – no one’s telling you what to do. You’ve got to figure it out yourself, but that’s an amazing opportunity in life. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0015** |
| **Biographical** | My impression is that each of us is born with distinct personal char­acteristics. It seems quite observable in our three daughters, and I think I see it in myself. I believe I was born to be a physicist of the kind who may be a little weak on the mathematics but has some sort of intuitive grasp of the science. I remember at a very early age pestering my mother to be allowed to put together the parts of the coffee percolator after she washed it. I enjoyed taking other things apart, though I did not always do so well in reassembling them. And I remember coming upon an explanation of compound pulleys in a schoolbook of one of my older sisters. I thought that was neat, and still do. To me physics is compound pulleys, all the way down. I also inherited or somehow acquired the tendency to dream, sometimes about physics. That may have been a little detrimental to my career, because dreaming can help postpone action. But on the whole, it was seriously beneficial.  I was born in what was then the city of St. Boniface. It was meant to be the francophone sister city of anglophone Winnipeg, in the province of Manitoba in the center of Canada. But the arrangement was unstable: as Winnipeg grew St. Boniface shrank to a charming neighborhood in greater Winnipeg. I began education in grade one at King George V school in Norwood, an anglophone suburb of St, Boniface. We moved to St, Vital, then a rural municipality just south of Winnipeg, where I attended Wind­sor junior high school and then Glenlawn Collegiate for grades 10 to 12. It was a small high school then; I guess about twenty-five graduated in my class. Glenlawn has grown a lot larger.  I realize now that I was not a satisfactory student in high school because I did not pay much attention to my teachers. I was not rebellious, just a dreamer, little motivated to do more than what was required to pass the examinations. I cannot remember whether I had given any thought at all to what I wanted to do with the rest of my life when I graduated from high school. My father had wanted to go to university, but the great depression and his overbearing father prevented that. So, my father took a job at the Winnipeg grain exchange, where several companies looked after the shipping and sales of the harvest from the great prairies to the west. It was not a very inspiring job, but important to have during the depression, and he stuck with it to the end of his life. He found solace in alcohol. My mother found solace in St. Mark’s Anglican Church. On the rare occasions when she was able to persuade me to accompany her, I was deeply bored. My father would have nothing to do with religion. He was a handy person, and I enjoyed working with him.  My older sister Audrey went to normal school, which in those days was preparation for teaching in public schools. Before me that had been the highest degree of education in my immediate family. While attending uni­versity I lived at home and took the bus in. My summer jobs generated enough money for books and tuition.  As I said, I had not given thought to what to study at the university. But I knew I liked to build things such as model airplanes, and I had the impression that engineers built things, so I enrolled in engineering at the University of Manitoba (which I will term the U of M for short). I liked many of the classes, and learned a lot, some of lasting value. I was exposed to calculus in what might be termed an engineering point of view, which suited me, for as I said I don’t have a strong intuitive feeling for mathematics. I remember with pleasure engineering drawing with india ink on linen fabric. We don’t do that sort of thing anymore, at least not in my field, but the manner of visualization it required was valuable and has stayed with me. And the courses allowed intervals of free time that I filled by playing hearts, the card game. I particularly liked the phys­ics courses. I remember, in my second year in engineering, complaining to a friend from Glenlawn Collegiate, Dale Loveridge, that I was running out of physics courses to take. He replied that I could transfer to physics. To the best of my recollection that thought had not occurred to me. So why, if I was born to be a physicist, did it take someone else to make me realize that I ought to transfer to physics? I can only say that I tend to be vague about such things. Anyway, I made the change in my third year at the U of M and felt at home. I guess I could have made my way through life as a mediocre engineer, but Dale directed me to something for which I am far better suited.  The courses in physics were fascinating and the students compatible with my inclinations, intellectually and socially. We spent a good deal of our spare time playing another card game, bridge. But we also spent a lot of time arguing about mathematics, which was OK, and physics, which I loved. I learned a lot from those discussions, and even more from the lec­tures. Our courses did not get very far into the 20th century, but that was fine for me. When I arrived at Princeton University as a graduate student, I found I had to work hard to catch up with what the other students knew about modern physics. But I think I had a better than average education in the foundations, including good old classical physics.  I remember the day my closest friend among the students in physics at the U of M, John Moore, came to me saying, Jimmy you have to meet Ali­son. That was because her surname is the same as mine. Maybe we’re related, but her line of Peebles came from Ireland, mine from England, so the relation looks kind of distant. We liked each other, and our physics friends saw us married and shipped off to Princeton in 1958. Al has been my best friend since we met.  Graduate study at Princeton was Ken Standing’s idea. He was a profes­sor of physics at the U of M and had been a graduate student in nuclear physics at Princeton ten years earlier. He formed the opinion that Prince­ton is the only place for me. I don’t imagine he could have seen how right he was. It was a real pleasure to talk to him the last time we met, in the Spring of 2016. But he had started to exhibit the symptoms of Parkinson’s Disease, which soon took him. Ken had a productive career in precision measurements of masses of macromolecules, which biophysicists value, and he loved to spend time in his cabin in the beautiful woods toward the eastern edge of Manitoba, in the Precambrian Shield. I owe a lot to Ken.  I entered Princeton with the intention of doing something fancy in par­ticle physics. I wrote one paper on that subject, which I see has gathered five citations, one of them mine. I was saved from a dismal future in that direction through the help of two fellow graduate students, both also from the U of M. Bob Pollock was a year ahead of me, we were friends while both of us were there, and he and Jean, and Al and I, remained good friends. Pollock was a gifted experimentalist. Soon after I arrived at Princeton I was approached by Professor Donald Hamilton, who wanted to discover whether I was another Pollock, and if so whether he could persuade me to join his experimental atomic beams group. A short con­versation revealed that I am no Pollock, and we parted as friends. I did not know Bob Moore while at the U of M; he was a few years earlier than us. But Moore led me to Professor Robert Henry Dicke’s Gravity Research Group.  After war research on radar and other electronics, Bob Dicke spent a decade at the laboratory bench in Princeton on what might be termed quantum optics. But then he decided that the study of the physics of grav­ity was seriously neglected, and that the great advances in electronics during the war would allow many of the classical experiments in gravity physics to be done better and would allow new experimental probes into the nature of gravity. He quite abruptly changed his direction of research to the empirical study of gravity physics. The first twelve PhD disserta­tions he guided had nothing to do with gravity. The last of these is dated 1959. The next, dated 1961, is mine. The twenty-six dated after 1960 include only one that has nothing to do with gravity.  The abrupt switch of direction may seem bold. But at about the same time another member of the faculty, John Archibald Wheeler, decided to turn the direction of his research to the theoretical study of gravity. This cannot have been entirely coincidental, but the two had quite different philosophies. Wheeler accepted [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)’s general theory of relativity and explored to great effect its consequences and ways to reconcile it with quantum physics. Dicke seemed to be almost personally offended by the scant empirical support for general relativity, and he enthusiastically explored questions that many had considered settled without empirical support at the precision possible then. Is the period of a mechanical oscil­lator, measured at rest relative to the oscillator, really independent of its motion relative to distant matter? Is the period defined by a spectral line quite independent of the atom’s motion? Are parameters of physics such as the strengths of the gravitational and electromagnetic interactions really independent of motion? Might these parameters be evolving as the universe expands?  I was impressed by what Wheeler was doing and enjoyed interacting with his many graduate students and postdocs, but I was not inclined to join his group. Bob Moore took me to the weekly evening meetings of Dicke’s Gravity Research Group. The group cannot have been much more than a year old when I arrived, in the autumn of 1958, but graduate stu­dents and postdocs had already started ambitious experiments, while oth­ers were looking into such arcane things to me as the dating of historical eclipses, for the purpose of checking the orbits of the moon around the earth and the earth around the sun. Dicke had some of his graduate stu­dents and postdocs working with him on a repetition of the Eötvös exper­iment that demonstrates that the gravitational acceleration of a free test particle depends very little if at all on its composition. Eötvös had to observe his balance from a distance, using a telescope. Dicke buried his balance and used his elegant feed-back techniques to monitor the electro­static force needed to hold the balance fixed. The measurements have since been done even better, but Dicke showed the way. We heard pro­gress reports and discussions of these projects, and thoughts about what other things might be investigated. Some of Wheeler’s students, who were looking into the theoretical side of general relativity and quantum phys­ics, sat in on the Gravity Group meetings. And Dicke brought occasional visitors. It was a fascinating tour of physics. The Gravity Group meetings showed me what I wanted to do and taught me a lot about how to do it.  Dicke directed me to the issue of whether the strength of the electro­magnetic interaction, represented by the fine-structure constant (in the old-fashioned units I still use) *a=e2/hc* (1) might be evolving as the universe expands. This led me to learn a lot of nuclear physics, because if a evolves then the decay rates of long-lived isotopes change, increasing or decreasing according to how a change in a changes relative energy levels. And that could mean the radioactive dating of minerals and meteorites based on the assumption of constant decay rates would produce inconsistent results from different isotopes. So I read a lot of geology, and learned fascinating things such as the great extinc­tions. And I cooked up a relativistic classical field theory that allowed a to evolve without serious violation of the Eötvös experiment. All of this went into my doctoral dissertation. My bounds on the possible rate of change of the value of a are modest compared to what has been done since by observations of the spectra of galaxies and quasars at redshifts well above unity. And I have never reexamined my theory of how a might evolve, to see if it truly makes sense. But this was an excellent learning experience.  I learned the standard thinking about the expanding universe from the book, *The Classical Theory of Fields*. It is part of the marvelous series on theoretical physics by Landau and Lifshitz. The books in this series do not deal much with phenomenology. The closest I find in *The Classical Theory of Fields* is in a footnote (on page 332 in my edition, the 1951 trans­lation from the 1948 Russian edition). It cautions that the validity of the assumption that the universe is close to homogeneous and isotropic in the large-scale average remains an open question. That was a very sensi­ble remark. My other reference was Tolman’s *Relativity, Thermodynamics and Cosmology*, published in 1934. It too is thin on phenomenology. I was left with the early impression that the subject of cosmology was pretty much free of the empirical physics I enjoy applying, and I saw lots of room to go about creating some physics.  Dicke had suggested that the universe may have expanded from a hot dense early condition, leaving a remnant sea of thermal radiation that was cooled by the expansion. I saw that this would imply interesting thermo­nuclear production of light isotopes. Toward the end of 1964 I learned that I had been reinventing the wheel; George Gamow published most of my ideas in 1948. But he left room for more detailed analyses of the evolu­tion of the isotope abundances. And I did hit on new ideas about how the sea of thermal radiation would affect the gravitational assembly of matter into galaxies and groups and clusters of galaxies. I discuss all that in my [Nobel Lecture](https://www.nobelprize.org/prizes/physics/2019/peebles/lecture/).  I gave a one-term graduate course on these ideas about physical cos­mology in the fall of 1969, and John Wheeler insisted that I turn my lec­tures into a book. To that end he took notes that he gave me at the end of each lecture. The sight of that great physicist taking notes in his elegant hand so unnerved me that I promised to produce a book. I meant its title, *Physical Cosmology*, to indicate that I did not intend to get into the subtle­ties of what might be termed astronomical cosmology: evidence from stellar evolution ages and the extragalactic distance scale. I don’t think I thought of it at the time, but the title also helps distinguish this book from the bloodless approach in Tolman’s *Relativity, Thermodynamics and Cosmology* and [Landau](https://www.nobelprize.org/prizes/physics/1962/landau/facts/) and Lifshitz’s *The Classical Theory of Fields*. I meant to explore the physical processes that are observed to operate, or we might imagine operate, in an expanding universe. At about the time of publication of my book, in 1971, [Steve Weinberg](https://www.nobelprize.org/prizes/physics/1979/weinberg/facts/) published his book, Gravitation and Cosmology. It presents more complete theoretical consid­erations. Mine is more complete on the phenomenology and the physics that might show how the phenomenology all hangs together. The two books mark the start of the growth of physical cosmology from its near dormant condition in the early 1960s to a productive branch of physical science by the end of the 1960s. But my role in how that happened is dis­cussed in my Nobel Lecture. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0015 = JP**  James Peebles: Hello.  Adam Smith: Hello, my name is Adam Smith. Many congratulations on the award.  JP: Thank you.  AS: You are of course a cosmologist, but I suppose you are also an explorer, an explorer of the universe. Does it feel as if you’re an explorer with your mind?  JP: Oh yes. We think, we ask ourselves questions, some of us start computing, others start measuring. It is an exploration of course. Very different from exploring far reaches of the Earth, but explore in so many ways.  AS: You mention that interplay between observation and theory. That is very important, isn’t it?  JP: It certainly is. We must always bear that in mind. It’s so easy … well, I will make a small sermon … it is so easy for us theorists who build wonderful castles, beautiful ideas. Sometimes, it is remarkable, sometimes these beautiful ideas prove to be close to what the observations tell us. But often and also they turn out to be wrong. No great surprise, but time will tell, and it is the measurements that tell us. Of course we must bear in mind that measurements without theory are equally empty. It’s stamp collecting.  AS: It’s a great joint enterprise.  JP: That is the whole point.  AS: And of course you’ve been instrumental, as the committee said, in changing cosmology from, in their words, speculation to science. One of the results has been your discovery that we don’t know what most of the universe is made of.  JP: Yes.  AS: May I ask, would you hazard a guess as to when, how, or indeed if, we’ll know what it is made of?  JP: One of the wonderful things about this exploration is that of course we don’t know what we will see. And it is true here. I hope that we will be surprised by what is found to be the nature of the dark matter. It might be something that has already been considered seriously. If so, the demonstration will be a detection, perhaps in the laboratory. There are remarkably sensitive experiments now hoping to detect the interaction of dark matter with ordinary matter. It might be through its annihilation that releases energy that can be detected as radiation. But my romantic dream – I guess I am romantic about these things – my romantic dream is that we will be surprised yet once again. I’m hoping that will be the case. And so I cannot tell you at all how it will be discovered that we know what the dark matter is. It will have to appear.  AS: That was beautifully said, yes, and I like the idea that one doesn’t know where one needs to look. It could be in the laboratory, it could be out in space. One just has to keep looking.  JP: That is very appropriate, you don’t know where to look. It means that these beautiful experiments to detect dark matter must decide on a direction and then work exceedingly hard over many years to explore in that direction. It takes a tough mind to do that because you consider that they might be looking in the wrong place. I mean that in no way to be a negative statement – I deeply admire the people who are doing these experiments to detect, one way or another, to detect the dark matter – but they know, and they are resigned to the fact, that they don’t know where to look. So they choose a direction, you make that tough decision and you work hard at it.  AS: Do you ever find it overwhelming, all this mystery?  JP: No, no, no. Overwhelming? No, no, that has never come to my mind. Wonderful? Yes. Fascinating? Yes. Eager to know more? Absolutely.  AS: I so much look forward to speaking more when you come to Stockholm in December. It’s very exciting news.  JP: [Laughs] It is indeed, we are excited.  AS: Thank you, thank you very much indeed. It was a huge pleasure speaking to you and we look forward to meeting you in December.  JP: You’re very welcome.  AS: Thank you, bye bye.  JP: Goodbye. |
| **Interview** |  |
| Q16 | **What did you want to be when you were younger?** |
|  | James Peebles: Until I reached university, I was rather directionless. Never a rebel but rather a dreamer. I didn’t pay too much attention to classes, I am sure I annoyed my teachers, or to what I would want to do after I graduated from high school. So, I didn’t have a direction until I reached the University of Manitoba. There I learned that I love physics. I’m sure there were signs before I reached the university that I loved physics, for example I do remember as a youth I would read anything that was before me, the cereal boxes. I read in one of my older sister’s schoolbooks how compound pendulum works, and I still remember thinking how wonderful that is, and it was a hint, and I liked to build models. Because I liked to build models, I entered the university in engineering. I enjoyed it. I guess I could have made my ways through life as a mediocre engineer, but to my eternal gratitude of friend who I can name, Dale Loveridge, since I complained to him that we were running out of physics courses to take in engineering: “Why don’t you transfer to physics?”  Perhaps you know the phrase a “Duh” moment? Well, that was a “Duh” moment. Why didn’t I think of that? So, I entered physics and loved it from the start. I owe a lot to the faculty and I owe a lot to the students too. You know, students learn a lot from fellow students, we don’t give that enough attention, I think. Nothing I like better than to see students arguing over how to work a problem. Also, my fellow students introduced me to Allison. So I was married and off to Princeton university as graduate student. The faculty member who told me I would go to graduate school in physics to Princeton, Ken Standing, died just a year or so ago. It was a great pleasure to meet him prior to that. He had his own distinguished career in the structures of large biological molecules, biochemical molecules, I am not sure the phrase. Anyway, I love physics, it’s just grabs me and still does. |
| Q3 | **What do you enjoy about physics?** |
|  | James Peebles: One way to put it, I think, is that event with the compound pulley. A compound pulley is that the one in the same time rather subtle, but yet explicit within the framework of the game. Similarly, physics is layer upon layer of concepts such as a compound pulley. Each compound, complex, what am I trying to say? Each element of physics is, when looked at from afar, a compound pulley. It’s a simple concept, well defined and subtle but yet direct straightforward in many ways. And physics is just a hierarchy of such things. I find that neat. I suppose I am also satisfied by the fact that in physics you get to settle arguments, because you can do experiments and you can find out whether or not this concept make sense. It’s also relative to say, oh, biology much simpler and I do treasure that simplicity. Again, my impression of a biologist is someone who’s waiting through depths and complications, it’s just so subtle. Whereas in basic physics, which is the kind I enjoy, you can actually start from the fundamentals and work your way up, layer upon layer of compound pulleys. You can’t do that with biophysics because it is just too, too complicated. I guess I could have made my way through life as a biophysicist too, but I wouldn’t has been as happy. |
| Q5 | **Was there a particular person that influenced you?** |
|  | James Peebles: Yes, I can mention two. I have already mentioned one, Ken Standing, at the University of Manitoba. You know I reflect back, and I don’t remember him ever saying: “I suggest you go to the University of Manitoba, Princeton for graduate study”. He was rather: “You will of course go to Princeton”. It set my life. If I hadn’t gone to Princeton I wouldn’t have met my second and really top adviser, professor Robert Henry Dicke, Bob to everyone, who was an inspiration when I arrived. He has doing gravity physics which of course lead to cosmology. He has just the sort of skills that I really take a delight in. He understands physics very well, but he understands also how to apply it and to be very, very much a person who likes the combination of theory and practice which totally grabbed me and still does. Bob Dicke I have called for many years my professor of continuing education. He’s alas no longer with us but through the years he taught me so much. Not only about physics, but also, don’t be messy with your physics. The one time he would get hostile was when you were sloppy in your thinking. And that certain cured me of being sloppy. |
| Q2 | **Can you tell us how you discovered you’d been awarded the Nobel Prize?** |
|  | James Peebles: Do you know that in the US, perhaps around the world, there are sites in which you can place money, wagers, on events, such as who will get the Nobel Prize and it turns out that these sites have a pretty good record. You understand that the more money placed on a candidate for the Nobel Prize, the lower the pay-off. So, my university and I think many others, keep track of the odds on faculty members and so, for the last two years I have had an enigmatic message from our department of public relations: “If you need help with publicity we will come to your aid”. No explanation, no mention of Nobel, but at this time of year you wonder, so I was slightly prepared, and you know, to be honest, I think it was a good choice. I’ve been riding this wave my entire career, so, that was great. They called at a definite hour here in Sweden, which may be a very different hour at the Laureate’s home. So, five o’clock, the call, and you know at that hour either it’s something really bad or something really good, so, I was somewhat prepared when I picked up the telephone. Allison’s … my first words were: “Oh God!”. The university was prepared, and they had laid on elaborate celebrations through the day. Totally exhausting. But still, utterly rewarding. |
| Q1 | **Do you have any advice for young scientists?** |
|  | James Peebles: There is one piece of advice that I keep advising, keep offering: “Don’t judge your career by the number of prizes and awards. I have so many. It’s wonderful and the Nobel Prize is absolutely spectacularly wonderful, but to get such a prize requires not only dedication and creativity, it requires eventualities. The cards must line up just so, in order to make it appropriate for a prize-winning committee to recommend you, don’t count on those eventualities. Judge your career by how well you did at it. And of course, don’t be sloppy. |
| Q1 | **Do you have any advice for young people entering science?** |
|  | James Peebles: My advice, my central advice to a young person considering entering science of any sort, say in natural science: look around, discover what really interests you. It may not be the first thing that you notice, you may find something mildly interesting, but if you look a little harder, you’ll find something even better. Don’t jump into a particular line of research until you have looked around quite carefully and discover that which really fascinates you. If you are fascinated, you’ll do well. |
| Q3 | **Why do you like teaching?** |
|  | James Peebles: You understand that teaching, the students learn from a good teacher and the teacher learns from the students. Also, you know, there is the comment of Samuel Johnson, “Nothing quite concentrates the mind like a prospect of being hanged” and that quite a serious, but the prospect of having a student ask a question that you haven’t anticipated, is something that makes me very uneasy. So, of course, when I teach, I prepare, and I try to think of all of those odd little side issues that I never perhaps thought through. They happen and I learn from that. Also, of course, it’s so lovely to see young people who are interested in something, so interested so they sit here and take notes from what I say. And, of course, I emphasize time and again, they are learning from each other. Nothing I love more than to see a group of students arguing over how to work a problem, so that’s rewarding. And because I love physics, you are not surprised to learn I enjoy talking about it and so, yes, I have taught both students who are advanced in physics, who are deeply interested in physics and those who take the course as a requirement.  The last is a far by far the most difficult. How do you persuade these people that physics is not simply a hurdle to pass so they can get on to do something they really want to do, perhaps medicine? How do you convince them that this is a fascinating subject? In part, you know, I think we have to be fatalistic about that, some people are charmed by compound pulley and some are not. Perhaps you don’t particularly care about compound pulleys, but they are neat, and perhaps not neat for you, but some for they are. So, I guess I do have the seat-of-the-pants feeling, feeling that different people are suited for very different activities, that’s obvious isn’t it, must be. So, my test might be, offer them a compound pulley, do you think that is neat or would you rather that I would stop talking about it. So, most of the time I have taught people who think compound pulley is neat even though they’ve never considered that before. By far, as I say, the most difficult is teaching those who’s not so sure compound pulleys are neat. The most depressing question is: “Is this going to be on the exam?” |
| Q8 | **What are the most important qualities for being a teacher?** |
|  | James Peebles: The most important quality of being a teacher, I think, surely is the enthusiasm for what you are trying to teach. And perhaps it’s equally important, you should pay attention to the students. That seems pretty obvious, but I suppose if you are a teacher who don’t enjoy your job, then I think it would be good of you to find another line of work. If you enjoy your job it means you are just love transforming, transmitting information to the student. And you will notice the student either understanding what you’re saying or not and if the latter you’re going to try hard to get the student’s attention. That was when we used to have an hour and a half long lectures, to stop half way in between was a good way, not only to get them awake, because it’s rather hard to sit for an hour and a half, but also I found, there are students who have questions but they don’t want to ask them, because they don’t want to be the subject of attention and the breaks were a wonderful way for a student who was diffident to approach me. I don’t know how to encourage these people to speak up, no one is going to bite them, some people are just like that. The break was always a good way, not only to refresh all of the students but also to give the diffident ones to get a chance to talk to me. |
| Q9 | **What do you enjoy doing in your free time?** |
|  | James Peebles: As I have reached the golden years, I find that I enjoy physics, so I don’t often take much time off. I love going into the laboratory and writing and calculating or reading. I used to enjoy gardening quite a lot. In recent years physical effort is becoming more difficult. The fact that the market gardens have tomatoes in immense abundance when I have my pitiful few tomatoes, I stopped gardening. However, I spent the last two years full time on a book on how we got from where we were in the past to now in cosmology. The book is now done. It will be out next spring. I have still some deep jobs to do in checking proofs, but that labour is pretty much over and I think I will not get myself involved in another project that is quite that intense and that maybe I will start gardening again. I think maybe flowers rather than vegetables. |
| Q2 | **Where do you do your best thinking?** |
|  | James Peebles: I’m deeply fortunate that we live just one mile from my office. It’s a mile through almost entirely quiet residential streets, so walking to and from the lab is to me refreshing and a chance to let my mind wander. I’ve always, when walking, paid very little attention to where I put my feet and so the mind can wander in totally random, irrelevant silly directions, but sometimes it will land on a little point: “Why didn’t I think of this?”, rather a “Duh”-trip moment. Though it’s interesting, I discovered a few years ago that I was tripping and, you know, a face-plant when you fall, it happened to me three times in one year. No injuries at any time but it’s such a chock to suddenly find yourself prone. So, I’ve started looking where I put my feet to the detriment of my thinking. Which should I do? I think I will continue to watch where I put my feet, I will put a little more attention to that, but I also will continue to enjoy walking and letting my mind wander. I hate running. I can’t imagine operating a treadmill or pumping weights, that’s sounds so dull. But to me a walk in the woods or in the city is pleasant – always is. |
| Q14 | **What do we still have to discover?** |
|  | James Peebles: In our field we are leaving to the future generations a lot of interesting research problems. It is rather difficult to convince non-scientists of the fact that we at the same time have a reliable science, well-established and yet there are simple questions they can ask of you that you can’t answer. It’s particularly notable in cosmology when you consider that we postulate this dark matter, we postulate this cosmological constant or dark energy. They surely have deep physical meaning, that we do not understand, what a glorious opportunity, explain this. It is as I say, a subtle business to explain that we have both great open problems and yet a securely established physics. The point is, of course, all of our physics are approximations, we have no complete theory in any branch of physics or in any other natural science. We instead have approximations that are good or more or less well-established depending on the evidence. We have so much evidence for cosmology that I think it is almost a dead cert that the dark matter’s there. We know that its properties must be in a defined range, but that range is pretty broad. We’re sure that there must be there and the great triumph would be to identify it. Lots of experiments are going on attempting to do that, watch your local newspaper, for announcements. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0016** |
| **Biographical** | Iwas born in 1942 in Lausanne, Switzerland, a small town along Lake Leman where I started elementary school. My father was a police commissioner with positions in different cities. When I was six years old, my family moved to Cully, a superb village in the vineyards along the same lake and four years later to Aigle, a small town of less than ten thousand inhabitants in the upper Rhone Valley, in the midst of the mountains.  During my 5 years at school in Aigle, when I was aged 11–16, I had an exceptional teacher of science, Edmond Altherr. He was the Director of the College and a teacher, but nevertheless he continued to do research at home, on the nematodes, during his whole life. Nematodes are an animal phylum that includes some worms that are parasites of the human body. Although that does not look like a charming subject, he was able to convey a lot of enthusiasm about the study of nematodes and so he was able to stimulate our interest in science. A great pedagogue, really.  Apart from school, I was an active member of the scouts: hiking, skiing, camping in high altitude mountains and every kind of outdoor activity. As a teenager, I began doing one of my most exciting sports: climbing. With Aigle so close to mountains, living there allowed me to regularly practice these sports. Climbing provides me with immeasurable pleasure but… at one stage I was very happy to be rescued from a deep crevasse without too much damage!  I was obliged to move back to Lausanne to go to high school, as there was none in Aigle. After high school, I went to the University of Lausanne to study theoretical physics. When I was a child, I was always interested in science, but did not dream of being an astronomer. I was fascinated by every domain of sciences: wildlife, geophysics, geology, the Alps … But I loved mathematics, so I chose to study physics and mathematics at the University of Lausanne. After the first year I had to choose between them, and I chose physics. Professor Karl Gerhard Stuekelberg was my most important professor for theoretical physics. Students were fascinated by his very profound vision on physics and I chose to do my masters degree in theoretical physics (in 1966). My thesis was on the interaction of particles with large spin and was based on formalism of [Steven Weinberg](https://www.nobelprize.org/prizes/physics/1979/weinberg/facts/). In mathematics I had the privilege to have Georges De Rham as one of my professors. He is known for his major contributions in differential topology, but also as a great alpinist!  In 1966 when I finished my studies at the university, I got married to Françoise. She was then a student in the natural sciences at the University of Lausanne. We have been blessed with three children, Anne, Claire and Julien and today five grandchildren.  For our children, Haute-Provence Observatory was a second home as the whole family came with me for numerous observing runs (at least during school holidays). It seems that our children have acquired the virus of scientific curiosity by listening to the discussions with friends at home or at Haute-Provence Observatory. Today, all of them share the same passion for research but in quite different domains.  **First steps in science**  After graduation, I got a position as a PhD student in the field of galactic dynamics at the University of Geneva.  The origin of galactic spiral structure had been a long-standing problem: if it was a result of a strong differential rotation, as appeared, then such galaxies would not keep their shape, but would wind themselves up. Professors C.C. Lin and Frank H. Shu proposed a theoretical explanation in 1963. I wanted to test the consequences of their idea on local stellar velocity fields to see how the spiral structure perturbed the mean flow of stars. This was the start of my interest in stellar kinematics, and I started my PhD study in this domain.  By the end of my PhD I was looking for observations to test these ideas, but the stellar velocities in catalogues were not adapted to my question. I needed new data. This was my critical motive for moving from theory to instrumentation, with a specific focus on the determination of stellar kinematics. By chance, at an N-body Colloquium in Cambridge, I met Roger Griffin and we talked about a new spectrographic method to measure stellar velocities by cross-correlation (Fellgett 1953, Griffin 1967). I was really impressed by this technique. It was evident we could make much progress in this domain, and I realized that this was what I needed to get my data. Back in Geneva, I discussed it with the director of our institute, who was in favor of this as a way to develop the observatory. But I think he wondered how I, a theorist, would dare build an instrument.  I needed advice, and André Baranne − professor of optics at Marseille Observatory in France − provided it. In fact, he found the problem interesting as he designed the optics… Sometimes it is easy to collaborate with people like this case, in science. This was my first cross-correlation spectrograph with more technical possibilities than Roger had envisaged and with a computer that made it very efficient.  A first CORAVEL instrument was installed in 1977 on our 1-meter Swiss telescope at the Haute-Provence Observatory and a second one on the 1.5-meter Danish at the ESO La Silla Observatory in Chile to have access to the southern sky. The huge efficiency gain of this new kind, fully optimized instruments opened for me the domain of the stellar kinematics. During more than 15 years, I have visited so many different domains: dynamics of globular clusters, cepheids and supergiants in the Magellan Clouds, stellar rotation and with Antoine Duquennoy double stars of the solar vicinity.  (Duquennoy and Mayor, 1991). The statistical properties of binary stars are fossil traces of stellar formation processes. The mass-ratio distribution of double stars was one of the observed results. The precision of the CORAVEL was just sensitive enough to detect companions at the very bottom of the main sequence… close to the domain of brown dwarfs and giant planets. The opportunity offered by the Haute-Provence Observatory to develop ELODIE at the very beginning of the nineties opened the path to the detection of brown dwarfs AND giant planets. Based on our long radial velocity monitoring of stars of the solar vicinity we have selected 142 single G and K stars as the stellar sample for our ELODIE program.  What conditions played a significant role in that discovery? The impressive efficiency of the cross-correlation spectroscopy, the on-line reduction of the measurements with its impact on the observing strategy, the large size of the stellar sample, the number of observing nights allocated to our program by the OHP observing committee and obviously the precision of the spectrograph. One hot Jupiter among 142 solar-type stars corresponds to the frequency of this kind of giant planets… we have not been especially lucky!  The discovery of 51 Pegasi b in 1995 completely changed the priorities of my research.  I was happy to have been associated with that first epoch of exoplanet discoveries.  After this first detection, not only have we continued our search in the northern hemisphere but in 1998 we have initiated a systematic search for exoplanets in the southern sky.  CORALIE, a spectrograph almost identical to our northern instrument, was installed on our quite new 1.2 m EULER telescope at La Silla observatory. Small telescope… but today having discovered more than 150 exoplanets.  At the very end of the nineties, in an answer to a call for proposals issued by ESO, I took the lead of the development of a new spectrograph to achieve a radial velocity precision of 1 m/s. Only after 3 years, in March 2003, we got the first light and the contractual goal of a precision of 1 m/s. For our consortium having built that instrument, the reward as measured by the number of 500 observing nights allocated for five years was at the level of the challenge.  Five hundred nights on a 3.6 m telescope devoted exclusively to a comprehensive program to detect and characterize exoplanets was a superb adventure for my colleagues and me.  Many of my PhD students are actively working in the field of exoplanets with outstanding results, for example Willy Benz, Didier Queloz, Nuno Santos, Christophe Lovis and Pedro Figueira.  The Kepler space mission has provided an exceptional harvest of transiting planets.  The comparative planetology requires having exoplanet radii (via transits) and masses (via Doppler spectroscopy). As the Kepler field is in the northern hemisphere, we have been obliged to develop a northern copy of our HARPS instrument (an instrument developed with Francesco Pepe as principal investigator). This program of that spectrograph installed on the 3.5 m Galileo telescope at La Palma Island, Spain, was focused on the physics of very low mass planets.  In 2007, I became emeritus professor at the University of Geneva with the privilege, still today, to continue contributing to transforming the old dream of Greek philosophers in the very active domain of present-day astrophysics. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0016 = MM**  Michel Mayor: Hello?  Adam Smith: Oh hello. My name is Adam Smith. I’m calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  MM: Yes.  AS: Where are you now?  MM: I’m in the bus going to the airport.  AS: Oh I see, okay.  MM: In Spain, I’m in Spain.  AS: May I just ask you, how did you hear the news of the Prize?  MM: Oh, completely by chance because just leaving my hotel, I connect myself to the computer. And I received the news from that, about one hour ago. Very nice, very nice connection, very nice surprise! So we have some difficulty to speak as I am in a bus in Spain, and the quality of the telephone is very bad …  AS: … But anyway, you’ve made it to the airport at least. It must be a bit frustrating to be away from home when it happens.  MM: Yes, it’s a little bit more complicated, and okay but I will be with colleagues in Madrid and in different place for giving some discussion on exoplanets so it’s okay, it’s a good moment with colleagues.  AS: Of course, many celebrations to come.  MM: Yes, I hope so!  AS: Do you recall the excitement of the moment when you saw the first exoplanet 24 years ago?  MM: Yes, I believe, you know this is not a discovery made at one special moment, because you need to accumulate a lot of measurements during several months, and only after that you have some hints that maybe you have something interesting. Because due to the lot of announcements made before I have decided to wait for the next season to repeat the measurements to be absolutely sure. The problem was not the quality of the measurements but was the interpretation of what we are discovering. So we wait ’95, in the middle of ’95, the second season, and everything was fine: the same period, the same amplitude, the same phase. So with Didier we said, “OK, now we are sure this is an extrasolar planet.” It was a great moment.  AS: It’s so unexpected that it orbits in just four days,  MM: Yeah.  AS: Such strangeness. It must have taken great courage to believe in your data.  MM: Apart to have the confirmation that extrasolar planets exist, this was maybe the most important aspect of this discovery, the discovery of the orbital migration. So that if planets are formed some distance of the star then after the interaction between the planet and the [protoplanetary] disk create a spiralling migration of the young planet towards the star, and this was a major ingredient of the scenario of planetary formation. And today, all scenarios have to include this kind of phenomena.  AS: And that’s the point isn’t it, that the discovery of all these exoplanets, over 4,000 now, teaches us a great deal about the way that solar systems form, and indeed even about our own Earth.  MM: Yes, yes, sure. In fact this is a different … we are living in a planetary system so all the planets of all solar systems belong to the same kind of scenario.  AS: Yes, because there’s often a great focus on the search for life, but we could learn a lot more than just whether there’s life out there by looking.  MM: Scientists are interested in the formation of planetary systems and so on, but for most, the majority of people, what is behind is the question of life. At the present time this is the most exciting question we have facing us, and for the next generation I don’t know how many years we will need for that, but this is the extremely important question for laymen.  AS: When do you think we might be ready to see the first signs of life if they’re there?  MM: It’s the problem of the detection of biosignatures. We know how to do it. But it’s so difficult, technically, so difficult. I believe maybe 10 years or more, I don’t know exactly, but I don’t expect to have an answer next year. Greek philosophers were discussing the plurality of worlds and the possibility to have some of them inhabited 2000 years ago, so I believe we can wait 20 years more.  AS: Well said, yes. We look forward very much indeed to welcoming you to Stockholm in December.  MM: What I have to say, ‘tack’?  AS: Exactly, ‘precis’! Thank you very much indeed.  MM: Thank you.  AS: Congratulations, bye bye. |
| **Interview** |  |
| Q16 | **What did you want to be when you were younger?** |
|  | Michel Mayor: When I was a child, I was in fact interested in science from the age of 10 years. Not specifically astronomy, but science. I was really a fan of quite different subjects: geophysics, plants and so on. When I was obliged to start a choice at university, it was for me very difficult to choose between mathematics and physics, so for the first year I chose both. After that I had to choose one and I chose to study theoretical physics. When I finished in 1966, it was quite easy to get a position as a PhD student because every laboratory was increasing, recruiting young people and so on. It was a little bit by chance that I choose astrophysics because on the same day, you have several opportunities, in statistics and mechanics, rather different domains. I said, “Okay, astrophysics, I will go and see if it is interesting”. I chose to continue and do my PhD in the domain of theoretical astrophysics. It was related to the problem of spiral waves and spiral galaxies. You are looking at a nice picture of a galaxy, you have sometimes two big arms. I did my PhD in this domain, absolutely nothing related to planets or velocities. This arrived only later. |
| Q3 | **Was there a particular moment that sparked your interest in science?** |
|  | Michel Mayor: I’m not sure to have a specific moment where I started to say I will do my full life in the domain of science. I believe it was the responsibility of a very good teacher. At some moment between 12 and 16 I had a fantastic teacher in science who was doing experiments, extremely stimulating. It was not a moment, but the contact with this man having really stimulated the interest of myself but also all the younger colleagues. At some moment, he tried to show us some chemical reaction with chlorine. It’s a very dangerous gas and probably today it would be completely forbidden to do this kind of experiment. He said, “Oh, it’s a little bit dangerous, so I suggest you go outside in the back yard to do the experiment outside to not smell the chlorine”. He did a fascinating experiment, putting some copper inside this gas to show that it started to burn, and things like this. We also did quite a lot outside visits to the forest to see flowers and observe them. This was the important moment.  Maybe I can suggest another aspect of this relatively old professor. During his lifetime as an elementary school teacher, he continued to do his own research at home. He was studying nematodes, this nasty worm you can have inside your body. He was a renowned man in this field. For us, it was very impressive to this teacher having at home continuing research, receiving dirty water from any place in the world to identify new species of nematodes. Eventually I did not choose to study nematodes, but extrasolar planets. |
| Q3 | **What do you enjoy about science?** |
|  | Michel Mayor: I believe science is one very fabulous way to satisfy your curiosity. I still have the same curiosity today when I’m reading some journal, equivalent to the *Scientific American* or general /- – -/. It’s so fascinating to see such the huge diversity of the work going on in the fundament of science, geophysics, archeology, medicine and so on. For me, it’s better than any fiction. I’m still continuing to have the same interest for the kind of research done by colleagues, and here it’s a good opportunity to meet people working in different domains. I love this. |
| Q7 | **What are the traits needed to be a scientist?** |
|  | Michel Mayor: Curiosity is the most important point to be a scientist. If a student is good enough in all the basic tools of science, mathematics, physics and so on, and love to study natural problems, to see what kind of very unusual phenomena exist in the nature. “Why is this like this?” Sometimes you are looking at the sky and you have some clouds and you see some bright spots apart from the sun. “Why is it like this?” If he has this kind of mind, he could do science. |
| Q3 | **Do you enjoy teaching and mentoring?** |
|  | Michel Mayor: I was appointed as a professor at the university during more than 20 years, so I believe I enjoyed it. I had to do some mandatory courses in astrophysics, but in addition to this, I had the choice to teach some different topics as the master degree, so you can choose. I would like to learn a little bit more in this domain, so you work to prepare the course and I liked to do that.  I’m very proud that I had I believe 18 PhD students in my life and I still have very good relationships with all of them. I was very proud that during many, many years, all of them got positions in science and continued to work in science. This was part of the pleasure of the Nobel Prize announcement. A few days ago, we had a special celebration at the observatory in Geneva. Some of my PhD students went from Chile, from South America, from Brazil, from the south of Italy to participate in this celebration. So, I believe, it’s good friends. |
| Q7 | **What do you look for in a PhD student?** |
|  | Michel Mayor: I don’t know how we have to select people because it’s not always because they are absolutely extremely good at the university that they will be the best. I believe it’s how to select. Sometimes you believe that they will be extremely good and some are not so good, and sometimes you have someone who’s absolutely fantastic, asking good questions and so on. We don’t have a checklist to select people after, we have to try and see. |
| Q2 | **Can you tell us about your relationship with co-laureate Didier Queloz?** |
|  | Michel Mayor: Didier was one of my undergraduate students first. After that he did his master thesis with me in the domain related to spectroscopy and things like this. Thereafter it was time for his PhD he chose to continue with me as a supervisor. It was just the beginning of a collaboration with some French colleagues to build a new instrument. It was a good opportunity so I asked him if he would like to collaborate with this development. It was the start of this long-term collaboration with him. He developed part of the software to analyze the signal we received. After the success at the end of a few years, before his PhD, there was the discovery of the first planet, and for him at least, it was a start in this domain, it’s evident. We have continued to work together for several years and still today we have some collaboration, part of the time in Cambridge, but also part of the time in Geneva we continued to collaborate. |
| Q2 | **How important is it to be open to new ideas and unexpected findings?** |
|  | Michel Mayor: In principle, this is one of the most important points of science, to be prepared for the unexpected. If you are not, only working on what you almost know before, it is not a way to make progress. You must always be prepared for the unexpected. In the beginning the program was to search for a low mass companion to solar-type stars. It was mentioned brown dwarfs and extrasolar planets. Brown dwarfs are a very small stellar companions, having a mass too small to ignite and to start a nuclear reaction. But at the time, nobody knew what could be the period for this set of objects, so in some sense, it was a chance for us because we had adapted a rhythm of measurement to either kind of period. Then it was a chance because we discovered an object with the period of only four days. If we had started only from the theory, the period of a planet would be more like 10 years. We would have done quite sparse sampling of measurements. The fact that we had open eyes created a chance for us. |
| Q2 | **How do you deal with doubts in science?** |
|  | Michel Mayor: Part of this doubt is a normal discussion in science. When you have new facts, I believe you have to be sure to analyze if it’s really strong. You have to remember that the first planet is a giant planet, like Jupiter, but with an orbital period of only 4.2 days. The theory at that time said that we cannot have a giant planet with a period shorter than ten years, because you need to agglomerate an ice particle and an ice particle does not exist close to a star. So it was a discovery by factor 1,000 with a theory. I believe it’s evident a normal discussion in science when you have a so big discrepancy with the theory, to be a little bit cautious.  This period for me was not so stressful, it’s not true. I believe it was superior to say, “What could be the other explanation? Is it a rotating star with magnetic activity? Is it a pulsation? For me it was only part of the normal discussion in science. You have something you need to understand, what could it be? Then after we had decided to postpone the announcement by one year, to wait to the next season, to repeat the measurement, to check if everything was still there – the same period, the same amplitude, the same phase and so on. It was only the first week of July, the only hypothesis to explain this is a companion, a low mass companion, plant three domain. So, we started to write the paper. |
| Q9 | **How did you discover you had been awarded the Nobel Prize?** |
|  | Michel Mayor: I knew that many years ago I had been nominated for the Nobel Prize. But you know, I was not the only one. Several other people were also nominated, so I wasn’t surprised to not receive anything. I decided with my family that we didn’t mind all this, so we took vacation in October. We were taking professional commitment and things like this. This was the reason why we were in Spain and I was absolutely not looking or waiting for that. Then just before the announcement by professor Hansson we were leaving San Sebastián for the airport and I connected my computer. Suddenly I saw the beginning of the press conference at the academy and the first word was something like, “This this year, the prize will be given to three people having contributed to a better knowledge of the cosmos.” I said, “Oh, interesting domain”, and then after I heard the three names and then it was time to leave for the airport and then we tried to have a connection with the Nobel Foundation. |
| Q12 | **How has your family supported you personally or professionally?** |
|  | Michel Mayor: The life of an astronomer, going frequently to observe in many places in Europe and in addition to scientific conferences, it will be many months away from the family. Certainly when you have small children all of the burden of this situation was on the shoulders of my wife. From time to time we have been lucky enough to have the possibility to move all the family in south of France or later on in Chile. But nevertheless I had the huge support of my wife and three children working later on in science. Probably science was not too badly considered by my children, if they’ll have children, to continued, not in astrophysics, but in some domain of science. |
| Q8 | **What do you do in your free time?** |
|  | Michel Mayor: My hobby, when I was young enough, I really loved climbing – high altitude sports, rock climbing, high altitude skiing and things like this. Evidently today, I like to continue hiking but no more climbing. But I still like to do downhill skiing, cross country skiing and things like that. I feel that when you are hiking, you have time to think on new ideas. I hope you spend some time just to admire the countryside, but also, I believe it’s the activity to walk. It’s very nice to have time to think, you have no e-mails, you have nothing. It’s good. I believe this is an important part of this kind of activity. |
| Q3 | **What fascinates you about the universe?** |
|  | Michel Mayor: If you are in a dark place, on a /- – -/ or the top of a mountain and you’re looking at the sky, it’s difficult to escape good questions. Still today, I’m really fond of astrophysics.  Like many people in the past, you ask, “What is this?” This is a good aspect of astrophysics. You always have the possibility, if you are away from light of dawn, to ask this kind of question. For example, when you are in Chile, the center of the Milky Way at some moment, it is just at the zenith of where you are observing. You have the Milky Way here, it’s absolutely fantastic to see this, because it’s much brighter than in the northern sky. |
| Q14 | **How long do you think it will be before we detect life beyond Earth?** |
|  | Michel Mayor: During the last 20-25 years, we have been lucky to explore all the diversity of the planetary system. Already today many colleagues are working on this specific new domain. Do we have such capabilities and possibilities to detect life? I’m quite sure that we know how to detect life. But we still do not have the good instrumentation to do it, but we have the path of how to do it. It’s a matter of years, money and maybe to have new, better ideas. I would say in one generation, it’s not for next year. It’s very difficult, but I’m quite confident. I don’t know the answer, but we will have the instrumentation to search and it’s good, it’s a good question. In science, it’s absolutely fantastic to still have extremely huge questions not solved. Cosmology, black holes, life in the universe, these are not details, so for young people, I believe it’s so nice to have huge questions still facing us. |
| Q14 | **Could we live on these other exoplanets that have been found?** |
|  | Michel Mayor: The question of the possibility for humanity to move to an extrasolar planet is a very important question for me. I’d like to stop this kind of dream. If you consider that man went to the moon in three days, light needs one second. Let’s consider that a very optimistic possibility is to have maybe one good planet. Good in the sense that we can have good conditions at 30 light years. 30 light years is really our neighbour. Nevertheless, approximately one billion light years, one millionth second for the light to go. One billion times three days is completely crazy, that’s millions of years. So, there’s no way to go. Some people say we can accelerate the spacecraft, no, the energy to accelerate is impossible. Our planet is alive, there is not another one. We don’t have the possibility to immigrate. I’m not discussing the solar system, but extrasolar planets are not a possibility to immigrate for humanity. And Earth is so beautiful – why? |
| Q4 | **How special is Earth?** |
|  | Michel Mayor: We are lacking the experience to compare another planet to ours. The only things we can say is that we have 200 billion stars in the Milky Way. If only one planetary system among 100 is convenient for a rocky planet it is still a big number. Let’s imagine how many planets in the universe are convenient for life. This is the next big question to search for. Not for very distant planets, but to analyse if life exists on much closer planets – and it is not going to these planets but just analyzing the luminosity and the spectra of these planets. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0017** |
| **Biographical** | Didier Queloz is a professor of physics at the University of Cambridge’s Cavendish Laboratory and professor of astronomy at the University of Geneva (part time). He is one of the originators of the “exoplanet revolution” in astrophysics. In 1995, as part of his PhD, he and his supervisor announced the first discovery of a giant planet orbiting another star, outside the solar system1. The planet was detected by the measurement of small periodic changes in stellar radial velocity produced by the orbiting planet. Detecting this small variability using the Doppler effect was possible thanks to the development of a new type of spectrograph combining stability, high resolution and a creative approach to measure precise stellar radial velocity2. For his work he was awarded, with Michel Mayor, the 2011 BBVA Foundation Frontiers of Knowledge Award of Basic Sciences for “developing new astronomical instruments and experimental techniques that led to the first observation of planets outside the Solar System.”  This seminal discovery spawned a revolution in astronomy and kickstarted the research field of exoplanet systems. Over the next 25 years, Didier Queloz’s main scientific contributions have essentially focused on expanding the detection and measurement capabilities of these systems to retrieve information on their physical structure. The goal is to better understand their formation and evolution by comparison with our solar system. In the course of his career, he has developed new astronomical equipment, novel observational approaches and detection algorithms. He has participated in and conducted programs leading to the detection of hundreds of planets, include breakthrough results.  Early in his career, he identified stellar activity as a potential limitation for planet detection. He published a reference paper6 describing how to disentangle stellar activity from a planetary signal using proxies, including new algorithms that have become standard practice in all planet publications based of precise Doppler spectroscopy data. His teami and his Geneva colleaguesii established standards to optimise measurements of stellar radial velocity that are still in use today.5  Shortly after the start of the ELODIE planet survey at the Haute-Provence Observatory (OHP), he led the installation of an improved version (CORALIE), on the Swiss Euler telescope in Chile. Very quickly this new facility started to detect exoplanets on stars visible in the southern hemisphere.4 In 2000 he took responsibility, as project scientist, for the development of HARPS7, a new type of spectrograph for the European Southern Observatory (ESO) 3.6m telescope. This instrument, commissioned in 2003, was to set a new standard in the business of precise Doppler spectroscopy8. HARPS’ performance, allied with the development of new analysis software inherited from data gathered by ELODIE and CORALIE, would considerably improve the precision of the Doppler technique. Eventually it would deliver spectacular detections of smaller exoplanets in the realm of Neptune, super-Earth systems10,11 before NASA’s Kepler space telescope would massively detect them and establish their statistical occurrence.  After the announcement of the detection of the first transiting planet (in 1999), Didier Queloz’s research interests broadened, with the objective to combine capabilities offered by transiting planets and follow-up Doppler spectroscopy measurements. In 2000 he achieved the first spectroscopic transit detection of an exoplanet using the so-called Rossiter-McLaughlin effect.3 This type of measurement essentially tells us about the projected angle between the stellar angular momentum vector and the planet orbital angular momentum vector. The pinnacle of this program would be reached 10 years later, after he led a significant upgrade of CORALIE, and established collaboration with the Wide Angle Search for Planets (WASP) consortium in the UK. With his PhD studentiii he demonstrated that a significant number of planets were surprisingly misaligned or on retrograde orbits, providing new insights about their formation process. 2017 he received the 2017 Wolf Prize in Physics for his “seminal work on revealing an incredible diversity of exoplanets and his contribution to the discovery of more than 250 additional exoplanets, including several multi-planetary systems and measurement of the first Rossiter-McLaughlin effect for a transiting planet, which allowed the measurement of the projected angle between the stellar spin axis and the planets orbital axis […]”  The special geometry of transiting planets, combined with precise Doppler spectroscopic observations, allows us to measure the mass and radius of planets and to compute their bulk densities in order to gain insights about their physical structure. In 2003, Didier Queloz − recently appointed to a faculty position − with his teamiv pioneered and established the combination of these techniques by first measuring bulk density of OGLE transiting planets9. They also looked for transit opportunities on known radial velocity planets and they found the first transiting Neptune-sized planet (GJ436b).13 In the course of this program and in collaboration with his colleague Shay Zucker from Tel-Aviv University, he developed the mathematical foundation to compute residual noise they encountered during the analysis of transit they were trying to model. They established a statistical metric to evaluate “red noise.”12 Today this concept is widely used in the field to estimate systematics in light-curves and transit modeling.  In 2007 Didier Queloz became an associate professor. Over the next 5 years following his appointment, his research program − based on a combination of spectroscopy and transit detection − intensified. He took the lead in the spectroscopic follow-up effort of the WASP consortium and the Eureopean Space Administration’s Corot space mission.v The combination of WASP and Corot data with follow-up observations using Euler-Cam (a CCD imager he developed), CORALIE, HARPS and other main ESO facilities was amazingly successful. It led to more than 100 publications, some of them breakthroughs providing us with new insights on formation and nature of hot Jupiter-type planets. Further, in the same period, the detection of COROT-7b − combined with intensive follow-up work − established the first detection of a planet with a bulk density similar to a rocky planet.14  All the follow-up expertise he developed naturally extended to the Kepler mission era, with the HARPS-N consortium confirming the Earth-like bulk density of Kepler10.16 In ground-based transit programs, Didier Queloz was deeply involved in the design and installation of a new generation survey telescope: the Next Generation Transit Survey (NGTS) observatory. His role was decisive during system tests in Europe and in establishing the facility at Paranal, Chile.20  At the time Didier Queloz moved to Cambridge, he essentially focused on setting up comprehensive research activities directed to the detection of Earth-like planets and life in the universe and to the further development of the exoplanet community in the UK. When he left Switzerland, he was co-directing a major national initiativevi which eventually got funded. At Cambridge, with the help of his colleagues at the Institute of Astronomy (IoA) and the Department of Applied Mathematics and Theoretical Physics (DAMTP), he established the Cambridge Exoplanet Research Centrevii to stimulate joint coordinated efforts and collaboration between departments. In the UK he organized the first “exoplanet comunity meeting” and installed the idea of a regular yearly community workshop. In the European context, he is leading at Geneva (through his joint professorial appointment) the development of the ground segment of the CHaracterizing ExOPlanet Satellite (CHEOPS)viii space mission and he chairs the science team.  His most recent research highlights are related to the search for transing Earth-like planets near low-mass stars and for extraterrestrial life. This program, carried out in collaboration with Michael Gillon of the University of Liège, is the origin of the detection of Trappist-118, a planetary system of potential interest to further searches for atmospheres and signs of life. Another successful avenue of research is the characterization of the rocky surface or atmosphere of hot small planets, including the work on 55 Cnc.17 The recent extension of this program towards extraterrestrial life is being carried out in the context of an international research initiative supported by the Simons Foundation. One major result of this collaboration is the definition – combining chemistry and astrophysical constraints – of minimum conditions for the origins of RNA precursors on exoplanets (“abiogenesis zone”).19  Discoveries of exoplanets attract a lot of attention from the public and the media. In parallel with his research and teaching activities, Didier Queloz has been involved in numerous documentary films, has written articles and has done TV and radio interviews to share his excitement, explain his findings and promote a general interest in science. |
| **Autobiography** |  |
| **Podcast** | **0017=Queloz**  No script |
| **Telephone**  **interview** | **00017 = DQ**  Didier Queloz: Didier, yes.  Adam Smith: Hi, my name’s Adam Smith. I’m calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  DQ: Oh, hello.  AS: Hello.  DQ: Yes, I’m in the car, is that fine?  AS: That’s fine. First of all, many congratulations.  DQ: Thank you very much.  AS: I know lots of people have been searching for you. How did you actually hear the news?  DQ: It came up as a complete surprise, because I got a call from the press office of Cambridge and I was in the middle of a scientific meeting with colleagues, and then I stopped breathing … [Laughs]  AS: [Laughs] And you’re breathing now or only barely?  DQ: I’m still shaking a lot, I must say. The good stuff is that since I was in a scientific meeting, I mean, that we had already a scientific celebration with my colleagues, which I think was exactly what I … was appropriate and what I needed at that time. I mean it’s so big, I mean it’s so emotional, like a kind of a wave. It came up so much as a surprise to me, so it’s just unbelievable. I’m still completely stunned by the news. It’s just an invaluable and fantastic news for this field of research which is now growing so fast, about dealing with exoplanets and life in the universe. I’m so glad, I mean I’m so glad the Nobel Prize committee for this. It’s just amazing.  AS: It’s 24 years and 2 days since you announced the first exoplanet in Florence at that meeting. Does today’s excitement rival the excitement you felt when you saw that first evidence?  DQ: Absolutely, there’s something amazing in the field. The excitement for the field has never decreased from the beginning. And the emotions to get award to the Nobel Prize for that 25 years after, in a way it’s just the acknowledgement of the excitement of what we have been doing, and definitely the emotion that I’m having right now is kind of reward of all this work that we’ve been doing. I mean we have to remember that at the beginning, I mean most of the people were very sceptical about all these discoveries so it is just this … it’s a climax of the story right now.  And I’m still trying to understand exactly what’s going on frankly.  AS: It’s a lovely validation of the whole field, yes indeed. And you found that planet as your PhD project; I mean most people would be happy with just a Nature paper, but now you have a Nobel Prize from your PhD as well.  DQ: Yes, indeed, you know I’ve been talking a lot with Michel Mayor, my supervisor in these days, and Michel always say something very nice to me; He said “I mean the quality of the work and the creativity, I mean it’s not related to the age of the people.” So I feel pretty easy with that, I mean I don’t really care and anyway in my mind I still feel like a PhD student when I’m doing research. So I’m fine with that.  AS: How lovely. There’s enough excitement around already, but we now know of more than 4,000 exoplanets and I think they’re being discovered all the time. What are you most excited about currently in the field?  DQ: I agree we know a lot of them, but we’ve still so much to learn. I mean, they’re very little planets that look like the Earth right now and we’re just barely trying to understand the planetary formation as a whole. And tomorrow there will be people addressing the question of the possible atmosphere content and evolution of the atmosphere, and one day, eventually, we’ll be talking about life. And all this is as much exciting as detecting the first planet. And the detection of the first planet was the trigger of all this, and that’s … I guess that’s what the Nobel Prize is acknowledging right now.  AS: Indeed. It’s such a joy to speak to you, thank you. And I look forward to speaking more when you come to Stockholm in December.  DQ: Yeah, sure, with pleasure.  AS: Thank you so much.  DQ: Okay, thank you, have a good day.  AS: You too, bye.  DQ: Bye. |
| **Interview** |  |
| Q3 | **Where do you get your passion for science?** |
|  | Didier Queloz: I think my passion for science may be something that I had in the blood early on, because I think I was born very curious. I’ve always been very curious. I believe I’ve always been very creative. I love to play and doing stuff and always very curious about understanding ‒ how does it work? When I was a kid, I used to break apart stuff just to understand how it works. Of course I was not able to bring it back. It was not something very nice sometimes for my mother. But I think I’m a born scientist, I’m very curious and very math gifted, so I went naturally into physics because that’s something I feel at ease and that’s something that was responding to my internal voice that maybe that’s something I should be doing. |
| Q3 | **What do you like about science?** |
|  | Didier Queloz: I can never stop asking myself, ”Why is that?” or ”How does it work?” and ”Why did we do that and not that?” I’m also curious about … It’s not about physics only, it can be about psychological patterns, about the way society works or the way the financial system works. I think I’m very eclectic, and it’s kind of a soft madness in a way that I always want to answer and obviously I very rarely got any answers, but I really want to answer. That does cause this kind of emotional drive or maybe even a bit obsessive drive that brought me to science. |
| Q5 | **Was there a certain person that influenced you growing up?** |
|  | Didier Queloz: I cannot identify a specific person that had a specific influence, but I have had a lot of people that may have played some role. First, I have a very supportive family, so I think this is essential. I don’t think if you are under stress and under threat in your family, it’s not possible to do that, you have to get a free mind. I can really do what I want, which is what I wanted, which was certainly a good starting point. I have had very interesting teachers sometimes because it was the seventies, new teaching in the seventies, a new PhD theory, so they were kind of funny teachers. Maybe it helped to develop some creativity in me. I cannot really say I have had amazing physics teachers, but I remember very well my mathematic teachers and I liked them very much, I mean they really brought me to mathematics and then I went to physics. I realised that there were a lot of cool topics that I didn’t know. I read a lot of books, some of the almost public oriented writers like Carl Sagan for example, was one of them. I got very thrilled by this and all this together was a nice music to my ear. They all together brought me to to become a physicist and then working on astrophysics. |
| Q2 | **What was it like to work with your co-laureate Michel Mayor?** |
|  | Didier Queloz: That’s the interesting aspect of working with Michel. I think I picked Michel because I think we are very similar in the way we operate, we’re very instinctive. We are kind of emotional and maybe we can say Michel is not very academic in that style, so it’s all by touch. It’s a bit like doing paintings. I cannot say something specific, but the whole touch paintings that you get by working with Michel ‒ bits here and bits there ‒ I think was amazingly useful. Still today, I just love being with Michel. I love listening to his perspective on things. He sometimes has an awkward perspective and I like it. I think we all need what I call a mentor, an inspiration. Michel was certainly a mentor to me. I learned a lot and he helped me at least to develop some skills, not directly but just by being with him and maybe having some discussions about something, and then they would help me to think in a different way and to develop other skills. I must say I’ve been very fortunate to work with Michel and I hope the same for him because I think the discovery was an amazing teamwork. |
| Q1 | **What advice do you give to your students?** |
|  | Didier Queloz: It’s always difficult to give advice to students, but usually the way I interact with my students is that I try to do what’s called “light touch supervision”. This means you are always there when they really feel under stress, because that’s why we are here really, to do a net. But it’s also up to them to find a way. You can show the way, we can suggest the way, but very quickly I try to promote this kind of spirit, let’s say an artistic spirit, that they try their own way to go through with what they’re doing. In a bit of a provocative way, if I try to translate that it’s, don’t listen to your supervisor, in a way. Do it as you feel. Of course, you should not apply that strictly speaking, but I think it’s part of this kind of light touch. That helps to develop confidence in yourself, that you can do something without being told that you can do it. It also helps you to listen and to try to grab the advice that is the one that fits you the most. I think to me that’s a perfect interaction you should have between a student and a supervisor, and that’s something that all students have to understand, they should really develop their own perspective on things. They should not be afraid to build confidence. Quite often I’m facing a lack of confidence. The confidence is a matter of our self respect and also self build up, and the best way to build up confidence is just to do the things, so do the thing. Don’t listen too much to your supervisor, listen to your guts. |
| Q5 | **What do your students teach you?** |
|  | Didier Queloz: The reason why I love working with students is that every day they teach me something. First, their brain is much more fresh than mine. They come with a very fresh perspective and sometimes bring new ideas. I got too spoiled because I know too much, which is bad if you want to be creative at some point. But they come with so much energy that is so wonderful. I just love working with students, PhD students and working teams. I think that makes my day really happy. |
| Q14 | **You received a Nobel Prize for your PhD. How can someone follow in your footsteps?** |
|  | Didier Queloz: I was a PhD student, that’s true, but you know, in all discovery it’s a matter of luck, it’s a bit of a gamble. We have been lucky. We could have been scooped, other people could have found this before us. We have been lucky, we work right. I think the fact that I was a PhD student certainly played some role because I came up with maybe a fresh perspective. In another way, maybe that Michel would’ve done it, so this kind of teamwork that we had with someone way more experienced and somebody way more energetic and maybe more naive and creative in a way, was a terrific team. But there’s no rule, just do what you think.  When I was doing that, I never thought about a Nobel Prize at all. I just thought it was cool, what I was doing, because I just loved it. I was doing something new. I was pushing the boundary of the knowledge, and it was great because we were doing this work. We were excited about what we were talking and discovering and announcing to the world, but at no point, I thought I would build a career and have a Nobel Prize. So I think it doesn’t really matter if you’re a PhD or if you are a junior scientist, just do it because you believe you have to do it and just have fun when you’re doing it. |
| Q9 | **How did you discover that you had been awarded the Nobel Prize?** |
|  | Didier Queloz: Oh, my day was really awful. I had a problem with my bike and I had to fix my bike and I was late and the day was starting really on the wrong foot. Then it turned completely in another direction after this phone call. Not from the Nobel committee, actually, they failed to reach me, but by the PR office of Cambridge, because I was at Cambridge, they called me and told me after the announcement, because I was not at all looking at that, I was in the middle of a meeting. I was really … The Nobel Prize was the last of my worries, and they told me, “Are you aware that you got the Nobel Prize?” At first I thought it was a joke, and then I realised it was the 8th of October and I said, “Are you sure?” I was not so convinced, in the way I asked them, “Are you sure?” For a moment, they said, “Oh, just wait for a minute, we’re checking,” and they came back. “Oh yes, that’s you, we see your face.” “Okay,” I said, “Oh, my god”, then I lost track of the day. They asked me what I would do with the prize, and I was so annoyed with this bike, I said, “I’m going to buy a new bike”. That definitely is in the plan, so I’m looking for buying a new bike. |
| Q6 | **Is there a Nobel Laureate that has influenced you?** |
|  | Didier Queloz: This is a difficult question, see if there is any other past Nobel laureates. When you look at the list, there is so many great figures, so many great physicists. I cannot identify some specific one, but there are certainly a lot of great names that, when I was a student in physics, I learned the history and I learned what they did. Certainly the Nobel laureates is a good pool to pick a great lot of people from, but I don’t have a specific name to tell you right now. |
| Q9 | **How does it feel to be a Nobel Laureate?** |
|  | Didier Queloz: I’m still learning what does it mean right now, to become a Nobel Laureate. It seems so extravagant right now, what is going around this. I don’t really know, I just saw the first effect. Everybody seems to be interested by me and they all want my signature, that’s what I see right now. I’m waiting a little bit for all this to go down because I don’t feel that very different. I’m still the same person. I don’t think my science will be very different. But certainly I do feel a sense of responsibility with this prize, and it will take a bit of time for me to digest absolutely everything, what it means and what I can do with this prize. |
| Q8 | **What do you do in your free time?** |
|  | Didier Queloz: I don’t have a lot of free time, that’s my problem. The time I have left I try to dedicate to my family and to my wife. We love just taking care of the house, doing gardening and when we can, traveling a little bit. I save a bit of time to go in the mountains because I love to ski, so to do skiing. We are trying to save a bit more time to just go to a nice place when we can, just go to the beach and enjoy, a bit more relaxing time. That’s really optimizing the few time I have. Most of my time, it’s absolutely obsessive about science, it’s just a disaster, it’s just so obsessed. |
| Q1 | **Why should young people go into science?** |
|  | Didier Queloz: I think people that have an interest for questions and like the rational reasoning and even if you are gifted for logical thinking and mathematics, which is not everybody, but if you are gifted, you have to do science because that’s where you will develop all these gifts the best you can. Science is essential to the society, it’s everywhere, all the science. Our society where we’re living is the outcome of science. Your cell phone is full of science. I think we need scientists and what we need the most is not only scientists, it’s scientists in the government making policies. Because when you look in the parliament, when you look in the governments, there is an absolute lack of scientists compared to the weight of science in our society. I really encourage people to do science, and if at some point they decide to become prime minister or president or parliament members, they should do it, because we need these people there to rebalance a bit of society. Really, and please do it, and please, please, please, if you are a woman, don’t feel science is not for you. This is all wrong. I mean, science is for everybody. We need all the bright men and we need you. We are missing women right now because we are missing certainly gifted women for science. For some social and family pressure, they don’t go into science, which is an absolute disaster. I think they should really join it and try. Diversity in science is essential to the science. When you look back, science is about traveling, freedom of travel, freedom of expressions and getting the best people in the world. There is a limited number of people gifted. They’re all around the world and we need to have them. If diversity is not represented in science, it means we’re losing capability, we’re losing people, we’re losing brains. |
| Q14 | **Do you think there’s life out there in our universe? How long will it take to find it?** |
|  | Didier Queloz: We have so many planets that the question of life is obvious, that’s the next step. I’m absolutely convinced there must be life somewhere else because I can’t believe life is unique in the universe. There’s too many stars, too many planets; that’s pretty clear. Now what is still not very well understood is what is the origin of life? And what you need as ingredients? Life is chemistry. In a way it’s a kind of chemistry that is turning bad because the chemistry that leads to something that is evolving and then by itself, evolving to something more complicated. Now the question is, what do you need? We definitely can look at this in the lab, here on earth, and we can look at this on Mars, on Venus and Enceladus, there’s couple of other satellites in the solar system where you can look at that. Then you can try to look on other planets.  The question of what exactly do you look at is not very clear, but it is believed that at some point the geophysics of a planet should tell you something about whether there is life or not. It’s a long way, it’s a long path because we don’t even connect exactly the origin of life on the solar system. It’s something I’ve started spending quite a lot of time on. I started this project a couple of years ago because that was my main focus. When I moved to Cambridge I was trying to establish a new path here, and there has been progress. The interesting part of this progress, is that it doesn’t come from only astrophysics. It comes by combining what the astrophysicists are doing, with what other people not really dealing very much with stars, because they’re dealing with molecules in labs. It’s called molecular biology.  There has been a lot of progress on the origin of life from the molecular biology perspective, the ingredients, the unit, what is the simplest unit and how do you build them? What is the condition for that? Then the condition is something that the astronomers can provide. We’re trying to get an idea, what is the atmosphere about, what is the kind of planet we’re talking about? Do we have plate tectonics? Do you have magnetic field – all this does it. What is the role of all this to initiate the chemistry, to have the chemistry right? That’s a new field that I’m talking about, which is at the boundary between different fields, that connect different disciplines together. This is my next big goal. I’m not claiming I will find the answers to that, because these are big questions, but I will definitely work on this, I am working on that already.  The most obvious element that we are missing in the /- – -/ right now is to find another earth like us. We have failed right now because we have not been able to deploy the right equipment to do that. We know how to do it, we just need to implement it. I have built up an international program on that called Terra hunting. A lot of universities in the world are having new equipment being built, and we will dedicate a quest for that kind of object, a telescope doing only that, and there may be others in the future. Then we have to build up the equipment to analyse the atmosphere of these planets. There will be a next generation of space equipment and people are now talking about this. This is really my main activity where I’m moving right now. I’m trying to accrete people, young people on this, and hopefully this Nobel Prize will be helpful for that, to stimulate these new developments.  People are convinced that this is what is coming next, but it’s difficult to implement it because since we are building a new science it doesn’t really fit in the usual box. When you have a usual activity, it’s easy, but when you do something really new, like life questions, so, there must be life somewhere. We are going to make progress on the origin of life on earth and possibly to find life that could be like the life we have on earth or another kind of life, other kinds of chemistry certainly within the next 50 to 100 years. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0018** |
| **Biographical** | Arthur Ashkin’s father, Isadore Ashkenazi, was born in Tsarist Russia in 1891. Isadore was brought up with his older brother, Harry, both shown in Figure 1, in an orphan asylum and never told of the circumstances of how they ended up there. As it turned out, his brother made the mistake of not leaving Russia by the age of 18. That meant that he was subject to army service, which at that time was akin to a death sentence or prison. The solution to this problem was to use the passport of his younger brother and leave immediately, in 1910. After getting to America he could simply mail the passport back to Isadore for his use in leaving Russia. A serious problem arose when the passport never arrived. Isadore was now stateless, but he found a man who, in exchange of his accumulated savings, would arrange passage for him on a sealed train to Holland, and then steerage on a boat headed to New York. Despite his doubts, the arrangement worked out and Isadore found himself on Ellis Island, greeted by his brother, and accepted in the country. An agent changed his name to Ashkin to make it sound more American.  Since Isadore was a “good boy” in the orphanage, he was trained as a dental technician. Once in the United States he found employment and soon opened his own business, which flourished.  He was soon able to marry Anna Fishman, also an immigrant, born in 1895, who arrived at 2 years of age with her family from what is now Poland. They can be seen in Figure 2 as a young couple at Coney Island. They found lodgings in Brooklyn and soon had a family: a boy named Julius, a second son, two years younger, named Arthur, and a daughter named Ruth, five years younger. Anna and her three children can be seen in Figure 3, a photo taken in the late 1960s. Julius was a brilliant student, skipping several grades in elementary school and eventually earning a PhD in Physics from Columbia University at age 22. During World War II he worked on the atom bomb at Los Alamos and after the war taught at Rochester University and Carnegie Tech. He eventually became the head of the physics department at Carnegie Tech.  Their daughter, Ruth, Arthur’s younger sister, majored in Greek and Latin, and a number of years later she earned a master’s degree in Greek. However, she became a school teacher and devoted herself to teaching underprivileged children in a Brooklyn elementary school.  Ashkin is the product of Brooklyn’s public school system during the Great Depression. He showed an early interest in science. As a ten year old he was fascinated by the Crookes radiometer. Seeing one in a pharmacist’s window, he soon had one of his own. He understood the thermal effect of light causing molecules of air, under low pressure, to cause the movement of the vanes forward from the blackened side. A few years later, after exposing the radiometer outside for some time to make it hotter, he brought it back inside the house. It started to spin in the opposite direction, the same direction as if it was driven by light pressure.  In 1940 he enrolled at Columbia College with a major in physics, but his education was soon interrupted by the entry of the United States in World War II. He was drafted into the army but spent the war years as a technician at Columbia University’s Radiation Lab, where he built magnetrons for radar. While at the Radiation Lab he was mentored by Sid Millman, who later became a department head at Bell Labs. After the war he completed his bachelor’s degree and earned his Phi Beta Kappa key in his junior year. As a graduate student at Cornell University under the G. I. Bill, he earned his PhD doing electron-positron scattering under Professor Bill Woodward.  After getting his PhD, Arthur Ashkin was remembered by Millman at Bell Labs, who hired him. At Bell he initially worked on electron tubes for communication. He felt working in nuclear physics would put him in competition with his brother. He resented being called “Ashkin’s brother Ashkin” by his brothers’ colleagues. He switched to laser work in the early sixties. He had long been interested in how the momentum of light photons could be used to move matter. He formed the two-beam laser trap early on and in 1970 published his first paper on optical trapping.1 The subject of the paper was the two-beam trap, but it also spoke of the possibility of trapping atoms, molecules and small particles. As a manuscript to be submitted to *Phys. Rev. Lett.,*it had to be reviewed by the Theoretic Physics Department in order not to besmirch the Lab’s high reputation. It was rejected on three grounds:   1. There was nothing new; 2. There was nothing wrong with the work – a comment reminiscent of the “Pauli insult”\*; 3. It could be published somewhere, but not in *Phys, Rev.*   Ashkin’s boss, Rudi Kompfner, was irate and simply said, “Damn it! Just send it in.” It was immediately accepted. It was published and became the most referenced paper of that year. At over 5,000 citations it was one of the one hundred best atomic physics papers of the last century, as named in “The Physical Review the First Hundred Years, a Selection of Seminal Papers and Commentaries.” In 1971, Ashkin achieved levitation of small glass spheres, which were used in many important experiments, such as the optical [Millikan](https://www.nobelprize.org/prizes/physics/1923/millikan/facts/) oil droplet experiment. Finally, he made his famous single beam tweezer trap 2 in 1983. He can be seen near the optical tweezer apparatus in Figure 4, a photo which was taken soon after the discovery for which he would be awarded the Nobel Prize.  His work has been both widely used and widely recognized. He is a member of the National Academy of Engineering and the National Academy of Sciences, as well and the National Inventors Hall of Fame. He has been elected Fellow of the APS, the OSA, the IEEE, and the AAAS. He has received numerous awards, among them the Harvey Prize, the Rank Prize, the Keithly Prize, the Townes Award, and the Ives Prize. His most recent award is the 2018 Nobel Prize in Physics for Optical Tweezers for Application to Biological Systems. At 96 he is the oldest recipient of the Nobel Prize. It is generally recognized that he should have been included in the 1997 award, since optical tweezers were used to trap atoms in the prize-awarding experiments. After difficulties with atom trapping, Ashkin and Dziedzic trapped 20 Angstrom plastic spheres to demonstrate the ability of optical tweezers to trap particle of approximatively atomic size. Subsequently, successful atom trapping using the same technique was demonstrated.  Art Ashkin and his wife, Aline, have been happily married for almost 65 years. They met at Cornell, on the shore of Lake Cayuga. She was on a Cornell Outing Club excursion and he was occupied with his prospector’s pick, splitting rocks of shale looking for fossils. She was curious about what he was doing. A conversation was struck up and they both realized that something special was happening. He asked for her phone number and she willingly gave it to him. She was a junior at Cornell, majoring in chemistry and he was finishing his PhD in nuclear physics. Two years later, after she graduated in 1954, they were married. They settle in Bernardsville, New Jersey and had three children. When Bell Labs opened a new laboratory in Holmdel, New Jersey, they moved to Rumson, New Jersey, where they have been living for the past 52 years. Once the children were all off to school, Aline started working as a high school chemistry teacher. She pursued that for fifteen years, obtaining a master’s degree in science education along the way.  Their children did well. The oldest, Michael, and the youngest, Daniel, both went to the University of Pennsylvania. Michael’s major was in Hebrew and Arabic; Daniel’s was in Earth Science and Geology. Their daughter, Judith, went to Cornell to major in Spanish and Portuguese.  Today, Michael is chair and professor of art in Cornell’s School of Art, Architecture, and Planning. Judith teaches Tai-Chi and recently completed a Master’s Degree in Acupuncture. Daniel, with a PhD in Ceramic Science from Rutgers University works on ceramics for CoorsTek, a company run by the same family who also make Coors beer.  Art and Aline are the sole survivors of their families. The parents of both are deceased as are their siblings. His brother and sister both died in their early sixties, as did her brother. Art and Aline are still alive, enjoying their five grandchildren and two great grandchildren. A recent photo of Aline and Art (left) is shown in Figure 5. It was taken on October 9, 2018, at the top of Crawford Hill in Holmdel, New Jersey, part of the laboratory of the same name at Bell Labs. They are in company of [Bob Wilson](https://www.nobelprize.org/prizes/physics/1978/wilson/facts/) and his wife Betsy (right). Bob who also spent his career at Bell Labs, did the work for which he was awarded the 1978 Nobel Prize in Physics on that same hill. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0018 = AA**  Arthur Ashkin: Hello.  Adam Smith: Good morning.  AA: Yeah, who is this?  AS: Well my name is Adam Smith, and I am calling from Nobelprize.org, which is the website of the Nobel Prize in Stockholm, Sweden.  AA: OK.  AS: First of all, many congratulations on the award of the Nobel Prize.  AA: OK, thank you.  AS: May I ask how it feels to be the oldest ever awarded Laureate?  AA: [Laughs] I didn’t realise I’m the oldest ever! So I just about made it, huh? Because you can’t be dead and win … If you’re a winner of the National Inventors Hall of Fame you can be dead. I won that prize a couple of years ago and I was very proud of that. That is the most, I would say, most important prize I’ve won.  AS: May I ask, are you still experimenting in your home lab?  AA: I am. In fact I’m writing a paper now that you guys are disturbing in my … I’m going to send it into Science and hope that they’ll accept it.  AS: Well I guess that’s the secret of a successful research career, to concentrate on work rather than distractions.  AA: Well that’s my hobby, more or less. I was interested in science since I was a kid, so I tell my wife that’s the only thing that I’m really good at.  AS: And presumably you’re amazed and delighted to see the variety of applications that optical tweezers have been put to?  AA: Well, I anticipated that it was pretty important from the day go. The thing that I … well one of my heroes is this guy, this Dutchman Antonie van Leeuwenhoek, and he’s the guy who discovered animalcules. And he kept writing to the Royal Society telling them and sending them pictures, and nobody paid any attention to him until very much later they wanted a sample of his microscope and he said no, he said if you want one you have to do it yourself. He’s a hero, and there are other guys like Michael Faraday, who’s another one of my heroes.  AS: It’s good to have scientific heroes. So you knew when you developed them that you were developing the instrument which would allow you to probe molecular processes?  AA: Yes. Well look, I was interested in trapping molecules with light a long time ago. My famous paper in 1970 – that’s the most famous paper I ever wrote. In that I mention [unclear] molecules with light. But I never thought living things – light was supposed to kill tissue. They used light to heal wounds and it was considered to be deadly. That was very much a surprise. That was a big surprise and once I … look, well I should, I’ve got a lot of old stories to tell about what happened. When I described catching living things with light people said, ‘Don’t exaggerate Ashkin’!  AS: You certainly weren’t exaggerating. How exciting. Well, we look forward to hearing some more of these stories. Will you be coming to Stockholm in December to receive the …  AA: I’ll come if I… if I can.  AS: Sure. Are you going to celebrate?  AA: Well, look, I’m writing a paper now and I’m not celebrating about old stuff. I’ve got something new and important. I’m working on solar energy and I think I’ve gotten some important stuff. And the world badly needs science in climate change.  AS: Indeed.  AA: OK?  AS: Thank you very much indeed, that’s an important message. I very much appreciate you talking to me. Thank you and, once again, many congratulations.  AA: OK, you’re welcome.  AS: Thank you. Bye bye.  AS: OK, bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0019** |
| **Biographical** | Yesterday i had the honour of drinking coffee with Madam [Joliot-Curie](https://www.nobelprize.org/prizes/chemistry/1935/joliot-curie/facts/)’. Those were the words of my father, who was an engineer in charge of the electricity network of The Three Valleys in Savoy. He went on to say that she was a very important personality who had received the Nobel Prize. I was about five years old and it was the first time that I had heard the Nobel mentioned. The rapid electrification of France after the war made it necessary for my father to travel. I was born in 1944 in Albertville, Savoy, with my two sisters, Jacqueline and Michèle. A few years later we moved to Moûtiers, the gateway to the Tarentaise and The Three Valleys, with its well-known skiing resorts, Courchevel, La Plagne and Val d’Isère, etc … I keep in mind an unforgettable memory of the town of Moûtiers, braced between those majestic mountains. Its cathedral and medieval bridge spanning over the tumultuous Isère were impressive and fascinated me. I learnt how to ski at Moûtiers on the slopes of the Champoulet hill. In springtime there were walks with the schoolmaster or priest on Thursdays in the mountains, so high up for the child that I was. In summertime my parents sent me to a boarding house with an authentic Savoyard family living in a chalet in which the main room was divided by a curtain into two parts separating the cows from the inhabitants. This was pure bliss, and those moments will always remain with me. I was nine years old when my father was transferred to La Voulte-sur-Rhône.  The Rhône valley replaced my beautiful mountains. I had to leave my skis and my sledge behind. It was heartbreaking for me, reminiscent of the child Kane’s Rosebud scene in the magnificent film “Citizen Kane”. La Voulte, with its famous rugby team, was very sports-oriented and I replaced skiing with swimming, which I loved.  My father, being an electrical engineer, knew about electro-magnetism, which is a starting point for many fields in physics. He enjoyed sharing this knowledge with me in the way of small problems. I was fascinated by light and the speed of light, which travelled distances seven times the circumference of the Earth or the distance from the Earth to the Moon in one second. That is how he came to explain that we were nowhere near seeing Martians on Earth, contrary to what sensational newspapers were announcing. He taught me how to play chess, which we often did in the evening.  The summer holidays were either spent in Provence with my grandparents on my mother’s side or in the Aude at Espéraza near Carcassonne. In Provence we spent our holidays with our cousins in the medieval village of St Martin-de-Pallières where the castle dominates the village and its park offered us a marvellous playground. The rhythm of our daily life was set by our grandmother, Mané, who we all loved and respected. It was a simple and modest way of life. We would go and retrieve drinking water from the fountain at the bottom of the village. The village school-teacher was my uncle, a voracious reader with an enormous library. Before the advent of television, he along with the school-teachers from the neighbouring villages had created a cinematographic club which regularly featured what today are the great classics. This period certainly awoke in me my deep interest in classic films. Sometimes we would have a visit of one of my mother’s first cousins who had two daughters: Andrée and Marcelle. Marcelle, with whom I had much in common, was later to become my wife.  In the Aude at my grandparents’ on my father’s side, my grandfather was a fascinating character due to his joviality, his kindness, his talent for story-telling and his dexterity. My grandparents were very endearing, and it was a pleasure to spend my holidays with them. My grandmother was an excellent cook and as to my grandfather, he was a brilliant do-it-yourself man with golden hands. He had a workshop where we spent a lot of time together inventing and innovating. Although he had only received a primary education, he loved history and we spent many hours visiting Cathar castles or talking about how the locks along the marvellous Midi Canal worked. He was also an excellent gardener with two gardens at both ends of Espéraza, Fa and Couiza. As we left the table after one of those Sunday meals prepared by my grandmother, my grandfather would invariably ask which way we would prefer to go? To Fa or to Couiza? This was to amuse me a few years later when reading Proust and Madame Verdurin would also invariably ask her hosts the same question: a walk on the Swann side or on the Guermantes side? I was overwhelmed to stay with our relatives, hearing their typical and irresistible southern accent and expressions. It was a bliss to be soaked with my cousins during few weeks in a medieval town like Carcassonne or in a cathar city like Fanjeaux, where I had my first taste of cassoulet de Castelnaudary and blanquette de Limoux the ancestor of champagne.  My father wanted his son to have a technical education, and a very renowned school near Grenoble at Voiron offered advanced technical studies in preparation for the School of Arts et Métiers. It was the Voiron National Technical Lycée, in the Grenoble region, commonly known as the Nat. We sat a difficult exam in order to gain entry to the school in the fourth year. We studied general subjects: mathematics, French, English or Italian but also a number of technical subjects such as industrial design, mechanics and workshops. I was an average pupil, a dreamer and easily distracted. At that time these defects terrified my parents, especially my mother, but they would become precious assets later on in the course of my career as a researcher.  After a Technical High School Diploma and the baccalaureate, in 1963 I entered the University of Grenoble, where I passed a Master’s degree in physics in 1967. Meanwhile Marcelle, who was an excellent gymnast at a national level, entered the École Normale Supérieure of Physical Education (ENSEP) which was then at Chatenay-Malabry near Paris. In the same year we married and went up to Paris, Marcelle in order to complete her training at the ENSEP, and I to start a postgraduate degree in physics (DEA) in order to do a Ph.D. We were living in Chatenay-Malabry near Sceaux. It was predestined. Our apartment belonged to the same landlord family that rented to [Pierre](https://www.nobelprize.org/prizes/physics/1903/pierre-curie/facts/) and [Marie Curie](https://www.nobelprize.org/prizes/physics/1903/marie-curie/facts/) in Sceaux, south of Paris. The landlord was a charming talkative man who was proud to tell me that he had manuscript letters from Mme Curie. One day as we were in the garden, I asked him if it would be possible to see the letters. He went back to the house to bring me a note written by her, complaining of a leaking faucet that needed to be fixed urgently. The great couple was not working only on lofty problems. They had also their pedestrian moments. As I was in search of a post-graduate subject, I came across Jacques Ducuing, who had just come back from Harvard and who was starting a post-graduate degree in Non Linear Optics. That was in 1967 and it was the first time I had heard this term which, associated with the laser, immediately captivated me. I applied for this post-graduate degree straightaway.  Now I had to find a research laboratory. At the Optics Institute I came across an advertisement looking for a researcher in the laser field. It offered a position in research at the Ecole Polytechnique in Professor Vignal’s laboratory directed by Alain Orszag. My career as a researcher was launched.  **My first scientific works at the Ecole Polytechnique**  Of course, during the first months I was very impressed to find myself at the very spot where so many great scientists such as Ampère, Fresnel, Fourier and many others had distinguished themselves.  This was only 7 years after Theodore Maiman’s laser demonstration. The work I was assigned was in fact a prelude to the work that led to the Nobel Prize. It consisted in the analysis of the frequency of a Q-Switch laser. The latter was not constant and varied during the pulse time. It presented a frequency drift, which in English is called a ‘chirp’, during the pulse time. The idea of this project was to exploit this ‘chirp’ in order to reduce the duration of the pulse by compression. This experience in the nanosecond regime did not present much interest, the ‘chirp’ being too weak and unable to lead to spectacular results. However, it was very important for my experimental formation and would be demonstrated 10 years later with success and thus permit the production of the first femtosecond pulses corresponding to a number of optical cycles. Combined with amplification it would be the basis for CPA (Chirped Pulse Amplification), for which I was awarded the Nobel Prize in 2018.  After passing my DEA in 1968, in conditions complicated by the May demonstrations, in 1970 I passed my doctoral thesis of the 3ème cycle (equivalent at that time to a Masters thesis) on the drift frequency of Q-switched lasers. During this period we also had our first child, Julien.  **The call of North America, my first steps in the picosecond field**  In 1970, I began my military service. At that time, it was possible to serve as a scientific cooperant in French-speaking countries. My thesis director Georges Bret, who founded the Quantel company, introduced me to Professor Marguerite-Marie Denariez-Roberge from Laval University. She was one of the first people in North America to have a picosecond laser. For three years I studied the kinetics of dye in the picosecond field. I was passionate about this field. I was still enrolled at the University of Paris and in contact with Professor Guy Mayer, with whom I prepared and passed my doctoral thesis (Doctorat d’État, which is equivalent to a PhD) in October 1973. During this period our second son, Vincent, was born.  After my thesis, I, together with my little family left for a post-doctoral year at the University of California in San Diego. Employed by Professor Michael Malley, an extraordinary man who had a picosecond laser but above all also had one of the first Optical Multichannel Analysers, OMA. The OMA allowed us to do away with photographic plates. It was far more sensitive while permitting the recording of light signals in real time.  Equipped with a detector well-suited to the task, I was the first to discover the manner in which to observe the movement of molecules or to measure the fluorescence time of the latter in the picosecond field. Personally, I consider this to be my first major discovery. I was experiencing epiphanic moments which attracted me to the field of ultrafast physics in a definitive and irreversible way.  **Return to France, introduction to ultrafast physics on the Palaiseau plateau**  Ever since I had left France, I remained on very friendly terms with Alain Orszag. We agreed that after my post-doctorate year in California, I would return to the Laboratory of Applied Optics, the LOA. We thus returned to France in 1974 and I introduced ultrafast optics to the LOA with the help of André Antonetti and also Gilbert Bourdet. At the same time the École polytechnique (’X’) was moved to the Palaiseau plateau and the LOA relocated to the ENSTA (Ecole Nationale Supérieure de Techniques Avancées) premises. We were living in this nice town of Dourdan in the south-west part of Paris. Our house was at the foot of the beautiful castle built by Philippe August in the XIV century. Julien and Vincent had a wonderful time.  At that period, I read David Auston’s article about the use of the picosecond laser for electronic switching with just a gap on intrinsic silicon. The switching was realised by the creation of carriers, produced by the laser photons in the silicon gap. I marvelled at the simplicity and the elegance of the device which could produce perfectly synchronised high voltage pulses without any laser ‘jitter’. Among the applications, I saw the switching of streak cameras which had a serious ‘jitter’ problem.  The first experiments on switching were realised at the LOA with Antonetti but also Alain Migus who had joined us with financial help from the CEA (Commissariat for Atomic Energy). During this period, we had the pleasure of welcoming Michael Malley on a sabbatical year at the ENSTA. Michael was assembling the first femtosecond ‘dye’ laser with Alain Migus and Jean-Louis Martin, who had joined us, bringing with him information and advice given by Erich Ippen from the Bell Laboratories. Our switching experiments were presented in 1978 to the Conference on Lasers and Electro-Optics (CLEO) in Washington. My results attracted the attention of Wolf  Seka from the Laboratory of  Laser Energetics (LLE) at Rochester, NY. After an animated discussion, Wolf understood the interest of introducing femtosecond pulses in inertial fusion to diagnose implosion. A few months later, I received a phone call from Wolf telling me that the LLE was offering me a position as a scientist. I had thoroughly enjoyed my sabbatical year in the US and  also had the feeling that I would evolve more easily in this field that appealed to me in the US. After talking it over with Marcelle, a few weeks later we decided to accept the offer and in September 1977 we left for Rochester along with our two children. However, both of them were sad to leave beautiful Dourdan.  **Rochester**  The LLE had just been built. This laboratory was financed essentially by the Department of Energy (DOE) and dedicated to inertial laser fusion. My first impression was that I had made a big mistake. Laser fusion was not my field. I preferred smaller projects, less programmatic ones. However, a few months later my opinion was to change radically.  The University of Rochester in the 1980s played an important role in the development of the field of ultrafast science and technology. The Institute of Optics and the Laboratory for Laser Energetics (LLE) occupied centre stage. The Institute of Optics provided exceptional students and LLE a unique technical platform. Many of the techniques that the researchers in the field use today, like THz generation, picosecond electron diffraction (PED), electro-optic sampling (EOS), chirped pulse amplification (CPA), and jitter-free synchronization, were conceived and demonstrated by the ultrafast science group. The Institute of Optics students – Wayne Knox, Theodore Sizer, Irl Duling, Janis Valdmanis, James Kafka, Donna Strickland, Maurice Pessot, Jeffrey Squier and John Nees – formed the core. Their enthusiasm was infectious and contributed much to attract students from physics, such as Steve Williamson, Theodore Norris, and Kevin Meyer, and from electrical engineering, Daniel Blumenthal, John Whitaker, and Doug Dykaar, as well as faculty like C. W. Gabel, Robert Knox, Charles Stancampiano, Thomas Hsiang, Roman Sobolewski, Adrian Melissinos, Joseph Eberly, and David Meyerhofer.  In the early 1980s ultrafast science was dealing with eV energy-level phenomena. Our group extended its range into the meV on one side, with the introduction of THz beams and electro-optic sampling (EOS) techniques, and to the MeV-GeV on the other side, with chirped pulse amplification (CPA) and its ability to produce relativistic intensities. Work in this area mainly started at LLE in 1978 after my arrival. LLE was running a highly programmatic effort on inertial confinement fusion. At that time the director and founder, Moshe Lubin, and later Robert McCrory understood the importance of creating and supporting in parallel to the main research activity a group that would work on weakly related laser fusion projects,which could offer the flexibility and the type of environment that PhD research demands. I would be in charge of this group, known as the ultrafast science group.  I was impressed by the work of Dave Auston at AT&T Bell Laboratories, which demonstrated that electrical signals could be switched with picosecond precision. Here I had the opportunity to demonstrate that this simple technique could find some important applications in laser fusion because of the need for synchronised high voltage pulses for active pulse shaping or for jitter-free streak cameras.  An exceptional undergraduate student, Wayne Knox, shared an understanding of the importance of this line of research. At Wayne’s high speed of progress, we extended Auston’s work to very high voltage and applied it to the synchronisation of streak cameras. For the first time the streak camera could be used in accumulation mode. Weak luminescence signals could be accumulated, improving their signal/noise ratio. The jitter-free streak camera found immediate applications in photobiology with the group of Wayne’s father, Professor Robert Knox. This technique is now routinely used in synchrotron-based femtosecond x-ray diffraction experiments. High voltage switching also has applications in active pulse shaping, as demonstrated in collaboration with John Agostinelli (student of C. W. Gabel), and in contrast improvement with Wolf Seka. This technique is still used today in high field science.  **Electron diffraction**  A streak camera is a beautiful photon-electron transducer. It makes an electron replica of the photon pulses. The electrons are deflected across the phosphor screen, leaving a phosphorescent track. I was mesmerised by the thought that we could use this perfectly synchronised photoelectron pulse to perform time-resolved electron diffraction in the picosecond time scale by simply locating a sample under study in the camera drift region. We could study solid-liquid transformation simply by using a short optical pulse to produce the phase transition and the electron pulse to probe the structural change that would follow.  I asked a new student with great passion for research, Steve Williamson, if he would be interested in this project. This was an enormous challenge, as none of us had any kind of electron diffraction experience in steady state let alone in the transient regime. But Steve was a superb experimentalist, and in one year he built a complete “streak camera” and demonstrated the concept. We applied it by performing the first time-resolved structural transformation in the picosecond domain. It was the solid-liquid phase transformation of aluminium. Further work was conducted by Hani Elsayed-Ali, notably on surface melting. The activity was extended later to gas electron diffraction by [Ahmed Zewail](https://www.nobelprize.org/prizes/chemistry/1999/zewail/facts/) (Nobel Prize in Chemistry 1999). More recently – twenty years later – our picosecond electron diffraction experiment on aluminium was repeated by Dwayne Miller from the University of Toronto with a superior laser and shorter pulses. Note that Dwayne was at the University of Rochester in the chemistry department with a joint appointment in Optics when, in 1982, Steve did his seminal experiment. Today, time-resolved electron diffraction is becoming a very active field, rivalling time resolved x-ray diffraction.  **First step of single cycle THz generation**  We knew that the picosecond rise time produced by photoconductive switching could be used to produce THz transients, either from the gap itself by putting a switch in a coaxial waveguide transition or by exciting a microwave antenna. This simple experiment was performed by a dedicated undergraduate student, Daniel Blumenthal, from the electrical engineering department in collaboration with his adviser, Charles Stancampiano, and André Antonetti from the Ecole Nationale Supérieure de Techniques Avancées in France. The THz field became a very important domain once it was realised by Auston that the electric field could be time-resolved by the laser pulse itself. The field amplitude and phase could be measured, and a new THz spectroscopy technique was born that would replace infrared Fourier-transform spectroscopy. Besides spectroscopy, applications of these transients include THz imaging. Also, the methods of generation have been vastly advanced as demonstrated by X-C Zhang.  **Electro optics sampling: measuring electrical signals with subpicosecond resolution**  We could switch electrical signals with rise times in the subpicosecond domain, but it was difficult to measure them. Wide band sampling oscilloscopes could only go to 25 ps and the only way to measure the picosecond pulses was to use a second photoconductive gap with a fast photoconductive semiconductor. Of course, one solution was to try to use the electro-optic effect. The EO effect can have a purely electronic reaction with a sub-femtosecond response. But there is no free lunch, and this ultrafast response is paid for in terms of sensitivity. Kilovolts are usually necessary to detect a signal. So, it appeared the EO effect could not be a contender for fast measurements, as it was not sensitive enough. Janis Valdmanis, who had the idea to use lock-in detection in conjunction with the electro-optic effect, demonstrated this to be false. With his “golden hands,” Janis showed that sub-millivolt, subpicosecond signals could be measured. The EOS technique became an indispensable tool to visualise THz electrical signals. For the first time, direct propagation of picosecond electrical pulses on transmission lines, both normal and superconducting (with low and high-Tc) could be investigated. EOS was also used in the measurement of the fastest transistor rise times and the switching of Josephson junctions. It was also used in the direct investigation of subpicosecond carrier dynamics in semiconductors, such as velocity overshoot. Most of the activity was coordinated by D. Dykaar and involved many students, like J. Whitaker, visiting scientist Roman Sobolewski and Professor Thomas Hsiang, from electrical engineering, as well as Kevin Meyer, a student from physics.  **Chirped pulse amplification**  The generation and amplification of short pulses was, however, our main activity. Short pulses were used for everything. At that time, Ti:sapphire had not been invented, and dyes like rhodamine 6G were the main amplifier media. The leading laboratories were at AT&T Bell Laboratories with the group of Charles Shank, and with Erich Ippen and Hermann Haus at MIT. In our group, outstanding students were working on dye-based generation and amplification of ultrashort pulses. They were Theodore Sizer, Irl Duling, James Kafka and Theodore Norris. During one of our constant and endless discussions about novel ideas and concepts, we discussed in 1982 with Steve Williamson a possible way to get larger energy per pulse by using better energy storage media. Strangely enough, 1982 also marked the birth of our daughter Marie, which convinced me that I could do two very different things at the same time: generate a Nobel Prize-worthy idea and engender with Marcelle an adorable girl. From a bandwidth point of view, Nd:glass can in principle amplify subpicosecond pulses. However, unlike in dye, Nd:glass is almost too good of an energy storage medium. The major problem is that the pulse energy becomes too large, leading to high intensities and nonlinear effects. The nonlinear effects contribute to destroying the beam quality and ultimately lead to the “destruction” of the optical amplifier. Dyes, on the other hand, do not have this problem. They are mediocre energy storage media, due to their large amplifying cross-section. Therefore, the pulse energy stays below the critical intensity level where the nonlinear effects dominate. We were greatly influenced by the work of Dan Grischkowsky (IBM Yorktown Heights) and Anthony Johnson (AT&T Bell Laboratories) that demonstrated that by propagating a relatively long pulse in a fibre, the pulse will be the subject of broadening and stretching by a combination of self-phase modulation (SPM) and group velocity dispersion (GVD). As a result, the pulse is stretched with the spectral content of a much shorter pulse. It exhibits a linear chirp. At this point it can be compressed by using a Treacy grating pair, which exhibits a negative GVD to a value one hundred times the value of the input pulse. It looked to me that it would be simple to try to amplify the pulse in order to extract the amplifier energy and compress it later when the energy would be fully extracted. I asked a new student, Donna Strickland, if she would like to do this experiment. Donna was excited about it but also concerned that it might not be good enough for a Ph.D. thesis. She quickly demonstrated that this concept was working to the millijoule level.  It was at this time that Marcel Bouvier from Albertville joined the group. He was a shrew electrical engineer who made some impressive contributions. Notably by inventing a key device, called 1kHz Pockels cell that revolutionized the field. This laser component is now in the exhibit of the Nobel Museum. He also started the company MEDOX Inc. with Phillippe Bado a laser scientist in the group.  *The key to CPA: the matched stretcher-compressor.*The first approach to CPA was rudimentary and relied on an unmatched stretcher-compressor system. It was not perfect. After a certain amount of stretching, the compressor could not compress the pulse without causing significant wings on the pulse. The fibre-grating pair system was not matched over all orders. What we needed was a matched stretcher-compressor system so we could extract the energy better and compress it better. The matched stretcher compressor became our “Holy Grail.” I was continuously thinking about it. One day I was skiing at Bristol Mountain with my wife Marcelle, and on the chairlift, I started to think about a paper I read the day before from Oscar Martinez. This paper was describing a compressor for communication applications at 1.5 mm. At this wavelength the GVD in fiber is negative and the pulses exhibit a negative chirp where the blue frequencies lead the red ones. To recompress the pulse at the fiber output, Martinez proposed a compressor with positive GVD that was a combination of a grating pair and a telescope of magnification unity. I realised that the Martinez compressor in the positive GVD region was in fact the matched stretcher of the Treacy compressor. This was exactly what we were looking for. I interrupted my day of skiing and went back to the laboratory, where I met Maurice Pessot, a new student in my group. I asked Maurice to drop what he was doing and show that the Martinez stretcher and the Treacy grating pair were matched. In a beautiful experiment, Maurice showed that an 80 fs pulse could be arbitrarily stretched 1000 times by the Martinez device and recompressed by the same factor to its initial value. A major hurdle in CPA was overcome. Fifteen years later, this stretching-compression system is still part of the standard CPA architecture.  *En Route to the Petawatt.*The stretcher-compressor was integrated in our first Joule level Nd:glass system by a visiting scientist, Patrick Maine, and a post-doctoral fellow, Philippe Bado. With Donna they demonstrated a pulse with one joule in 1 ps., i.e. 1 terawatt on a table top – called the “Maine event” since. It was at night and we were jubilant. Robert McCrory, the LLE director, was as usual working late and heard our noisy celebration. He poked his head in the laboratory curious to know what was going on. I told him that we had just demonstrated the generation of one TW with a new amplification technique. It was a thousand times improvement in power over standard techniques, and moreover, this technique could be scaled to a much higher energy than the kJ level using the glass development laser (GDL), a prototype chain at LLE. At that time, we paused and asked ourselves what the next scientific prefix after “tera” was. Nobody knew. We went to Bob’s office and discovered that it was “peta.” So, from now on, our next goal would be the petawatt. The first article on the possibility of producing petawatt level pulses was described in a French scientific journal, “En Route Vers Le Petawatt” and the first petawatt pulse was demonstrated by Michael Perry at Livermore ten years later. At that time, we decided with Patrick and Donna to call this new amplification technique chirped pulse amplification (CPA). Of course, Wayne, who was at AT&T Bell Laboratories by that time and always has something to say, called me to argue that people would get the acronym mixed up with “certified public accounting.”  It was a great time with visits from bright people, like Michael Campbell and Michael Perry from LLNL who understood immediately the revolutionary nature of CPA. We had big plans to go together to the PW level. Also, we had See Leang Chin who came for a sabbatical and was the first to propose with Joseph Eberly to use T3 for the study of light matter interaction in the high intensity regime. With Henri Pepin and his group, Mohamed Chaker and Jean-Claude Kieffer, the contingent of Quebecois from INRS was growing. INRS would play an important role later in our decision to move to University of Michigan. Also, I don’t want to forget the group of Adrien Mellinos who had the first the idea of using T3 on SLAC to demonstrate pair generation on SLAC.  We worked a lot to extend the technique to other materials, such as Alexandrite with Jeff Squier and Don Harter. That was before Ti:sapphire. Alexandrite was at that time the only broadband high-energy storage material available. A lot of the CPA work continued after our move to the University of Michigan with Ted Norris, Jeff Squier, François Salin and Gary Valliancourt producing the first kHz Ti:sapphire source – the workhorse of many ultrafast optical laboratories today. Let’s also not forget Marcel Bouvier, our indispensable and reliable electrician.  **Michigan**  However, by inventing CPA we created a new field with characteristics diametrically opposed to the fusion field, the LLE main mission. Our success was highly appreciated but it created some tension. One day I received a call from Duncan Steel and the dean of the College of Engineering from the University of Michigan, Charles Vest, inviting me to move all my group and their families to the University of Michigan Ann Arbor. After one month of negotiation, in August 1988, my group moved to Michigan. This coup was apparently perceived very positively by the MIT search committee looking for a new president. A few months later, Chuck Vest became President of MIT, a position that he held for 16 years. With me, Henri Pépin’s group followed with their equipment. They were the initiator of the ultra-high intensity field at Michigan.  In 1990, two years after our arrival we had been able to attract prominent scientists/professors like Janis Valdmanis, Donald Umstadter and Philip Bucksbaum from Bell Labs. We responded successfully to a call from the Natrional Science Foundation to build an NSF Center. We named it the Center for Ultrafast Optical Science (CUOS) based on femtosecond optical pulses that can provide the shortest controlled bursts of energy, yet produce and enable the highest laboratory peak-power densities ever generated. These two characteristics have opened access to a number of new fields of research not previously available to basic science and applied technology. In the original establishment of the Center, it was pointed out that “ultrafast optical science is an inherently interdisciplinary effort implying scientists and technologists working on laser and optical physics, atomic and condensed-matter physics, chemistry, optical fibres, and electronics.” The first important results on high field physics in gas and solid were obtained by the group of Don Umstadter and J.C. Kieffer with high energy electrons acceleration … It was at CUOS in the early 1990s thanks to M. Bourier Pockels cell the kHz Ti: Sapphire was demonstrated by Jeff Squier and François Salin. This system became the workhorse of femtosecond research. As I was presenting in Bayreuth the kHz laser, Georg Korn introduced himself and expressed the desire to come to CUOS. I accepted and Georg stayed a few years with us where he participated in many important experiments. CUOS now includes researchers in all fields, as well as in plasma physics, accelerator physics, materials science, biophysics, and medicine, all working closely with scientists developing new ultrafast laser sources and measurement techniques – in short, in a “centre mode” of research.  In 1994, we spent 4 months on sabbatical at the University of Tokyo Roppongi at the laboratory of Shantaru Watanabe. We enjoyed immensely the time in Japan. We were hosted by Professor Hiroshi Takuma. It gave me also the opportunity to meet Professor Toshiki Tajima, the inventor of the wake-field accelerator, whom I did not know before. It was an epiphanic moment that started a fruitful collaboration between us that has had few discontinuities since. Marcelle started to take some Ikebana classes and later became a sensei of the SOGETSU Ikebana school.  **Femto-micromachining and eye surgery**  Ultrashort laser pulses offer both high laser intensity and a precise laser-induced breakdown threshold with reduced laser fluence. The ablation of materials with ultrashort pulses has a very limited heat-affected volume. The advantages of ultrashort laser pulses are applied in precision micromachining of various materials. Ultrashort-pulse laser micromachining have a wide range of applications where micrometer and submicrometer feature sizes are required.  With Ron Kurtz, Tibor Juhasz and students, we investigated refractive cornea surgery in vitro and in vivo by intrastromal photodisruption using a compact ultrafast femtosecond laser system. Two students, Detao Du and Xinbing Liu, demonstrated that in the femtosecond regime, photodisruption is associated with smaller and very deterministic threshold energy as well as reduced shock waves and smaller cavitation bubbles than with nanosecond or picosecond lasers. Our reliable all-solid-state laser system was specifically designed for real world medical applications. By scanning the 5 micron focus spot of the laser below the corneal surface, the overlapping small ablation volumes of single pulses resulted in contiguous tissue cutting and vaporisation. Pulse energies were typically in the order of a few microjoules. Combination of different scanning patterns enabled us to perform corneal flap cutting, femtosecond-LASIK, and femtosecond intrastromal keratectomy in porcine, rabbit and primate eyes. The cuts proved to be highly precise and possessed superior dissection and surface quality. Preliminary studies show consistent refractive changes in the in vivo studies. We conclude that the technology is capable of performing a variety of corneal refractive procedures at high precision, offering advantages over current mechanical and laser devices and enabling entirely new approaches for refractive surgery.  **Back to France**  In 2004 I was invited for the scientific evaluation of the Laboratory of Applied Optics (LOA). I was approached by the research director of the École Polytechnique, Maurice Robin, who asked me about the possibility of returning to France. Although I was very happy at Michigan with CUOS, it was an excellent opportunity for us to come back and almost 30 years later I returned as the director of the LOA in 2005.  The same year, ESFRI was in the process of updating its roadmap of large-scale research infrastructures. I took advantage of the opportunity to propose the Extreme Light Infrastructure (ELI) as a Pan-European facility. At the same time the Ile-de-France region also had a call for major instruments and so we proposed the Apollon laser facility as well. These laser facilities, for me, were the extension of the ultra-high intensity at CUOS by seeking the tens of PW level almost 100 times more powerful than the Hercules laser at CUOS. It was a beautiful opportunity to fulfill our dream with Tajima. We succeeded in obtaining both projects. I was the initiator and PI of the ELI Preparatory Phase that started in 2008.  After an agonising debate between the École Polytechnique, CEA, CNRS and the Institute d’Optics, it was decided to build the Apollon laser at the CEA site at L’Orme des Merisiers in an old accelerator facility that had been dismantled. It was decided also that the LULI would build and run the facility by 2019–2021.  The ELI project has as its goal to build an infrastructure of facilities providing the most advanced peak power laser systems in the world. This gargantuan power will be obtained by producing kJ of power over 10 fs. Focusing this power over a micrometer size spot, will bring forth the highest intensity. By producing, firstly, the highest electric field, secondly the shortest pulse of high energy radiations in the atto/zeptosecond regime and thirdly, electrons and particles with ultra-relativistic energy in the GeV regime, the laser signalled its entry into Nuclear Physics, High Energy Physics, Vacuum Physics and in the future Cosmology and Extradimension Physics. More precisely, ELI will be the first infrastructure dedicated to the fundamental study of laser-matter interaction in the ultra-relativistic regime (I > 1024 W/cm2). The infrastructure will serve to investigate a new generation of compact accelerators delivering energetic particle and radiation beams of femtosecond (10−15 s) to attosecond (10−18 s) duration. Relativistic compression offers the potential of intensities exceeding greater than 1025 W/cm2, which will challenge the vacuum critical field as well as provide a new avenue to ultrafast attosecond to zeptosecond (10−21 s) studies of laser-matter interaction. After long debate it was decided that ELI will have three pillars located in three European emerging countries. Each countries will work on coordinated different topics: Czech Republic for the development of high energy particle radiation Beam Line, Hungary for Attosecond Source and Romania for Nuclear Physics.  In 2010, following the advice of Alexander Sergeev, I applied for a Russian Megagrant and was one of the winners. I had a joint appointment between the Institute of Applied Physics at Nizhny Novgorod and the University of Nizhny Novgorod. We were housed in a studio next to Sacha and Marina sharing our breakfast and meals together. We had long and friendly discussions between us on all subjects, sometimes exposing our French and Russian differences. Our collaboration started with the Russian Excel Laser and still continues with fine scientists like Efim Kazhanov and Sergey Mironov.  **IZEST: orbital debris, going beyond the horizon**  For ELI, 2011 was the end of the preparatory phase and the beginning of the construction phase. The respective countries managed the three facilities. I was 67 and had to become professor emeritus. With Toshiki Tajima, we proposed to create a unit to explore the prospective for Extreme Light, IZEST for International Zeptosecond Exawatt, Science and Technology. IZEST is devoted to the investigation of Extreme Light beyond the Horizon set by the ELI infrastructure.  Among IZEST’s achievements, we note: The International Coherent Amplification Network (ICAN) project, a laser system characterised by a novel architecture, based on the coherent combination of many CPA fibre lasers. ICAN, provides the laser with high peak power, high average power and good wall-plug efficiency. This is paramount to applications such as particle colliders, nuclear waste transmutators and space debris mitigation. The development of the working prototype is the XCAN program and is directed by Jean-Christophe Chanteloup.  After seeing the movie “Gravity”, it occurred to me that the ICAN system could play a key role in space debris mitigation. The first conceptual demonstration was made by Rémi Soulard and Mark Quinn.  Finally, my future goal is dedicated to the increase of peak power towards the Schwinger regime. Here with Toshiki Tajima and Jonathan Wheeler, we are aiming to test a new paradigm: instead of producing high peak power by increasing the laser energy, we will increase the peak power by shortening the laser pulse to the atto and subattosecond regime. In this way we could produce high energy, attosecond single cycle pulses in the x-ray regime. The peak power could be exawatt, the wavelength in the x-ray and the intensity in the Schwinger regime, enough to produce PeV particles and vacuum materialisation. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0019 = GM**  Gérard Mourou: Hello.  Adam Smith: Hello, my name is Adam Smith from Nobelprize.org.  GM: Yes.  AS: Yes. Many congratulations on the award of the Nobel Prize.  GM: Thank you very much.  AS: It must be … it must be an amazing environment.  GM: It’s an amazing moment, I tell you. And nobody’s prepared for that kind of moment.  AS: I must say it sounds quite calm around you now.  GM: Yeah.  AS: In the background it sounds calm.  GM: The background, yes, because I’ve been put in a room so nobody can bother me.  AS: Going back to that breakthrough in 1985, was there a eureka moment when you suddenly realised how you could improve the pulse length, shorten the pulse length?  GM: It came in different steps, in different steps, and in fact it came very, very kind of naturally with what I was doing. So we wanted to amplify very, very short pulses in order to get more peak power. OK, because power is energy divided by time, and so if you want to get large power, big power, then you try to get pulses shorter and shorter and shorter, OK. So trying to do that, when you amplify your pulse, come to a point where material is breaking down on the laser.  AS: Yes, so you have to reduce the power somehow.  GM: You have to reduce the power but not change the energy, right, I mean you have to reduce the power without changing the energy, without changing the total energy of the pulse, OK, because you want to do that efficiently. I just came say well maybe we can stretch the pulse, stretch the pulse, and we knew how to do that because we were working on, recently on very short pulses, and you knew how to stretch pulses by using diffraction gradings and so on. So we stretched the pulse, and of course immediately the peak power decreased and then we could amplify the pulse much better, much much better. And then we had to compress it back, again by steps.  AS: Yes, and what … give me an example of one of the most exciting things we can do with these ultrafast lasers.  GM: Well, what we can do is to accelerate particles. We can accelerate particles with really stunning efficiency, so instead of using kilometres to accelerate particles, like at CERN we could use a system with lasers which will already only take centimetres.  AS: Indeed, indeed.  GM: Accelerators have a lot of applications, in the medical world, OK, because you want to create for example therapy, OK, therapy you [unclear] maybe use maybe radio isotopes, but every time when you want to do that you have sometimes to go outside, so this radio isotope, for instance, are made, by reactors which are far away, and so on, so it’s difficult to bring them back at the patient’s bed.  AS: Yes.  GM: But now if you really make this accelerator very compact you can put that then in hospitals, and because they are compact, you can multiply them, and you can have one per hospital.  AS: Powerfully described. It’s a most lovely example of such a successful interaction between basic science and applied science. The whole development …  GM: Oh absolutely, absolutely, absolutely, yep.  AS: And how utterly co-dependent they are – you cannot do it without the basic research.  GM: Yeah.  AS: Yeah, and it’s so exciting that you’ve been awarded together with your graduate student.  GM: That too, yes, yes, yes. When I proposed this idea to Donna Strickland she said: ‘Well that’s so simple, this is not a PhD’. You know … [Laughs]  AS: Well …  GM: No, it’s not a PhD, it’s Nobel Prize material!  AS: I don’t know of another example where somebody’s first published scientific paper leads eventually to a Nobel Prize. That sets a pretty high standard for other graduate students to achieve.  GM: Yes. [Laughs]. Yes.  AS: Will we be welcoming you to Stockholm in December?  GM: Absolutely. AS: Good, we very much look forward to it.  GM: Absolutely, of course.  AS: Thank you very much for speaking to me and congratulations.  GM: Thank you. Bye bye.  AS: Bye bye. |
| **Interview** |  |
| Q10 | **What are the benefits of working in an international setting?** |
|  | Gérard Mourou: I think that I always believed that science should be used to unify people and I’m a strong believer in this and it’s like music and everybody speaks the same language and science is the same thing. In science we all speak the same language and we work on the same problems and we try to make this world better. |
| Q3 | **Where does your passion for science come from?** |
|  | Gérard Mourou: The passion of lights, in fact, comes from the laser. The first time I’ve seen the lasers, I said “Wow”, I couldn’t believe, to see this light, so well behaved, this beam of light, with pure colours. That was direct captivating for me and kept me ongoing for 45 years. |
| Q9 | **How did it feel to discover you had been awarded the Nobel Prize?** |
|  | Gérard Mourou: Of course the culmination of a career as a physicist is winning the Nobel Prize and then when you get this phone call from Stockholm … I knew of course that the Nobel Prize for Physics is always given on the first Tuesday of October and before noon. And I was kind of waiting, even if I had a small chance, waiting and also interested to know who was going to get it. But anyway I’m a harried swimmer, so I was on my way to go to the swimming pool and my secretary just waved at me and said “Gérard, could you come back. We have a phone call for you from Sweden, from Stockholm”. Oh, wow /---/, and so I picked up the phone and of course it was coming from Stockholm and so I almost crashed because it’s dynamite [laughing] when you hear it and that change your life also. |
| Q2 | **How relevant has persistence been to your career?** |
|  | Gérard Mourou: Oh, this has been extremely relevant. One thing I’ve done … we have invented a new type of laser with Donna Strickland, my co-laureate and student. We invented the lasers and in a process of aligning the laser my student, not Donna, but another student, got the beam in his eye, and so we took the student … he was not wearing goggles like he was supposed to, but accidents happen and so we took him to the hospital where is an ophthalmologist. The ophthalmologist looked in his eyes, his retina and so on and he said: “Hm, yeah, you got the laser in your eye, in your retina. Your retina is burned.” but he said: ”What type of laser is it?” and the student told him it was a new type of laser, he said, and he asked him “Why are you asking these questions?” “Well, because the damage you have in your eye is perfect”. And that started what now is femtosecond ophthalmology. There is millions of people now using the femtosecond laser for surgery. So it was really clearly a mistake. It was not planned at all. That opened up a completely new field and you have millions of people who are benefitting from this application now. |
| Q4 | **Are environmental issues close to your heart?** |
|  | Gérard Mourou: This is very close to my heart. All the environmental issues, I will say, is on the top of my preoccupations because we see every day of course that science brings a lot of *goffe* technology, brings technology on, but now it is science. We invented in fact a new terms for this type of science, should be ‘toilet science’, do you understand, ‘toilet science’, because some of the science help to destroy the planet with all the technology that came out of that, so we should really now think about really cleaning the mess, basically, which has been done, we have to say that as a consequence of this new science and technology. |
| Q18 | **What responsibilities do scientists have to the environment?** |
|  | Gérard Mourou: I would say yes, we have to be very careful about what we do. I think it’s of course … you have sciences then you have engineers and technologies and then you have products and consumers and so on. I think for scientists we really need to discover work on improving cleaning the planet for instance in nuclear power we have always nuclear waste. We are working with lasers on trying to basically improve the situations because nuclear energy is certainly a very important source of energy which could be very clean at one conditions, we have to clean up the waste after that. If we come up with reasonable solutions to clean up this waste, then that will be a fantastic improvement. |
| Q14 | **What is the scope for future applications of lasers?** |
|  | Gérard Mourou.: The scope for our lasers is enormous, it’s enormous. In 1960 the laser was demonstrated by Ted Maiman but it was invented by Townes and as soon as the laser was invented people were saying “What’s going to be the use of this laser?” So they tried to find applications for the lasers, they tried to find applications In fact when we also demonstrated the very ultra-high intensity lasers that we got the prize for, also we weren’t sure about the applications and now we have of course enormous amount of applications. And what I like, of course, is applications in the medical, in medicines and so on. It’s very important. It’s on the top of my agenda. |
| Q1 | **What is your advice for young scientists?** |
|  | Gérard Mourou: I think I have only one advice for students who would really like to get into science and researching on that. You have to be passionate. If you don’t, if you are not passionate about it, you should do something else. It has to come from the heart of you because it’s very hard. It’s very hard for you but also, I have to say, for the family because you are always thinking about your research, because you are passionate. So you are thinking only about one thing and so sometimes it’s tough for the people who are with you. |
| Q7 | **Besides passion, what are the qualities top scientists need to have?** |
|  | Gérard Mourou: I mean the type of quality you need, of course, is the ability to endure being long time in the lab and be patient and obstinate. You have to be focused. |
| Q8 | **What do you do in your spare time?** |
|  | Gérard Mourou: My swimming is very important. I’m swimming. Well, I’m a skier. I was born in the French Alps. This town was named Albertville and where we have the Olympic Games and so skiing is basically a natural thing for me but also I love swimming. Swimming for me is like yoga, it’s very peaceful. I’m talking about open water swimming, and I love that. |
| Q2 | **When swimming, you aren’t thinking about your research?** |
|  | Gérard Mourou: That’s right and I have a problem because, when I think about this, I’m in a state when I’m swimming. You are totally relaxed but also you are thinking about your research and that’s a problem because some time I forget the number of laps I’m doing. |
| Q5 | **Who inspired you in the beginning of your journey?** |
|  | Gérard Mourou: Because my dad was working in an electrical power company in the Alps, the French Alps, so we were always talking about current, power, amperes, voltage and things like transformers and all that because this is what he was doing and he was always explaining to me how these were working … with my dad, playing chess and doing, I mean learning about physics, yes. |
| Q3 | **What do you enjoy about working with graduate students?** |
|  | Gérard Mourou: Working with graduate students is like with your family, being with really your family. I mean that’s a way I always treated my students. We stay in the labs together, we discuss in the lab, weekends we spend together and so on, I mean, always thinking about what we want to achieve and that brings the students and the faculty together and we are aging, the faculty is aging, but we’re renewed, our students are renewed, so we are always close to the very young students and I’m very pleased also that many of my students, a few, well many, about ten will be attending the Nobel Lecture and the events. |
| Q14 | **How has your research been applied over the years?** |
|  | Gérard Mourou: Since the very beginning I was very attracted by trying to really produce very, very short pulses. In the beginning it was the picosecond and then femtosecond and then attosecond and so on. So picosecond is 10-12 so it’s one thousandth of a billionth of a second and a femtosecond is a billionth of a millionth of a second. And I was always fascinated by what you could do because we could really, for the first time, we could see things moving in these time scales and of course we are not moving in these time scales, but molecules, atoms, electrons and all that, they move in this time scale and of course your eye is too slow to follow them. So trying to understand the world with this new tool, this tool of having very short pulses so you can track down the evolutions of very small systems to understand reactions and that, this is really extremely fascinating.  Then came the second application and the second application. If you are producing these very, very short pulses, then you can produce, if you are smart, very high intensity, very high peak power, and this is where we had a problem, with Donna, is a fact, because the pulses are short, the power that you are trying to get, can be very high, but at one point you are breaking down the laser because the power, the peak power is too high. And that’s of a clamp down the laser power that you can produce and it’s where we came up, with Donna, with this idea of CPA which is chirped pulse amplification, which could circumvent these problems of the laser being destroyed as we were amplifying the pulse and getting more peak power. |
| Q4 | **Can you summarise your discovery in 30 seconds?** |
|  | Gérard Mourou: My Nobel Prize was awarded because of my work with laser, with very, very short pulse laser, and why do you want to have very short pulse lasers? It is because these pulses will be also important for communications, very important for communication in general, very important also in the medical world, very important for many other disciplines. Because with these very short pulses you will be able really to observe or to track down reactions which happen in cells, but also which may happen also in a transistor for instance for communication, fast electronics and so on and also because this very short pulse gives you viability to produce, over a very short time, extremely high peak power so you can use that for nuclear physics and maybe also trying to understand events which are happening in the cosmos. Because you can really produce the highest temperature, the highest pressure, the highest field, the highest gravity, so you can really simulate and bring back into the lab what is happening in the cosmos and in life in general. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0020** |
| **Biographical** | **Life**  Donna Theo Strickland was born on May 27, 1959, in Guelph, Ontario, Canada. She studied engineering physics at McMaster University in Hamilton, Canada, and optics at the University of Rochester in the United States. She earned her doctorate in optics in 1989. Her PhD supervisor was Gérard Mourou, future Nobel Laureate. She worked at the National Research Council, and then lived in the United States where she worked at Lawrence Livermore National Laboratory and Princeton University. She has been affiliated with the University of Waterloo in Canada since 1997. Donna Strickland is married with two children.  **Work**  Professor Strickland developed chirped pulse amplification (CPA) with Gérard Mourou, her doctoral supervisor while at the University of Rochester. CPA enables the most intense laser pulses ever and the research has led to tools with applications in medicine, industry, science, the military and security.  **Autobiographical**  On the day I was born, my father marked the day by buying a copy of a newspaper. He performed this same ritual to mark the birth of each of his three children. My mother was a very organized person and kept all of my mementos. Near the end of her life, when she sold the family home, she gave me her collection of my childhood memorabilia. This newspaper was preserved in a plastic bag. Along with it was a note from my mother, pointing out an article that she thought I would now find particularly interesting.  It was a piece on the first female engineering graduate from the University of Toronto. She was the only woman in a graduating class of 450. The accompanying photo shows her seated, holding a bouquet of roses, and surrounded by several of her male classmates. It looks a lot like Marilyn Monroe’s number *Diamonds are a Girl’s Best Friend*from the movie *Gentlemen Prefer Blondes.*The men are all looking at her adoringly and each is holding up a glass. According to the article, they were toasting their queen. The story describes her as a girl, a maid and, as I say, a queen. Not once was she called a woman.  Back then, my home town had a population of less than 40,000 people. Guelph’s most notable former resident is John McCrae, the field surgeon in World War I who penned the poem *In Flanders Fields.*It’s also known as the Royal City, Guelph being the surname of several royals, including George IV, the king at the time the city got its name.  My mother trained as a teacher but was a homemaker when I was a child. My father was an electrical engineer. I’m the second of their three children, appearing between my older sister Anne and my younger brother Rob. I would say that we had the stereotypical good, middle-class life. I was a daddy’s girl. When I was young, I was always climbing up on my dad’s lap, like in the family photo. I was always asking him to read my favourite story, *Molly Whuppie*. It is an English fairy tale about three sisters having to fend for themselves. Molly was the youngest, but she used her wits and her gumption to save all three of them from the horrible giant. My dad always asked, “Again? Can’t you pick a different story?” But he always read me *Molly Whuppie.*He started calling me Molly Whuppie, which got shortened to Whup and that is what he always called me from then on. My dad was an avid newspaper reader, reading it from cover to cover, including the comics. He liked the comic *Cathy*, because she reminded him of me. I have several of the *Cathy*strips that he cut out of the papers. They are about the dad just wanting Cathy to be happy and Cathy mostly wanting to be an independent woman, more interested in a career than settling down and starting a family.  I have always loved going to school. I was one of those rare kids who was happy to have summer vacation over so I could go back to school. I started this love affair with school at Victory Public School. I met my new best friend, Susan, in Grade 1. We went all through school together, including skipping Grade 3 together. We went onto Willow Road School for Grades 7 and 8 and then Guelph Collegiate Vocational Institute, GCVI, for Grades 9 through 13. In the earliest grades, I excelled at reading, but as I got older, math and science became my favourite subjects. I would say writing was what I found hardest to do, so I shied away from subjects like history. In high school, when I made a comment to my friends about how I was glad I was done having to take any more history and geography courses, the teacher overheard and asked what courses I wanted to take. When I responded that I wanted to take math and physics, she told me that they were boys’ subjects. I could not believe she would say such a thing. I had never once thought there were girls’ and boys’ subjects. Most of the top math students in my year were girls and none of my classmates thought that was odd. There were boys in the class who were excellent at writing essays and stories and I certainly didn’t think that was strange. This was the 1970s and women’s lib was all the rage. We girls were told we could do anything we wanted, and I believed it.  Education was important to both of my parents. My father’s father had grown up in a small fishing village in Newfoundland and had no formal education. He learned to read and do math after he retired. Both of his sons went to university, which was rare coming from a small town on Cape Breton Island in Nova Scotia. My mother grew up on a farm outside a small village in Ontario. Her brother took over the family farm, while my aunt and my mother went off to university. Again, this was rare for girls from small villages to go off to university. Growing up, our parents never said to any of their children, “if you go to university.” It was always, “when you go to university.” So, the three of us understood from a very young age that we would be well educated. We went on many family car trips, mostly down east to see the Strickland family, in Cape Breton. My mother would bring along a large binder to share facts and history about the places we would see. We went to museums, historical sites and toured mines. My mother liked to tell the story of one family trip to the science centre about an hour from our home. As my mom told the story, when my dad was looking at one of the displays, he called me over to him and said I am going to want to see this because this is the future. It was a laser. So according to Strickland family lore, it was my father who introduced me to what would eventually become my life’s work.  I was incredibly shy in high school. I had a few very close friends, but I wasn’t very well known in the high school. I remember one day when I was asked if I was Edith’s daughter, followed by someone else asking if I was Rob’s sister and finally someone else wondered if I was Anne’s sister. I didn’t seem to be known by anyone as me. Even though I enjoyed math and physics I always felt it made me seem very nerdy to the other students. I remember when I won the school’s prize for the highest mark in Grade 11 physics, I dreaded going up on stage to receive the award. I thought it would label me a supreme nerd. It turned out to be a learning moment for me. The other kids were wonderful about it and said things like it must be nice to be so smart. While at high school I enjoyed being in the school band, even though I really couldn’t play the clarinet. I also enjoyed the outers club, which went on camping trips. The rest of my family didn’t really like to rough it, but I enjoyed camping. Winter camping was my favourite because you didn’t have to deal with the millions of mosquitoes that were present in the spring, or spiders in the tent.  While I was in high school, my dad received a diagnosis of terminal spinal cancer and was told he only had a year to live. I was spared from knowing this devastating news at the time. My mother realized that she would have to be the breadwinner for the family. She felt very fortunate when a teaching vacancy opened up at the high school where she had taught before getting married. This high school was my high school, so we were both at GCVI when I was in Grades 12 and 13. My father was not a quitter. He researched his own disease and asked his doctor about a new radiation treatment he’d read about. The physician wasn’t optimistic, but my dad wanted to try it. He was one of the lucky ones and the radiation therapy worked. My mom kept her teaching job, since it would be a few years before they would know if the remission was permanent. We had my dad for another two decades.  With my mother’s paycheque, my parents were able to afford a cottage on Lake Huron after my father sold the family home that he had inherited in Cape Breton. The family has enjoyed many summer weekends together up at the cottage ever since. You can see the family photo taken there in 1985, the year I published the paper that earned me a Nobel Prize.  So many times in my life I heard my mother talk about how she wished she had gone into science or mathematics. She was always sure that she would have found university easier taking the subjects that she had been very good at while in high school. People discouraged her when she was young because they felt women just didn’t go into those disciplines. I think hearing her story of regret made me determined to pick a career in a field I was good at and enjoyed, regardless of what others thought or said. I was very clear in my own head about where my strengths were and what I wanted to do.  I would like to acknowledge my homeroom teacher, Jim Forsyth, who was also my physics teacher in Grade 13. When I returned to Canada as a faculty member at the University of Waterloo, he read that I had developed chirped pulse amplification. He contacted me through my mother asking if I would be willing to be placed on GCVI’s wall of fame. I wasn’t sure that I belonged on this wall that included John McCrae. Jim said that he wanted to have a female scientist on the wall as a role model for the female students. I agreed to his request and he made it happen. I have been on GCVI’s wall of fame for two decades for the development of CPA. They recently have rewritten the citation to say that I have received the Nobel Prize for CPA. Now it doesn’t seem so strange for me to be on GCVI’s wall of fame.  I decided that when it came time to pick a university, I would go where my closest friends were not going. My best friend was going to take engineering at the University of Waterloo, where I teach physics today. My sister was already there. I knew if I went there, I’d stick with them and continue as I had been. I wanted to get over my shyness, and going away would force me to meet people and stand on my own.  While deciding what to take at university I couldn’t make up my mind whether I wanted to pursue engineering like my father and my sister or if I should take physics, which I thought would be more fun. I looked over the course calendars of all the Ontario universities and I found that McMaster University had an engineering physics program. I decided that program would let me walk the line between engineering and physics. When I saw that one of the engineering physics programs was about lasers and electro-optics, I just thought what fun it must be to study lasers. That clinched it for me and off to McMaster I went. I made wonderful new friends at MAC. My first-year roommate, along with another pair of roommates from down the hall in the first-year residence, recently took a trip to Nashville together to celebrate turning 60. I was actually only turning 59, but I didn’t let that stop me from joining the birthday bash. It was this same group of girlfriends who helped me shop for the gowns and dresses I needed for Nobel Week. I graduated from McMaster in 1981 with a Bachelor of Engineering degree. Unlike the woman who had graduated in 1959, I was not the only woman in engineering, and in fact there were three women who graduated in the discipline of engineering physics that year.  Even as a child, I knew I belonged in school and decided I would get a PhD, since I was told that was the ultimate in education. Towards this goal of going to graduate school, I took a research job at McMaster the summer after second year. I worked in the laser group supervised by Brian Garside. I had two different research projects in two different labs. The labs in the subbasement of Burke Sciences Building were undergoing renovations and the air conditioning had been turned off. This was a very hot summer and the men were working with their shirts off. I didn’t see that I wanted to do this too, so I decided that I would spend most of my time on the other project in another building. I was to try to melt germanium onto gallium arsenide to make a p-n junction to be a fast optical detector. I had to clean out an old evaporator, including cleaning and fixing a diffusion pump. It took me most of the summer, but I did get to try to make one sample. To test it I had to silver paint some electrical connections to it. I have shaky hands and the small test sample flew out of my tweezers onto the floor littered with solder drops that all looked the same as my device. I was down on my hands and knees looking for it when one grad student came in and asked what I was doing. He got down on the floor with me and started looking. One by one as the men came in and asked what we were doing they each got down on the floor with us and looked. We found it! I wondered if the guys would do this for any summer student or did it help my cause that I was the only woman in the group. I remain very good friends with the grad students I worked with that summer.  Unfortunately, when I finally tested my device, it was not a diode and my summer project was a bust. Luckily, I had impressed my supervisor anyway and he was willing to write me good references for my next summer job and for grad school. The summer after third year, I worked at the National Research Council in Ottawa with John Rolfe. That year my project was investigating polishing fiber bundles. The group needed a way to get light down a cryostat that did not have windows. I don’t remember being much more successful that summer than I had been the previous summer. I made many fiber bundles, but I don’t remember ever getting to shine light through any of them to see if they worked.  I knew I wanted to keep working with lasers and I asked the grad students where I should go. I was advised by one to consider the two optics schools in the United States: The Institute of Optics at the University of Rochester, or the University of Arizona Optical Sciences Center. I didn’t get through the foreign student admissions at Arizona, but Rochester accepted me and off I went. During my first week at the Institute of Optics, I met a Canadian graduate student who offered to show me around the campus. When he learned that I was interested in working with lasers, he introduced me to Gérard Mourou at the Laboratory for Laser Energetics. I went into his ultrafast laser lab that had a red dye laser pumped by a green laser beam and I thought what fun it would be to work on such lasers. They reminded me of a Christmas tree and Christmas is my favourite time of year. Gérard was not a professor at the time and so he became my research supervisor. By the time I was in third year, the institute realized that so many students wanted to work with him, that he should be a professor and so I became one of Gerard’s first PhD students. He is also the person with whom I now share half of the 2018 Nobel Prize in Physics.  While working in Gérard’s group I met my future husband Doug Dykaar. He was a graduate student in electrical engineering. The group he worked with used the ultrafast lasers in Gérard’s lab. As Doug describes our first meeting – the lights were off, except for the flashlamps that pumped the infrared lasers – anyone else would think it was romantic. Optics labs are often dark to help us see the laser beams. Doug and I became good friends one week when we worked together to the wee hours of the night on a project about electro-optic sampling with infrared light. We used my laser with Doug’s electro-optic sampling apparatus. Doug and I share a fondness for dessert and a great new dessert place had opened up near the lab. We quite often took a break in the afternoon and went and ate cake together. We didn’t actually start dating until five years after that first meeting, but we think the group thought we were dating long before that.  Gérard gave me a theoretical paper by Stephen Harris of Stanford University. It was about high order harmonic generation that could yield coherent radiation out in the extreme ultraviolet. Lasers do not work at these high frequencies. I decided that it looked like a good PhD project for me to work on. To make it happen, we would need a more intense laser than Gérard’s group had at the time. At first, I tried pulse compression of a high energy laser, but it didn’t work. At the time, it wasn’t possible to increase the intensity of the beams without damaging the laser itself.  Gérard came up with a new idea that we now call chirped pulse amplification, CPA for short. The simple idea that Gérard came up with was to stretch the pulse by chirping it. That means the frequency changes through our laser pulse. In a bird’s chirp, the sound frequency changes in time through the note. It was the stretched, chirped pulse that we would amplify.  It took me about a year to build the amplification system. To get a lot of energy into a short pulse, we needed to first stretch it to make it a long pulse, amplify the long pulse and then compress it. I want to thank Marcel Bouvier for helping me with this project. Gérard had managed to get an old laser amplifier that I could use for the project. The electronics needed to be fixed and Marcel was the electronics engineer for the group. Marcel had also developed a new type of Pockel cell driver for the lasers in the group. I used one of these Pockel cells in the regenerative amplifier I built for the CPA system. Marcel went on to start a company to sell these Pockel cells, and I still use one of his Medox Pockel cells to this day in my lab at Waterloo. I also want to thank my friend and colleague Steve Williamson. I did not have any way to measure the duration of the amplified compressed pulses. Steve had a streak camera that would work. He brought his streak camera into the lab one night and together we measured the pulse duration. CPA worked. The amplification process did not distort the pulse chirp and we had a short intense laser pulse.  To answer a commonly asked question, I never wondered at the time if I would be awarded a Nobel Prize for this. I was just trying to do one of the world’s best PhDs. That was my goal. We were quick to publish our results. We wanted to be sure we were first, so submitted to *Optics Communications,*a journal with a relatively quick turnaround. When I finished writing, the article came to only three pages in length. I simply wanted to state the idea, explain the laser details and give the data. *Compression of Amplified Chirped Optical Pulses*by Strickland and Mourou came out December 1, 1985. It was my first published paper. It was awarded the Nobel Prize in Physics 33 years later. At the time we received the award, the paper had more than 4,000 Google citations, with about 200 new ones per year. I would like to note that during the first few years after publication, it wasn’t that highly cited. Large laser systems take time to develop and were done mostly at the large labs. CPA didn’t really become mainstream until titanium doped sapphire was developed. This allowed academic researchers with small labs to build CPA systems. Then the field took off.  CPA formed the basis of my PhD, but I had to do a scientific study with it for my PhD. The high harmonic generation was going to be too difficult to finish in time. For a six-month sabbatical, See Leang Chin from Université Laval in Quebec came to Rochester to study multi-photon ionization with the CPA laser. Gérard agreed that I could work with See Leang on the ionization studies for my thesis. In the end, there was a group of us working on this project that included a new student, Steve Augst, a new professor David Meyerhofer and a theorist Joe Eberly, who was a professor in both physics and optics.  Long before finishing my PhD, I knew that I wanted to be Paul Corkum’s second post-doctoral fellow. His first post-doc had been a grad student at McMaster when I was an undergrad with the group. Paul worked in the laser and plasma section of the physics division of the National Research Council in Ottawa. Paul was already considered to be Canada’s leading expert in ultrafast optics. Now he is renowned for his development of the new field of attosecond science, but he didn’t get to that until after I worked with him. I took so long to get my PhD that I almost lost the opportunity to work with Paul. He called me in January 1988 and told me that if I could promise to be done by the end of the summer, he would hold the job for me. I was so happy when See Leang showed up and suggested that we all work together on multi-photon ionization. That helped me keep my promise and I was able to join Paul’s group in September 1988. Technically I did not graduate with my PhD from Rochester until February 1989. With Paul, I worked on the link between continuum generation and self-focusing. I still find continuum generation to be like magic seeing all the colours appear out of nothing, even though I helped discover why all the colours were generated. After that we worked on Coulomb imaging of iodine. It was a wonderful three years. I tell everyone that doing a post-doc is the best. You get to do research full time. While at grad school, you are busy doing classes and worried about things like qualifying exams. As a professor, you are the one responsible for finding the money to do the research, while also teaching and doing committee work. As a post-doc, you have only one responsibility and that is to have fun doing research.  Doug and I were married in 1991 right after I finished my post-doc. After leaving Rochester, Doug was lucky enough to have found a job he loved working at Bell Labs in New Jersey. I wanted to find a science job in New Jersey so I could live with my husband, but I didn’t manage to find one. I took a job at Lawrence Livermore National Laboratory in California, where I again worked on developing new CPA lasers and also using the 10TW CPA laser that they had in the group to finally study high harmonic generation. My supervisor at Livermore was Mike Perry. I worked on developing CPA in chromium doped LiSAF with Todd Ditmire, a fantastic young grad student. Todd went on to build the second Petawatt CPA laser as a young faculty member at the University of Texas. I worked with John Crane on the harmonic generation.  I had a fun year at Livermore, but I wanted to live with my husband and working at a lab in the United States as a non-citizen is difficult. Since Doug had a dream job, I kept looking for a new job in New Jersey. I took a job as a member of technical staff at Princeton University’s Photonics and Electro-Optic Materials Center. I worked with Warren S. Warren there and Stephen Forrest, who was the director of the center. It turned out to be a blessing that I had this position. I went through both of my pregnancies while on this job. Pregnancy made me nauseous morning, afternoon and night. I would have found it hard to teach as a faculty member during this time. I somehow managed to get my job done even though I did spend quite a bit of time in the sick room.  Doug and I have two children, Adam and Hannah. They are both adults now and the best of friends, despite being quite different in their temperament and interests. They have always been close, ever since they were tiny children. Doug and I always encouraged them to follow their interests so long as they are doing what makes them happy and that plays to their strengths. Hannah seems to be following in her parents’ footsteps. She is currently a grad student studying astrophysics. Adam has taken his own route and is studying comedy writing and performance.  Doug and I continued to both look for academic jobs. The University of Waterloo was the first university that offered me a job. I started there in 1997. Since I had followed Doug to New Jersey, he agreed to follow me to Waterloo. He took a job in industry. I set up an ultrafast laser group. Teaching is rewarding and I enjoy sharing my excitement about physics with budding scientists. In addition to teaching and conducting research, I believe strongly in serving the scientific community. Among my professional activities, I was president of the Optical Society of America (OSA), and was on the board of the Canadian Association of Physicists as the director of Academic Affairs.  The early-morning call from Sweden on October 2, 2018, changed my life. I am grateful that the Royal Swedish Academy of Sciences recognized my work and I feel so honoured to be among the many eminent scientists I join as a Nobel Laureate. With one phone call my work is out in the world more than ever before, my name is mentioned in the same breath as trailblazers [Marie Curie](https://www.nobelprize.org/prizes/physics/1903/marie-curie/facts/) and [Maria Goeppert Mayer](https://www.nobelprize.org/prizes/physics/1963/mayer/facts/), and I have had dinner with royalty. I will use the platform the Nobel Prize affords me to continue to advocate on behalf of science and the many scientists who devote their lives to probing the fundamental questions. |
| **Autobiography** |  |
| **Podcast** | **0020=Strickland**  **No scirpt** |
| **Telephone**  **interview** | **0020 = DS**  Donna Strickland: Hello.  Adam Smith: Hello, is this Donna Strickland?  DS: Yes it is.  AS: Hi, this is Adam Smith calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  DS: OK.  AS: First of all, congratulations on the award of the Nobel Prize.  DS: Thank you very much.  AS: What was your immediate thought on hearing the news?  DS: Well, obviously, I think like many people  said we wondered if it was a prank. I knew it was the right day – it would have been a cruel prank, but that is what I was thinking.  AS: Were you sleeping when the call came?  DS: Yes, 5 in the morning!  AS: It’s fairly amazing, I don’t know if there’s another case of somebody being awarded a Nobel Prize for their very first scientific paper. That’s an extraordinary …  DS: No, I don’t know that either. [Laughs] No, I know, I sort of feel like I just lucked into this whole thing. I have to say that. Obviously I did luck into the whole thing!  AS: You must have been a naturally gifted experimentalist. Where did it come from do you think, the gift?  DS: I don’t know, I mean it was … it was just a fun thing to do, and so I enjoyed putting many hours into it. It is the one time in my life that I worked very, very hard! And … but … you know, it was a fun time in the field of short-pulse lasers, and it was a fun group to be in and … I don’t know, I put in the long hours and it was fun most of the time. Most of the time!  AS: It’s the classic vision of the experimental physicist, sort of tinkering around in the lab.  DS: That’s right.  AS: And, of course, you are the first female laureate for 55 years.  DS: Yes, and Goeppert-Mayer, yes, I couldn’t think of her name.  AS: That’s right.  DS: Marie Curie everyone comes to, but I was thinking “Oh, I even quoted her”. I cited her work in my thesis.  AS: Right, that’s a nice connection.  DS: Yes.  AS: What message do you think it sends to people?  DS: I don’t know how to answer that because I’m not a woman whose been looking at these prizes thinking “Why isn’t there a woman?” I sort of haven’t thought like that, but I know a lot of people, I guess, do look at that. I don’t know, I hope … I mean I certainly tell the Maria Goeppert-Mayer story and I’m happy that life isn’t like that. I’m glad there were trailblazers like her and Marie Curie.  AS: That’s right, because she …  DS: She didn’t get to have a paid job for the longest time, right? She didn’t really get to be recognised as a scientist, even though, you know, she was doing incredible work, right. I think she won hers in the 50s, right, and yet …  AS: It was ’63 she was awarded.  DS: ’63. And the work I cite for her started the whole field of multi-photon ionisation, which was the first thing that our laser was used for in my thesis, and that work was done in 1939. It’s not what she won the Nobel Prize for, but again it totally changed what could be done, right?  AS: Yes.  DS: And you just think “How can you go more than 20 years?” You know, I don’t know, that was sort of, not to get a Nobel Prize, but just even be recognised as a scientist. So I think things have totally changed. So I think it will come around and change.  AS: Yeah, yeah. I suppose one thing that is going to happen is that you’re going to be very much in demand. How do you feel about that?  DS: Yes, I’m a little scared about that because I was talking to a previous Nobel Prize winner a few years back and he told me how he flew 100,000 miles, you know. And scientists don’t even get to fly first class so that’s hard. [Laughs] Like in a year, like you know, and my goodness that’s a lot! So, you know, I don’t know.  AS: Well maybe put in your bid early if you want to go first class. Yes, well anyway, I guess …  DS: I’m not sounding like a Nobel Prize winner at this point I realise!  AS: I think you sound exactly like a Nobel Prize winner at this point. This is a pretty surprising point to be at, I mean for anybody. It’s a … you only hear just a few minutes ago. Let me ask just very briefly about ultra-fast lasers – what’s your favourite example of what they can do?  DS: OK, well right now I’ve taken over doing the undergraduate labs and I’m trying to think of, you know, what was the most exciting thing for me to see, OK? So you’re probably thinking bigger things. I just think white light generation is just one of these remarkable things to see, and actually, you know, one colour of light goes in to just water or any clear anything and out comes all the colours of the rainbow when the pulses are short and intense enough. And it’s just remarkable to sit there and go “What? Where do all those colours come from?” It’s not that we don’t understand that in science, but it’s one of those things that’s just really cool to see.  AS: Well, it was good enough for Newton if you see what I mean.  DS: Yes, but it’s not like, yeah, it’s different than Newton seeing all the colours that are in white light. We actually just start with a very narrow bandwidth and create all of those colours through the non-linear interaction with the medium.  AS: Gosh, that is magical, yes.  DS: It is magical and, you know, it took many years for scientists to even figure out what was going on, which is also always a fun, you know, thing for scientists to do. That’s what we like to do, is puzzle as to why something is working. But it’s, you know, it’s useful. White light generation is used also everywhere, so …  AS: As you say, what a wonderful demonstration of what this prize is all about – a laser, light and matter interaction.  DS: That’s right.  AS: Yeah. Well, goodness, we’ll learn a lot more about this when you come to Stockholm in December. We very much look forward to welcoming you.  DS: Well, thank you very much.  AS: It’s a joy to speak with you and I very much look forward to meeting you in December.  DS: Thank you, and I certainly look forward to getting there. [Laughs]  AS: Well good luck with what will undoubtedly be a very busy day.  DS: Yes, thank you very much.  AS: Bye bye.  DS: Bye bye. |
| **Interview** |  |
| Q3 | **How did you become interested in science?** |
|  | Donna Strickland: I have been asked that question about how I got interested in science quite a bit. I think it just is my education system. It’s hard to say, I mean, my dad was an engineer and my mother was a high school teacher and so I think education was very big for both of them, so they did take us to things like the Toronto Science Centre and they did take us to these Audubon lectures about birds. And I have to say, it did not spark any interest in birds with me so I don’t know … But not everybody gets raised by parents that are as interested in education and studies that way. But I think it is mostly because in school, I did very well in maths and science. I was one of those kids that thought science class was fun and most people don’t, so, I don’t know why, I just did. |
| Q5 | **Did you have a teacher that particularly inspired you?** |
|  | Donna Strickland: In high school I think had very good science teachers. There was Ms Bowman in grade 9 whose husband was a chemistry professor and she would spend her summers in her husband’s lab learning the latest in chemistry. I had her for grade 9 science and she would like to have things exploding in the front of her room and she was wonderful in a lot of ways. I had her again for grade 12 chemistry, and she brought in chocolate bars for us one test I had to miss because I was in the high school band, we were had gone to England during the one test. And she just “Before you start you must have your brain working”, and so she gave each a chocolate bar to get our brain working. But also, I must say, Jim Forsythe, was my physics teacher in grade 13 and I was in his homeroom for the entire five years of high school. So, I sat in the physics room, every morning to start the school day. He also is the one who put me on … I’ve been for twenty years on my high school wall of fame and it was my high school physics teacher that did that for me. |
| Q12 | **Did your mother have an interest in science too?** |
|  | Donna Strickland: My mother was raised in a small Canadian farm town, she was raised on a farm. She had an older brother and an older sister. The brother inherited the farm and did not go to university, but the two girls and this was back in the 1940s, were both sent to university. The oldest one had thought about going into medicine but switched into nursing and my mother wanted to go into maths and sciences, that was what she was good at. But before her sister went ahead of her, and my mother got advised that as a female she would find going into the sciences is very hard and that she would be better served by going into the arts. So my mother listened to these people, because coming from a town of one thousand, she just felt overwhelmed about the idea, and so she chose to go then into the arts and then regretted it ever after. And then again, I was raised by a very strong woman who made it clear that she wished she had followed her heart and soul and not listened to everybody else. |
| Q6 | **When did you first encounter your fellow female Physics Laureates, Marie Curie and Maria Goeppert Mayer?** |
|  | Donna Strickland: I think [Marie Curie](https://www.nobelprize.org/prizes/physics/1903/marie-curie/facts/) is just one those icons that you hear about so I can’t possibly tell you when I would have first heard of her. [Maria Goeppert Mayer](https://www.nobelprize.org/prizes/physics/1963/mayer/facts/) I didn’t even know was a woman when I cited her in my thesis. I just knew of, you know, Goeppert Mayer’s work and it is very funny that I actually said ‘he’ in my thesis, that is how uneducated I am and one of the people reading, the theorist who would help me at the end, he enjoyably stroke it off and said, “Donna, shame on you”. |
| Q11 | **How has being a woman in science changed over the years?** |
|  | Donna Strickland: I think since Maria Goeppert Mayer has won that there are many more women in science than when she was there. I think the fact that she didn’t get a paying job as a scientist until, I think, the 1950s, when she did her Nobel Prize winning work, is amazing. So, I have been payed all along. I never had a female professor, not as an undergrad, not as a grad student and now there are six of us, I believe, in my department of 40. So that is still only, you know, 15 percent and it is not as high as it should be, but the fact that there are even six women instead of zero is quite a change. So, I think things are changing for women all the time, certainly. I grew up in the 70s and we were all about women’s lib and I remember being told over and over again: “Women, you can do anything”, so it never entered my mind that I couldn’t. |
| Q11 | **How can we encourage more women to do science?** |
|  | Donna Strickland: I think everybody in the world should be told that do what you love and what you think you are good at. I don’t want to really distinguish men from women, I think there are jobs that are stereotypically women jobs and I don’t see any reason that men shouldn’t do those. I don’t think women are more caring than men and yet we hear, you know, so many jobs are for women because they are the more nurturing and I think that’s just as wrong on the other side. So, I think every person should be exposed to every type of field and find out what they are good at and everybody should get to do what they are good at and what they love to do. |
| Q16 | **What advice would you give to the younger version of you?** |
|  | Donna Strickland: I feel like I was born under a lucky star and life just worked out for me. Then I will tell other women who ask me this, because people are always wondering when should you have your children, and I said, you should just do everything when it feels right for you. There is no one life plan that works for everybody. Because my husband and I the first year of marriage actually lived biocoastal – so that we both could have good jobs – that got old fast and so I did give up on the academic track and I took a job as a member of technical staff and it turned out that is was perfect for me because I had my two children. I went through my both pregnancies on that job and I was someone who was nauseous all nine months, all day long and I realized that when I got my first teaching job, which was right after my second child was born, I thought I could not have lectured through those two pregnancies. I could have not stood in front of a classroom, I was so ill trough all those two sets of nine months. So, to me, my life just keeps working out. I thought, maybe I was giving up on my carrier, but I hadn’t and someone at the University of Waterloo was willing to hiring me – after four years being out of the academic track. So, this is what I always tell women, you have to do what is right for you at the time is right for you and hope that life works out. I don’t know, maybe I am the only one born under such a lucky star, but my life seems to keep working. |
| Q16 | **How has your career been impacted by being married to another researcher?** |
|  | Donna Strickland: I think this is the toughest thing about being a female scientist. I think once we become 50/50 women and men, then it won’t be quite the same. But almost every female scientist is married to a male scientist. Male scientists can’t be married to female scientists, there is not enough of us to go around for them. And so, it does bring in the two-body problem. And most couples have to find jobs together, but many other jobs exist in almost every city whereas science jobs only come up rarely, here, there and everywhere. So, if you want to do the type of science you want to do, you have to be willing to look worldwide and so it is very hard for two scientists to find jobs together. And I must say, my husband followed me to Canada and took a job with industry it’s not, wouldn’t have been his choice either. And so, we each took our turn. As he says he has now been 22 years living in my country, near my family, with me doing the job I want to do and I was only four years in New Jersey, near his family, in his country with him doing the job he wanted to do. So, he kind of keeps asking when it is his turn – for me to go back and follow him. |
| Q11 | **Why is diversity important in science?** |
|  | Donna Strickland: I think diversity is a good thing whether you are in science or in anything.  Again, it goes along with not only should every gender be exposed, every person has the right, it shouldn’t be a privilege, it is a right to do what you want to do, and what you are good at and I think, and I’ve said this before, the world works best if we all do what we are good at so that no group of people should ever be discouraged from doing what they are good at- the world works best if we all get our opportunities and we all do it. |
| Q9 | **How does it feel to be awarded the Nobel Prize for your very first paper?** |
|  | Donna Strickland: It is unusual that I am winning Nobel Prize for my very first work. I, you know, call it a one hit wonder quite a bit of the time when I give talks and people always want to hear about this one paper rather then my newer work. It’s unusual, most of the time I think supervisors get the credit for the work, and I also think that Gerard Mourou, my supervisor, deserves more credit, maybe, for this prize than I do, because it was his idea. As the student, it’s my job to take the idea and make it a reality and so a lot of work goes into that. People over the years have said that the students should be credited with the fact that though you are given one grand idea, you still have to figure out all the other details to make the idea work. So, everybody should be rewarded, I think possibly, I am one of the people that are getting rewarded as a student because it was as an individual project and so, there were just the two names on the paper as opposed to a large group of names and therefore, when it is a large group of names, one person represents that group and that one person would have to be the supervisor, so … |
| Q8 | **How did you find your time as a PhD student?** |
|  | Donna Strickland: When I went to graduate school, I really went with the attitude that I wanted to do one of the world’s best PhD’s. Now, I didn’t think I was unique in that, I would think that every student going for PhD must hope to do one of the world’s great PhD’s. I don’t think anybody goes to do a PhD for society, to say great job. I think there is many other jobs where your parents are telling you to be a doctor, be a lawyer, there are things, at least, in North American culture which are held up on a pedestal and science isn’t one of them. I think, you go in and do a PhD in science because you just love it and want to do it. And if that is why you are doing it, you just want to do the best. So, there was Gerard’s group, or just a group of fabulous students, and we were all there trying to do the best. I think we all urged each other on and we all helped each other but there was also that competitive spirit in all of us and we all knew the other one was working hard, so we must work hard, but as well when anybody struggled we all figured out a way around each person’s struggle so that they could keep moving forward, so it was, even though I am the only one getting credited, it was quite a team effort. |
| Q2 | **How important is collaboration in science?** |
|  | Donna Strickland: I think that science is a team sport. I think that you have to talk to somebody else about it. It helps you to form your ideas and it also will spark something, especially if you are in a dead end trying to figure it out, there would always has to be these conversations back and forth. Whether it is from student to supervisor, student to student, student to anybody, from scientist to scientist – it just helps to talk about what you are doing. |
| Q3 | **Do you enjoy mentoring students?** |
|  | Donna Strickland: Of course, it’s fun. I particularly like to having students in my group and talking to them and trying get them to see it from their own perspective and get them to the point where they are telling me the new things and that I can learn from them. That is always the goal of every supervisor, to bring your student to the point where they’re letting you know what I should already know, so that’s the joy in it. |
| Q1 | **Do you have a favourite piece of advice for students?** |
|  | Donna Strickland: No, I don’t, I don’t think I have one piece of advice to give to a student. I think each of us is unique and each of us sees it from a different point of view and so, what one person needs, another person doesn’t need. I will tell you though, that quite a long time ago, before I even came to Waterloo, there was a student who was to give a talk, a female student, to give a talk. And this female student was particularly nervous and I pointed out, I said, this is one of the challenges that I think women have to overcome, that boys at the time certainly were raised to have bravado, boys are raised to say “Look what I did”. Boys, whether it’s through sport or anything, they are to stand up and almost be beating themselves on the chest saying, “Look how good”. Women are raised to stay quiet and demure and women are raised to not show off. And I was given this advice early on as well, I said: “No, you must learn that to get your PhD you must be able to stand up and give a talk. And if you’re giving the talk about your research you are basically saying ‘Look at me, this is what I did’”. Now, you don’t say it in those words, but this is why it is harder, quite often, for the females to get up and give effective talks because they want to always be so humble about it and they have to get over that. |
| Q3 | **Is it important for science to be fun?** |
|  | Donna Strickland: I personally think that, since you spend so many hours a day at work you should, if you have the chance … Unfortunately a lot of people have to work just to get a pay-check, but scientists do not. Scientists are doing it – they could do something else that would make the more money, so I think if you don’t love what you are doing, you are not going to be doing it. That’s all, I think. You can call it fun, you can call it excitement or you can call it enjoyable – I call it fun. |
| Q3 | **Why do you enjoy science?** |
|  | Donna Strickland: For me the fun of science is that even though there’s a lot of hard work to get an experiment to happen and then you can spend … The one that I am being awarded for took a year of work to get done. But there is something so exciting about data being shown on a screen or a laser that you have been trying to struggle to make work, finally it works, and you see it. And laser physics is one that you actually see happening. There is all kind of things when you see one colour of light change to another colour of light or what have you that’s exciting to see. And brand-new data that you know you have, and other people don’t have – it’s such an exciting feeling. That’s why, I just think it is fun. And I like building, I have said before, Lego was one of my favourite toys growing up and so building lasers, falls under that category of feeling like a toy, you’re simply playing with a new thing. |
| Q4 | **What application from your discovery are you most proud of?** |
|  | Donna Strickland: There was only one commercial application of this laser jet and that is cutting or machining of transparent material, it can be glass used on a cell phone, it can be the cornea of your eye. That’s the one application and I was actually quite surprised that started just 10 years later after the laser was built. It’s usually takes a little bit longer from a research tool in the lab to become an actual product, so I am surprised by that. But I think going forward, the exciting one would be if we can actually get laser acceleration to work. There is a number of people working on laser acceleration, I myself not at this point, but I think if  can start to either compete with the hospital accelerators so it could be used for medicine or even the large CERN – that would be the exciting one I think. |
| Q18 | **Is it important for scientists to do work that impacts society?** |
|  | Donna Strickland: No, I don’t feel that the work you do should be directly applicable to society. Again, it falls under my idea that we should all do what we’re good at and I don’t think everyone of us is good in taking an idea out to there or knowing what society should do, but again it goes back to the facts that the scientists have to constantly have dialogues and so it’s a whole train of people. And it doesn’t matter if it is something to be used for society or just an application for whatever. It’s a nonstop continuum people working together. So hopefully there’s other people that are looking at this who want to help society and work back to find the right things. |
| Q9 | **How did it feel to discover you had been awarded the Nobel Prize?** |
|  | Donna Strickland: I think it’s a shock, it’s a stunning shock to be woken up to be told that you are Nobel Prize winner. Again, this work was done over thirty years ago so it’s not something that I am living and breathing every day that I did pulse amplification for my PhD all those years ago. No one is expecting, in my position, to win a Nobel Prize. So you get woken up at 5:00 in the morning. My husband is closer to our landline phone that is in our bedroom and so he answered the phone. And certainly, in the middle of the night when you get woken up, it is usually a fear thing, that someone has been hurt. So, I’m saying “What is it, what is it?” and he goes “They’re asking for Professor Strickland”. So, then you kind of know that no time am I called ‘Professor Strickland’, even usually at the university and then this is an important call from Sweden and then you are put on hold, although actually they hung up on me. But I thought I was on hold, I’m hanging on to this call, it’s from Sweden and it’s October 2nd and I think I won the Nobel Prize, this can’t really be true. So that’s all it feels like, this can’t be true. |
| Q12 | **How did your friends and family react?** |
|  | Donna Strickland: So many things. My daughter … I texted four people before the press conference and after I’d received the call. I texted my brother, my sister, my son and my daughter. I knew that my brother and sister would get it right away. My sister was in Europe, so would be the daytime and my brother wakes up. I know my two kids were not get it right away and the funniest one was my daughter, who unfortunately had her e-mail hacked in the summer before this, so when she got the text saying, all I said was “I won the Nobel Prize”, she goes, “Oh, mom’s phone had been hacked”. And even her girlfriend called her and said “Congratulations on the Nobel Prize”. And she goes, “No, my mother’s phone has been hacked”. She had to be told it’s in the news, so then she googled, and she was just, she was in tears, screaming at me in the phone call. So that was exciting. I think at my press conference at the university the first day my entire department showed up and the former chair of the department who I was associate chair under, stood up to congratulate me on behalf of all of them. He almost had me in tears because that’s when I really looked, I think I was like a deer caught in the headlights with so many cameras on me. I had never faced such a thing before and when I looked at each face, they were just all beaming at me. It was almost too much. |
| Q9 | **How has the Nobel Prize affected your life so far?** |
|  | Donna Strickland: My life got turned upside down with winning the Nobel Prize. I now realise, I don’t think at the time, I realised how reclusive I was and how much I enjoyed spending time all by myself until I found out that was not something possible after winning the Nobel Prize and that all of sudden, people want my autograph and people want to take selfies with me or, you know, like I said everybody in my department, now when they see me, a big smile comes on their face and yet, it’s like really ‘cause you have known me for so many years’. So things are very different for me, I kind of hope they go back to just that normal way. I don’t think people can live on this height of excitement for very long, but it has been quite different for the last two months. |
| Q7 | **What qualities are needed to be a successful scientist?** |
|  | Donna Strickland: A scientist needs, first curiosity, they need to be excited about trying to learn something different. They have to be able to communicate well to be a good scientist, because you do have to talk back and forth so that you get these new ideas. I think an experimental physicist needs incredibly patience. And so, some students try it for a while and realise they simply don’t have the patience to do that kind of work and then of course things break, and you have to start again, and it can lead to a lot of frustration. So, you have to have that kind of patience, but again, if it’s work you enjoy it is not as hard as it is although frustration is still hard to deal with. |
| Q4 | **How do you deal with scientific challenges?** |
|  | Donna Strickland: I’m somebody who knows what I am trying to get to at the end of my project, so it doesn’t matter how many hurdles get thrown in. Soon it’s another hurdle one must figure its way around, it but that is a job of a scientist. Usually the hurdles are a technical challenge or a scientific obstacle that you weren’t expecting, but the that is was science is about. If you already knew how everything worked, you wouldn’t have to do the project to start with. So, each one is a new puzzle and you have to … To me I just think of it like a puzzle rather than a problem, but then I also like to work on projects that not very many people are working on rather than being part of the race. So, this is why I have not worked on chirped pulse amplification (CPA) as much. I have two assistants in my lab, but I look for projects that I find really fun to do, but the whole rest of the world aren’t necessarily doing them, so I don’t ever feel that I am in a time race. |
| Q4 | **Can you explain your Nobel Prize-awarded discovery in 30 seconds or less?** |
|  | Donna Strickland: In 30 seconds might be very hard. This is a way to make sure that you can have very intense pulses and the idea is like, I call it, a laser hammer and that if you can push on a nail with all your might it won’t go into a piece of wood, but if you hit it with a hammer quickly it goes in. And this is the idea that we can we make our laser pulse both energetic and short, but we didn’t have the hammer inside the laser destroying the laser**,** which was why it never worked before. But we can do it in a way that the laser was fine and then make the pulse short after amplification so that could be like a laser hammer. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0021** |
| **Biographical** | My father came from a well-off German Jewish family in Berlin with connections to the Rathenau family that had begun the Allgemeine Electrische Gesellschaft (AEG). As a young man he had become an ardent and idealistic communist. After finishing medical school he worked in a communist workers’ hospital as a neurologist in Berlin. My mother came from a Protestant family of government workers and lawyers on the Rhine near Koblenz. She had come to Berlin with ambitions to become a professional actress. I was born on September 29, 1932 in Berlin to this unlikely and unwed pair.  At the time Berlin was balkanized into sectors that were Communist, Nazi and Weimar. My father had gotten into difficulties with Nazis infiltrators at his hospital and had been taken “prisoner” by a Nazi gang. My mother’s family, with still some connection to the civil authorities, managed to get him released and sent him to Prague, Czechoslovakia. After I was born she joined him in Prague. It was not a convivial relationship even though they had gotten married and had another child, Sybille, by 1937. A critical moment came in September of 1938 when Chamberlain gave Sudetenland to Hitler and effectively opened Czechoslovakia to the Nazis. We heard the decision on a radio while on vacation in Slovakia and joined a large group of people heading toward Prague to attempt to get a visa to emigrate to almost anywhere else in the world that would accept Jews. There were not many places to go. We were extremely lucky in gaining the support of the Stix family of St Louis who gave bond for about ten thousand Jews who were professionals to gain favored entry to the United States.  The family came to the United States in January 1939, landing in New York. It took my father about 4 years to pass the New York State examinations to practice as a medical doctor. During those years he worked as a medical aide in a New York City tuberculosis hospital and my mother took counter jobs at department and drug stores. The family stabilized by the late 1940s, with my father becoming a psychoanalyst in the Horney group. My sister became an actress and is now a successful playwright at New York University.  Initially I went to New York City public school but through a refugee relief organization associated with a neighborhood church I received a scholarship to the Columbia Grammar School, a private school in mid-Manhattan at one time associated with preparing students for Columbia University. I started there in 5th grade and remained to graduate as a high school senior in 1950.  Music, science and history were my favorite courses. Mathematics had to be motivated by real problems. In part due to my father introducing me to magnets and batteries, I became interested in electricity and, especially, radio and electronics. By 13, at the end of the Second World War, I could go to Cortland Street in New York and for pennies buy vacuum tubes, transformers, capacitors, resistors and for a few dollars buy complete assemblies such as radar receivers with oscilloscopes, servo controllers for gun mounts, crystal oscillators. All of this war surplus was arriving by the truck load and effectively being dumped into the street. With these components and magazines such as *Popular Mechanics and the American Radio Relay League Handbook for guidance*, one could build ham transmitters, audio amplifiers, even an FM radio. I made pocket change by fixing radios and other broken electronics. During that time, I built an audio system for the school gymnasium and the transmitter for a school ham station, W2ZIQ. A significant opportunity for even grander electronics occurred when a Brooklyn movie theatre had a fire behind the screen and a large number of coaxial tweeter/woofer speakers were destroyed. A friend and I were allowed to unscrew six of the speakers and take them home via the subway. The heat of the fire was not enough to depolarize the magnets nor destroy the tweeters; all that was needed to restore the speakers was to buy new cones and voice coils from the manufacturer.  By 1948 I had assembled a high-fidelity audio system consisting of an FM tuner, a Williamson power amplifier and one of these movie house speakers. At that time the New York Philharmonic was being broadcast live from Carnegie Hall on an FM station. I invited some immigrant friends of my family with an interest in classical music to come listen. They were truly impressed at the sound, it was like being in Carnegie Hall for the concert. They asked, if they paid for the parts, would I make them such a system. That is how a small business began − they had friends and their friends had friends and so on. By the time I was a senior in high school I had more orders than I could handle but had run into a problem that challenged my “street” electronics knowledge.  At the time the very best phonograph records for classical music were being made by the British company Decca FFRR (Full Frequency Range Recordings); they were 78 RPM and made of the smoothest shellac available. Even so the roughness of the surface made it difficult to enjoy piano music due to the high frequency noise generated by the needle running in a rough groove. The noise was tolerable in a fast loud movement but it was all one heard in a quiet slow movement and spoiled the listening, especially in a wideband high fidelity system. To deal with this problem, the thought I had was to make a frequency variable filter that depended on the amplitude of the sound – to reduce the high frequency response at low sound volume and open the bandwidth when the music was louder. I never got the idea to work satisfactorily. It just seemed difficult to vary the bandwidth smoothly in such a way to avoid swishing noises and adding more distracting sounds. I didn’t know enough about filter theory and mathematics and decided it would be useful to learn electronics more formally by going to college.  MIT accepted me as a Freshman in 1950. At the time MIT had a rigid curriculum for Freshmen consisting of what was considered a basis for all scientific and engineering disciplines. The choice of a major was deferred to the sophomore year. I chose Electrical Engineering as it seemed closest to the problems I was trying to solve. The Electrical Engineering curriculum was unfortunately also rigid. All had to first take a course in power engineering and mechanical structures followed by the elements of circuit theory. Electronics and more interesting courses in noise and signal processing were reserved for juniors and seniors. By the second term sophomore year I had shifted into Physics because it had a more flexible curriculum.  Furthermore, vinyl phonograph records with much smoother surfaces had begun to replace shellac records and the problem I was worrying about had been eliminated by this new technology.  The summer between sophomore and junior year I worked for a small-time entrepreneur who had a cost plus contract with the Air Force to design and construct an automatic blood cell counter. The idea was to enable quick triage in the event of a nuclear war with Russia, to save those people who still had reasonable red cell counts and viable white cells to survive. This was part of a national effort of civilian defense to allow misguided military planners to contemplate the use of nuclear weapons, at a time when some Americans who could afford it were being encouraged by our government to build fall-out shelters.  I designed pulse counter circuits with pulse width discrimination. Others were designing a microscope with a rotating stage and a fast photodetector at the optical output which would drive the electronics. I don’t think the project was ever completed. When the summer ended I made a bicycling trip to Nantucket which changed my life.  On the trip I met a girl who literally swept me off my feet. She played the piano, folk danced and had a very sensible attitude to what was important in life. We spent several days together in Boston before she had to go to Northwestern University in Chicago to continue her education. The relation was initially maintained by frequent letters and came to a high point during Christmas when we met each other’s families. After she returned to Chicago and I to Boston the letters were less frequent and I went to Chicago in the middle of the school term to try understand why. To a more world wise person it would have been obvious. She had found a more interesting guy, and I went into what is best described by Schubert in the song cycle “Die Winterreise” as the disappointed rejected lover who could think only of “her” − saw and heard her in the trees, in the waterfalls, the sunsets … The result was I failed all my courses at MIT and had to leave as a student.  In the spring of 1953 I became an electronics technician in the Atomic Beam Laboratory of Physics Professor Jerrold Zacharias in the Research Laboratory of Electronics (RLE) at MIT. I had a union card and punched a time clock. My colleagues were machinists and lab technicians, Frank O’Brien, John McClean, Mark Kelly and a collection of graduate students some of whom were still veterans from WWII. I learned how to machine, do sheet metal work, soft and hard solder, Heliarc weld and design equipment around those things available in metals stockrooms and hardware stores – the art of improvisation in experimental science. The science being done in that laboratory was exquisite. The experiments were looking at the properties of isolated single atoms and molecules unperturbed by neighboring systems. Each atom was the same as the next and it was possible to ask fundamental questions about their structure and the interactions that held them together.  I started by helping the graduate students design and build the electronics they needed for their thesis projects and eventually began working directly with Jerrold on the Cesium atomic beam clock. The laboratory developed a prototype clock with potential of a precision of 10-12 in one second of integration time. The clock was commercialized by the National Company and then became the standard of time for the Bureau of Standards (now the National Institute of Standards and Technology, NIST) and the United States Navy.  Jerrold had bigger ideas. He wanted next to make an atomic clock with about 100 times better precision so he could make a direct measurement of the Einstein gravitational red shift on the Earth. His idea was to increase the observing time of the atom in the region where the instrument translated the internal oscillations of the Cesium atom into a radio frequency signal. In the initial clock the atoms were flying through this region in milliseconds since they moved with the velocity of sound horizontally. His new idea was to make the atoms travel vertically so that the slower ones in the Maxwell distribution would be turned around by the gravitational field of the Earth – they would follow the same parabolic trajectory as a ball thrown vertically. The observation time could become a decent fraction of a second. The concept was called the Zacharias atomic fountain. When the new clock was operating, Jerrold and I would go to Switzerland where we would put one clock in the laboratory on top of the Jungfrau and another one in the valley below and compare their rates by sending signals between them.  Unfortunately, the fountain clock did not work. The first attempt was made in a vertical vacuum system about 3 meters high. Although we were injecting about 1018 atoms/sec into the fountain we saw less than 10 background atoms/sec hitting a detector on the opposite side of the fountain. The same results when extending the height of the apparatus first to 6 meters and finally to 9 meters – there were just no slow atoms in the beam. It seemed the Maxwell distribution was not satisfied in a beam. In 1956 Jerrold began a project to revitalize secondary science and mathematics education in the United States and I had the free run of his laboratory. I did want to understand why the fountain had failed and set up a fast shutter near the source of atoms and a detector at the 6-meter-high point in the upward going beam. I found that the Maxwell distribution was already deficient at 1/3 the average velocity atoms and that there were simply no atoms at 1/20 of the average velocity we were hoping to use to make the fountain. The problem was the copious fast atoms in the beam were hitting the slow ones and throwing them out of the beam.  It is worth noting that now, with the ability to laser cool a gas of atoms, it is possible to make a Zacharias fountain with heights of less than a meter and clocks that can measure the Einstein gravitational red-shift over a height difference of a few cm.  With Jerrold’s help I finished my undergraduate degree and became a graduate student working in the same laboratory. I kept trying to make better clocks. The next idea was to increase the standard frequency using molecular rotation states of light molecules at 50 to 100GHz rather than the hyperfine structure of Cesium at 10GHz. There I had to invent a way to detect all kinds of atoms and molecules rather than just alkali atoms which ionized easily on a hot wire with a work function higher than the ionization potential of the atom. I designed and built an electron impact ionizer with high current densities and a scheme to use the electron space charge to collect and focus the positive ions. The device was able to convert a neutral atomic or molecular beam to a collimated positive ion beam with 20% efficiency. Next I built an electric resonance molecular beam apparatus to use the rotation states of C12O18 as the basis of the new clock with the fancy ionizer as the molecule detector.  While I was waiting for the O18 enriched sample of carbon monoxide to be produced in an Israeli reactor, I worked with Lee Grodzins on a [Mössbauer](https://www.nobelprize.org/prizes/physics/1961/mossbauer/facts/) experiment. The Mössbauer effect had just been discovered, affording a way of measuring fractional energy shifts of 10-13 in a simple apparatus. The idea we had was to test a somewhat zany hypothesis of Finlay-Freundlich (this in the epoch of the also wild but seductive hypothesis of the steady state universe) who had noticed that spectral lines in bright stars were more red shifted than in dimmer ones. He attributed this to a photon/photon scattering (not predicted by quantum field theory) where a photon from a spectral line in the star was reduced in frequency by colliding with the background thermal photons generated by the star. He furthermore made an estimate of the average photon field in the universe and provocatively attributed the Hubble cosmological red shift to this new type of scattering which got called the “tired light” hypothesis. We built a Mössbauer apparatus where we passed the gamma rays through a hot oven to look for a frequency shift. By comparing the Mössbauer line shift with the oven hot and cold we established no frequency shift at a level that would have mattered for the Finlay-Freundlich hypothesis (several years later the experiment was done again with light and a microwave cavity again showing no frequency shift – this was the beginning of precision interferometery, more on this later).  Just as we were finishing the Mössbauer experiment, I was told that I had exhausted the funding for a PhD candidate and that I had to finally do a PhD thesis and graduate. Furthermore, my wife had become pregnant and a real income had become more important. (My wife, Rebecca, was a plant physiologist at Harvard and later became a children’s librarian.) Jerrold managed to get me an instructor’s job in the Physics department at Tufts University in Medford, Massachusetts. I taught in the day and worked on the thesis at MIT at night. The ambitious CO clock was dropped and instead I did a boring but useful measurement of the electric dipole moment of HF and its hyperfine structure in a set of low angular momentum rotational states.  All this was done by May 1962 when Sarah, our daughter, was born. (Sarah has become an ethnomusicologist. A son Benjamin was born in 1967, he is now an art historian.) Tufts had made me an Assistant Professor and it seemed I could have stayed as a faculty member, but I wanted to work with Professor Robert Dicke at Princeton who had become interested in gravitation. These were the years of a renaissance in General Relativity, in good measure due to the vast improvements in the technology since 1915. It was now possible to contemplate measuring the tiny deviations of [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)’s theory from Newton’s. Gravitation, because of this, was making a transition from mathematics back into physics.  When I arrived at Princeton, Dicke and his group had just finished a modern version of the Eötvös experiment showing the equivalence of the inertial and gravitational mass, one of the cornerstones of general relativity (the weak principal of equivalence), to a part in 10–11. Dicke was working on a new theory of gravitation which combined a scalar field to the tensor field of general relativity. The motivation was to better incorporate Mach’s principle into gravitation. He asked another post doctoral scientist, Barry Block, and me to consider an experiment to measure the excitation of the Earth in the spherically symmetric 0S0 mode by scalar gravitational waves coming from astrophysical sources. The mode has a period around 20 minutes with a Q about 3 000 as had been seen after some strong deep focus earthquakes. We made a quartz gravimeter and placed it in the same temperature regulated pit used earlier by the Eötvös experiment. We did not set interesting limits on the spectrum of scalar gravitational radiation. Early in our observing run, it became clear that geophysical excitations were going to severely limit the sensitivity of our measurements.  Even though the experiment was unsuccessful, the two years at Princeton were profoundly important in my scientific development. During my stay a range of experiments and experimental techniques were being tried. These included: a successful measurement of the Einstein gravitational red-shift in the sun (the first really believable measurement), an experiment that showed the equivalence between passive gravitational mass (ability of mass to respond to gravitational fields) and the active gravitational mass (ability of mass to make gravitational fields), an experiment to try to answer how round is the sun, and a precise absolute measurement of g, earth’s gravitational acceleration, using a freely falling corner cube in an interferometer. Lots of new ideas were being talked about at the group meetings such as the notion of putting optical corner cubes on the moon to allow precision measurements of moon-earth dynamics as well as the early thoughts about the heat that might accompany the origin of the universe. (The actual work of looking at the microwave spectrum of the sky started shortly after I left.) The critical and lasting knowledge was how one designs an experiment to get to its fundamental limits. Dicke was a master at this. I tried to learn a little formal general relativity from courses taught by Dicke (very many diversions) and [Wigner](https://www.nobelprize.org/prizes/physics/1963/wigner/facts/) (too abstract) but have to say they were interesting but not successful.  In 1965 Jerrold invited me to come back to MIT as a faculty member in the Physics department, with research support through the Joint Services Research Program in the Research Laboratory of Electronics. The program was not fussy about the actual research topics it supported but was dedicated to training more scientists and engineers as a resource for the national defense. At the time MIT was a better place than Princeton to do experimental work, as there was still the legacy of machine shops and store rooms filled with equipment from the wartime radar lab. It was easier to start a new experimental program at MIT. I began a laboratory dedicated to Cosmology and Gravitation. One of the first research goals was to try to establish if G, the Newtonian constant, was varying in time by a fractional amount 10-10/year. Both [Dirac](https://www.nobelprize.org/prizes/physics/1933/dirac/facts/) and Dicke had suggested that due to the expansion of the universe G was getting smaller with time. The way we were going to measure this was with an absolute gravimeter based on a plate held against Earth gravity by electric forces whose strength was determined by the Stark effect on molecular states in a beam. Thereby g would be turned into a frequency that could be compared to an atomic clock. It was also necessary to take sample measurements of the shape of the Earth to estimate its change in radius with time. The idea was to develop kilometer long laser strain gauges with absolute knowledge of the wavelength by comparison to an optical molecular resonance reference. The proposed program was long range and probably too ambitious for a starting (untenured) faculty member, although I felt it fit well into the capabilities of the MIT infrastructure.  We started with the laser frequency stabilization when Schaoul Ezekiel, an aeronautical instrumentation graduate student, became the first student to join the new group. We made an Argon ion laser in the RLE facilities and frequency stabilized it against narrow molecular iodine resonances to a relative frequency accuracy of 10-12. At about the same time, two experiments were done to look at the quantum noise of the laser. One experiment measured the fundamental phase noise in a laser due to the spontaneous emission as had been predicted by [Townes](https://www.nobelprize.org/prizes/physics/1964/townes/facts/). The other (mentioned previously as a redo of the tired light hypothesis) was a table-top [Michelson](https://www.nobelprize.org/prizes/physics/1907/michelson/facts/) interferometer operating at significant power (~100mW) with the phase measurement limited by the shot noise (quantum noise) at 10’s of KHz. The fringe was maintained by a servo system and the signal was translated from the 1/f region of the amplitude noise of the laser to higher frequency by modulation techniques − all direct applications of the Dicke methods for precision measurement.  The absolute gravimeter never got constructed as I began to realize that the lunar laser ranging observations and the solar system radar astronomy, which gave the critical radial dimension to the dynamics measurements, would lead to more reliable measurements of changes in g at the necessary precision.  In 1966 I was asked to teach a graduate general relativity course. I describe the difficulties and also some of the things learned in my [Nobel lecture](https://www.nobelprize.org/prizes/physics/2017/weiss/lecture/), especially, the beginnings of thinking about gravitational wave detection by interferometric methods. The other new topic for me was general relativity applied to cosmology. It was love at first sight (even though we all learn bits of this as we go along) finally understanding Bondi’s book on Cosmology and working with the Friedmann-Robertson-Walker equations was magical. One of the students in the course was Dirk Muehlner, who had some experience with far-infrared physics and, furthermore, knew some astronomy. At the end of the course we began talking about the new measurements that had been made by [Penzias](https://www.nobelprize.org/prizes/physics/1978/penzias/facts/) and [Wilson](https://www.nobelprize.org/prizes/physics/1978/wilson/facts/) and their interpretation by Dicke and his group as the red shifted relic heat of the cosmic explosion. We both thought it would be critical to show that the radiation actually exhibited a [Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) spectrum, but it was only talk until Bernard Burke, the head of my division in the Physics department, and I had a heart to heart conversation about my future in the department. Burke felt that these laser and gravity experiments would not lead to interesting results soon enough for decisions that had to be made in the department. He suggested why not really do cosmology and measure the spectrum of cosmic background radiation. His radio astronomy colleagues could help, since they had some experience with flying balloons in the stratosphere above most of the water in the atmosphere that would disturb such measurements.  Dirk joined the lab and we began to explore the possibility of making a measurement of the spectrum of the cosmic microwave background. At the time there were only measurements in the [Rayleigh](https://www.nobelprize.org/prizes/physics/1904/strutt/facts/)-Jeans low frequency part of a 3K thermal spectrum. The thermal peak is near 180GHZ, while the highest frequency measurements at the time was at 32GHz.  There was an optical measurement made in the 1930s of the rotational excitation of CN (Cyanogen) molecules in stellar atmospheres, which could be interpreted as due to the molecules being in equilibrium with thermal radiation at 3K. The lowest rotational state energies were close to the thermal peak and could have been excited by the radiation but also by local charged particles. I describe our effort in a book edited by Jim Peebles, Lyman Page and Bruce Partridge, *Finding the Big Bang*1.  The laser science and technology were taken over by Ezekiel as the research area in a new group in RLE. Starting in 1967 we began a program to measure the spectrum of cosmic background radiation from high altitude balloons. The research was supported by NASA.  Between 1967 and 1982 we flew around 20 flights, first to measure the spectrum and then the isotropy of the cosmic background radiation. The program was the mainstay for graduate student theses as it had both significant technical development (new mm detectors and filters, cryogenic instrumentation) but also astrophysical science results that could be published. Initially the spectrum measurements were done in three frequency bands, one at low frequencies in the Rayleigh Jeans region of the black body spectrum, which overlapped with the ground-based measurements, a second embracing the black body peak and a third in the Wien part of the spectrum above the peak. Even at an altitude of 40km in the atmosphere the ozone and water emission lines had to be accounted for and modeling was necessary to recover the cosmic background spectrum. At the end more channels were added to specifically measure the atmospheric emission lines. Eventually we were able to establish the black body nature of the spectrum (there was a peak in the spectrum) but not with much precision. We began to realize that a precision measurement would require a satellite mission.  Next, we turned to measurements of the angular distribution of the radiation. The first goal for these measurements was to see a dipole distribution of cosmic background radiation in the sky determined by our motion relative to the average rest frame of the universe or, another way to think of it, as relative to the last scatterers of the radiation at a red shift of about 1000. The largest term would come from the rotation of our galaxy, which gives a v/c of about 10-3 producing a variation of the radiation temperature over the sky, being hottest in the direction of the velocity and coldest in the opposing direction.  High sensitivity to small changes in the temperature (in the intensity of the radiation) was required in these measurements, but one could relax the requirements for absolute calibration so important in the spectrum measurements. Our first flights made an unfortunate discovery and indicated a significant problem for the future of these measurements. In the channel embracing the black body peak and the high frequency channel we saw anisotropies easily 10 times larger than the expected dipole and also discrete sources tied to the sky and not to the atmosphere. Eventually we realized we were measuring dust emission from these discrete sources as well as broadly distributed dust throughout our galaxy with the strongest emission from the galactic plane. We had to become astronomers to get at the cosmology, In fact it took many flights viewing much of the sky to actually measure the dipole in the low frequency channel (least effected by the dust) using corrections from the higher frequency channels. The galaxy had replaced the atmosphere as the worst source of contamination. Other groups measuring the isotropy of the cosmic background from balloons and aircraft with channels at lower frequencies, less sensitive to the dust but more sensitive to free-free and synchrotron emission by electrons, were able to map the dipole better.  By the early 1970s it was clear that a satellite mission would be more definitive in making measurements of the spectrum and the isotropy of the cosmic background radiation. By placing the instruments outside of the atmosphere in Earth orbit, one could get long integration times to improve the signal to noise but also have time to test for systematics. Furthermore, without the absorption in the atmosphere, it would be possible to add enough wavelength coverage to separate the cosmic background radiation from the emission of nearer astronomical foregrounds. [John Mather](https://www.nobelprize.org/prizes/physics/2006/mather/facts/) recognized this and acted on it. As a graduate student at Berkeley he conceived of COBE (Cosmic Background Explorer) and after graduating he pulled a team together including me to actually do it. He and John Boslough wrote a book about COBE, *The Very First Light*2 which includes much of the story of the project.  I became the COBE science working group chairman, in part since I was the oldest but also because of the experience I had gained with being on many NASA advisory committees dealing with science policy and management, though COBE took close to 20 years from John’s conception to results. The results were significant: the cosmic background spectrum was found to be thermal to a 10-4 between 90 to 600 GHz , an intrinsic anisotropy of the universe at a level of 10-5 K was discovered at angular scales 7 degrees and larger which indicated that there were quantum fluctuations in the beginning of the universe that created a structure maintained through the universal expansion by the gravitational interaction of dark matter, and it found that the galaxies were filled with dust at close to 5K. For these discoveries, two COBE scientists, Mather and [George Smoot](https://www.nobelprize.org/prizes/physics/2006/smoot/facts/), won the Nobel Prize in Physics in 2006.  Research in experimental gravitation did not end in the laboratory once the cosmic background measurements began. The same graduate course in general relativity led to a gedanken experiment which became a real experiment in 1972 to try to detect gravitational waves from astronomical sources. The experiences of the earlier research on laser frequency stabilization and the characterization of the fundamental noise in laser interferometry found application in the design and construction of a prototype interferometric gravitational wave detector. By the mid-1980s, Stephan Meyer took over the cosmic background radiation research and I became more involved with the gravitational wave detection project, as is described in my Nobel Physics Prize Lecture. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0021 = RW**  [Rainer Weiss] Hello.  [Adam Smith] Good morning, my name is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  RW: Yeah, very good, I already talked to some of your … with your colleagues this morning.  AS:  First of all congratulations on the award of the Nobel Prize.  RW: Well thank you.  AS: It must be very special, in particular because you came up with the idea of this detector.  RW: Well be careful with that. Other people also had thought of detectors, so be careful with that. I mean, in fact there was a group in Russia in 1962 – Gertsenshtein and Pustovoit – I can’t pronounce it for you well. They wrote a little paper in a Russian journal which none of us knew about that, not to do it by interferometry so much, but to use light as a way of doing this. And then it turns out, Joe Weber, unbeknownst to us, but also Joe Weber, who was sort of the first person to start thinking about this.  AS: With his Weber bars.  RW: Yeah, the Weber bar, but at the end of the Weber bars he also has some notes about that maybe we should do this with interferometry. The whole world tried to reproduce the Weber experiments. I don’t know, you’re probably too young to know that, but the thing is that in ’60 … Weber made his big announcement in ’69 and … where he showed that he had, in three bars, he had seen gravitational waves and then that got, virtually everybody, well many, many groups, both in Europe and Asia and in the United States, tried to reproduce this and to everybody’s disappointment nobody saw the same thing that Joe did, Joe Weber did. And, the way it happened in my life is I was teaching a course in general relativity, in the middle of that epic, sort of 1967, and I couldn’t explain the way a Weber bar worked. Mostly because I just didn’t know enough, ok, but it was that … I thought that there must be an easier way to explain how a gravitational wave interacts with matter.  If one just looked at the most primitive thing of all, 3D floating masses out in space and look at how the space between them changed because of the gravitational wave coming between them. And I gave that as a problem in the course, you know, and the kids in this course did it, because it’s a fairly straightforward calculation. And that was sort of ’67 and by about ’72, ’71 it turned out that many people were not seeing, I mean it was already quite clear that the bar technique and Weber’s experiments were not being seen by others. And so I spent a summer thinking about, maybe this idea that I gave as an exercise in a course would be a nice way to try and do this because it was so easy to understand it. And that then turned into LIGO, but other people had thought of it. I didn’t know that.  AS: Nevertheless a journey from the early seventies to now. The sense of achievement and excitement must be quite …  RW: Oh no doubt. I mean look … the thing … it has nothing to do with pride. I did something that others didn’t do. I actually did a calculation of what might be all the things that get in the way of being able to do it. Which actually turned out to be very useful. You know the different noises that would make it impossible to see it, or possible to see it, you had to solve a whole set of problems, and that was my contribution to it in the early days.  AS: Well that’s it. It’s quite mind-boggling to think of how precise this piece of equipment is.  RW: Yeah. [Laughs] That’s true, and it’s … that’s what took by the way. I think the easiest way to say it is that the concept is very straightforward. You measure the time it takes light to go between two orthogonal directions in the gravitational wave. And you measure that time very carefully, and that idea’s sort of trivial. I mean most people who know a little bit of physics can make that calculation. On the other hand what happens to make it actually happen because the sizes of things is so small and I think the easiest way to say it … Are you familiar with exponential notation, can I use that?  AS: You can yes.  RW: Well ok, I think the best way of saying it is this way. There are two factors of 1012 that had to be solved. One of them was that the light wavelength itself is 10-6 meters and so you had to devise a way to make light, which has this wavelength of 10-6 to go, to be good enough so you could measure 10-18 meters, and that is a factor of 1012. And that was not the hardest problem, but that was one of the major problems between, let’s say 1972 to 2015. But the other one is another factor of 1012 which is just as serious and that’s much harder to solve. And that took longer. And that took more effort. And that was that even though you may have this wonderful method of now breaking up a light fringe so you could do a part in 1012 of it, you still don’t know that the thing that you’re measuring is not being pushed around by forces that make it move much, much more, that are not gravitational, that are not gravitational waves. But other forces like thermal noise, like seismic motion, or god knows all the different things that happen in the world, that you were not being …  That, that same mirror that you’re looking at is not being pushed around by, by things that make it move more than 10-18 meters. And that is another factor of 1012 about because it turns out that ground motion is about a few microns, 10-6 meters again. So you have to devise a wonderful way to get rid of the ground motion and then get rid of the thermal noise and now it’s really at the point where we’re worrying about the quantum noise. So … But, it was all pretty well organised, I mean in the sense that people knew what the problems would be. It’s just that it takes time to do a thing like that.  AS: The contrast between the minute precision to make the measurement and the size of the actual gravitational wave which is …  RW: Yeah, yeah yeah. It’s really … What it tells you is something really interesting. It tells you that space is very, very stiff to distortion. You know the [Einst](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html)ein waves can be thought of as a distortion of space, and time. But the way we see it, we see it as a distortion of space. And space is enormously stiff. You can’t squish it, you can’t change its dimensions so easily. And it turns out that I think the easiest way to see that, or say it is that, it’s …  I’ll give you an example so that you can use it or think about it. If you put this whole thing that was detected, you know, back in the first detection, and put it not at a billion light years away , but rather put it at the sun, the distance of the sun. Suppose the sun had, somehow, put out those gravitational waves. You would have had a motion at the earth of, a motion of over a km, of about only 10-6 m. It’s still tiny. In fact you could just about hear it in your ear, but the amount of power that went through you is something like 1024 watts per cm2. It’s huge. In other words, it’s … you know the sun puts out about 104 watts.  AS: So you can translate that into what, though?  RW: Yeah, yeah, in fact we did translate that in that initial paper into the amount of power was sort of brighter than anything in the universe, by 50 times brighter for the few moments in which that gravitational wave was actually, you know, travelling through you. It’s an enormously stiff, the system just does not like to make, you cannot distort space very much but you do get a little bit and that’s what we measure.  AS: That is a really beautiful concept to mention on this call, thank you so much. We will hopefully discuss all these things more when you come to Stockholm.  You will be coming to Stockholm in December?  RW: Of course I will, and I intend, at least if you can manage it I would like to … I prefer really often to talk to high school students, mostly because I think they’re the future for us. And, I know I have to give lectures, I’m very happy to do that and so are my colleagues. But if you think that there’s some high school students that would benefit from understanding this a little better I’d be very happy to do that.  AS: We will most certainly work to fill rooms with high school students for you. That’s a great objective. We very, very much look forward to welcoming you to Stockholm. How do you feel about the coming day which will be completely taken over by this.  RW: What today? I don’t know, I’m at home, still not completely dressed! Well I am now, a little but, yes I know that I have to confront all my colleagues and it’s a charming thing to do, it’s just a little awkward that’s all.  AS: Good luck with finishing getting dressed and we look forward to meeting you in Stockholm in December.  RW: Yeah I’m very happy to come. I’ve been there once with – you gave a Nobel Prize to one of my colleagues named [John Mather](https://www.nobelprize.org/nobel_prizes/physics/laureates/2006/mather-facts.html) and [George Smoot](https://www.nobelprize.org/nobel_prizes/physics/laureates/2006/smoot-facts.html).  AS: Of course, because you also worked on the COBE project with them.  RW: Oh yeah I worked on that with them and I’ve been there and it’s really quite a pleasure to come there.  AS: OK, well we greatly look forward to meeting you. Thank you so much.  RW: Bye bye. |
| **Interview** |  |
| Q3 | **What do you enjoy about science?** |
|  |  |
| Q3 | **How did you become interested in science?** |
|  |  |
| Q1 | **What advice would you give to a young person interested in science?** |
|  |  |
| Q16 | **Is it ever too late to become a scientist?** |
|  |  |
| Q3 | **How do you keep going in science?** |
|  |  |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0022** |
| **Biographical** | Reflecting on my background, it is not at all apparent why my life did not go in other directions, rather than my becoming a physicist, and one dedicated to an academic career and pursuing research on some of the most fundamental problems in nature. My families’ backgrounds are not very well documented, but I do know that both my parents’ families immigrated to the U.S. from small shtetls in Eastern Europe, in the general area of Ukraine and Belarus. I know little about my ancestors there. They apparently were not very distinguished, and I do not know what they did or under what circumstances they immigrated to the U.S. at about the turn of the 20th century.  My mother’s family (maiden name Shames) settled in St Joseph, Missouri, where she was born. They moved while she was a child to Omaha, Nebraska, where her father, Max, opened a body and fender shop, where they fixed cars. My mother had one sister and they lived a middle-class life with their social life centered around the small, but tight knit, Jewish community in Omaha. My grandmother died at a young age and my grandfather, Max, remarried. My mother graduated from high school and had aspirations to go to college at the University of Nebraska, even earned a scholarship, but her father would not let her go. This was a disappointment she expressed her whole life.  My father’s father, two brothers and grandfather immigrated to the U.S. in 1901, and became homesteaders in North Dakota, under a program where the U.S. government gave away land to be farmed and developed. They did this for 18 months, sold the land for a good profit and moved to Sioux City, Iowa, where they eventually owned and ran a Ford agency until they had a falling-out with Henry Ford over his anti-Semitic attitudes. In 1929, just before the U.S. depression, the Barish family moved to Omaha, Nebraska.  My grandfather had died in Sioux City when my father was only 10 years old. Although his mother remarried, my father had to work from a young age to help support the family. As a result, my father also never went to college.  I was born in Omaha in 1936 and we spent the first 10 years of my life there. I had a brother, born in 1940. I was a quiet child, but a very good student from a young age. My father worked for his father-in-law in his automobile repair business. During World War II, my father worked at a factory near Omaha that built military bombers for the war. He was in charge of an assembly line. As a result of his war work, he did not serve in the military during the war.  After the war, my father did not want to work any longer for his father-in-law and, instead, decided to move the family to California, where he would work for his uncle. His uncle had moved his car business to Los Angeles. We moved to the Los Feliz area, a quiet neighborhood adjacent to Hollywood. I went to public schools through high school. As a young child, I loved to read and after consuming many mystery and science fiction books and I began reading good literature, including the classics. In my young teenage years, in addition to liking story-telling, I enjoyed writing. When was asked what I wanted to do when I grew up, it was to become a novelist. At this age, I did not have any special interest in science or mathematics but was always very good in math and won some contests testing skills.  I actually spent most of my non-school hours playing sports − football, basketball and tennis. Most of my friends were made through sports. By the time I entered high school, my sports interests focused on tennis and I played on my high school tennis team for the next three years, during which time, I won some honors.  Academically, I was always at or near the top of my class, but I had no real direction. Neither of my parents had gone to college, but they were convinced of the importance of education and were committed to my brother and me getting a college education. However, they were not able to give us much guidance in terms of what we might study. I also never happened to have a good mentor in high school and, as a result, I had little idea what to study. There was a general expectation in my family that my brother and I would become either doctors or lawyers. I was not very interested, but my brother did become a lawyer!  Lacking much good guidance, I decided I would study engineering, based on the fact that I had good math skills and liked what I knew about engineering. I entered the University of California, Berkeley, where as a Freshman I took two Engineering courses, Inorganic Chemistry, Physics, Mathematics and a French Literature course. This was my first physics course, because we did not have one in my high school, and I began my love affair with physics, immediately. At the same time, my engineering courses, drafting and surveying were too tedious for me. So, I switched to becoming a physics major, not really understanding what it meant to be a physicist.  I loved Berkeley from the beginning. I thrived in the academic part, made many new friends, including having my first serious girlfriend. I emerged from being a terribly shy young boy to being a socially (if not outgoing) active college student. My only disappointment was that I dropped tennis. I had aspirations to play tennis at Berkeley, but I was only marginally good enough to be on the tennis team, and the large demands for practice time interfered with my many labs in physics, chemistry and engineering.  In undergraduate school, I became interested in particle physics, stimulated by all the new elementary particles being discovered on the particle accelerators at the “Rad Lab” (now Lawrence Berkeley National Laboratory, LBNL) above the campus. I did some research there as an undergrad and spent time at the 184-inch cyclotron. As I approached getting my BA, I was very attracted to doing my graduate work in Berkeley on the large accelerators. The physics department discouraged their own undergrads from going to graduate school in Berkeley, but in the end they accepted me to their graduate school.  In graduate school, after my course work and candidacy exams, I went into particle physics and did my thesis on the 184-in cyclotron studying single pion production in pion-proton collisions tracing the production of the Δ33 resonance in the final state. My thesis advisor was A. Carl Helmholz, who was chairman of the physics department. During this period I learned not just about particle physics detectors, but also the accelerators in Berkeley, the 184-in Cyclotron and the Bevatron. In fact, when I received my PhD, my wife had a job she liked and we decided to stay for an extra year, me on a postdoctoral appointment working at the Bevatron.  My years in Berkeley were transformative. I entered as a shy young boy, good academically, but otherwise pretty lost. I found and developed the professional love of my life, physics, as an undergraduate in Berkeley. I did well enough in graduate school to land a really good postdoctoral job at California Institute of Technology (Caltech), which has been my professional home ever since. I also met the other love of my life, my wife Samoan, in Berkeley and we have made our lives together ever since, she as a psychoanalyst, me as a scientist, and with a daughter and son, and three grandchildren. I left Berkeley with happiness and confidence to take on the world!  I was hired at Caltech in the fall of 1963 as a postdoc for Alvin Tollestrup to work on a new experiment at the Brookhaven Alternating Gradient Synchrotron to study the annihilations of antiproton-proton → electron-positron pairs. This reaction probed the time-like form factors and we were searching for heavy resonances in that system, but found none. For the experiment, I designed and built an intense separated anti-proton beam as my primary contribution to the experiment. The experiment worked well and we made important time-like measurements of the proton structure for the first time. For me, it provided for the first-time, my identification with one of the major experiments in particle physics.  Then, in about 1965, I went to work on the new two-mile long linear electron accelerator at the Stanford Linear Accelerator Center (SLAC), working with Henry Kendall, Jerry Friedman of Massachusetts Institute of Technology (MIT) and Richard Taylor of SLAC. This long-term project involved designing and building a large 6-GeV spectrometer and preparing a three part program: 1) to measure the proton form factors at high momentum transfer; 2) to make a comparison of electron and positron scattering to probe two-photon exchange effects; 3) and finally, to measure electron proton inelastic scattering. These were the greatly anticipated initial experiments to be performed at the new SLAC accelerator. I participated in both the electron elastic scattering and electron-positron comparisons − highly anticipated and important studies of the proton form-factors to large momentum transfer. We found no big surprises, and I made a fateful decision not to participate in the inelastic scattering experiment, expecting that this long-term program would again provide nice measurements, but no big surprises. How wrong I was! The experiment surprisingly (at least to me) found very large scattering cross sections, which became pivotal evidence for the quark structure of the proton. My colleagues, Kendall, Friedman and Taylor, won the Nobel Prize in 1990 for this important discovery.  I left the SLAC experiment to return to the Brookhaven AGS for a follow-up experiment from our earlier anti-proton proton annihilation experiment this time to measure final pion production states, which had been an important background in the original experiment. I organized a small group from Caltech and collaborated with Adrian Melissinos and his group from Rochester University. It was the first experiment where I was the leader (or spokesperson). The experiment was successful in that we made a series of nice measurements of high energy antiproton-proton annihilation cross sections.  On the personal side, during this period my wife and I had two children and our lives became complicated by our trying to balance my demanding professional life that required lots of travel and a home life that involved raising small children and my wife, Samoan, pursuing her own professional life as a mental health professional. I also was promoted from post-doc to Assistant Professor at Caltech.  This was a very exciting time in high energy physics. The new high energy accelerator at the National Accelerator Laboratory (now called Fermilab) was nearing completion and proposals were being made for the initial scientific program. I teamed with my Assistant Professor colleague at Caltech, Frank Sciulli, to develop a proposal to make a narrow band neutrino beam and a detector to exploit it. The high energy protons at Fermilab provided the opportunity to produce intense high energy neutrino beams. Our experiment was approved, along with a wide-band neutrino beam proposal by David Cline, Al Mann and Carlo Rubbia.  We succeeded in measuring the first high energy scattering distributions and cross sections for both neutrinos and antineutrinos. These results were complementary to the SLAC electron deep inelastic scattering experiments in revealing the quark structure of the proton. But, even more importantly, Electroweak Symmetry Breaking or the, so-called, Standard Model for particle physics had been proposed by Sheldon Glashow, Stephen Weinberg and Abdus Salam. A unique prediction of that model was that it predicted the existence of weak neutral currents, or in other words, neutrino scattering without charge exchange (v + N → v + X). An event had been observed in the Gargamelle heavy liquid Bubble Chamber at the European Organization for Nuclear Research (CERN) that provided initial evidence for neutral currents, and our experiment at Fermilab provided the definitive proof. This was the lynchpin that established the Standard Model of particle physics, still the best description of high energy physics that we have today.  In the subsequent years, I was promoted and settled into having my career based at Caltech. Interestingly, I had considered going to Caltech as an undergraduate, also as a graduate student, then I finally went as a postdoc, and have made my entire career since then at Caltech. It has been a great place to me and for me! The smallness and dedication to being a place where great science is done has provided the encouragement and support for me to address the most fundamental problems experimentally possible at the different stages of my career. I have led a series of forefront particle physics experiments, including: the Neutrino Experiment at Fermilab (with Frank Sciulli), where we established the weak neutral current; definitive evidence for the Z0, the carrier of the weak neutral current at the SLAC Linear Collider; measurements of the fundamental parameters of the t-lepton (our heaviest lepton) at the Cornell Electronic Storage Ring (CESR) electron accelerator; the most sensitive search for Grand Unified magnetic monopoles at the Gran Sasso Laboratory in Italy in the MACRO experiment, as well as experimental confirmatory evidence for the existence of atmospheric neutrino oscillations discovered by Takaaki Kajita and collaborators in the Super-Kamiokande experiment in Japan.  In 1990, I joined with Bill Willis from Columbia to co-lead the design of one of the two large efforts (Gammas, Electrons, Muons = GEM) for the Super Collider in Texas. We did detector development R&D and simulation work to optimize a detector for the main particle physics goals in the multi-TeV range. The GEM collaboration was highly international and we developed a detector design that was very well reviewed shortly before the Superconducting Super Collider (SSC) was cancelled by Congress in the fall of 1993. This was a huge disappointment for those of us in particle physics in the U.S. A large contingent of my collaborators in GEM joined the CERN Large Hadron Collider (LHC) experiments, and both the U.S. Department of Energy (DoE) and National Science Foundation (NSF) approved funding for U.S. participation at CERN. In fact, some central design features and technologies from GEM have been implemented in the LHC detectors.  Following the cancellation of the SSC, I made a personal decision not to join the CERN experiments, but was not in a rush to decide what I wanted to do next. Being a professor at Caltech, I had no job issue and my main preoccupation in the months immediately following the SSC cancellation was finding jobs for some of the very talented scientists I had hired at the SSC and who now had no job. Some of them were absorbed in the broader U.S. high energy physics program, some left high energy physics for technical jobs or Wall Street or elsewhere, and some were unfortunately lost to science.  About two months after the SSC cancellation, I received a phone call from Charles Peck, the chair of Physics, Math and Astronomy at Caltech. Charlie was a long-time colleague and friend, who I had worked with on and off through the years. He asked if he could come out and visit me in Santa Monica, where we live (about an hour drive from Caltech) to take a walk with me on the beach. This was completely out of character for Charlie, and I had no idea what he wanted to talk about (maybe to commiserate over the demise of the SSC?), but I didn’t ask, just set a time for him to come. Charlie came and we took a walk during which he asked me if I would be willing to take over the leadership of the Laser Interferometer Gravitational-Wave Observatory (LIGO). This was completely out of the blue and a surprise to me. Charlie explained the problems that existed in LIGO and that NSF wanted a change of leadership, before they would consider funding the project. He told me that the NSF Director, the MIT and Caltech Presidents, etc. had conferred and they wanted him to ask me to take the project over. I told Charlie that I was flattered, supported Caltech’s and MIT’s initiative toward gravitational wave detection, but that I hadn’t yet considered what I wanted to do next. I said that to decide on LIGO, I would need a month to do my homework to determine whether I thought I could make LIGO a success. After a month, I was not able to convince myself that I could succeed, but on the other hand, I couldn’t convince myself I couldn’t. As a consequence, I accepted and became the LIGO Principal Investigator, and I plunged into this great challenge.  In the spring of 1994, we revised the LIGO proposal in ways that raised the costs substantially. The reasons were to strengthen the technical team, have larger staffs at the sites to operate LIGO, and to invest more in making the technical infrastructure as robust, flexible and forward-looking as was practical. We made some significant design changes, including investing in some modern developing technologies like solid state lasers, digital controls, computer interferometer simulations, etc. The revised proposal was submitted to the NSF and was reviewed by a distinguished panel by summer of 1994. The new proposal was strongly endorsed by the review committee.  The next step was for NSF to make a decision whether to fund our proposal. We had strong support in Physics from Rich Isaacson and Dave Berley, who were responsible for the NSF gravity program and for LIGO and from the physics leadership, Marcel Bardon and Bob Eisenstein. The Math and Physical Sciences Assistant Director at that time was Bill Harris. I met with this group several times over this transition period. Bill became convinced that LIGO was potentially a transformative project and that we were on a path to succeed.  Bill Harris convinced Neal Lane, Director of the NSF at that time and the National Science Board (NSB) chairperson that Kip Thorne and I should be invited to make a presentation directly to the NSB, to help them be able to make an informed decision whether to fund and proceed with LIGO. Kip Thorne and I were invited to a meeting of the NSB during the summer of 1994. I note that it was unprecedented for scientific proposals to be presented to the NSB by the proposers. In any case, the NSB agreed and Kip made an inspired presentation on the science potential of LIGO. In my opinion, this made funding LIGO irresistible, if the board could only be convinced the investment would be used in a way such that the project could succeed. I followed Kip’s talk by laying out our plan for how we would build and evolve LIGO over the coming decade, as well as how to have a clear path in the longer term to Advanced LIGO, a yet more sensitive detector. The NSB accepted our arguments and approved funding to construct LIGO.  We then organized the LIGO project and began to move toward construction. The project was (and is today) a Caltech-MIT collaboration, funded through a “Cooperative Agreement” between NSF and Caltech, and with MIT as a subcontractor. Although administratively MIT is a subcontractor, in fact operationally LIGO and Advanced LIGO were built and are operated by the LIGO Laboratory, consisting of staffs and Caltech, MIT, Hanford Washington and Livingston Louisiana. Some other university groups participate in particular technical areas under subcontracts with the LIGO Laboratory and non-U.S. collaborators thorough memorandums of understanding (MOUs).  The construction of Initial LIGO took place between 1994 and 1999. Some key members of LIGO who joined the project early during the construction period included: Gary Sanders, whom I had worked with at the SSC and who had experience with large projects, became our project manager; I recruited Albert Lazzarini from industry, and he took on the responsibility of being our systems engineer and integration manager; Dennis Coyne, who became our chief engineer, and Mark Coles, who became head of the LIGO Livingston Observatory (LLO). They joined the very strong team who were working on LIGO before I took it over, including Stan Whitcomb, who has been our chief scientist; Fred Raab, who became the head of the LIGO Hanford Observatory (LHO), Mike Zucker who led LLO for a few years, David Shoemaker and Peter Fritschel, who worked with Rai Weiss on the many responsibilities assumed by the MIT group.  Two important events occurred in 1997, while LIGO was in the midst of construction.  First, I invited Benoit Mours from Laboratoire d’Annecy and a senior member of Virgo to visit Caltech on sabbatical, and during that visit he worked with LIGO scientists to agree on a data format structure that would be used by both LIGO and Virgo. This was the earliest step toward these two collaborations working together to analyze data. The MOU to collaborate between LIGO and Virgo that has led to joint authorship of our discovery paper was formalized ten years later.  The second important event that took place in 1997 was the creation of the LIGO Scientific Collaboration (LSC), which brought in a broader group than Caltech and MIT to carry out the science from LIGO. The LSC was made by design to be a separate organization from the LIGO Laboratory, an organization where LIGO Laboratory scientists and scientists from other U.S. or non-U.S. universities and laboratories would work on equal footing with other individuals and groups that joined to do LIGO science. This has evolved over 20 years into the present LSC of over 1,200 members from 18 countries. The data pipelines for the various searches, calibration of data, data quality studies, scientific computing and the responsibility for scientific papers, talks etc. are carried out. The LSC has been a great success in enabling scientists from around the world to join together to carry out the science of LIGO.  In about 2000, we began commissioning LIGO and it became more sensitive than any previous instrument within about one year. Over the next decade we went through six cycles of ‘observing,’ which we called runs S1 through S6, each taking months of data, and each searching for gravitational waves and not finding any. Each of these observational runs was at significantly better sensitivity than the previous, enabling us to set better and better limits on gravitational waves. Finally, the last data runs reached our design sensitivities. One very important feature of this period is that there we had found no serious backgrounds or rejected possible candidates for gravitational waves. This meant that we were limited by our instrument sensitivity, not backgrounds that had features that could mimic gravitational waves.  At the time I proposed our plan for LIGO to the National Science Board in 1994, I asked for support to keep the key technical experts who developed Initial LIGO to do the R&D to develop the technologies for Advanced LIGO. By 2003, we had developed the basic technological concepts for Advanced LIGO and that conceptual design was reviewed and approved by the National Science Foundation. We developed an engineering design and developed the project plan and responsibilities over the next few years. Advanced LIGO was funded and we began construction soon after completion of our S5 data runs, when we had pretty much achieved the best sensitivity possible before a major upgrade to Advanced LIGO. A few changes for Advanced LIGO were implemented immediately and a final data run we called Enhanced LIGO, S6, was taken before the major construction was undertaken for Advanced LIGO.  During much of the period of Advanced LIGO construction, I was involved part-time on LIGO. I had been recruited to lead the design of the International Linear Collider, the leading concept for the next generation particle physics accelerator. For three generations, particle physics had advanced to new higher energy regimes through the complementarity of two accelerators, a proton-proton accelerator with good rates and ability to survey, and an electron-positron accelerator with capability of doing more precise measurements. Vigorous R&D programs were carried out at the Japanese High-Energy Accelerator Research Organization Laboratory (KEK), SLAC in the U.S. and Deutsches lektronen-Synchroton (DESY) in Germany to develop a design for an electron-positron machine to complement the Large Hadron Collider (LHC) at CERN. The fundamental problem is that traditional circular machines at such high energies for electrons and positrons radiate away too much energy for a practical size machine. This motivated the development of a new type of accelerator, a linear collider.  A linear collider is a very challenging concept, because it is a single pass machine, while a conventional circular collider has many cycles for the beam particles to collide. Therefore, to achieve comparable event rate (luminosity) the beams have to be focused into very small spots. Two possible solutions were pursued in major R&D programs in the 1990s, one using conventional room temperature cavities at SLAC and KEK, and one using superconducting cavities at DESY. I chaired a committee in 2004 that chose the superconducting radiofrequency (RF) cavity concept, based to a large extent on the promise of a developing technology, other applications, and some technical advantages. I was then asked by the International Committee on Future Accelerators (ICFA) to lead the design effort for the International Linear Collider. I accepted and led an international R&D and design effort resulting in a detailed technical design that was completed in 2012 and is under consideration by the Japanese government with a decision expected during 2018.  In 2002, I was appointed by President Bush to the National Science Board (NSB). The NSB has 24 members, 8 appointed by the sitting President every two years. I served from 2002 to 2008, then as a consultant for two more years. The NSB advises both Congress and the President on science issues and serves as the oversight board for the National Science Foundation. Just as examples, during my tenure we did a study and advised Congress on the issue of ‘rare materials,’ many of which have become essential for high technology electronics, etc., yet, are not being mined in the U.S. Other issues that we grappled with continuously included diversity issues in science education and science professions. As the oversight board for the National Science Foundation we advised on how to do the best science within a system that has become more and more accountable to the government and has a smaller fraction of successful grant applications. Both trends lead to more conservatism and lack of risk taking in the NSF grant program, while doing the best science inherently involves risk taking and willingness to fail or find nothing that is interesting. My tenure on the NSB was very broadening for me, working closely with very talented and knowledgeable colleagues with very different backgrounds and skills on interesting and important issues.  I returned full-time to LIGO as the construction was complete on Advanced LIGO and commissioning was underway. The commissioning went well and when an improvement of a factor of three or more over the best achieved sensitivity for Initial LIGO was achieved, we were ready to perform the first Advanced LIGO data run. It began in September of 2015 and we observed the merger of two Black Holes on September 14, 2015. This discovery was a direct result of the new technology we had developed to improve our isolation from the ground motion by adding seismic isolation to the passive isolation in LIGO. This technology was developed in the Advanced LIGO R&D program that immediately followed the construction of Initial LIGO.  My first reaction to the dramatic event of Sept 14, 2015 was concern that we might be either fooling ourselves or were being fooled. The Advanced LIGO interferometer was basically a new instrument and we had only officially begun to operate it a few days before the discovery. So, maybe there were ways that such event candidates could be generated in the new detector itself or by something external. The main point is that we could measure the improved sensitivity but needed to run the detector for some length of time to determine whether events as observed could be some form of background. To test that required running for about one month and looking at off coincidence bins to see if such events are generated accidently. After doing this test for one month, we determined the probability of the event being an accidental was infinitesimal. This relieved one source of concern. The other was “how could we be fooled.” In this case, the worry we had was that maybe there was some way that someone had ‘injected’ a fake event into our data stream. We created a ‘tiger team’ of experts to investigate this, led by Matt Evans of MIT. They did a thorough job of tracing the signal back to each detector (meaning that it would have had to be generated and injected into the data at our two LIGO detector sites within 6.9 milliseconds of each other. They showed that this could not have happened in any way they could conceive.  At this point, everyone in LIGO, including me, decided the event was real and we should proceed toward publication, keeping the discovery totally internal until we published, so that we could present it as professionally as possible. We spent another month doing the physics analysis for fitting to general relativity, parameter estimation, etc. By early November, we began writing the discovery paper. It was completed in early December and Physical Review Letters said they could not do an expedited review with Christmas vacation coming so soon. So, we held the paper until January. In the interim, on December 26 we saw our second ‘five-sigma’ event. The black holes for this event were somewhat lighter and therefore the event went to higher frequencies and had many cycles in our frequency band. Any doubt that might have lingered in my colleague and my mind were set to rest with this observation.  For me, despite the fact that the first event made an incredibly strong case, I let out a final sigh of relief. This was not due to any lingering doubts about what we had done, but a history in physics of the difficulty in claiming a discovery based on one event. I recall the discovery of the W-based on a single event in a hydrogen bubble chamber beautifully established the quark model of particle physics. On the other hand, a claimed discovery of the magnetic monopole, again based on a single event, appeared convincing, but was not confirmed in much more sensitive experiments.  The announcement of our observation was made on February 11, 2016. The LIGO press conferences was in Washington DC and simultaneously I gave the first scientific seminar on our result at CERN. We were, of course, very pleased by the acceptance and enthusiasm for our discovery by the scientific community. That has continued through our observations of several more black hole mergers, and more recently, a binary neutron star merger. The neutron star merger event had electromagnetic counterparts. This combined science has been amazing and is the beginning of a new astronomy – multimessenger astronomy, which I believe will lead to exciting new understanding about our universe in the coming decades and beyond.  As I write this, we are working hard to make LIGO an even more sensitive instrument, as well as doing R&D and developing concepts for next generation gravitational wave detectors. This will keep me busy for the rest of my career, and I eagerly embrace my future! |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0022 = BB**  Barry C. Barish: Hello?  Adam Smith: This is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize in Stockholm. Well first of all congratulations on the award of the Nobel Prize.  BB: Oh thank you. Of course I’m humbled and thrilled.  AS: How did the news come to you?  BB: I guess a telephone call about 10 minutes ago, just before they started the session I guess. So I learnt just before you learnt, I guess.  AS: It really couldn’t have arrived any faster, the news, because the announcement of gravitational waves was only made last year.  BB: [Laughs] Yeah.  AS: Putting LIGO together and getting this result took many decades and an awful lot of work. Where did that dedication come from?  BB: I think that’s a harsh question to answer. I think there’s a personal part – you have to be someone who doesn’t need instant gratification. But I think the scientific goals and the technical challenges were the two things that equally motivated me. The technical challenges were technical challenges that were not unbeatable; it was just that we had to learn how to do things, and how to build a sensitive enough device. That took us 20 years after we built the first version of the LIGO detector. And of course the science is unbelievable, so I think it is not hard to be motivated for 20 years to do the kind of science we’re starting to be able to do.  AS: The precision of this instrument is quite unbelievable, isn’t it.  BB: Yes it is, the size of the effect that we measured from the first event, the merging of two black holes, the actual size of the signal was about one thousandth the size of a proton, what it did to our apparatus. So we were able to measure a movement, or change of length of the apparatus, by the passage of the gravitational waves to that accuracy and then measure its form well enough to decide what that was. So that’s pretty unbelievable.  AS: It’s a testament to human ingenuity isn’t it?  BB: And a testament to modern technology and science. I think this couldn’t have been done 50 years ago, or 20 years ago, or 30 years ago. It’s taken the best modern lasers and control and engineering to be able to do it.  AS: Will we be welcoming you to Stockholm in December?  BB: Yes of course.  AS: Lovely. It was great to talk to you. Congratulations again.  BB: OK, thank you. Bye bye.  AS: Bye bye. |
| **Interview** |  |
| Q3 | **How did you become interested in Physics?** |
|  |  |
| Q5 | **Who has inspired you?** |
|  |  |
| Q5 | **How important is mentoring?** |
|  |  |
| Q9 | **How did it feel to be awarded the Nobel Prize?** |
|  |  |
| Q9 | **What does the Nobel Prize mean to you?** |
|  |  |
| Q2 | **What makes the LIGO team so special?** |
|  |  |
| Q2 | **How important is collaboration in science today?** |
|  |  |
| Q2 | **How difficult was it to lead such a big group of scientists?** |
|  |  |
| Q10 | **How important is government support?** |
|  |  |
| Q16 | **If you could choose to be involved in any other discovery, what would it be?** |
|  |  |
| Q2 | **Do you think it’s important to interest young people in science?** |
|  |  |
| Q1 | **Do you have a favourite piece of advice for young people?** |
|  |  |
| Q7 | **What qualities do you need to be a successful scientist?** |
|  |  |
| Q16 | **What advice would you give to the younger you?** |
|  |  |
| Q8 | **How do you like to spend your free time?** |
|  |  |
| Q2 | **Do you think science can be seen as a creative subject?** |
|  |  |
| Q22 | **What are you looking forward to during your visit to Stockholm?** |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0023** |
| **Biographical** | **My youth**  I was born in 1940 in Logan, Utah, USA, a college town of 16,000, nestled in a verdant valley in the Rocky Mountains.  My father, David Wynne Thorne, was a professor of soil chemistry at the Utah Agricultural College (since renamed Utah State University). Over his lifetime he had a major impact, through research and consulting, on arid-land agriculture, not only in the USA but also in the Middle East, Pakistan, and India. He was an intellectual inspiration to me.  My mother, Alison Comish Thorne, with a PhD in economics, aspired to be an academic, too. However, her career was thwarted by Utah’s nepotism law that forbad the wife of a University employee from also working for the University; so she devoted most of her life to community organizing and community activism, and to raising and mentoring five children. Her lifetime impact on the community led the University to award her an honorary doctorate in 2000, when she was 86; and in 2004 when she died, a giant headline in the local newspaper, the Herald Journal, read “Old Radical Dies”.  Our parents encouraged my siblings and me to pursue our own interests, treasure our individuality, think for ourselves, and not automatically accept the dictates of the culture in which we lived. This and much more about my youth are described in my Mother’s autobiography, *Leave the Dishes in the Sink: Adventures of an Activist in Conservative Utah.*  As a small boy, watching plows create snowbanks as high as 3 meters in front of our home, I aspired to become a snow plow driver. Then, when I was eight, my mother took me to a lecture about the solar system at the local Mormon church (Logan’s fifth ward), a lecture by a professor from the University. I was enthralled, so my Mother suggested we make a model of the solar system on the sidewalk alongside our home. We drew the sun as a circle four and a half feet in diameter (about a meter), and then she showed me how, mathematically, to take the solar system’s actual dimensions and scale them down to this 4.5-foot sun. With our calculations completed we drew each planet as a circle at the appropriate distance from our sun. It was amazing to me: the Earth was a half-inch diameter circle a bit beyond the fourth home north of ours; and Pluto was a tiny circle about 3 miles away, in North Logan. I was hooked. I began to devour everything I could find about astronomy in local libraries and bookstores.  Five years later I discovered, in a bookstore in Salt Lake City, a paperback edition of *One, Two, Three, …, Infinity*by the physicist George Gamow. It dazzled me. It revealed the role of astronomy as a subfield of physics, the role of mathematics as the language of physics, the beauty of [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)’s relativity, and the power of physical laws to explain the universe. I read it three times and decided I wanted to become a physicist, pursuing a quest to understand the universe. Fourteen years later, when I had started publishing my own research, George Gamow sent me a letter inquiring about ideas in one of my publications. Thrilled, I wrote back, telling him I was a physicist because of having read his book three times. In response, he sent me a copy of *One, Two, Three, … Infinity*in Turkish, with an inscription “To Kip so that he would not be able to re-read it a 4th time”. That book remains one of my most treasured possessions.  My mother encouraged each of her other children to pursue their own chosen dreams. My sister Barrie, two years younger than I, became a professor of sociology. My sister Sandra, eight years younger, became one of the first female forest rangers for the US Forest Service. My sister Avril, nine years younger, became a professor of psychology. And my brother, Lance, eleven years younger, became an artist in wood.  Our ancestors, on all genealogical lines, joined the Mormon church and migrated to Utah on foot, on horseback, or in covered wagons before the railroad arrived (1869). Throughout my youth, our parents, Alison and Wynne, taught an adult Sunday school class, focusing on comparisons between Mormon thought and culture, and other religions and the ideas of great philosophers.  For me as a youth, Logan and its Mormon culture and history provided an idyllic environment, and I still treasure my Mormon roots. However, in my teenage years, as I learned more and more about science and discovered its power for explaining Nature and the Universe in testable and tested ways, and for producing technology that can improve dramatically the lives of people, and as I contrasted this with the more magical and less verifiable character of religion, I gradually lost interest in religion and even in whether God exists. (Much later, when my mother was 75 years old, at her urging, she and all her children resigned our membership in the Mormon Church, because of the church’s discrimination against women. My sister Barrie had already been excommunicated for her feminist activities.)  As a teen ager in the 1950s, I had an active social life. I played saxophone and clarinet in a dance band, participated in exhibition dancing, edited the high school yearbook, and was on the high school debate squad, partnering with the future All-American and All-Pro football player Merlin Olsen. But my deepest passion continued to be physics. While others were building telescopes, I – having been captivated by Mr. Thomas’s high school course on axiomatic Euclidean geometry in two and three dimensions – formulated it in four dimensions. I recall my excitement upon discovering, through a sequence of lemmas and a theorem, that in four dimensions the intersection of two planes is generically a point, not a plane.  In the summer of my eighth birthday I was at loose ends, so my mother sent me to typing school at Logan High School. “This may be useful to you someday,” she said; and indeed it became very useful decades later, in the era of computers. In my fifteenth summer my parents enrolled me in a geology course and an analytic geometry course at the University, opening my eyes to phenomena I had not dreamed existed. Thereafter, throughout high school, I continued taking an occasional university course.  **My university student years**  Despite my university experience as a teenager, when I arrived at Caltech as a freshman in September 1958, I found myself overwhelmed. I had had no calculus, I was a slow reader, and it quickly became evident that my thinking was slower than that of most other Caltech freshmen. I stumbled and struggled for a year and a half, but gradually developed my own ways of mastering the physics and mathematics that were coming at me like water from the proverbial fire hose. Most valuable of all was a series of notebooks that I developed for myself – one for each major class that I took. In each I wrote down the most important ideas and results I was learning, in my own words and equations, and formulated my own mathematical proof and/or physical explanation for each major result. I continued this through graduate school, then abandoned it for about 15 years, and then started up again in the late 1970s, when I was trying to master new topics and tools relevant to astrophysics and to gravitational-wave experiment. I still find myself consulting those old notebooks from time to time.  By the middle of my sophomore year at Caltech, I got my feet under myself and started enjoying my studies thoroughly, and started moving through difficult material at a reasonable pace.  In the summers before my first, second, and third years of college (1958, 1959, and 1960), I worked as an engineer’s assistant in the Great Salt Lake Desert, designing solid propellant rocket engines for the Thiokol Chemical Corporation’s Minute Man Intercontinental Ballistic Missile – engines that would later power the space shuttle. This gave me my first taste of “big science”, it showed me how various components of an R&D program should come together on a predefined time schedule, and it showed me how Nature can confound a research program: hot, turbulent gas swirling near the entrance to the rocket nozzles kept eroding the slowest-burning solid propellant (the “inhibitor”) so rapidly that the turbulence ate into the rocket casing, blowing the nozzles off the engine. The explosions, in test after test of our evolving design, were spectacular and frustrating.  In the summer before my fourth college year (1961), I got a job doing theoretical astrophysics research under the inspiring mentorship of the astronomer Jesse Greenstein. The result was my first published paper, on “The Theory of Synchrotron Radiation from Stars with Dipole Magnetic Fields”.  Ever since reading *One, Two, Three, … Infinity*, I had been fascinated by relativity. During my fourth year at Caltech I decided that was the direction I wanted to go for my PhD, so I spent many hours in the Caltech physics library trying to read relativity articles in research journals such as *Reviews of Modern Physics*. It soon became evident that by far the most interesting research on general relativity was being done by John Archibald Wheeler at Princeton University and his students, so I applied there for graduate school – despite Jesse Greenstein’s warnings that the only significant application of relativity was the expansion of the universe. In Jesse’s view, and that of many other eminent astronomers and physicists of the era, relativity was a dead end.  At Princeton, John Wheeler was an even more inspiring mentor than I expected, and his young associate Charles Misner added to the inspiration. From Wheeler and Misner I learned about black holes, neutron stars, singularities, and geometrodynamics (the ill-understood nonlinear dynamics of curved spacetime). In parallel, I sat in on the weekly research group meetings of Robert Dicke, whose focus was experiments to test general relativity; and there I met and admired postdoc Rainer (“Rai”) Weiss.  In that era, when relativity theory was far ahead of experiment and was only weakly tested, I somehow understood that the interface of the theory with experiment could become a fruitful and exciting area of research, so I not only immersed myself in Dicke’s experimental-gravity milieux; I also spent much of my first year at Princeton getting hands-on experience with experiment. In the bowels of the Princeton physics building there was a cyclotron (particle accelerator) on which, under the mentoring of assistant professor Edwin Kashy, I explored the internal structure of the nuclei of Rhodium atoms. This was rather far from relativity, but that experience (like my earlier experience with big science at Thiokol) would turn out to be extremely useful later, when I embarked on gravitational wave research.  In the summer of 1963, I spent eight weeks in a relativity summer school at the *École d’Été de Physique Theorique*in the French Alps. There Wheeler and Dicke gave inspiring lectures, and I met gravitational waves in depth for the first time, in lectures by Rainer Sachs (University of Texas) on the elegant, mathematical theory of the waves, and by Joseph Weber (University of Maryland) on his pioneering experimental effort to discover gravitational waves from the distant universe. I hiked with Weber in the surrounding Alps, we talked at length about his experimental program, I became a convert to the importance and possibilities of gravitational wave experiments, and I became rather fond of Weber himself.  I completed my PhD in June 1965 and spent one more postdoctoral year at Princeton, honing my theory research skills. In 1966 [Willy Fowler](https://www.nobelprize.org/prizes/physics/1983/fowler/facts/) (who would win the 1983 Nobel Prize for explaining the origin of the elements in stars) invited me back to Caltech as a postdoc, and I jumped at the opportunity. In May, while driving from Princeton to Caltech to start my new job, I stopped in Chicago for discussions with [Subrahmanyan Chandrasekhar](https://www.nobelprize.org/prizes/physics/1983/chandrasekhar/facts/) (who would share the 1983 Nobel Prize with Fowler).  Over the following decade both Fowler and Chandrasekhar made major contributions to my chosen areas of research and influenced me substantially (Fowler on relativistic stars; Chandrasekhar on black holes and gravitational waves), and both became dear friends of mine.  **Early years as a Caltech professor**  When I arrived back at Caltech in 1966, there was a paucity of theoretical physics faculty working outside elementary particle theory. Particle theory was in the doldrums and I was bubbling over with research problems involving black holes, neutron stars, and gravitational waves, so a number of outstanding physics graduate students gravitated toward me, looking for interesting research problems. By late winter, although just a postdoc, I had built a research group of five graduate students and was having a wonderful time working with them. Then in the spring, to my great surprise, the University of Chicago – under Chandrasekhar’s influence – offered me a tenured associate professorship. To my great joy, Caltech matched the offer, and almost overnight I was a tenured member of the Caltech faculty.  One of the great things about Caltech is the support that the administration and one’s colleagues provide to young faculty members, to help them reach their potential. Maintaining a research group of five or six graduate students and several postdocs, as I was doing almost from the outset, is not cheap. Initially most of the expenses were covered by Fowler’s research grants from the National Science Foundation (NSF) and from the Office of Naval Research. In 1968, when Fowler became a member of the National Science Board, which oversees NSF, he arranged for me to take over from him as the Principal Investigator on his large NSF grant. Under my leadership, that grant was renewed time and time again over the next forty years and remained my largest source of research funding until my formal retirement in 2009, whereupon my successor, Yanbei Chen, became the grant’s Principal Investigator, and remains so today, after several renewals.  My group’s initial research topics – black holes, neutron stars and gravitational waves – were all subtopics in a brand-new field called *relativistic astrophysics.*This new field grew out of the discoveries of quasars (1963; Maarten Schmidt at Caltech), pulsars (1967; [Tony Hewish](https://www.nobelprize.org/prizes/physics/1974/hewish/facts/) and Jocelyn Bell at the University of Cambridge), cosmic X-ray sources (1962; [Riccardo Giacconi](https://www.nobelprize.org/prizes/physics/2002/giacconi/facts/) and colleagues at American Science and Engineering), and the cosmic microwave background radiation (CMB 1964; [Arno Penzias](https://www.nobelprize.org/prizes/physics/1978/penzias/facts/) and [Robert Wilson](https://www.nobelprize.org/prizes/physics/1978/wilson/facts/) at Bell Labs, and then Robert Dicke and his group at Princeton). Thanks to these observational discoveries, relativity was suddenly relevant to a whole lot more in the universe than just its expansion. The merger of these discoveries with the theoretical ideas of Wheeler (Princeton), Yakov Borisovich Zel’dovich (Moscow), Dennis Sciama (Cambridge), Fowler, Chandrasekhar, and others, gave rise to relativistic astrophysics.  Very early in the development of this new field (summer 1965; before moving to Caltech), I attended the Fifth International Conference on General Relativity and Gravitation, in London. There I met and initiated close friendships with a few physicists who would profoundly influence my life and career. Most important, perhaps, were Stephen Hawking (a student of Sciama) and Igor Novikov (a young colleague of Zel’dovich).  Hawking had contracted Amyotrophic Lateral Sclerosis only two years earlier. In London, walking with a cane and talking with modestly mutilated enunciation, he lectured about his recent insights into the big bang. I was mesmerized by his science and also his personality. We talked in the conference corridors and found ourselves kindred spirits. Although, in the subsequent half century, Hawking’s research on black holes and the big bang has greatly impacted my gravitational wave work, we have never collaborated on research, and when together we have spent more time discussing life and death and love, than physics; so I shall describe the details of our friendship elsewhere, not here.  In London, Igor Novikov lectured about new insights in relativistic astrophysics that he and Zel’dovich had been developing. I had studied the Russian language as a Caltech undergraduate, and in London I found that my Russian was about as good (or bad!) as Novikov’s English, so we stumbled along in a semi-coherent mixture of the two languages, exchanging astrophysics ideas and initiating a friendship that would soon grow strong and deep.  In 1968, with my new Caltech research group beginning to make an impact, I was well prepared to take advantage of the next international conference on general relativity, this time in Tbilisi (Soviet Georgia). There I met Zel’dovich in person for the first time, and Zel’dovich introduced me to Vladimir Braginsky, who was building a research program in gravitational wave experiment at Moscow University in parallel with Joseph Weber’s in America. This was the beginning of my career-long research collaboration with the groups of Braginsky (on gravitational waves and experimental tests of relativity) and of Zel’dovich and Novikov (on black holes and neutron stars, and later on wormholes and time travel). To facilitate our collaborations, Braginsky, Novikov and I began traveling back and forth between Moscow and Pasadena with typically one trip per year in one direction or the other – despite the raging cold war. For a few details, see my book *Black Holes and Time Warps: Einstein’s Outrageous Legacy.*  During my first dozen years on the Caltech faculty, 1966–1978, gravitational waves were only a modest portion of my group’s research portfolio. Our larger foci were black holes, and other astrophysical phenomena where gravity is so strong that it must be described by Einstein’s relativity laws rather than Newton’s laws – primarily neutron stars and dense, relativistic clusters of stars. My students and postdocs (sometimes with a little help from me) used general relativity to analyze the structures and astrophysical roles of these objects, and also how they would behave when disturbed – their pulsations and their emission of gravitational waves. This fed into the main thrust of our gravitational wave research: our evolving vision for the information that can be extracted from gravitational waves, when they are ultimately detected; and more broadly, our vision for the future of gravitational wave astronomy; see my [Nobel Lecture](https://www.nobelprize.org/prizes/physics/2017/thorne/lecture/).  In the next to last section of this biography, I describe the style in which we carried out this research. That style included extensive interactions with colleagues from other institutions, including Zel’dovich, Novikov, Braginsky, and also Leonid Grishchuk in Russia; Hawking and Brandon Carter in the UK; Wheeler, Chandrasekhar, Fowler, James Bardeen and James Hartle in the US; and many more.  **Caltech’s early research in gravitational-wave experiment**  In his Part I of our joint Nobel Lecture, Rai Weiss describes the early history of experimental research on gravitational waves, including (very briefly) at Caltech. Here I shall add some details about the genesis and early years of the Caltech experimental effort.  My early ideas about gravitational-wave experiment were influenced profoundly by Vladimir Braginsky. After Weber’s 1969 announcement that he might be seeing gravitational waves, Braginsky (1969–1972) was the first other experimenter to build and operate gravitational wave detectors using the “bar” technology that Weber had initiated, and was the first to fail to find the waves that Weber appeared to be detecting (1972), and among the first to move on toward second generation detectors (1974). In 1972, after Rai Weiss wrote his seminal paper proposing the gravitational wave detectors – “gravitational interferometers” – that would ultimately be used in LIGO (see my Nobel Lecture), I turned to Braginsky for insights and advice about future gravitational wave experiments.  It was my many discussions with Braginsky in 1972–1976, as well as those with Weiss, that convinced me gravitational wave detection was truly feasible and led me in 1976 to propose to Caltech that we create a research group working on gravitational wave experiment. My first choice to lead our Caltech group was Braginsky. After many months of struggling with the idea of moving from Moscow to Caltech, he told me *No*. Even if he managed to get himself and his family through the iron curtain to California, the consequences for his professional colleagues and friends left back in Moscow could be dire, he thought.  When I asked Braginsky whom we should go after to lead the Caltech effort, at the top of his list was the same person as Weiss suggested to me: Ronald Drever of the University of Glasgow. Why? Because of Drever’s high creativity and his experimental insights. (For example, Drever had already proposed operating the arms of gravitational interferometers as Fabry-Perot cavities, which has turned out to be a major improvement on Weiss’s original design.) So, I suggested Drever to the Caltech physics and astronomy faculty, and after many months of learning about him and other candidates, they chose him to initiate our new experimental effort. The Caltech administration made him an offer which after many many more months, in 1979, he ultimately accepted. The next year we recruited Stan Whitcomb from the University of Chicago to assist Drever in leading our experimental effort. (Today Whitcomb is the LIGO Laboratory’s Chief Scientist.)  As a precursor to Drever’s acceptance, the Caltech administration pledged roughly two million dollars of Caltech’s own private funds for the construction of laboratories and equipment for the new experimental group, including, most importantly, funds toward a prototype gravitational interferometer with 40-meter arms.  This was the first substantial investment in gravitational interferometer research by any institution in the US: Neither MIT (Weiss’s home institution) nor the National Science Foundation had yet been willing to commit significant funds for such research. With Caltech on board, Weiss, Drever, and I, working with NSF’s Richard Isaacson, were able to trigger significant NSF funding from 1979 onward.  [I take great pride in Caltech’s early and enthusiastic commitment to this field and unwavering support from the 1970s through today. Caltech’s atmosphere of collegiality, intellectual ferment, and easy communication across fields of science, and our administration’s enthusiastic efforts to help us find the funding needed for realizing our dreams, have anchored me to Caltech throughout my career, as they also anchored [Richard Feynman](https://www.nobelprize.org/prizes/physics/1965/feynman/facts/) and many others of my colleagues.]  For me, the late 1970s and early 1980s were a particularly exciting period:  Drever, commuting back and forth between Caltech and Glasgow, made several inventions that would significantly improve gravitational interferometers:   * *power recycling*(recycling unused light back into the interferometer – which was also invented independently by Roland Schilling in Garching, Germany). * *resonant recycling*(tuning the response of the interferometer to waves of different frequencies by recycling some of the signal back into the interferometer before extracting it. A few years later, Brian Meers improved on Drever’s version of this and it got renamed *signal recycling*). * the *PDH technique for stabilizing the frequency of lasers*(adapted by Drever from an earlier microwave idea by Robert Pound, and then first demonstrated by John Hall and Drever in Hall’s lab in Colorado). This is now widely used in other areas of science and technology.   While Drever was inventing and commuting, Whitcomb and the students and postdocs that he and Drever hired were focused on building and perfecting the 40-meter prototype interferometer on the Caltech campus, and with it exploring technical issues that had to be surmounted in any ultimately successful gravitational interferometer.  In parallel, Carlton Caves and my other theory students and I – with very helpful input from Drever and Whitcomb – embarked on *Quantum Nondemolition*research: an effort to devise ways to circumvent the [Heisenberg](https://www.nobelprize.org/prizes/physics/1932/heisenberg/facts/) uncertainty principle in gravitational interferometers and other gravitational wave detectors. This effort was triggered by insights from Braginsky, much of it was in collaboration with Braginsky and his group, and it continues to this day; see my Nobel Lecture for details.  **LIGO**  In 1984 – building on successes with the interferometer prototypes at MIT, Caltech, Glasgow and Garching, and building on a feasibility study for kilometer-sized interferometers that Weiss and his MIT group and Whitcomb had carried out – Drever, Weiss and I founded LIGO as a Caltech/MIT collaboration. MIT was unwilling to make any substantial institutional commitment to LIGO until a few years later, so Caltech became our collaboration’s lead institution. Weiss and Barish sketch the subsequent history of LIGO in their parts of our joint Nobel Lecture.  From 1984 to 1987, I served as the “glue” that held our Caltech/MIT collaboration together, mediating between Weiss (who understood clearly that collaboration was essential for success) and Drever (who needed to be in complete control of all he did in order to remain creative and productive, and so had difficulty truly collaborating). It was with great relief that I relinquished my mediation role in 1987, when the three of us turned over the leadership of LIGO to our first director, Robbie Vogt, who quickly molded us into a truly functional, joint Caltech/MIT team.  In the meantime, Braginsky – despite having endorsed Weiss’s gravitational-interferometer ideas in the 1970s – focused the energy of his research group unwaveringly on a variant of Weber’s “bar” gravitational-wave technology. Braginsky was concerned that, to succeed, gravitational interferometers would have to become extremely complex (which they indeed are today, with 100,000 data channels that monitor their subsystems and the environment); and he worried that this complexity might ultimately doom the interferometers to failure.  Throughout the late 1970s and the 1980s, Braginsky and I both commuted back and forth between Moscow and California, maintaining a tight collaboration (particularly on quantum nondemolition techniques and technology; see my Nobel Lecture). And throughout this period, Braginsky advised Drever, Weiss, and their colleagues about interferometer R&D and planning. In the late 1980s, when Braginsky saw the progress that was being made with the prototype interferometers and saw the Caltech/MIT plans for a proposal to the NSF to construct LIGO, he became convinced that the probability of success was reasonably high; so he went home to Moscow, shut down his bar-detector research, and initiated in its place a whole new research program in support of LIGO. This had a profound effect on me, bolstering my confidence at just the moment my Caltech/MIT colleagues and I were developing our proposal and plans for LIGO construction.  In the early 1990s, under Vogt’s leadership, we secured approval from NSF for LIGO’s construction and we took major steps toward construction. Then in 1994–2001, our second director, Barry Barish, transformed LIGO from a small Caltech/MIT project into a large international collaboration, and led us through the construction of LIGO’s facilities, the installation of LIGO’s first interferometers, and the writing of a proposal for the advanced interferometers that have now succeeded in discovering gravitational waves; see Barish’s part II of our joint Nobel Lecture.  In 1992, with LIGO starting to move forward, I wound down other efforts (including the theory of time travel) that my theory research group was doing and I refocused our research almost completely onto theoretical support for LIGO. This included analyzing sources of noise in LIGO’s interferometers and ways of controlling the noise (see my Nobel Lecture), a beefed-up effort on quantum nondemolition, and a renewed effort to understand sources of gravitational waves, and the shapes of their waves – their *waveforms.*  It was only then that I began to realize how difficult would be the analysis of LIGO’s data – finding weak gravitational-wave signals amidst LIGO’s noise, and extracting the information carried by the signals. Fortunately, my former student Bernard Schutz, at the University of Cardiff, UK, had recognized this as early as 1986 and had begun then to lay foundations for the data analysis (see my Nobel Lecture). To bring Caltech up to speed on the data analysis, we imported Bruce Allen from the University of Wisconsin; and he, together with a number of my students and postdocs, dove into the problem while I cheered them on. Soon thereafter, Barish, as LIGO’s second director, created the LIGO Scientific Collaboration (LSC), which facilitated expanding the data analysis effort to scientists at many other institutions; see Barish’s Part II of our joint Nobel Lecture.  To help educate the many hundreds of scientists who joined the LIGO effort in the late 1990s and the 2000s, I created in 2002 an online course in gravitational-wave physics that included videos of lectures about all aspects of the field, by the best experts.  By 2002, it seemed to me that I was no longer much needed within the LIGO Project. The students and postdocs I had trained, and other LSC theorists, could play the roles that I had been playing, and could do it at least as well I, if not better. So, with a sigh of relief (because by personality I did not really like working in a large project), I left day to day involvement with LIGO, and focused my attention largely on building at Caltech a research effort on computer simulations of colliding black holes and other sources of gravitational waves; see my Nobel Lecture for details.  One consequence of my departure from day-to-day LIGO work was my non-involvement in Advanced LIGO and its triumphant discovery of gravitational waves.  The credit for that ultimate success, and for all the rich insights about the universe that have begun to flow from it, belongs largely to the younger generation of LIGO/Virgo scientists and engineers, and also to my Nobel Prize co-laureates Rai Weiss and Barry Barish, who have continued to make major contributions in the Advanced-LIGO era.  I continue to help the LIGO Project whenever called on for help, but that is less and less often as time passes (and almost entirely on political issues and not technical issues).  **My students and postdocs**  Over the near-half-century of my career, my graduate students and postdocs have done much more important and impactful research, while in my group, than I myself. I take great pride in their accomplishments, some of which I describe in my Nobel Lecture.  In many cases they took research problems that I suggested, and with very little help from me, brought the problems into soluble form, solved them, and made major discoveries; an example is the work by Alessandra Buonanno and Yanbei Chen on quantum noise in Advanced LIGO interferometers (see my Nobel Lecture). In other cases, they identified important research problems themselves and, with little concrete input from me, brought the problems to fruition, with impactful results; examples are Carlton Caves’ work on the origin of quantum noise and using squeezed vacuum to modify it, and Yuri Levin’s work on thermal noise in interferometers (see my Nobel Lecture).  I patterned my style of working with students and postdocs after the styles of Wheeler, Dicke, and Zel’dovich (which I had observed up close) and of Robert Oppenheimer in Oppenheimer’s Berkeley/Caltech years (the 1930s). I gave the students a lot of room and time and freedom to explore things on their own, flounder, and ultimately find themselves, with an occasional nudge from me. But I also gave them an intellectual environment in which to learn from each other, and from students and colleagues elsewhere – an environment that included weekly group meetings typically two hours long and sometimes far longer than that, with frequent participation by experimenters from the Drever/Whitcomb group and later the LIGO Laboratory, and by outside experts. It also included frequent trips to Santa Barbara to interact with the superb relativity group that James Hartle had created there, and frequent visits to Caltech by research leaders from around the world – for example, members of Zel’dovich’s and Braginsky’s groups, and Stephen Hawking and members of his Cambridge research group. We had an *Interaction Room,*with a huge blackboard, a refrigerator filled with drinks, and comfortable couches and chairs, in which we would gather for spontaneous discussions as well as organized discussions.  Over my 43 years of mentoring students and postdocs, roughly 2/3 of our time and effort went into gravitational-wave-related research, largely connected to LIGO or what would become LIGO, but also connected to LISA, Weber-type bar detectors, and sources of gravitational waves in all frequency bands. I describe some of this research in my Nobel Lecture.  The other 1/3 of our time has gone into a wide range of other issues in relativistic astrophysics, or relativity, including a highly enjoyable period of several years in which we asked ourselves whether the laws of physics permit an infinitely advanced civilization to build wormholes for rapid interstellar travel and machines for traveling backward in time. (Although such questions may seem weird or flaky, they are useful tools for probing the laws of physics in domains where experiment is not yet possible. For example, our research, and that of Hawking and his students, have convinced both Hawking and me that the poorly understood laws of quantum gravity control whether or not backward time travel is possible.)  **A new career at the interface of science and the arts**  Since 2009 I have turned much of my effort in a very different direction: collaborations about science with artists, musicians and film makers.  Christopher Nolan’s movie *Interstellar*was one fruit of this, and with Stephen Hawking and my long-time Hollywood partner, Lynda Obst, I have a second science-inspired movie in the works. With the painter Lia Halloran, I am working on a book about the Warped Side of the Universe (objects and phenomena made largely or wholly from warped spacetime, most of them sources of gravitational waves). And I have been doing an occasional multimedia concert about the Warped Side of the Universe with composer Hans Zimmer and visual effects gurus Paul Franklin and Oliver James, using beautiful videos generated by numerical relativity physicists. I take great pleasure in these collaborations with brilliant and creative artists, who bring to our joint work talents and insights quite different from my own. These collaborations are my attempt to inspire nonscientists and especially young people about the beauty and power of science, in the same way as George Gamow’s book *One, Two Three, … Infinity*inspired me, 65 years ago.  **My family**  This is a scientific biography, so I have chosen not to discuss my two marriages (to Linda Thorne, 1960–1975; and then to Carolee Winstein, 1984–…), nor Linda’s and my children Kares Anne Thorne and Bret Carter Thorne (and his wife Regine Thorne), and granddaughter Larisa Anne Thorne. Suffice it to say that they all have been tremendously important in my life and have provided a balance to my scientific work that has helped make me more productive. They all went to Stockholm with me to share in the Nobel Week festivities. |
| **Autobiography** |  |
| **Podcast** | **0023=Kip S.Thorne**  **No scirpt** |
| **Telephone**  **interview** | **0023 = KT**  [Kip S. Thorne] Hello  [Adam Smith] Good morning, my name is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize in Stockholm. First of all congratulations on the award of this year’s Nobel Prize.  KT: Thank you very much.  AS: It could hardly have come any quicker. The announcement was just last year.  KT: Yes, that’s right. It is amazingly quick.  AS: [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html) himself probably didn’t think that gravitational waves could be recorded. But you have always been a believer I gather?  KT: Well I have been a believer but I began … I’m much younger than Einstein and by the time I came along there were lasers, there were massive computers, technology had changed, and our understanding of possible sources of gravitational waves had changed. Neutron stars and black holes which should be the strongest sources, Einstein had none of that to base his ideas on. So, yes, in his seminal paper on gravitational waves he indicated skepticism that gravitational waves would ever be detected.  AS: And they open a new window on the universe. What will we be able to see now that we can detect gravitational waves?  KT: I think over the coming decades we will see enormous numbers of things. Just as electromagnetic astronomy was begun in essence, at least modern astronomy, by Galileo pointing his telescope in the sky and discovering Jupiter’s moons. This is the same thing but for gravitational waves. This is … Gravitational waves are the only other kind of wave, besides electromagnetic that propagate across the universe, bringing us information about the universe, so initially we will see not just binary black holes. We will see neutron stars collide, tear each other apart, we will see black holes tearing neutron stars apart, we will see spinning neutron stars, pulsars, when the space-based LISA mission is operating hopefully by about 2030, we’ll be exploring basically the birth of the universe, the earliest moments of the universe. And there will ever so much more I’m sure, including huge surprises, as the years wear on.  AS: Talking of surprises, the one that you’ve just received, how did the news come to you?  KT: Well I think it was not unexpected that this opening gravitational wave window onto the universe would get a Nobel Prize. I was hoping that the prize would go to the LIGO-Virgo collaboration, which made the discovery, or to the LIGO laboratory, the scientists of the LIGO laboratory, who designed and built and perfected the gravitational wave detectors and not to Barish, Weiss and me. We live in an era where some huge discoveries are really the result of giant collaborations, with major contributions coming from very large numbers of people. I hope that in the future the Nobel Prize committee finds a way to award the prize to the large collaborations that make this and not just to the people who may have been seminal to the beginning of the project, as we were.  AS: That I guess is a conversation and a debate that is going to run and run, yes.  KT: I feel that I’m an icon for the LIGO-Virgo collaboration and the LIGO laboratory and I’m pleased to be that icon and represent what they have achieved.  AS: That’s nicely said. So will we be welcoming you to Stockholm in December then?  KT: Yes of course.  AS: [laughs] Good. Once again congratulations and we greatly look forward to meeting you in December.  KT: Thank you  AS: Thank you. Bye bye. |
| **Interview** |  |
| Q3 | **Why did you decide to become a physicist?** |
|  | Kip S. Thorne: I grew up in Logan, Utah, which is high in the Rocky Mountains, altitude nearly 2,000 meters. We had deep snow when I was growing up so obviously, and before I was eight years old, I wanted to be a driver of a snow plow because you could push the snow to such great heights. But then when I was eight my mother took me to a lecture about astronomy, about the solar system, and I became very excited about the idea of the solar system, so she did some projects with me about astronomy. And I started doing reading on my own, decided I wanted to be an astronomer.  But then when I was thirteen I found in the big city, Salt Lake City, a great big city, about a hundred thousand at the time, I found a little paperback book called ‘One two three infinity’ by a physicist cosmologist named George Gamow, and it described ideas for mathematics and theoretical physics and cosmology that I found even more exciting than astronomy, and so at age thirteen I decided I wanted to be a physicist but I would work on things connected with astronomy and so here I am. |
| Q3 | **What do you enjoy about science?** |
|  | Kip S. Thorne: The most enjoyable part of science is doing it. It is sometimes very hard, sometimes very frustrating but extremely rewarding when you suddenly understand something. It is an adrenaline rush when you suddenly understand something. It does not matter very much whether somebody else has understood it first or not. It is nice if you are the first person, but just to suddenly understand a puzzle that you have been struggling with for a long time is just fabulous. And it is remarkable that we as humans are capable of understanding the physical world around us in such detail that we can predict things that turn out to be true, that we can understand things that are very far from earth, such as the black holes that we have described colliding with gravitational waves. And that we can use the understanding we develop in the physical laws for technology for human benefit, so that aspect of it also is really quite wonderful. The power of science for understanding and for technology. But personally, the joy of discovery is the big deal**.** |
| Q2 | **How important are imagination and creativity?** |
|  | Kip S. Thorne: Imagination and creativity are really very crucial, particularly for the big leaps of understanding. But they are far from enough, because you may have imagination and creativity and suddenly think you understand something far beyond the frontiers of current knowledge, but you will usually be wrong, and you validate this through experiment and you validate it also by seeing how it fits into the well-established laws of nature and how it fits logically into this complex structure of all the well-established laws of nature. So you really, in order to validate the insights, you have to use these two additional things, experiment and detailed mathematical analysis. |
| Q5 | **Who are your biggest influences?** |
|  | Kip S. Thorne: There are a number of people that have influenced me and how I think and work as a physicist. John Wheeler, who is my PhD mentor and was a tremendous inspiration. We had very different views about the political world, but we were very much of the same mind about how you understand nature. And I learned so much from him, he was a professor at Princeton. Also at Princeton was Robert Dicke who is a superb experimental physicist who was a mentor to Ray Weiss, my colleague on the LIGO project. And I was there studying about black holes with John Wheeler and gravitational waves and also participating in the research group meetings of Robert Dicke, and I was learning about how experimental physics is done through Robert Dicke and his research group and Ray Weiss. So those two were among the handful of people who profoundly influenced me. |
| Q5 | **Which Nobel Laureates inspire you?** |
|  | Kip S. Thorne: Among the Nobel Laureates my colleagues that with whom I am receiving this prize. Actually, Barry Barish and Rainer Weiss and I are icons for a very large experimental team of a thousand people in LIGO. That team is so superb, but the people that really have inspired me working with them  are Weiss, Barish and Ronald Drever, who is no longer with us, he has passed away, whom I have worked with intensely on this. Also Robbie Vogt who was the first director of LIGO and helped us in the first step in the transition from a set of ideas into the real world of where LIGO is today. |
| Q2 | **How important is collaboration?** |
|  | Kip S. Thorne: Let me describe my personality. I am a person who likes to work on science quietly by myself or with one or two students, maybe a postdoctoral student. I am an introverted. I behave like an extrovert, I learned how to do that, but I am fundamentally an introvert. I get the greatest pleasure from that kind of work. But LIGO could only be done as a big collaboration, there was no other way to do it. So I gritted my teeth and I plunged into this and helped in every way that I possibly could to lay foundations for LIGO and to help Barry Barish wherever he needed any help from me. Grow LIGO from a small experimental project that I began with Ronald Drever at Caltech, and Ray Weiss at MIT, a small collaboration into what it is today. It could only be done as a big collaboration. It is so difficult, the things that go wrong are such number of different things it requires large numbers of experts and a variety of different pieces of physics and engineering to pull it off.  LIGO is the triumph of a thousand people, the superb experimental team. I think my biggest contribution was to understand where they had to go because I am a theorist and I knew about how strong the waves were. I understand how they interact with the detector, I understood what needed to be done. There is no way I could do any of that, but I could also convey to the funding agencies and the science community my faith in the experimental team and I could explain why I had faith in the experimental team, so that was probably my biggest contribution, to convince funding agency and physicists that this should go forward. It is a strange kind of a role but I think without that role it would probably not have happened. |
| Q10 | **How important is government support?** |
|  | Kip S. Thorne: For a project of this sort the only way it could be done was through a governmental agency. It was a project that cost up until now about 1.1 billion dollars. It is a lot of money. Not as much as some of the very biggest physics projects but by far, I think, the largest thing that the National Science Foundation in the US has ever done. It was absolutely crucial. So this really was a collaboration, initially between Caltech led by Ronald Drever and me, MIT led by Rainer Weiss and the National Science Foundation were the key person was Richard Isaacson who was our programme director, who himself had made an enormous breakthrough in the theory of gravitational waves, had solved a problem that had puzzled everybody from [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) on for decades. How is energy carried by gravitational waves? This was Isaacson who turned into government funding agent and he understood how things worked in Washington and he understood what we were doing because he is such as superb physicist himself. He was really the additional person who pulled this off. Without the National Science Foundation and Isaacson this would never, never have happened. |
| Q2 | **Can breakthroughs in physics still be done independently?** |
|  | Kip S. Thorne: You need to work on a breakthrough problem with whatever kind of team is optimal. And in many areas of physics a very small team is optimal. That is true particularly in condensed matter physics. It was true on this year’s chemistry Nobel Prize which was done by physicists. It is true in last year’s physics Nobel Prize and so I would say the majority of breakthrough work can still be done in small teams but there are certain things, and gravitational waves is one, that can only be done in a big collaboration. We in the physics community have learned, I think, how to function side by side as colleagues with some people working in huge teams, like LIGO with a thousand people, and others who are working in the manner that I prefer to work myself, in a very small effort with just one or two or three professors and a small number of students and postdoctoral students. Both are needed depending on the problem. |
| Q3 | **Do you enjoy being a mentor?** |
|  | Kip S. Thorne: My mentoring has been one of my greatest joys. I am proud that I have mentored fifty something, fifty-two or fifty-four, I do not even know the number, PhD students during my career and that they have gone in huge number of different directions because I mentored them broadly so that they had the tools to be able to work in anything from an analyst at the CIA on one extreme, to a very abstract theoretical physics, another extreme, in the computer industry, in management. I had a student, two students who moved into the financial world very, very successfully. And they look back and they say far more useful to them than Harvard Business School, which was a key piece for what they wanted to do, was doing a theoretical physics PhD under me. Because in doing a theoretical physics PhD they learned how to take a complex problem, break it down into pieces that could be solved, how to transform a problem into a soluble form, and that general skill that is the essence of success in physics is transferable into all these different areas of human enterprise. So yes, I am proud of my mentoring and I have taken great joy in it. |
| Q1 | **What is your favourite piece of advice for students?** |
|  | Kip S. Thorne: I have several favourite bits of advice. The one that I gave particularly to my granddaughter who is a physics PhD student at Carnegie Mellon University now. I told her when she said she wanted to be a physicist I said physics is a great springboard from which you can move in many directions, so fine, but you have to find a direction that you absolutely love. Because if you are going to spend a large portion of your life on something it has to be something you love. It also should be something that is important, something that can help the world, but you have to love it and this advice that I got from my own grandfather when I was about four or five years old. He told me, Kip, if you will succeed in life if you find a job that is like play. So that is one thing, you have to be willing to, ready to, eager to work very, very hard. It does not come easily. So that is the second piece of crucial advice.  Third piece of advice is you find your own way of functioning. My mind is much slower than most of my colleagues’ minds and I discovered that when I was an undergraduate. I struggled for the first year and a half as an undergraduate at Caltech where I spent most of my subsequent career, but I developed my own ways of learning things. Keeping records of what I was learning, working things out in my own way, and notebooks and so forth, that enabled me to achieve despite having a slow working mind. So I advise students, you find your own way, and you have to experiment of mastering material and your own directions that you can be successful in and everybody is different. We can all succeed in different ways. |
| Q8 | **How did you become interested in movies?** |
|  | Kip S. Thorne: Almost everything that I have done was something I never planned to do. That was true of LIGO, gravitational waves. I have watched through my entire career, my lifetime particularly as an adult, for opportunities, unexpected opportunities and that’s just basically what gravitational waves were. I came along at just the right time to do this together with Barish and Weiss and I jumped on the opportunity once I saw that it had a real possibility to succeed. Similarly, I never intended to be involved in movies, but I was single in southern California for about nine years and I dated in Hollywood and one of the people that I dated was a woman named Lynda Obst. She was a movie producer, had just arrived in Hollywood at the time that I was single and so our romance never went anywhere because perhaps I was too much of a nerd for her and she was perhaps too intense for me, but we became very close friends and she is a close friend of my wife Carolee as well today.  Many years later Lynda telephoned me, and she said Do you want to brainstorm with me for a movie? I thought for a very short time and said Yes. I could see immediately, one it would be fun, I would be working with brilliant people who were very different from me and that is particularly fun. And I would have a possibility to convey through a Hollywood movie the beauties and power of science to an audience what turned out to be about a hundred million people who bought a ticket to this movie. How else can a professor reach a hundred million people? So I said Yes. We brainstormed and we created the ideas for a movie, and she brought Christopher Nolan and Jonathan Nolan on board to write the screen play and direct it. They completely changed our story but kept all the science that we put into the movie. It turned out to be a wonderful collaboration between me and these filmmakers about a movie in which the movie is really based on and steeped in real science. |
| Q9 | **Is popular culture an important way to educate audiences?** |
|  | Kip S. Thorne: Popular culture has tremendous potential for inspiring people about real science. I do not know how effective we can be about educating people about real science. Interstellar was not an education process it was more of an inspiration process. I wrote a book to go along with the movie called ‘The Science of Interstellar’ which is my attempt to provide education in addition. But I think popular culture can provide tremendous inspiration about science and can convey some of the ethos of science but hard to convey the basic ideas with any precision obviously. |
| Q8 | **What is your favourite film?** |
|  | Kip S. Thorne: I think my two favourite films were not necessarily scientific, were the two that preceeded mine, that were also, had the science built into them from the beginning, ‘2001: A Space Odyssey’, where a physicist Arthur C Clarke provided the underpinnings for it. It was Stanley Kubrick’s film and then the movie ‘Contact’ where Carl, which was a collaboration between Carl Sagan and Lynda Obst, the same woman that I collaborated with to make ‘Interstellar’. It is the beginning for that film it was Carl Sagan’s film and, in both cases, again the science was embedded so deeply it was inextricably interwoven with the film and I have loved that. |
| Q2 | **Has working in films helped your own research?** |
|  | Kip S. Thorne: In small ways but not major ways. In ‘Interstellar’ I worked very closely with a visual effects team with computer graphics people at the company Double Negative were the lead person is Paul Franklin, who got the Academy Award for the visual effects in ‘Interstellar’. And in order to make the beautiful images of the black hole, gas around the black hole, a swirling gas around the black hole, the wormhole in that film, it was necessary to create a whole new way of going from a computer simulation to visuals on a screen.  Oliver James, who is the chief scientist at Double Negative, and I worked out this new method to do it which was necessary because you could not get the high resolution smooth images that were required for this science fiction movie in any other way. But the methods that we devised are now being used by astrophysicists as part of their visualization of simulations they do of things like black holes and accretion discs around black holes and neutron stars colliding neutron stars and so forth. So there is a feedback in that sense but I think beyond that the direction of the feeding is largely from the science into the film and through that to a popular culture. |
| Q8 | **How do you like to spend your free time?** |
|  | Kip S. Thorne: You know, my current career that interface with arts, the arts which is not just movies. It is music with Hans Zimmer and visual effects, multimedia, concerts, it is a book I am working on with my poetry and paintings by a superb young painter. My new career is my hobby in some sense. I thoroughly enjoy these interfaces with the arts. But also my wife and I just enjoy each other and we have a wonderful time. In very extreme moderation, we do not have that much time for it, but we hike, we scuba dive and we ski but not very much in the last few years. The last few years have just been too hectic. |
| Q2 | **Where do you do your best thinking?** |
|  | Kip S. Thorne: The kind of work that I do, whether it is working on a movie searching for ideas for a movie or in physics, I collect information, problems, issues that I am struggling with during the day. Day after day and I may be struggling with some issue. How do you depict something in a movie? Or how do you solve a particular physics problem? I may collect all this information related to it during the day for a few days. Then in the middle of the night the inspiration comes, somehow things connect together in the middle of the night. I get up going to the bathroom and write notes and go back to bed. Usually the notes are fairly coherent and often they have the key idea that I could not get in any other way. My mind has to go more or less blank and things have to just somehow naturally start fitting themselves together and in a semi-conscious state and that is where the inspirations come. |
| Q4 | **What projects are you working on now?** |
|  | Kip S. Thorne: Gravitational waves are a tool to explore what I like to call the warped side of the universe. These are objects and phenomena, they are not made from matter like you and I and people watching this video, but instead are made from warp space and warp time. A black hole is the prime example. Black hole is made from warped space and the diameter of black hole is huge compared to the circumference whereas normally circumference is bigger. Time slows to a halt near the surface of a black hole. You see this in ‘Interstellar’ where Cooper’s daughter Murph, Cooper being Matthew McConaughey, Murph being Jessica Chastain. Daughter Murph ages from age eleven to ninety-five while Cooper ages hardly at all because Cooper goes near a black hole where time slows. So near a black hole time slows down. Inside a black hole time flows in a direction you would have thought was a space direction toward the centre but that is the direction time flows.  Black holes drag space into whirling motion like the errand or tornado. So black hole is made from warped space and time. Colliding black holes create a veritable storm in the fabric of space and time. There are other phenomena on the warped side of the universe, the birth of the universe itself. The earliest moments of the universe space and time tremendously warped. Things called cosmic strings where the circumference around this sort of rubber band like object is less than π times the diameter. And these weird things that are made of space and time are a wonderful topic for a book about the warped side of the universe.  This book is a collaboration between Lia Halloran who is a young painter and photographer who is good enough to have sold pieces to the Guggenheim Museum in New York. She is in her thirties and so it is her paintings of these phenomena, the storm and the fabrics of space and time to produces gravitational waves, cosmic streams, that produce gravitational waves. Her paintings and my poetry about it, so I never shown anybody except my wife hardly any of my poetry. My attempts of poetry, if my poetry is so bad that it makes this book be a total loser and does not sell but two copies one to her and one to me, well that is alright. I have had success elsewhere and if I drag her down that is alright. She has had success elsewhere, so we are having fun doing something that is different for us. Perhaps people will enjoy it. I enjoy the process very much of working with creative people who are very different than I am and trying to do something different from what I have done before. |
| Q17 | **What do you find harder – poetry or physics?** |
|  | Kip S. Thorne: Right now for me writing poems is a lot harder than doing physics because it is new. That is why I am doing it. I have done physics for most of my life, I have been doing physics in a serious sort of way for more than half a century and so let’s do something different. That is also very hard because I am very new at it. So that is the challenge and that is the joy. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0024** |
| **Biographical** | **The Name**  The name Thouless is very rare. Fewer than 150 people with the name live in Britain, almost all of whom are connected to Norwich. This is because it is a relatively new spelling of an old name spelt variously Thules, Thewless, Thewlis etc. Five generations before David, his ancestor John was born Thules. When John’s son, James, was born, both parents were illiterate and the clergyman filling out the baptism certificate wrote what he thought he heard – ‘Thouless.’ It seems that almost everyone with this particular spelling of the name is descended from James.  **Mother’s Family Background**  David’s mother was born Ella Grafton Gorton in 1898. She changed her name to Priscilla when she was studying in Italy, where she found that having a name that translated to “She” in Italian was inconvenient. The family name originates in Gorton, a suburb of Manchester. Her father’s family had been Church of England clergymen back into the 1600s and continued this tradition through her generation. Her grandfather, father, three of her brothers and two of her brothers-in-law were all clergymen. The most prominent of these was her brother Neville Gorton, the Bishop of Coventry who was deeply involved in building the new cathedral after the war.  Priscilla was the sixth of seven children. She was taught by a governess who did not like mathematics and influenced her pupil to feel the same way. After her father died, she went to Altrincham High School and then got a scholarship to Manchester University. She gained a BA and MA in literature and taught English at Manchester until David’s older sister Susan was born in 1925.  David’s generation, which included 14 first cousins on his mother’s side, were not involved with the church or science. The most notable of his cousins was Assheton Gorton, who was an artistic director of a number of well known films such as *Blow Up*and *The French Lieutenant’s Woman*.  **Father’s Family Background and Scientific Interests**  David’s father Robert Henry Thouless was born in Norwich in 1894. Robert’s father Henry James Thouless married Maud Harper from Devon who was studying at a Teacher Training College in Norwich. Henry James was a company secretary at Barnards, a Norwich engineering firm. However, his passion in life was natural history, with a particular interest in insects, specifically moths. He served a term as the President of the Norfolk and Norwich Natural History Society and had a bungalow on the edge of a marsh in Wroxham, which was ideal for finding insects. He bequeathed a collection of insects he had collected, which included two named after himself, to the museum in Norwich Castle.  Robert had two sisters, Sybil and Margaret. Sybil became a nun and taught school in the order of Notre Dame. The younger sister, Margaret, also became a teacher. She studied at Oxford University before they allowed women to take degrees. Once women were allowed to take degrees, she returned to study Latin for a year, as this was a requirement for graduation. Margaret had wanted to study science, but she was considered too frail to do the lab work required. Instead, she taught English literature and foreign languages at St. Mary’s Calne, a girls’ private boarding school.  David’s father Robert attended King Edward VI School in Norwich. In 1912 he went as a scholar to Corpus Christi College, Cambridge. In 1914 he was awarded a bachelor’s degree in natural sciences. He joined the Royal Engineers as a signaller. After a couple of years, in 1917 he went to the Salonika Front, from which by his account he was lucky to have come home alive. He became a lifelong pacifist but it did not stop him joining the home guard to defend his own town, Cambridge, during the Second World War.  After the First World War, Robert returned to Cambridge and did a PhD in psychology. He then became a lecturer at Manchester University before moving to Scotland to start the Psychology Department at the University of Glasgow. While at Glasgow, he did his most important work on how an object is perceived, introducing the term “phenomenal regression” in 1931. In the 1930s this was a very unfashionable line of research, and it did not enter mainstream psychology until the 1950s. Robert was offered the resources to study this phenomenon in Australia after he had retired, but he replied that he did not have the strength and brain power he had had when he wanted to study the topic 40 years earlier. David has followed his father’s originality of thought, which sometimes came before the rest of the world is ready to engage in a topic.  Robert Thouless was also known for his radio programmes on how to critically analyse flaws in reasoning and arguments, which he later turned into the book *Straight and Crooked Thinking*. This is known as *How to Think Straight*in the US. It has been a required textbook for many generations of students of rhetoric. His grandson Christopher Thouless has revised the last two editions, so it has been in print for over 85 years.  Later in life, Robert concentrated his research efforts on studies of the paranormal. He was elected President of the Society for Psychical Research in 1942. Although a frequent result of his painstaking investigations was the detection of cheating in apparent cases of psychic powers, he continued to believe in the possible existence of such abilities.  **David’s Education with Emphasis on Mathematics**  Even as a four year old, David was precocious in mathematics. One day, after discussing with his father how far counting goes, David decided to take the experimental approach. His family was bored by the time he reached 500 and even more bored by the time he reached the second thousand.  Just before his fifth birthday David and his sister were evacuated to his grandmother’s house in Devon at the outbreak of the Second World War. While there, David taught himself to read and write, with the help of his grandmother’s housekeeper.  As soon as it appeared that a German invasion would not happen immediately, David and Susan returned to Cambridge and David started school. At this point David stopped asking how to spell words and started thinking about arithmetic. With the aid of a simple abacus he worked out problems for himself. He worked out the 2 times table, working out what 2 times 27 was before he got bored with the project. At age 7 he set himself the task of working out how many seconds there are in a year. From age seven through eight, he spent two years as the only boy in the top class of the school, with the rest of the class being large 9- to 11-year-old girls, which was not a situation he enjoyed at that age. Much to his relief, his next school St. Faith’s was a boys’ prep school.  David’s father had a big influence on his intellectual development. “When I was 5 my father taught me to play chess, at which I slowly acquired competence but not brilliance. I think I was a teenager before I had a good chance of beating him at chess, but I was generally much better than my friends.” In fact David continued to play chess until he was in graduate school, when he felt the mental effort was too similar to physics. He and his friend played chess in their heads on long Territorial Army marches.  My formal education in science was close to non-existent until I was nearly fourteen. I can remember one young man trying to teach chemistry. Half the boys knew what he was talking about, but I had no idea why a chemical should go in one particular direction, rather than to any other end-product that had the same number of each atomic species. Fortunately my father was always willing and able to fill such gaps in my understanding.  I cannot enumerate all the things I learned from my father. He certainly told me about Wegener’s theory of continental drift, which was very unfashionable at that time. His enthusiasm for probabilistic reasoning was something he shared with me quite early; he was an early follower of Cyril Burt in stressing the importance of careful statistical analysis of psychological tests. He showed me how base 2 arithmetic could be used to win the game of Nim. I saw, but never really absorbed, the Boolean notation he used to solve problems in logic.  David met interesting visitors his father invited to the house. “A frequent visitor to the house in the early years of the war was the philosopher Ludwig Wittgenstein. My father had been to his lectures before the war, and there was an annotated copy of the *Blue Book*among his papers when my father died. I also found notes on a series of conversations on philosophical and scientific matters between Wittgenstein, my father and Cyril Waddington. These were published in 2003 by James Klagge and Alfred Nordmann in the book *Ludwig Wittgenstein: Public and Private Occasions*.”  **Winchester**  St Faith’s School in Cambridge encouraged David to compete for a scholarship to Winchester College. He was not sure he wanted to go, so his parents made alternative arrangements in case it should prove too stressful for him. He won the top scholarship of the year with an outstanding mathematics result and very good English and Latin. David had not studied Greek, so he did not attempt that paper. He was the first ever student to come top in the scholarship exam having only done three out of the four papers.  It was also decided that I should take the School Certificate at the end of the first year, despite the fact that I would still be thirteen, because in preparation for the introduction of O-levels in 1950, there would be a minimum age for School Certificate in 1949.  As a result of this I took the exams in English Language, English Literature, Mathematics, Further Mathematics, History (Ancient Greece), Latin, Greek and Divinity (including St. Luke in Greek). I was still struggling with Greek, quite enjoying the struggle, and got Credit in Greek, but got Very Good in all the other subjects. I had no official science background, nor any modern foreign language qualification. I did not take any serious external examination in foreign languages until I entered the Cornell Graduate School on my 22nd birthday.  David got an excellent education in science at Winchester. “In all subjects there was a lot of emphasis on private study and assignments, and we spent relatively little time in class, perhaps less than eighteen hours a week, over about 36 weeks a year.” Time was found for a broad rounded education in addition to science. For example, “One of the joys of my second year was that the formmaster was Harold Walker, the head of the history department. His one-term course on American history left enough in my memory that I found no need to revise when I took the test for US citizenship. His scholarly but sceptical teaching of divinity was challenging and refreshing, particularly to someone like me who took religion rather too seriously.”  During David’s time at Winchester his termly reports did comment on his mathematical ability but expended far more space on his untidy work and handwriting. This may have led to his excellent habit of developing his equations on scrap paper and when he was satisfied with them copying them into hard backed numbered page note books. These have now been deposited in the archives of the Royal Society.  **Cambridge University**  David was fortunate that he did not get the scholarship he wanted to Trinity College, but did get one to Trinity Hall next door. Trinity Hall was a much smaller college, better suited to his introverted personality. He made a number of really good friends while there, some of whom he sees to this day. He became an honorary fellow of Trinity Hall in 2014 and enjoys participating in some of their activities.  Describing his undergraduate experience, he said:  I knew the Senior Tutor Charles Crawley well, as his son John was and is a good friend of mine. My own Tutor was the distinguished historian and theologian Owen Chadwick, and the other Tutor was Shaun Wylie, who supervised me in mathematics, and was probably the single most important influence on my academic development as an undergraduate. None of us were supposed to know the significance of Bletchley during the war and of Cheltenham later, but somehow or other, from various different sources, I had picked up a fair idea of what Shaun and his colleagues had been up to in those places.  The other piece of good fortune was that Trinity Hall did allow David to defer military service until after he graduated, which Trinity College would not have done. This led into his later studies with [Hans Bethe](https://www.nobelprize.org/nobel_prizes/physics/laureates/1967/bethe-facts.html) (Nobel Laureate, 1967). In June 1955, the Cavendish Professor [Mott](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/mott-facts.html) (later Sir Nevill) called David into his office and asked him what he was doing next. David said that he was going to do his military service, as he did not wish to defer until after graduate school because he did not want to do military research, which would have been the likely outcome once he had a graduate degree. Mott told him that he could continue to get a deferral as long as he continued his scientific work and that the requirement for compulsory military service was likely to be discontinued. This led to an interesting situation in which no one had time to take David on as a doctoral student but the Cavendish had money for a stipend for him. Professor Mott suggested that he work with Hans Bethe, who was on sabbatical at the Cavendish Laboratory, Cambridge University. After a year, Hans offered David the opportunity to go to Ithaca with him and study for a Cornell PhD, which David accepted.  **Cornell University**  David obtained a Fulbright Foundation scholarship, which paid for his Atlantic trip on the Queen Elizabeth ocean liner and a train trip on the Lehigh Valley railroad to Ithaca. He travelled with Ronnie Peierls, son of Professor Peierls (later Sir Rudolph) of Birmingham University, who was also going to Cornell to study with Hans Bethe. There were various students sitting at their dining table on the liner who were going to a variety of different universities. One of these kindly sent congratulations to David after the award of the Nobel Prize; even though they had not seen or contacted each other in the 60 years since the journey.  In David’s first week at Cornell he passed his modern language exams in French and German. He also passed the departmental qualifying examination with “flying colours.” He was particularly pleased as there were no required courses for physics graduate students in Cambridge and “Hans Bethe had been complaining about the poor knowledge of general physics shown by PhD students he had met in England.”  Cornell was unusual in having no graduate school course requirements, but the Physics Department required all its students to do two semesters of an experimental physics course. “I do not think any of my experiments came out right, but apparently the explanations I gave of what had gone wrong and what I needed to do about it were sufficiently convincing that I got the highest grade in the course, and was excused taking a second semester of experimental physics.”  While at Cornell, David met Margaret Scrase, a biology student in the College of Agriculture. They married and have now been together for 60 years.  Mathematician Mark Kac was on David’s doctoral committee and David said that “Getting to know Kac and to learn from him was one of the unexpected benefits of going to Cornell. I treasured his explanation of the difference between a physicist and a mathematician: that a physicist was interested in the simple properties of complicated systems, but a mathematician was interested in the complicated properties of simple systems.”  In the 1960s (according to John Rehr) a story went around the Cornell physics department that David asked Hans Bethe for a topic for his PhD and then showed up two years later with a completed thesis. The fact is that Bethe was a scientific advisor to President Eisenhower. He travelled back and forth by train from Ithaca to Washington D.C. every week, so it was hard for them to have regular meetings. David’s remark about this topic was, “if I had a good talk with him once a month, he left me with enough to think about for the next three months.” However there was some truth to the rumour. David did produce a finished thesis and ask for a year’s postdoctoral fellowship so that Margaret could finish her degree at Cornell. If David had shown Hans the thesis earlier the answer would have been yes, but as it was all of Hans’ money was committed so David had to look elsewhere. Instead he obtained a postdoctoral fellowship for a year at the Lawrence Radiation Laboratory in Berkeley, which allowed Margaret to complete her undergraduate degree.  **Postdoctoral Fellowships**  Cornell University had been such a marvellous experience for David and Margaret that whatever came next was bound to be a disappointment. David did not appear to have a preceptor in the Radiation Lab, but he did publish two papers and taught a course on atomic physics on the Berkeley campus which went quite well. Living in Berkeley was a pleasure and David and Margaret’s explorations of San Francisco, the surrounding hills and beaches and the Sierra Nevada would not have happened if either of them had been taking work more seriously.  David moved to the Department of Mathematical Physics at Birmingham University for two more years of postdoctoral research. He worked under Rudolf Peierls from 1959 to 1961. David was working very hard because there were a great many interesting physicists in Professor Peierls’ department in Birmingham University. David recollects, “I was probably more interactive with my colleagues during these two postdoctoral years in Birmingham than I was at any other period in my professional career.” David and Margaret’s two sons, Michael and Christopher, were born during this time.  David spent the summers of 1960 and 1961 at the Niels Bohr Institute and Nordita in Copenhagen. These were a pleasure for all concerned and helped with the parlous financial state that resulted from trying to support a family on a British postdoctoral salary.  **Cambridge: Churchill College fellow, university lecturer**  David went to Churchill College as a Director of Studies and a Fellow in 1961, the first year the college took undergraduates. He also became a lecturer in the Department of Mathematics and Theoretical Physics. He learned a lot, particularly about teaching undergraduates, but he said there was less to show researchwise for the four years in Cambridge than in his previous positions. This had something to do with the intensive Cambridge 8 week term. He would get exhausted and spend much of the vacation recovering from respiratory diseases rather than doing research. His health only improved after the family moved into a centrally heated house in Birmingham in 1966.  In March 1965, David went to an interesting conference in Novosibirsk. Russia had temporarily opened up and there were no Intourist guides in Novosibirsk. This allowed the Russian physicists to talk freely with the Western  physicists.  In David’s own words:  In the early spring of 1965 the most memorable scientific meeting I have ever attended took place. This was a conference on many-body problems, which was held in Akademgorodok, about 20 km south of Novosibirsk. The town was the centre of work on nuclear physics, and had been closed to outsiders until that year. Teachers at the local English language school had been invited to translate for us, but Bogliubov told them they were not wanted, because it was better for the Russians to practice their bad English rather than to rely on teachers with good English and no understanding of physics. The teachers sat in on the sessions and in the intervals tried to talk to the few of us who spoke English from the right side of the Atlantic.  We were able to go for walks in small groups, unobserved by security people. We met with people like [Abrikosov](https://www.nobelprize.org/nobel_prizes/physics/laureates/2003/abrikosov-facts.html), Gorkov and Dzyaloshinsky, whose book was making my own book out of date. This meeting was the first occasion on which I met [Vitaly Ginzburg](https://www.nobelprize.org/nobel_prizes/physics/laureates/2003/ginzburg-facts.html), who later spent time in Cambridge, and came, with his wife, to visit us in Birmingham. I also got to know A. B. Migdal and V. M. Galitskii, who were the authors of the Paper on Green’s function that had been so in influential on my work at Cornell in 1958. An outing led, I think, by Migdal, was my first experience of cross-country skiing, in bright sunshine, but with crisp spring snow. The only unfortunate thing about this trip was that I had a bad cough, perhaps the remains of a pneumonia attack I had during the winter. I flew back as far as Sverdlovsk with a couple of young Russian physicists, but when we came back to the plane after a short walk I was accosted by a furious Intourist official, who was supposed to have been escorting me back to Moscow.  I spent two days in Moscow, visiting the Landau Institute and Moscow State University, hosted by Pitaevskii and by Abrikosov. One morning I wandered round the Kremlin by myself, and I was stopped by a guard, probably offended by my scruffy duffel coat. When I said I was “angliskii” he smiled broadly, waved his arms, and told me to look around. Unfortunately my first visit to Russia was probably also my last.  A couple of months later I happened to see Ginzburg and, if my memory is correct, Khalatnikov wandering around the Cambridge market place, during a break from a relativity conference. We invited them both home for dinner and got my colleague Roger Tayler to meet them. The third guest was a lucky choice, as he had translated one of Ginzburg’s books.  **University of Birmingham Professor**  In 1965 David was appointed professor of physics at the University of Birmingham.  He has left six pages of detailed notes about the development of his research during the first three years at the University of Birmingham and one year of sabbatical leave when he visited Chalk River, Cornell, Stony Brook and several places in Australia.  Before leaving Birmingham on sabbatical in 1968 Margaret loaned their only car, a Bedford camper van, to neighbours. When Margaret and David got back a year later the friends had moved to Bristol, taking the van with them. The husband was in South America and his wife had a new baby just when Margaret and David needed their car back, so David had to go and fetch it. He stopped and had lunch with John Ziman, then a professor at the University of Bristol, which changed the future of his physics research. Two of Ziman’s students said they had disproved [Philip Warren Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/anderson-facts.html) and Nevill Francis Mott’s 1958 theory of electron localisation disorder, so David said he would look at their papers. In the end he convinced himself that Anderson and Mott were right; the Nobel Committee for Physics came to the same conclusion in 1977 when they awarded them the prize. The exercise of reading, analysing and rewriting Anderson and Mott’s work gave David opportunities to think about a topic that he had not thought about before and opened up connections within the physics world. David later thanked Margaret for changing the direction of his research life by lending their car.  Around 1970 [Michael Kosterlitz](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/kosterlitz-facts.html), a research fellow whose funding was not tied to any particular project, began to work with David on the interaction energy of a pair of vortices in a two-dimensional neutral superfluid. David commented on their relationship, “We worked well together, since I had the broad ideas and tried to understand the big picture, whereas Mike would find the holes in my arguments and ways to solve the problems I had ignored.” This collaboration resulted in Kosterlitz-Thouless transition theory, described in their 1972 paper, which is one of two cited for the [2016 Nobel Prize in Physics](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/).  The other events of significance in 1972 for Michael and David were the births of their daughters Karin Kosterlitz and Helen Thouless.  **Reasons for Leaving Birmingham University**  There has been a lot of discussion of the “brain drain” of the 1970s, which is often attributed to a lack of money for academics. However, David did not leave the UK for money, but because of difficulties with the university administration. When David arrived back in Birmingham from sabbatical leave in 1969 he had a meeting with the new Vice Chancellor, who asked David what he would be doing next. David gave the true but impolitic answer that he did not have any definite plans. This led to an ongoing saga which resulted in the Vice Chancellor eventually telling David that if he had a chance to leave the university he should do it.  Although David did not have any definite plans on returning from sabbatical, his curiosity and openness to new topics led to an extraordinarily fruitful period from 1970 through 1978. He published 16 of his most important papers in five distinct topics, including the work for which he was eventually awarded the Nobel Prize. As noted by Ana Mari Cauce, President of the University of Washington, David was known for his curiosity-driven research which, decades after the initial research, has led to many practical uses.  There were no theoretical physics chairs vacant in the UK at that time so David left the UK, much as he did not want to. David went briefly to Yale but clearly he did not talk adequately to whoever was in charge of making the appointment because Yale wanted David to build a research group, whereas David had always preferred to work with colleagues rather than being a group leader.  **University of Washington Seattle USA**  David’s life and work up to the year 1972 is known from his own detailed autobiographical notes. His story from that year forward is told without the benefit of such a first-hand account.  The University of Washington did not need David to build a big research group. There were enough other independent theoretical physics professors there to whom David’s students and postdoctoral fellows could talk if he were away. He mostly taught graduate students and upper class undergraduate courses. He had many graduate students from around the world, but never an American-born student.  Shortly after arriving in Washington in 1980, David wrote a grant proposal in which he described the work he intended to do, but also suggested he might investigate some entirely different topic if a more interesting one came along. David’s reputation for producing interesting work meant that he was awarded this grant despite the vagueness of the grant proposal; President Ana Mari Cauce has observed that this would be unlikely to be funded today, when grants driven mainly by curiosity do not get much support.  In 1982, David published a paper called *Quantized Hall conductance in a two-dimensional periodic potential*with research fellows Kohmoto, Nightingale and den Nijs (*TKN*2), which is the second paper cited for the Nobel Prize. The word topology is not mentioned in the title of the 1982 paper and does not appear in his titles until 1985. However, when David Thouless wrote the book *Topological numbers in nonrealativistic physics*in 1998 he said “Topological numbers crept up on the physics community before the community was aware of them. I did not think in these terms until I started working on the topological aspects on long range order in the 1970s, although I had been working on aspects of superfluidity that are not topological for several years before that.”  .  Marcel den Nijs has remained in Seattle and has been a great supporter of David but they have not published any more papers together.  In 1990 David was awarded the Wolf Prize in physics with [Pierre-Gilles de Gennes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1991/gennes-facts.html) (Nobel Laureate, 1991). Over the years he has received a number of other awards and honours. For example, he was elected a Fellow of the Royal Society (FRS) in 1979, a Fellow of the American Academy of Arts and Sciences (1981), a Fellow of the American Physical Society (1987) and a member of the US National Academy of Sciences (1995).  David enjoyed working and living in Seattle. He has never had many hobbies but he loved to hike in the mountains, camp, cross country ski and occasionally sail. His house had a 180-degree view of Lake Washington and mountains, including Mt. Rainier. Even though some of the surrounding trees have grown, a marvellous view remains. The garden faces southeast and has excellent soil for gardening. David’s biggest hobby over the years was reading. He read widely, but history interested him most. He was very happy in retirement reading in his chair and then resting his eyes on the view. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0025** |
| **Bioographical** | I was born in London in 1951, in a medical family who greatly valued science and education in general, but never tried to push their children to go into medicine, although my younger brother did choose that path. My father was a psychiatrist working in the newly-created National Health Service, and came from Scotland. He had wanted to become a psychoanalyst, but the war had prevented his planned training under Freud’s pupil Melanie Klein, and he was trying to find some way to apply techniques or insights inspired by psychoanalytic theory to the much more limited possibilities for psychotherapy in an NHS practice. My mother was a Carinthian Slovene from a bilingual region in southern Austria, who had met my father when he was an army doctor in the British Occupation Forces there. She was a medical student working in a hospital when she met him, but never managed to complete her studies after coming to Britain, because all the exams she had passed in wartime Vienna would not have been recognized, and she would have had to restart all the medical training from scratch, in what was, to her, a foreign language. Instead, she had a family. My parents’ backgrounds gave me a multicultural heritage, with relatives in both Scotland and in Austria, where we often visited for summer holidays, so I became reasonably fluent in German, but sadly my command of Slovenian remained very basic indeed. My mother was proud of her heritage, as was my father of his, and he would wear a kilt on formal occasions, so although I grew up in London, without a trace of a Scottish accent, I self-identified as half-Scot, half-Slovenian.  I was sent to private schools, first a mixed elementary school a short walk from our house in Bedford Park, in west London, where I appear to have excelled in subjects like arithmetic and spelling, but always lost out on my handwriting skills, which remained messy and irregular, despite my being made to copy out pages of text again and again (or so it seems in my memory). When I was ten, I was sent to the “preparatory school” (Colet Court) for St. Pauls School, and then to St. Pauls itself, which is a well-known “public” (i.e., private) boy’s school noted for a rigorous educational curriculum. The school was very cosmopolitan, and was mainly a day-school with pupils coming from many parts of London, with a small boarding component. I was one of the one hundred and fifty-three scholars at the school (the number has biblical significance as the number of fish miraculously caught by the apostles), and because of this, I wore a little silver badge in the shape of a fish.  I always remember being interested in mathematics and science. In English schools, at least at that time, one had to specialize early. Looking at the list of General Certificate of Education “O levels” that I took, they were English, Latin, French, mathematics, “physics-with-chemistry,” with the only unusual one being “physical geography and elementary geology.” At some point I had to choose between continuing with history or geography, and the rocks and minerals seemed interesting and I was fascinated by the crystal collection the school had (perhaps an early attraction to “condensed matter”?).  For “A” levels, I just have mathematics, physics, chemistry, so somehow, I never studied biology (I think one only studied it if one was planning to go to medical school?). Of course, as these last years of school were during the late sixties, there were lots of distractions for teenagers in London during that period, but I managed to keep my academics on track. In my final year at school, I had a very enthusiastic and inspiring physics teacher who got me interested in the subject, while previously I had found chemistry definitely more interesting.  Somehow, I managed to combine interest in science with interests in rock music and sixties counterculture. I had a gap of nine months after leaving school, and before starting University, and decided to travel. I worked for a while at a book publishers’ organization extracting data on names and fields of study of faculty members from German university catalogs, and with my savings, and  a large backpack, then set off on the then-well-traveled overland trail to India and Nepal via Iran and Afghanistan (and back!) – a journey impossible today! (I would later get to see India (and Nepal) from a rather different perspective during visits as a professional Physicist.)  I was admitted to “read” Natural Sciences at Christ’s College, Cambridge where I “matriculated” in October 1970. Three subjects plus mathematics were required, so I finally had the chance to learn some cell biology as well as physics and chemistry, but I found I was not so gifted in the laboratory, and after an experience when I accidentally swallowed some nasty chemical I was supposed to measure out a small dose of using a “mouth pipette” (I do not believe such things still exist with today’s work-safety rules) I decided I should opt for prudence and focus on theory!  In my third and last Cambridge undergraduate year, 1973, I took a class called something like “advanced quantum mechanics” taught by [Phil Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/anderson-facts.html), where, if I remember correctly, he talked about the problem of localization by disorder, the Kondo effect, and other inspiring things. These were deeply conceptual quantum problems different from the diet of scattering problems which seemed like mathematical exercises in partial wave expansions and spherical harmonics that the more conventional classes had been feeding us. I was hooked and decided that if I was accepted to stay on at Cambridge as a graduate student in the Cavendish Laboratory (which happened), I would like to work with Anderson. I also considered working with Michael Green on an intriguing problem of “massless spinning relativistic strings”: since string theory as a model for the hadrons was abandoned shortly thereafter, and took ten years “in the wilderness” till it was repopularized as a possible theory of quantum gravity, my choice to work with Phil seems a fortunate one, at least for one made in 1973! It is probably the case that any successful research career can be traced to “accidentally” making a series of non-obvious choices at the right time, and various chance events. I think it was the concreteness of condensed matter, in that it was much easier to experimentally realize systems that exhibit all sorts of remarkable effects, that kept me on the condensed matter theory trail. In some sense, particle theorists have only one physical vacuum, with its beautiful but highly constrained Lorentz point-symmetry group, to play with, while condensed matter physics can “build” a huge variety of model vacua with different symmetry groups and “elementary particles” (elementary excitations), and play with them experimentally.  In the TCM (Theory of Condensed Matter) group at the Cavendish, Phil gave me the problem of “valence fluctuations” in the Anderson model of a magnetic impurities to look at and a reprint of his Les Houches lectures about the Kondo impurity spin model, including the “Anderson-Yuval-Hamann” renormalization group treatment of the mapping that turns the path-integral of spin-flips of the impurity into a coulomb gas of charges of alternating sign that interact with a logarithmic potential. This had a “renormalization group” (RG) treatment that provided the precursor for the method developed by Kosterlitz and Thouless for the Nobel Prize-winning solution of their famous problem. I also had to study Phil’s less-complicated “poor-mans method” that rederived the same RG scaling equations for the Kondo model. Phil spent part of the year in Cambridge and the rest at Bell Laboratories, so I had to work through these mysterious texts by myself. The majority of the TCM group were interested in accurate computation of material properties, especially surface properties of metals with different kind of atoms or molecules absorbed on them for catalysis, so in my advisor’s absence, I tried to learn from them and did not understand his “toy model” approach, which was that accurate details really do not matter if one is trying to understand the essence of some phenomenon, provided that the ingredients retained in the model are indeed the ones that matter.  I remember puzzling over the Kondo, Anderson and Wolff models which were all representations of something like a transition-metal *d*-orbital deep inside the core region of a transition metal atom, in which there are strong electron-electron interactions, mixing with a weakly-interacting metallic conduction band derived from outer *s*-like orbitals. I even got hold of a self-consistent Hartree-Fock program written in FORTRAN-66 line by line on a huge stack of IBM punched cards that had to be fed into a card-reader hopper to submit the job to a mainframe computer, and tried to puzzle out how the real orbitals of the notional metal atoms would behave as charge leaked off or onto the impurity atom from the metal background. Needless to say, all this was quite pointless, even though it was some kind of learning experience. When Phil returned again, I still had not figured out what the toy models really meant physically. For example the Wolf and Anderson models seemed to be mathematically equivalent, depending on whether the extra “*d*” orbital was interpreted as being part of the conduction band or orthogonal to it.  But instead of helping me struggle with these niggling details, when Phil returned, he gave a marvelous course of lectures that became his book “Basic Notions of Condensed Matter Physics” where he sketched his ideas of “adiabatic continuity” within phases until critical points were reached, and that all points within the same phase shared the same essential “fixed-point” independent of all the fine details. Through hearing him flesh out his ways of thinking, and going to see him about some details I was missing, and instead having him share with me his interesting thoughts about some apparently quite different but essentially related issue, I began to see his point of view that tries to identify what is needed to see the “big picture,” when trying to understand the physics of strongly-correlated systems. Somehow, that was what having a “mentor” was all about.  In the middle of my second year as a graduate student Phil announced that he would be exchanging his half-a-year at Cambridge, half-a-year at Bell Laboratories position for a similar one that replaced Cambridge University with Princeton University. I and Phil’s other student, Ali Alpar, who was working on pion superfluidity in neutron stars, never learned the reason for the move. This was in any case a very interesting change for us both, as Phil arranged to take us with him to the very different world of Princeton, New Jersey, starting with a few summer months at Murray Hill, New Jersey, the location of Bell Telephone Laboratories, then in its heyday. This was a tremendous privilege for a graduate student.  In September 1975, I moved down from Murray Hill to Princeton, and Ali Alpar and I shared an office on the fourth floor of Jadwin Hall, which was a larger office divided in two by partition, on the other side of which was Natan Andrei, working on particle theory with [David Gross](https://www.nobelprize.org/nobel_prizes/physics/laureates/2004/gross-facts.html), and not yet on the Bethe Ansatz that he would go on to use to unexpectedly find the exact solution to problems I worked on such as the Kondo model, which I would tell him about. Other contemporary students on our floor included Ed Witten and Steve Girvin, who was working with John Hopfield on the “X-ray edge singularity” problem (which like the Kondo problem involved singular behavior at the [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/fermi-facts.html) level, especially the “orthogonality catastrophe” discovered by Anderson that affects dynamical degrees of freedom that excite particle-hole pairs in a metal that they couple to).  There were many blackboards on the fourth floor. One slightly disconcerting feature of the environment was that [John Nash](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1994/nash-facts.html), the future Economics Nobel Laureate, who was in the middle of his illness, would gain access to the building at nights or weekends and systematically cover all the blackboards with mysterious equations connecting politics, pop culture, and numbers. [Frank Wilczek](https://www.nobelprize.org/nobel_prizes/physics/laureates/2004/wilczek-facts.html) had just become an Assistant Professor, and he gave a many-body class about the 3D interacting Bose fluid that I took. Barry Simon and Elliot Lieb were working on the stability of matter, which I also took a class on. Through Princeton and Phil Anderson, I was privileged to meet so many of the leading theorists who were at Princeton, or visiting; for example I was invited to dinner at the Andersons when Phil’s old friend David Thouless visited, meeting him for the first time.  It was an intellectually exciting time to be at Princeton, and in that atmosphere, I finally understood what I was trying to achieve with my extension   to Anderson’s treatment of the Kondo problem that allowed valence (charge) fluctuations as well as spin fluctuations of an Anderson model impurity. The renormalization-group treatment showed a novel effect that there was a logarithmic temperature dependence of the energy level of the impurity orbital as a consequence of the interaction.  The renormalization group can be viewed as a way to resum a divergent series derived by perturbation theory, in this case in the mixing between the impurity orbital and the metal in which it is embedded. This means that the results can be validated by a detailed examination of the structure of the perturbation series. The test required that the sum of pieces of each of about forty distinct fourth-order terms in the series should exactly cancel. With the aid of the huge table of integrals by Gradsteyn and Ryzhik, I set out to do the test, but it did not quite work, the cancellation was just not happening. I checked and rechecked each of the forty terms time and time again, to no avail. Finally, after about two months of intense struggle, and being convinced that my results were correct, I realized that one of the complicated integrals I was taking from G&R could not possibly be right, because a simple approximation produced a lower bound that the printed result violated. When I worked out the integral for myself, there was a missing factor of two in the formula given in the tables, and the correction finally made everything work as expected. (When the next edition of G&R was published, there was indeed an erratum that corrected the printed formula!) The experience gave me confidence in standing by results I believed to be true, as well as a lifelong antipathy to doing high-order perturbation theory!  During my last year of graduate studies, the French physicist Philippe Nozières came to give a seminar, and Phil introduced me to him. Later, just when I was wondering where to apply for a postdoctoral position, I got a letter offering me a five-year position in France, at the Institut Laue-Langevin in Grenoble, a city with a large number of research laboratories. The ILL is a neutron-scattering facility, a joint consortium between France, Germany, and Britain, but had a theory group as well as experimental groups who used the neutron source. The idea of experiencing a new country, France, was very appealing, and especially as the dollar was at a low point of the exchange rate, the job looked very attractive, so I accepted. I finished writing up my thesis, and before leaving for France, I had the great opportunity to attend, with Phil’s recommendation, a workshop on strongly-correlated electron systems at the Aspen Center for Physics, in Aspen, Colorado, and then spend a month with Sebastian Doniach at Stanford University.  My brother came to visit, and shared the driving in my old VW beetle from Princeton to Aspen, and it was quite amazing to experience transcontinental driving! That year was a special year at Aspen, as a high-powered delegation from the Landau Institute in the USSR, led by Lev Gorkov, was also attending the workshop. My future colleague Sasha Migdal was among the Soviet party, and it was very interesting to witness the internationalism of science. (There was also a lot of speculation about who was acting as the KGB minder who it was assumed had to be there to keep an eye on the rest of the delegation!) This was followed by a further drive through the spectacular western scenery to Stanford, where I met my future long-term collaborator Ed Rezayi, then a graduate student with Doniach, and then another transcontinental drive back across the US to New Jersey, from where I left for France.  I had perhaps foolishly shipped my American-model Volkswagen to France, but picked it up at the port of Le Havre, and was driving down the autoroute to Grenoble when I heard on the car radio that Phil Anderson was to share that year’s Nobel Prize for Physics with his advisor [John Van Vleck](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/vleck-facts.html) and [Nevil Mott](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/mott-facts.html), who had brought him to Cambridge in the sixties.  I soon found that while my years of French language studies at school had prepared me to decipher street signs and read menus, understanding what people were saying was another matter. On the other hand, the multinational work environment at the ILL was mainly English-speaking, which did not help to improve my French. This was remedied after I met Odile Belmont, a native of the Grenoble region who would later become my wife.  In learning about the Anderson-Yuval-Hamann treatment of the Kondo model I learned about the X-ray-edge singularity problem, which Nozières and de Domincis (ND) had solved in terms of singular integral equations, and the much simpler later variant treatment by Schotte and Schotte using “bosonization,” a remarkable and mysterious representation of electron creation operators apparently just using harmonic oscillator variables, related to those used by Tomonaga in his 1950 treatment of sound waves in a one-dimensional Fermi gas. The two treatments agreed at weak coupling, but differed at strong coupling, where the ND treatment seemed more complete, but in fact the model assumptions used in the two treatments were slightly different, so the models were different at strong couplings. I became aware that Daniel Mattis had claimed to solve the Wolff model exactly using bosonization techniques, but I knew that, at least formally, the Wolff and Anderson magnetic impurity models were equivalent, and from my thesis work, felt something was not right with the proposed bosonization solution. One of my new colleagues at ILL, Hans Fogedby, was also working on the Wolff model with Mattis’ technique, and I determined to try an understand the bosonization technique, and find out why it was giving results I disagreed with, including a phase transition to a magnetic state as the short-range (contact) interaction strength (usually denoted “*U*,” by analogy to the Hubbard model, a widely-used “toy model” for studying magnetism) was increased.  The Anderson and Wolff models feature a single “impurity orbital” in which there is a Hubbard “*U*” coupling. Because the [Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/pauli-facts.html) principle prevents two electrons with the same spin from being present in the single impurity orbital, there is no direct interaction between electrons with the same spin, and these were explicitly discarded in the bosonization treatment. However, while the ND treatment of the X-ray edge problem, preserved the contact-type nature of the interaction, the bosonization treatment was secretly treating a long-range interaction which could couple electrons of the same spin, so it was not valid to simply discard same-spin interactions. This subtlety was hidden in the now-explicitlyspecified “ultra-violet cutoff ” structure, invalidating the bosonization treatment of the Wolff model, but I wanted to “clean up” aspects of the bosonization technique, which had been recently also been used to great effect by Luther and Emery for one-dimensional metals, and by Luther and Peschel for the spin-1/2 easy-plane spin chain.  At this time, the correctness of the Kosterlitz-Thouless treatment of the topological phase transition had not yet been universally acknowledged, and there was a counterproposal by Luther and Scalapino based on bosonization of a 1D quantum spin chain. I attended a workshop at NORDITA in Copenhagen, where Luther had moved to, where this was a heated subject of discussion. At that meeting I also first met my future colleague Kazumi Maki, and there was also a Soviet contingent, including Igor Dzyaloshinksky of the famous Landau-Institute AGD ([Abrikosov](https://www.nobelprize.org/nobel_prizes/physics/laureates/2003/abrikosov-facts.html)-Gorkov-Dzyaloshinsky) triumvirate, who had produced the foremost text on diagrammatic perturbation methods in condensed matter theory. Igor was an old friend of Philippe Nozières, and I got to know him well when he subsequently came for an extended visit to Grenoble. He had produced an interpretation (with Anatoly Larkin) of bosonization in terms of standard diagrammatic perturbation theory, which was a useful alternative viewpoint.  In my investigation of bosonization, I found that its exact formulation needed two action-angle variables to replace the absent zero-wavelength sound-wave mode, and the lack of this in the earlier formulations such as Luther’s had been “patched up” with a cutoff that was not really consistent. The new variables added topological winding-number excitations with their own distinctive energies to the well-known Tomonaga sound waves, and allowed me to formulate what I called “Luttinger liquid theory,” first as a replacement for [Landau](https://www.nobelprize.org/nobel_prizes/physics/laureates/1962/landau-facts.html) Fermi-liquid theory in one dimensional electron systems. However, because “2*kF*” for a spinless Fermi fluid would also be the Bragg vector if the fluid crystallized, it also applies to 1D Bose fluids and gapless uniaxially-anisotropic spin chains. As I described in my [Nobel lecture](https://www.nobelprize.org/nobel_prizes/physics/laureates/2016/haldane-lecture.html), this led to a a deeper understand of spin chains, including my very expected discovery in early 1981 that the spin-1 isotropic antiferromagnetic chain had a gapped spin-liquid state that is now recognized as an early example of topological quantum matter.  I was quite surprised when analysis starting with the Luttinger-liquid approach, supplemented with the mapping of the Kosterlitz-Thouless transition to (1+1) dimensional quantum mechanics, led inescapably to my surprising conclusion. I was even more surprised at the resistance this received from the quantum magnetism community when I submitted the paper for publication: it was rejected by multiple journals, and was labeled a “conjecture” even though it was, in my mind, a clear prediction. I recall that one referee pontificated that my claims “were in manifest contradiction to fundamental principles such as renormalization and continuity”! Of course, my predictions were later vindicated both by numerical studies and experiments.  While in France, I received an unexpected invitation to visit the University of Southern California in Los Angeles for a job interview. It turned out that Kazumi Maki had written to Philippe Nozières asking for suggestions for candidates. I visited, and was seduced by the beach and palm trees. I had not yet actively started to look for a faculty position, but at that time, the news I was hearing from British friends was anecdotally rather pessimistic about the UK physics job market, and government research funding. So by default, I inadvertently joined the “brain drain” to the US. A lasting legacy from my time in France was my French life-partner Odile, who agreed to try out the California lifestyle with me.  In my last year at the ILL, I was fortunate, perhaps as a result of my “Luttinger liquid” work, to be invited to one of a small group of “promising young scholars” invited to a Taniguchi Symposium in Japan where the Japanese philanthropist Toyosaburo Taniguchi envisaged they would come together to interact in ideal and luxurious surroundings, in this case a lodge next to Mt. Fuji. Not all the “scholars” were that young, and I had the chance to meet and discuss with John Hubbard, who had introduced a key “toy model,” the Hubbard model for strongly-interacting electrons, who was also there, and seemed to be enjoying the meeting. (Tragically, it was his last meeting, as he died just after returning home.)  By the time I got an extensively rewritten paper on the spin-chain finally published (in Physics Letters A) I had been in California for over a year. During that time I received two papers (from the same journal) to referee (first by Takhtajan, then by Babujian) both describing an exactly solvable gapless *S*= 1 spin chain with a Bethe Ansatz solution very similar to the gapless *S*= 1/2 chain. The exact solutions were claimed to represent the generic behavior of arbitrary-spin [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/heisenberg-facts.html) antiferromagnets, and they apparently completely contradicted my theory! I must admit I had about ten minutes of self-doubt when I received the first of these papers, but soon saw that the solvable model was a modified model with a large non-Heisenberg unphysical “biquadratic exchange” term, and did not represent the standard Heisenberg model I had treated. Furthermore, though they were gapless, I could not fit them into my “Luttinger-liquid” picture. Around this time, (1+1)-d conformal field theory was starting to be developed. Eventually it emerged that “Luttinger liquids” were related to Abelian conformal field theories, that can have continuously tunable critical exponents. The new *S*> 1/2 Takhtajan-Babujian solvable models were critical, but correspond to nonAbelian conformal theories that require fine-tuning the couplings to exactly cancel “relevant” perturbations, so do not represent generic spin chains. The  *S*= 1 case represents a critical point between the generic “Haldane-gap” nondegenerate symmetry-protected topological (SPT) state, and a non-topological gapped broken-symmetry two-fold-degenerate dimerized state.  An apparently unconnected series of surprises were independently discovered in those years. First, [Klaus von Klitzing](https://www.nobelprize.org/nobel_prizes/physics/laureates/1985/klitzing-facts.html) discovered the integer quantum Hall effect (QHE). As soon as it had been concluded that, in two dimensions, localization by a disordered potential would always lead to integer quantization of the Hall conductivity, [Dan Tsui](https://www.nobelprize.org/nobel_prizes/physics/laureates/1998/tsui-facts.html), [Horst Störmer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1998/stormer-facts.html) and Art Gossard discovered the *fractional*quantum Hall effect. This was far more of a shock for theorists, as the understanding of the integer QHE showed a fractional effect could only occur as a consequence of interactions. Furthermore, at the time it was generally believed that second quantization and diagrammatic perturbation theory was the principal tool for understanding interaction effects. In fact these techniques are only useful if some adiabatic connection can be found between a non-interacting system and the interacting one, which was not the case for this problem. The Soviet physicists at the Landau Institute outside Moscow were the world’s leading practitioners of diagrammatic techniques in condensed-matter physics, and interestingly, the fractional QHE was the first problem to which they were unable to make many contributions.  Of course, the key breakthrough was [Laughlin](https://www.nobelprize.org/nobel_prizes/physics/laureates/1998/laughlin-facts.html)‘s discovery of his eponymous state, apparently through carrying out a numerical diagonalization of a threeparticle system projected into the lowest Landau level. Perhaps his training in band-structure calculation allowed him to take this direct route to investigate the problem. The key experimental clue was that the QHE states occurred at Landau-level filling *v*= 1/3 but not at *v*= 1/2. I had been thinking about some kind of “supersolid” picture, when in early 1983 I received Laughlin’s paper to referee. Within ten minutes I knew he had found the right (Nobel prize-winning) explanation, an incompressible quantum fluid with fractionally-charged excitations, that was later realized to be topologically ordered. In addition, it was fundamentally disconnected from free-particle Slater-determinant states, so there seemed to be no hope of understanding it based on diagrammatic perturbation theory. The most convincing detail was that it provided a natural explanation based on Fermi statistics for why it occurred at *v*= 1/3 but not *v*= 1/2. The wavefunction also provides a clear picture of what was later called “flux attachment.” The Laughlin state had a huge effect on the way I thought about condensed matter physics.  Later that year, there was a meeting at Bell Laboratories to celebrate Phil Anderson’s sixtieth birthday, and I stopped over in New Jersey on my way to France, for a summer collaboration with Rémi Jullien, Robert Botet, and Max Kolb, who done the first numerical studies to test my claims about the integer*S*antiferromagnetic spin chains, and had also attracted skepticism when they reported results supporting my predictions. I had a very interesting discussion about the Laughlin state with Phil, who noted that if three units of flux were injected at a point to create three concentric quasiholes, the resulting state was the same as that resulting from locally removing an electron from the Laughlin state. Thus adding three units of flux plus one electron would just change the *N*-particle state to the (*N*+ 1)-particle state, in analogy to a Bose condensate where particles were composite objects. Independent of Laughlin’s work, a numerical exact-diagonalization study had also independently been carried out with (quasi)periodic boundary conditions by Yoshioka, Halperin and Lee (YHL) in an anisotropic basis which seemed to suggest a liquid state with a three-fold degenerate ground state, but was not as revealing as Laughlin’s picture.  I had been wondering how to do a numerical calculation that incorporated isotropy, without the problem of boundaries, which YHL avoids. That night, I was staying as a guest in Chandra Varma’s house, and woke from a dream in the middle of the night with the image of a spherical surface around a magnetic monopole, which solved the problem, and turned up to be an incredibly powerful tool for numerical investigation of the fractional QHE. I suppose my brain had been churning over my discussion with Phil Anderson earlier that day, to produce this Kekulé-like experience. Having woken up, I worked out all the details there and then.  I flew on to France, but instead of working on spin-chains with Rémi Jullien and his group, I found that their spin-chain programs were built with arbitraryrange exchange, which allowed me to use them “as-is” (for bosonic Laughlin states) to test ideas suggested by the spherical geometry, such as the powerful pseudo-potential idea, and the idea that the Laughlin state was an exact eigenstate of a “toy model” that retained only short-range components of the interaction potential, analogous to the Hubbard model, except without a background lattice.  In France, I learned the basic techniques of the Lanczos sparse-matrix diagonalization method. When I got back to Los Angeles, I was coincidentally contacted by Ed Rezayi who had just moved there, and we began a fruitful collaboration on numerical studies of the fractional QHE. Because of the inapplicability of diagrammatic methods for this problem, these have been the only quantitative source of information about energies and stability in the problem, to date.  The next year I received an interesting job offer to join Bell Laboratories as a member of technical staff. I took a leave of absence from USC, and we moved to New Jersey, one month after our son was born. It was just the time of telephone deregulation, and of the split between AT&T Bell Laboratories, and BellCore, the part of the research division going to the new local telephone companies, and who were still in the same building as us for several months more. There was fantastic research going on at the Bell Labs, but in the end I decided that I missed the academic environment of a university and accepted a position at the University of California, San Diego, starting at the beginning of 1987, where I stayed until mid-1990. The effect of breaking up the Bell telephone monopoly inevitably led Bell Labs to decline to a shadow of its former self over the succeeding thirty years.  In January 1987, we moved to La Jolla, California, with its beautiful weather and beaches. At that time I was working on both quantum magnetism and the fractional quantum Hall effect. While at Bell Labs, as soon as I heard a rumor that Ian Affleck, with Tom Kennedy, Eliot Lieb and Hal Tasaki (AKLT) had come up with a variant spin-1 magnetic chain model for which the ground state could be exactly found (the AKLT state), I correctly immediately knew, with no further details, that it had to work by the same “pseudopotential” idea that made the Laughlin state an exact eigenstate of a truncated short range interaction. At UCSD, Assa Auerbach and Daniel Arovas, who were postdocs at the University of Chicago had asked to come to visit La Jolla during the Chicago winter, and do some work on quantum magnetism. They found a beach motel to stay for a month, and we were able to get very nice insights into the excitation spectrum of the AKLT model using methods borrowed from the fractional quantum Hall effect, in the process starting a lifelong friendship. Interestingly, this work provided the first clue that there could be some relation between the spin-chain and the quantum Hall effect: this is now clearer, as both are now recognized as forms of topological quantum matter.  In this period at UCSD, I came across various interesting results, such as an exactly-solvable spin chain model with long-range exchange (independently discovered simultaneously by Sriram Shastry, and now called the “Haldane-Shastry” model), in which the “spinons” of the spin-1/2 chain are especially simple.  I also came up with the second discovery that the Nobel committee mentioned: I called it the “zero-field quantum Hall effect,” but it is now usually called the “quantum anomalous Hall effect” or the “Chern Insulator,” and is the first member of the topological insulator family, but one with broken time-reversal symmetry, unlike the later time-reversal-invariant topological insulators. The idea was started when I read a 1986 paper in *Physical Review Letters*(PRL) by Eduardo Fradkin, Elbio Dagotto, and Daniel Boyanovsky (FDB), called “Physical Realization of the Parity Anomaly in Condensed Matter Physics.” I am not sure if I understood it properly, but it seemed to propose a quantum Hall effect in the absence of a magnetic field *and*with unbroken time-reversal symmetry, on a domain wall in a semiconductor with strong spin-orbit coupling. This interesting paper also stimulated Frank Wilczek to think about axion electrodynamics in a condensed-matter context. But thinking about it, I realized that there was no problem with a QHE in the absence of magnetic field, *provided time-reversal symmetry was broken*, which explicitly was not the case in the FDB paper. I tried to make this point by submitting a “comment” to PRL on the FDB paper, but as is often typical in this kind of “Comment/Response” dialog, it really became two monologs, where neither side understands what the other is saying.  In the course of sharpening my arguments, I looked for as simple and transparent a model as possible with which to make my point, and since Gordon Semenoff has used a “graphite monolayer” (*i.e.*, graphene) as the condensedmatter backdrop for [Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/dirac-facts.html) points, I used that too. The 2D Dirac points are stable if both spatial inversion and time-reversal symmetries are unbroken: Semenoff broke inversion symmetry to get an entirely-unremarkable insulator that had a field-theoretic description as two copies of a massive Dirac equation. With a bit of magic involving complex second-neighbor bonds, I broke time-reversal symmetry and ended up with a topologically-non-trivial state exhibiting a “quantum anomalous Hall effect” where “anomalous” in this context means that the Hall effect is not driven by a uniform magnetic flux density, but arises from magnetization. At this point I realized that this effect was extremely interesting in its own right, especially if could be realized experimentally in a real material. I dropped out of the Comment/Response cycle, which in any case was getting nowhere, and published the result in its own right.  The model of graphene with a “mass gap” due to breaking of time-reversal symmetry, conceptually provided by an additional ferromagnetic degree of freedom with a magnetic moment normal to the graphene sheet, was a simple and transparent enough “toy model” to be used for a number of model calculations. As well as the gapped quantum anomalous Hall regime, it had a metallic regime, with the Fermi level inside a band, which could model a 2D version  of a metallic (unquantized) anomalous Hall effect, and David Vanderbilt and coworkers later put it to good use to find and test a general Berry-like formula for the magnetization of a material in terms of its bulk bandstructure. I later also used it to guide me to new expressions for the anomalous Hall effect in 2D and 3D metals as a pure Fermi-surface formula, which is relevant to the currentlyhighly-studied “Weyl semi-metals.”  Later in 1988, I had two very interesting foreign trips, one to the People’s Republic of China, where T. D. Lee organized a meeting at Beijing University with a cast of colleagues such as Bob Laughlin, Steve Kivelson, Ganapathy Baskaran, Dung-Hai Lee, and others. This was when it was just becoming possible to travel to the PRC, and the Beijing streets were still rivers of bicycles, unlike today. In the second trip, I was invited by David Pines, to join a party sponsored by the National Academy of Sciences to visit the USSR, in particular the Landau Institute at Chernogolovka, which had long been off-limits to westerners (we were in fact the second group of western visitors to visit). There I met such future condensed colleagues such as Paul Wiegmann, who also independently solved the Kondo problem (and who a year later was able under Perestroika to get a passport to come with his family to a visiting position at UCSD), and my future Princeton colleague Sasha Migdal, as well as meeting senior Soviet physicists such as Gorkov, Abrikosov, and Khalatnikov. Everyone in the visiting party at the Landau Institute also wanted to meet my renowned future Princeton colleague Sasha Polyakov, but then, as now, he was a fanatical jogger, and was out running somewhere in the woods and could not be found! After a day at the Landau Institute, we went on to a meeting in Tbilisi, which was greatly enjoyed by our hosts, as the alcohol ban that Gorbachev had decreed in Moscow did not extend to Georgia. While meetings between physicists from the US with those from Russia and China are commonplace today, at the time these were quite exceptional experiences.  In 1990, Princeton University successfully enticed me away from California, and with a new baby daughter, we moved back to the East Coast. Princeton, long known for elementary particle physics, was building up its condensed matter group. In 1992, I spent a half-year sabbatical at the École Normale in Paris, and after giving a seminar on the mysterious symmetries of the Haldane-Shastry model, which a year earlier had led me to formulate a novel “fractional exclusion statistics” a suggestion from Vincent Pasquier and Denis Bernard led the identification of an unusual form of the “Yangian quantum group.” In 1993, while attending a workshop, I unexpectedly learned from Steve Kivelson that I was that year’s Oliver Buckley Prize winner for the old quantum spin chain work: it turned out that that had been announced a few weeks earlier, but I had mistaken the large white envelope with the APS letter for some routine circular, and left it unopened, and no-one else had told me. This must have been the days before email was widespread! A few years earlier, David Thouless had told me he was nominating me for Fellowship of the Royal Society of London, and (perhaps because of the Buckley Prize) I was finally elected in 1996 and had the honor of signing the parchment Charter Book, with entries going back to Newton.  For a long time, nothing had happened with my 1988 graphene-like toymodel for the zero-field quantum Hall effect. In 1999, work by Ganesh Sundaram and Qian Niu (a former graduate student of David Thouless) revived the longignored work of Karplus and Luttinger on the anomalous Hall effect in ferromagnetic metals, showing that it had a modern interpretation in terms of Berry curvature. This re-energized the study of Berry curvature effects in band structures. My 1988 model satisfied the “TKNN” topological result of David Thouless, with coworkers Mahito Kohmoto, Marcel den Nijs, and Peter Nightingale, that was cited by the Nobel committee as David Thouless’s seminal contribution to topological matter. When the gap was opened by breaking time-reversal invariance the conduction and valence bands had Chern invariants ±1 respectively.  In the early 2000s after attending a seminar by John Joanopoulos on the new subject of “photonic crystals” where the flow of light is modified by passing it through engineered spatially-periodic “metamaterials,” I realized that, at least as far as “one-way” edge states were concerned, some of the physics of the quantum anomalous Hall effect could be transplanted into the field of photonic crystals, which could also have Chern invariants. Still it took some time to come up with an explicit photonic bandstructure that would this. Eventually, in early 2004, while I was on sabbatical at UC Santa Barbara, my student Srinivas Raghu, who came with me, found a candidate structure inspired by the same hexagonal graphene structure that exhibited the electronic effect in my 1988 model. A calculation confirmed that it indeed would show the effect, and the new field of “topological photonics” was born.  At that time, there was also a lot of discussion about a “spin Hall effect” in systems with spin-orbit coupling and unbroken time-reversal symmetry. As a toy model, it was natural to combine conjugate copies of the 1988 model for what could now be called the “quantum anomalous Hall effect” to form a time-reversal invariant structure that would exhibit a “quantum spin-Hall effect.” While at UCSB, I played with this model, but because it had edge modes that traveled in opposite directions, I assumed that it could not represent a true stable topological phase because spin-non-conservation by generic Rashba spin-orbit coupling would surely mix and destroy the edge modes because the total Chern invariant satisfied 1 − 1 = 0. This is a good lesson for not assuming things without actually doing a calculation! Charles Kane and Eugene Mele has the same idea, but actually tested it with a numerical calculation, and realized that the quantum spin-Hall state was indeed topologically stable because of a previously-unrecognized topological invariant. Furthermore, a few years later, in 2007, it was simultaneously realized by a number of groups that this new invariant could be extended to three dimensional materials, now called “topological insulators.” This was shortly followed by the discovery by Liang Fu and Charles Kane of an extremely simple formula for determining whether such insulators with additional inversion symmetry were “topological” or not, leading to many experimental discoveries of topological materials, and an explosion of interest in the field.  In this period of the discovery of time-reversal-invariant topological insulators, my own work focused on rather different problems of the role of geometry rather than topology in the fractional quantum Hall effect, but in 2008, my student Hui Li and I discovered remarkable topological features in what we called the “entanglement spectrum” of quantum states, showing how the detailed structure of the entanglement revealed by its Schmidt decomposition contained far more information than just the single number characterizing entanglement entropy. This has turned into a widely-used diagnostic for studying the topology of entanglement.  In 2012, I was very gratified when the role that the 1988 “zero-field Hall effect” model had played in the topological insulator was recognized when I shared the prestigious International Centre for Theoretical Physics Dirac Medal with Charles Kane as well as Shoucheng Zhang, whose work with Laurens Molenkamp had led to a physical realization of the 2D quantum spin-Hall effect.  Finally in 2013, Shoucheng Zhang’s collaboration with the experimental group at Tsinghua University in Beijing, where magnetic material was deposited on the surface of a layer of 3D topological insulator, finally led to the experimental realization of the quantum anomalous Hall effect envisaged in my 1988 paper. Because of the robustness of the unidirectional edge states, these materials are potentially even more useful than the time-reversal invariant topological insulators.  Finally, this chapter of my story ends in October 2016, when I was awakened by the 5:00 a.m. phone call from Stockholm, followed by the magnificent ceremony and banquet there on the 10th of December. While my mentor Phil Anderson was not able to travel to be in Stockholm in person, he passed on tips and observations he had made when he received his own Nobel Prize in 1977. John Van Vleck, Phil Anderson’s thesis advisor, who shared the 1977 Nobel Prize for Physics with him, had as thesis advisor advisor Edwin Kemble, who, while he himself did not win the Nobel Prize, had an advisor [Percy Bridgman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1946/bridgman-facts.html) who was the sole Physics Laureate in 1946. So I discovered I have an illustrious “academic gene line,” stemming from fortunate choices I made back in 1973! |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0025 = DH**  Duncan Haldane: Hello  Adam Smith: Hello, this is Adam Smith calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  DH: Ah ha.  AS: Well, first of all many congratulations on the award.  DH: Thank you.  AS: How did you hear the news?  DH: They called me up at the usual 4:30 telephone call, at local time here anyway, in the morning.  AS: Your immediate reaction?  DH: Well I was aware that there was a vague possibility but I didn’t think it would happen.  AS: What did you do after hearing the news, immediately?  DH: Had a cup of coffee. [Laughs] I mean I’m a bit British, or phlegmatic, about these things so I didn’t kind of faint or anything.  AS: Do you think that there’s any significance in the fact that all three of you Laureates were born and initially educated in the UK and then all moved to the States?  DH: I suppose in the late 70s I think there was a bit of a de-emphasis by British funding things on fundamental research as opposed to useful research. I think it is a very bad thing when government agencies start to say… we should never say things like “What’s it used for?” Because all the big discoveries of really useful things don’t really come about because someone sits down and thinks “I want to discover something useful”. They occur because someone discovers something interesting and it turns out to be tremendously useful. I mean that’s the history of everything, in the transistors. The surprise in everything is that quantum mechanics is so much richer than we dreamed. Quantum mechanics is so bizarre! The things it can do, we didn’t discover them earlier because it was just difficult to actually even imagine that quantum mechanics might do these kinds of things. And now we’ve found a whole lot of new topological physics and quantum mechanics and it’s starting to become a big field. Basically, the world is more rich than we … Basically there must be all kinds of things out there that actually happen or can happen but we don’t see them because we haven’t been able to dream that such things are possible, and that was really, probably a surprising effect in all this. It’s very difficult to know whether something is useful or not, but one can know that it’s exciting.  AS: That’s a very important message to deliver. I shouldn’t keep you much longer because I imagine that people are going to be battering down your door any second.  DH: OK, I think I hear somebody else trying to come through on call waiting.  AS: Let me just ask you, will you be coming to Stockholm in December to receive your Prize?  DH: Yes, I will be, certainly, yes.  AS: Ah, splendid, well we very much look forward to seeing you then.  DH: OK.  AS: Thank you so much for speaking to us.  DH: Thank you so much. Bye. |
| **Interview** |  |
| Q19 | **I noticed that you brought an artefact for the museum, what is it?** |
|  | Duncan Haldane: Well, when I initiated the work, which this prize was given for, one of the two works which I received this prize for took a long time for to be published, because it was actually contradicting conventional wisdom at the time. Perhaps I didn’t explain it clearly enough, but in any case, this paper was rejected by a number of journals. And in fact, the original arguments which I discovered that magnetic chains had what we now know is a topological phase, when the quantum spin with an integer-spin, but not a half-integer-spin, which was … no one had thought that was an important feature. Why I discovered this really accidentally, I mean I, as a consequence of another piece of work I had done earlier. And it turned out that people’s understanding of magnetic chains was confused and that there were two sources of it.  One was kind of that semiclassical picture that thought of spins like little compasses, little arrows, that pointed and wanted to be parallel/antiparallel. And that works very well in high dimensions when long-range order occurs at low temperatures. But it was actually when it had been known mathematically that a one-dimensional chain, a very long, thin chain of atoms couldn’t, it ought to be destroyed by quantum mechanical fluctuations at any finite temperature, but also at zero temperature. On the other hand, there was a mathematically exact solution of a toy model, that [Hans Bethe](https://www.nobelprize.org/prizes/physics/1967/bethe/facts/) had discovered, before he went on to, you know, do huge work in understanding why the sun, how the sun works, and working on nuclear energy. But his early work on magnetic chains had this interesting solution which he guessed, and it turned out to be correct. And he probably didn’t understand why it worked, because he thought he would apply the same methods to higher dimensional magnets, and he promised that in his original paper. But of course, it turns out that what he’d locked on to, involves some very deep mathematics that took about 50 years to understand.  So, most of the scientists who work in magnetism, they knew of the existence of Bethe’s exact solution for a chain of magnetic atoms where the spin took the smallest possible value of 1/2, and locked superficially exactly like the spin wave theories that worked for, that were basically semiclassical, that worked in higher dimensions. And so, they assumed that ok, there was a little kind of, some kind of technical problem with the spin wave theory, but it had to be morally correct; the detail, this little problem didn’t really matter. But they were completely wrong, because the resemblance was purely superficial, and we now know that Bethe’s solution describes very interesting excitations which you call ‘spinons’ now, that it should carry spin-1/2 why the spin wave would carry spin-1, and they got nothing, it’s just an accidental coincidence of the formulas.  I actually, by covering this from a completely different angle, which was that there had been a, again going back to the 30ies, a remarkable relation between fermions and spins, a spin-1/2 object could take two possible states; up and down. And if I have an orbital which you can put a fermion in, it can either be empty or filled, and there is a kind of mapping between the two. But fermions, they have this fundamental property that if you exchange two of them you get a minus one sign, so fermions are said to … the operators that create them are said to anti-commute well. You get a minus sign if you add or remove them in different orders, while the spins don’t have that. But Jordan and Wigner in the 30ies had discovered a nice little way to map the two by putting in, what’s now called a string, in front of the things in the operator. So, they had a remarkable relation between spins and fermions that worked. So, it was pointed out that you could map the spin half chain problem that Bethe solved into a problem involving fermions. And at some limit these were non-interacting fermions, so you could understand the thing.  Based on that language, a completely different language, which I had followed on some work that Alan Luther and Vic Emery and I guess Luther and Ingo Peschel had done in the early 70ies. This allowed one to calculate things – not exactly – but in an approximate method. While the Bethe solution, the one that gave you the energy levels, no one at that point had ever worked out how to calculate anything else out of these very complicated wavefunctions that Bethe gets. It took another, probably 20 years, before, finally the solution of how to calculate with Bethe’s solution was found. So, using these methods, which a normal physicist, who isn’t a very abstract mathematician, could actually do a calculation. So, in this language of the fermions, I made a kind of theory called ‘Luttinger liquids’, which gave a general scheme for what interacting fermions should do in one dimension, and then applied it to this spin-1/2 model. So, Luther and Peschel had made a large progress in the spin-1/2 model; by treating it as a fermion field theory we could actually do a calculation.  But they had missed a detail, which was if the spins like to be in the line plane, like compass needles, you could treat it easily this way, because one of the limits is non-interacting fermions. But the interactions in the fermion picture when the spin is to stand vertically upwards and down, like point towards the North pole or South pole, and they’d missed out why the transition happens at a certain point. From doing numerical calculations, in fact, by applying my general theory to the Bethe “Ansatz” solutions, I saw within them exactly what had been missed. And so, it gave me the understanding of how to properly do a treatment of spins. And then I could actually treat any spin, not just spin-1/2, and immediately I did it. I found that in fact, using arguments based on what [Kosterlitz](https://www.nobelprize.org/prizes/physics/2016/kosterlitz/facts/) and [Thouless](https://www.nobelprize.org/prizes/physics/2016/thouless/facts/) had done, that the conventional argument was just wrong. And something quite different happened for the next spin up, spin 1, and all spins which were integer values rather than half-integer values. So, that was it. In fact, this paper states the first lines of stating that, you know, this leads to a very unexpected conclusion; that the spin-1 chains behave completely different from what the Bethe “Ansatz” solution said/did, and it was nothing to do with the spin waves.  So, this was kind of against the orthodoxy … because a lot of people … I mean spin chains were kind of obscure, but in the 70ies a lot of people started working on them because there was some possibility of actually making materials that did this. And, in fact, spin chains had grown a lot of current research on understanding how thermal equilibration happens; people are using spin chains for all kind of things now. But, anyway, this started getting popular, but I was unable to get this published. I sent it to one journal, got turned down with three dismissive reports. So, I just sent it to another one, and I got essentially, I don’t know if it went to the same referees, but essentially, I got the same kind of reports back. So, it took another couple of years, to get the thing published, I mean I, perhaps I didn’t explain it in the language that these people could understand. But in fighting these referees’ reports, I rephrased the arguments in a much more, perhaps, a better way. But the original way I discovered this got completely lost from it. So, this paper was actually, was referred to in the literature because people went on and basically validated the results in here.  There wasn’t an archive at those times, and somehow, I, in moving around, I didn’t have this paper. The people who worked on this problem, and actually validated that I had found what they referred to, that they never had any copies anymore. The institute in Grenoble, the institute in Laue-Langevin, where I did this work, there was an official number of pre-print, but of course they had cleaned out all their cabinets. And I was searching in the boxes in my basement. In fact, the Swedish television people wanted me to meet to show them the boxes, because I have a problem with throwing out papers. So, I have lots of boxes, unopened boxes, which had accompanied me in various moves that was accumulated, and I thought that it might be in there. But in fact, a Hungarian physicist Jenő Sólyom, who worked this problem, eventually came up with the missing pre-print. So, it was actually very interesting cause it actually, it does make a connection that I had forgotten about completely with the work of Kosterlitz and Thouless.  Initially you wouldn’t even think there was a connection because their work was based on, essentially, classical problem of a super fluid film in two dimensions. And Kosterlitz, when he talks about this, he makes it clear that he just deals with classical mechanics, and quantum mechanics introduces too many complications. But, in fact there is a remarkable relation between two special dimensions and one space- and one time-dimension, which is kind of like the relation between relativity in space-time; three space dimensions and one time-, and you get the four-dimensional space-time continuum. Well, a one-dimensional system, where you have a one-dimensional space-time continuum, one plus one dimensional, you have time and space. And there is a remarkable mapping that maps statistic, classical, statistical mechanics of systems at finite temperature described by the Boltzmann factor, with probability weight, and quantum mechanics, which is described by an amplitude for a process to happen. So, the vortices in two dimensions, which if you wonder around the vortex, the spins rotate by two π, turns into, in one space-time dimension, what’s called an “instanton” process, or “tunnelling” process, where, if the spins are all kind of untwisted at one time, and they twist through one turn around a plane and a second plane, and if I do a walk in space time around that, the spin rotates by two π. But the remarkable thing is that quantum mechanics is far richer than the statistical mechanics, cause in the Boltzmann’s formulation of statistical mechanics, the weight factor, the probability factor is always positive. Probabilities, in classical mechanics, are positive, where in quantum mechanics, there are in general complex numbers. But if there is some kind of time-reversal-invariance property, they can be their real numbers, which are plus or minus. But, when things can be both plus or minus, when you combine them, they can cancel, which cannot happen in that.  So, it turned out that the basic process, which should be, the general process was the one I discovered, and the spin-1/2 was a very special case, which was behaving differently, so, the question was not why the spin-1s behave differently from the spin-1/2s, but why the spin-1/2s behaved the way they did. And that’s basically because there was a minus one factor when cancel things. So, this relation to Kosterlitz-Thouless was actually how I realised the thing is a mapping from the classical Kosterlitz-Thouless transition in two dimensions, to the quantum version, in one plus one, which has been incredibly fruitful for all the people working in one-dimensional systems too. So, despite Kosterlitz’s disembowel of quantum mechanics, in his work it has actually been very important also in quantum mechanics. So, this paper has that in it, and as to say, the published version, which is two years later, that connection was completely lost. So, it’s very interesting to remember this and how I discovered things. I was very pleased that someone finally, after I had drawn a blank everywhere, for copies of my own work … |
| Q2 | **Did you ever doubt yourself; did you think that you must have made a mistake or must be wrong?** |
|  | Duncan Haldane: No, I was always confident. I mean people were kind of telling me things like this is a … some referee reports were saying this was obviously wrong and in complete contradiction to fundamental principles of physics, such as continuity, or something like that. About that time … and other people called this the ‘Haldane conjecture’, I never, actually I looked in the paper and I used the word ‘conjecture’ about some other tiny detail, which is not the main thing. Somehow this got built as, not a prediction or a theory, but a conjecture – as if it was a guess, a speculation. And of course, it wasn’t. But sometime around that time, the Bethe’s method was applied to a modified spin-1 chain. In fact, two authors in the Soviet Union seemed to have found this spin-1 chain solution about the same time. So, there was two versions of this, and I was the referee of both, actually. And I guess they, of course, found something that looked very much like the exact solution of the spin-1/2 chain. But it wasn’t solving the regular problem, there was a slightly modified problem; an extra piece was added on. And, I suppose I might have had about ten minutes worth of soul-searching when I got the first of these to referee.  But finally, I realised that they were doing a slightly modified problem that was special. And in fact, that turned out to be the case; that if you add this extra piece to the problem, and vary in strength; if you switch it to zero – nothing, it’s the same as what I predicted, but as you switch it up, there’s a critical point at which a phase transition to another kind of state, called the dimary state happens. The solvable, the models aren’t exactly solved by Bethe’s method; they’re usually special, and this one was exactly on a critical point. Now, it turned out that these models were extremely interesting for conformal field theories, which were turned out. So, lot of developments happened in this whole business. And those were very interesting models, but they weren’t generic case, so, I suppose I had ten minutes worth of doubt. But I quickly recovered. It was so clear to me that I couldn’t quite understand why people wouldn’t appreciate my arguments. But maybe we feel that we are clearer than we really are … |
| Q5 | **Is there any person that you have worked with that has been a huge inspiration for you?** |
|  | Duncan Haldane: I was very fortunate to work with [Philip Anderson](https://www.nobelprize.org/prizes/physics/1977/anderson/facts/), who was a 1977 Laureate in Physics, or one of the three. In fact, he got the Nobel Prize with his graduate advisor, [Van Vleck](https://www.nobelprize.org/prizes/physics/1977/vleck/facts/). So, I guess that puts a big pressure on my students to make sure they don’t break the chain, right? So, he had a very unorthodox way of thinking, and maybe that rubbed off on me in some way. I mean, I was extremely inspired by the way he thought about things. I think obviously that was a big influence on the way I developed as a physicist that I had a good chance to interact with Philip Anderson, who has done an amazing amount of different things. With Philip, the Nobel Prize he got wasn’t for the thing he did which was the best; a bit like [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/), who got the Nobel Prize for the photoelectric effect and not for the gravitational theories. |
| Q9 | **What do you feel, do you feel that you’ve gotten the prize for the thing you are most proud of?** |
|  | Duncan Haldane: Well, I’m proud of a number of things. I guess I, this was interesting; it’s probably only in the magnetic chain stuff I always felt was ok, it was interesting, but it didn’t have any obvious applications. It was maybe changing what you thought about things, but more of a kind of theorist thing, right? But it was interesting to a special … some set of people. But a lot of people were very impressed by it. I suppose, partly because … if a lot of people had post it, they had to say, well, it was either, if it was obvious – it meant they were stupid. So, I had to be brilliant instead, to have discovered it, I suppose. But no, I’ve done a number of things, but, the general theme I think, has been this topological matter. And, the other piece of work I did, which was to find, to realise, that you could have a quantum Hall effect without a magnetic field, just due to some kind of magnetic interactions in the system. It is potentially a very practical, useful thing. But it has taken some time to actually be creative in real materials. You know, it doesn’t really matter to me what they gave it for I suppose. It’s kind of a great honour, and it is great for our field, actually.  What I think has happened is that, when I look back and see what, the way I was taught about physics, in the 70ies, and the way that condensed matter was feud by people. I think, my advisor Phil Anderson, were one of the few people who took a very different view point. But the way we think about it has just changed. The things that went in the textbooks, that people thought were important, they’re kind of just rather boring details. And all the new stuff is absolutely absent. So, it’s been a, you know, great honour I think to be involved in laying the groundwork for this complete change of the way we look at things. So, I suppose that’s probably why this prize has happened. And partly because of what happened about ten years ago; some of this work was generalised to so called “time-reversal-invariant topological insulators”. It suddenly turned out that there were real topological materials that had been sitting on people’s shelves for many years without being noticed. And, just the power of theoretical ideas to, reveal something, very worthy, big calculators and experimentalism that’s just not noticed, it’s amazing, right?  So, it’s a question of really, I think we need the imagination to see what quantum mechanics can do, and we need to understand all about quantum mechanics, it’s laws, but we don’t really understand this potential. In the past things were studied by basically hitting them with a hammer, but now we have arrived at where we are actually able to try and tweak or nudging things around, and understand how quantum mechanics influences what happens. And I think my basic line on this, is that quantum mechanics does stuff much cooler than we could’ve even imagined, and part of this emerges, part of this work. And much more, cooler, things have been emerging since then. And people have dreams of quantum computers and all kinds of quantum information technologies and things. And I am not sure what’s going to happen, but something’s going to happen, because so many people now are looking at this, and it has really become … I mean, once you’ve got real things, then people start taking it seriously. And it has become incredibly inspirational and exciting to lots of young people. And everywhere one sees, an experimentalist gives a talk and he views a little video of a coffee cup turning into a doughnut and backwards, a physicist, and you can find this on YouTube and …  The idea that topology has something to do with this material – it just, makes such a big impact on people. It’s just incredible how this thing has taken off. So, these ideas were kind of a sleeper for a long time, and it’s really the final thing where you’ve taken – it’s got three mathematical backgrounds to it, which, you know, I don’t really understand. But, I understand some of it, but just knowing the abstract mathematics doesn’t lead to something either. The mathematics was actually around for a very long time, since the 40ies, and it was only when some mathematician realised, the mathematical physicist realised, that, the formulas for example that David Thouless, and Kohmoto, Nightingale and den Nijs had found, were actually, they found that this topological, this number that didn’t change, and it was recognised, oh this is Chern’s integral of a curvature of a manifold, right?  Once you kind of name the mathematics, then of course there’s lots of tools around. But to actually find it, you’ll have actually be able to do, a concrete little, what they call a toy model calculation. So, the mathematics is often too abstract to actually pin down. But in what I have done, and what Thouless did, is to be able to do very simple calculations, that you can actually do, preverbally on the back on an envelope. Perhaps you’ll need a computer a little bit to do something simple. But to actually see what a model does you need to strip all the irrelevant details out of something and go down to the simplest, possible model, that contains the physics that you’re interested in, right. And I take this line in seminars that it is almost like contract law. Because if you have a contract, very often it’s got a little phrase in it that says that “anything that’s not written in this contract cannot be considered to be part of it” and the contract is the entire document, right? Once you’ve got your model down to be very simple, and it still exhibits the physics you are after, then it’s not omissible to discuss, to attempt to use anything if you have already thrown away as part of the explanation of the thing. So, this is a technique to getting down to the cleanest, simplest example of things. Then, if you can actually do a calculation, then you can be concrete. And then maybe you can see *how* it works with the deep and beautiful mathematics that gets exhibited in the actual solution.  But the final thing you need, on top of that, is  once you have actually shown it can be made in a model, in a toy model, it’s remarkable that the material science has got to the point where, if you think it can be made, someone’s going to make it. Before the toy models were attempts to make, you know, text-book thing for modelling complicated real matter, and the large school of *ab initio* calculation, to get everything right, all the details right. And of course, the people who worked on that, often had a poor opinion of toy models. But it turns out they missed all these fundamentally, simple and beautiful things. So, toy models are great, but once someone actually makes it, then you got the three ingredients; you got the mathematics, calculation that a kind of ‘simple working physicist’ can understand, and then, an actual material. Once that started to happen about in the last decade, once these three things came together, this field has just taken off, and who knows where it’s going to go. |
| Q3 | **How have you been able to keep your enthusiasm up within this field during your career?** |
|  | Duncan Haldane: I think we’ve just kept on, things just got cooler. I think in this whole field, I mean, it’s been so fruitful. It’s been actually great for young people at every stage in their career in this field. Other fields, like high-temperature superconductivity has really been the graveyard of many people, because it has been unclear, it’s complicated, and there’s lots of rival theories which fights this and that, and there’s people knifing each other… But in this field, every time a problem came up it got solved and something very interesting and new came up, and people said “fine”. But it’s renewed, it has kept on; new things have kept coming on, and in fact, it’s just been a sequence of progressively more and more cool things coming out. So definitely it keeps one’s interest up. And of course, you try and understand the big picture; the big picture is kind of gradually being assembled. So, it is still an exciting field and it is still going to continue to be an exciting field. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0026** |
| **Biographical** | **Childhood**  Iwas born on June 22, 1943 in wartime Aberdeen, Scotland and lived there for the first sixteen years of my life. My parents, Hans Walter and Johanna Maria Kosterlitz (Gresshöner) had fled Hitler’s Germany in 1934 because my father, a non-practicing Jew, came from a Jewish family and was forbidden to marry a non-Jewish woman like my mother or to be paid as a medical doctor in Berlin. Under the circumstances, my father decided that it would be in his best interests to leave Germany and accept the offer of a lectureship at Aberdeen University. My mother, who came from a conventional Christian German family, decided that she would follow my father to Britain so that they could be married, which they were in Glasgow.  I had a happy childhood in Cults which used to be a small village just outside Aberdeen and separated by farms and fields from the city but now is just another suburb of a much larger city. I was raised as a British child unaware of my German origins, although I was aware that my parents were different from those of my friends because they had a secret language used to communicate when they wanted to exclude me. My parents would have nothing to do with Germany and spoke only English at home except occasionally under special circumstances. As a result, I grew up speaking only English and had to wait several years before learning some basic German at school. In fact, for several years I did not know I was actually of German Jewish origin nor did I know what being Jewish meant. The only thing I knew was that a boy in my class got some extra vacation because of his Jewish religion. When this became known among the class, everyone wanted to change their religion for the extra holidays. My father had no interest in religion and left all instruction in these matters to my mother, who was a devout Christian. I was a nominal church going Christian until I left home for Cambridge University on a scholarship when, to my great relief, I could drop all religion and become my natural atheist self.  **Education**  My early schooling was in Aberdeen at a semi private school, Robert Gordon’s College, which I attended from kindergarten up to age sixteen. There I had a broad education including the sciences, mathematics, history, geography, Latin and French with a strong Aberdonian accent. Some of the teaching left much to be desired particularly in physics where even I, at the age of fourteen, could tell that the teacher did not understand the subject. My parents decided that I showed some talent for academics and that I was worth grooming for Cambridge or Oxford University. In 1959, I went to Edinburgh Academy where the English A and S level subjects were taught. There I was able to specialize in the sciences and mathematics. With the improved teaching, I found I could do best in physics and mathematics. Eventually, I concluded that the reason for this was that my ability to make logical deductions compensated for my unreliable memory.  At school, I was fairly average at the humanities but excelled at mathematics and the sciences. Chemistry was the science I enjoyed most because we were allowed a lot of freedom in the laboratory at school, where I would carry out various forbidden syntheses of explosives and other noxious substances. I remember a few occasions where the lab had to be evacuated when one of these experiments went wrong and a noxious gas escaped. Despite the enjoyment these chemistry “experiments” gave me, I was not very good at the subject because of the memory required and I had to make too many guesses, especially when we studied organic chemistry where the chemical formulae were too complicated to memorize. However, despite the rather boring experimental part, physics was where I excelled at school. It satisfied my six fact memory limit because I was able to deduce correct results more often than not. Also, about this time in my life, I discovered that I am red green blind and that this disability does not fit well with chemistry. Some of the experiments required me to distinguish between several test tubes each containing a different reddish fluid. They all looked the same to me, but my classmates assured me that they were all quite different. At this point, I decided that chemistry was not for me despite all the fun I had mixing the various chemicals I could access.  While I was at Edinburgh Academy, along with several fellow students, I sat the Cambridge University scholarship examination and, to my great pleasure, I was awarded a major scholarship in the Natural Sciences to Gonville and Caius College. A condition for attending Edinburgh Academy was that one joined the army cadet corps. Once every week in Edinburgh, I wore my kilt feeling foolish. Despite being born and raised in Aberdeen, Scotland, I did not have a drop of Scottish blood in me and had never worn the kilt before. I came to understand Edinburgh’s reputation as a windy city by parading through the streets in winter in my kilt in the usual cold horizontal rain which is an experience never to be forgotten.  **Undergraduate Years**  As an undergraduate at Cambridge from 1961 to 1965, I did the natural sciences tripos which covered most of the science subjects of that time. I chose to do physics, mathematics, chemistry and biochemistry because I enjoyed the chemistry at school. My color blindness again made this very frustrating and my poor memory made organic chemistry a bit of a nightmare as there were many situations where guessing did not work. I remember one situation in a biochemical experiment, where I ended up with a test tube containing some nondescript fluid. I was staring at it wondering what I should see in it or if I should do something else to the contents when, suddenly, it started to change color. Instinctively, I averted my gaze just before the test tube exploded. To this day, I have no idea what happened but that episode confirmed that chemistry was not for me and that the less dangerous and less memory intensive subject of physics would be my best bet. For fun, I joined the Cambridge Climbing Club which ran a bus or minibus to Derbyshire or North Wales every weekend. I discovered I was good at rock climbing and enjoyed the thrill of being high on a cliff with almost nothing to stop a fatal fall. From that moment, climbing at weekends became like a drug and I became obsessed by the sport.  About this time my grandmother died at age ninety-two and left me a small bequest which I promptly spent on a car and the necessary insurance. With the help of my little car a typical weekend would be scheduled as: Friday evening – drive as fast as possible to Llanberis pass in North Wales or to the Lake District or, in the cold winter, to Glencoe or Ben Nevis in Scotland for the ice climbing. Climb on Saturday and Sunday, if not raining, and drive back to Cambridge late Sunday night and early Monday morning. Then I would sleep until I woke too late to go to class, thus not having any lecture notes. The weekly tutorials kept my nose to the grindstone and rescued my academic career.  My life settled into a routine which was close to my father’s dictum of work hard and play hard. By now Berit had become part of my weekly routine. Although she did not climb herself, she enjoyed being with me in the mountains. On Friday evening, Berit and I would jump into the car and drive as fast as possible to North Wales and Berit blames this period in our lives for causing whiplash injuries to her neck. We would return to Cambridge or Oxford late on Sunday after a hair raising race through the dark and I would try to stay awake in class until the following Friday. I would often leave for an afternoon’s climbing on some closer rocks once a week or perhaps do a bit of climbing on some nearby buildings. I had so much fun with these extra curricular activities that  I seriously neglected my studies, especially in my final year. Also, in the Part 1 exams at the end of my second year at Cambridge, I did rather well in line with the expectations of me as major scholarship holder and I now expected to obtain a first-class degree in my third year with ease.  However, I did not perform as expected in the all-important final year and ended with an upper second class degree. I am a bit surprised that I did so well because I hardly studied, nor did I attend many classes mostly because I had done so well in the first two years without studying much. I thought to myself “Michael, you are some sort of genius”, but the final year taught me differently. Shortly before the final exam, I realized that I did not know what had been in the syllabus and panicked. I borrowed lecture notes from friends as I did not have any and read and read for most of each day as I watched time passing inexorably towards the final examinations. I struggled with questions I half understood, knowing that my enjoyable undergraduate days of climbing and pubs were now exacting their price. In the summer, I went on a climbing expedition to the Peruvian Andes where I could forget my Cambridge failures.  While I was in Peru, my father arranged for me to have an extra year, 1965– 66, at Cambridge to do Part III mathematics to try to improve my disappointing performance. He was an eminent academic who well understood the importance of a Ph.D. degree. This year was a mixed success because I did not appreciate the almost rigorous approach of the applied mathematicians and physicists teaching the courses. Much of what I learned I found useful later. Unfortunately, I had not learned my lesson and my obsession with rock climbing again prevented me from spending the necessary time and effort my studies really needed. Again,  I had to be satisfied with an upper second class performance. At the end of the year, I was lucky to have the necessary qualification for graduate school at Cambridge, but not in high energy theory which I was determined to do. I was offered a position with [Nevill Mott](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/mott-facts.html) in experimental solid state physics but I turned this down in favor of an offer from Oxford in high energy theory.  **Graduate and postdoctoral years**  I spent the next three years, 1966–69, in Oxford sharing a rented house with several medical students and my future wife, who spent the time complaining that she had to work too hard keeping several messy males tidy, clean and fed while I worked on my Oxford D.Phil. We quickly fell into a routine where Berit left for work at 8:00 am and I at 10:30 am to arrive for the 11:00 am coffee at Rudolph Peierls’ department of theoretical physics. My D.Phil. supervisor, John Taylor, left me alone to do my own research and find my own problems which upset me at the time but, in retrospect, this was excellent training for my later career. Whatever his reasons, I am eternally grateful to John for putting up with my foibles at Oxford. I managed to write three papers on Regge poles and the Veneziano model, a precursor to modern string theory, with other graduate students.  These were the subjects I would continue to work on in Torino and later at Birmingham until I changed fields. In 1969, I managed to write my thesis, imaginatively titled “Problems in strong interaction physics”, which, I suspect, has never been read. Of course, the weekends and vacations were still reserved for my climbing obsession, which still occupied all my leisure time. At this time, I spent all summer vacations climbing in the French or Italian Alps and even Yosemite Valley in the USA and managed to get quite a reputation as a mountaineer.  Our next adventure was when I managed to obtain a Royal Society grant for a postdoctoral fellowship which I could use anywhere in Europe. I decided on the Istituto di Fisica Teorica, Torino, Italy because Sergio Fubini, one of the pioneers of modern string theory, was there but, more importantly, it was close to the Alps where the best mountains such as Mont Blanc are situated. Neither Berit nor I spoke any Italian but we were young enough that this was just a minor challenge to be overcome. We had many other interesting challenges to overcome of which renting an apartment and furnishing it, all in Italian, was one. Another was to deal with the local car drivers. I learned to love alpine skiing, made contact with outstanding local climbers and created a climb in the Val d’Orco which bears my name, “Fessura Kosterlitz” which was not repeated for a decade. My achievements in physics are somewhat less well known but I did do some very long calculations on a precursor to modern string theory. This resulted in one paper “The General N-Point Vertex in a Dual Model” with a fellow postdoc, Dennis Wray. I did not realize at the time that I was in the forefront doing research in what was later to become string theory.  While I was in Italy, there occurred a pivotal event which led to my Nobel Prize. I applied to CERN for a postdoctoral position for 1971–1972 but failed to submit the necessary paperwork in a timely fashion and was turned down. Panic set in as the prospect of unemployment hung over me. Berit walked to the main train station to buy a British newspaper which contained advertisements for academic jobs. There was one for a three-year postdoctoral position at Birmingham University in England and I dutifully applied for this although I did not really want to go there for high energy physics. I got offered the job and duly accepted.  Another, even more pivotal, event occurred in Italy. Berit and I had by now been an inseparable couple for seven years. The question of marriage arose on and off during those years, but we decided that I was too immature and the three-year difference between our ages was too great. By now we felt differently and I suggested that our relationship seemed very stable and we might as well make it legal. We married in Torino in September 1970 after a long battle with the local bureaucracy. Also, this avoided the problem of obtaining work permits for Berit. Since then, we have had more than a few adventures and three children who are now settled in Boston and Providence in New England. We have also been blessed with five grandchildren.  I spent the next three years, 1970–1973, as a Research Fellow at the Department of Mathematical Physics at Birmingham University. I continued my calculations on the dual resonance model of Veneziano and was about to write up my calculations when a preprint by a group at Berkeley doing exactly what I had done appeared on my desk. Needless to say, I was rather annoyed but shrugged my shoulders and started a new long and laborious calculation. I completed this and started to write it up when another Berkeley preprint arrived on my desk. When this happened yet a third time, I did get rather upset and went from office to office asking the occupants if they had a problem I could look at or if I could help in some way. Eventually, I found myself in David Thouless’ office listening as he described concepts and ideas I knew nothing about. He talked about superfluidity in 4He films, crystals in two dimensions, vortices, dislocations, topology and many other related ideas.  Although this was all new to me, as was statistical mechanics which I had ignored as being unnecessary for high energy physics, the ideas made sense to me. After I left David’s office with my head spinning with all these new ideas and concepts, I returned to my own smaller office and began to work on these new wonderful ideas which David had introduced to me. The central idea was that the only way a flow in 4He can dissipate is by the creation of vortices and their subsequent motion. A superfluid can be characterized by the absence of free vortices and a normal, dissipating fluid by the presence of a finite concentration in thermal equilibrium. In two dimensions, the problem becomes equivalent to the equilibrium statistical mechanics of a set of point charges interacting by a Coulomb potential. David and I introduced the concept of a vortex as a topological excitation or defect. The same ideas can be used to discuss the melting of a two-dimensional crystal with point dislocations playing the same role as vortices in a superfluid. David and I wrote two papers on this [1, 2] where we discussed the basic theory of defect mediated transitions. About this time, David casually directed my attention to some papers by [Phil Anderson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/anderson-facts.html) and coworkers on the Kondo problem and its mapping to a one dimensional 1/*r*2 Ising model [3] which introduced me to renormalization group methods, although this terminology only came into use later. I did nothing for six months but reading and re-reading this seminal paper and reproducing the calculations to try to understand it. During this time, Berit tried in vain to tidy my office but was firmly told to leave all papers alone. A year later, based on this research, I published a paper which discusses a renormalization group treatment of the two-dimensional planar rotor model of superfluid 4He [4] which is the basis for the exact prediction for the superfluid density [5].  My next position arranged by David Thouless was as a Postdoctoral Fellow at LASSP at Cornell in 1973–74. There I met Michael Fisher and his young, very smart graduate student, David Nelson. Even at this early stage in his career, David demonstrated that he was going to become something special. I was excited by the prospect of learning about phase transitions and critical phenomena from the Cornell experts, Michael Fisher and [Ken Wilson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1982/wilson-facts.html). Field theoretic methods and the epsilon expansion of Wilson and Fisher [6] had permitted enormous progress in understanding a huge variety of phase transitions and I badly wanted to be one of the pioneers in this. Working with Nelson and Fisher opened my eyes to what physics is all about, how important experimental data are and how to choose the problems to work on. In the 1970s, critical phenomena was a field which was at last opening out by Wilson and Fisher’s epsilon expansion methods. With Nelson and Fisher, I worked on bicritical points using renormalization group methods. To our great pleasure, we were able to understand in great detail the shape of the phase diagram in the vicinity of a bicritical point and why the various phase boundaries had the shape of experiments on anisotropic antiferromagnets. This was at the height of the development of critical phenomena in 4 − Ɛ dimensions and I was excited to be in the middle of it with the leading authorities in the field. I learned the importance of testing one’s theory against the ultimate authority in physics, experiment.  During all my postdoctoral years I kept to my mantra: first climbing, then physics and last family. In fact, when I was in my twenties, I was one of the best climbers in Britain and even considered giving up physics in favor of a professional climbing career. My teaching duties prevented me from going on any of the Himalayan expeditions I could have joined. However, on thinking about the possible consequences of this choice, sanity and my wife finally prevailed. I realized that, although I was technically good enough, a career in academia and physics would allow me enough vacation time to indulge in my climbing obsession. Some of my climbing acquaintances had chosen to become professional mountaineers and a few succeeded but most did not. I decided that I would probably not succeed in this.  **Tenured years: Birmingham and Brown**  I returned to Birmingham University in 1974 as a tenured lecturer, then was promoted to Senior Lecturer in 1978 and finally to Reader in 1980. I continued working on phase transitions and critical phenomena while teaching two courses at the same time. David Nelson and I managed to produce our important prediction for the superfluid density of a thin film of 4He [5], but my significant output slowed down although I produced several papers on critical phenomena. David Thouless was still at Birmingham, during which time we continued our collaboration on spin glasses until he moved to the USA. I spent a semester in 1978 as a visiting professor at Princeton, Bell Laboratories and Harvard respectively, bringing my family. My stay at Harvard was especially productive as David Nelson and I wrote our paper “Universal Jump in the Superfluid Density of Two-Dimensional Superfluids.”  By 1978 we had two children in Birmingham schools and my wife and I were happy and thought we were settled there forever. I was doing what I loved, climbing, immersed in physics, and spending the remaining time with my growing family. However, this contented period of my life was not to last, because I contracted the nasty autoimmune disease of multiple sclerosis. I awoke one day in September 1978 and was unable to stand up because my balance did not function. I was admitted to hospital where I spent one week while the doctors tried to figure out what was wrong. Eventually, a solemn neurologist said that there were two possibilities, a brain tumor or multiple sclerosis, of which the latter was the better alternative. It turned out I did indeed suffer from MS and life as I knew it was forever changed. Needless to say, I did not react well to this news as I assumed it meant the mountaineering half of my life was over and I would have to live the rest of my life without it. My wife was not as upset as I was because, by this time, a number of my climbing friends had died in climbing accidents and she was relieved that this would not happen to her husband. However, this thought was little consolation to me who could not envisage life without the mountains and I went into a deep depression which lasted for several years. The professor of neurology offered these kind words of encouragement, “There is no cure, some people live longer than others. If you can look back after 25 years you will know how bad a case you are.” Needless to say, this information also affected my physics productivity for a few years.  In 1979 I was offered a position as a tenured full professor at Brown University and Birmingham counter offered a promotion to a research professor as an incentive to stay. This would be at a Center of Excellence centered at Birmingham, which I was inclined to accept. I was about to refuse the offer from Brown when Birmingham abruptly withdrew their offer. Combined with my illness, for which I subconsciously blamed Britain, this was the last straw and I immediately tendered my resignation and left for Brown, where we have been since 1982. My wife and I finally became citizens of the USA in 2004 because, in that year, Sweden permitted dual nationality and my wife did not wish to give up her Swedish nationality. As a British citizen, I had no difficulty because Britain has always permitted dual nationality. After 9/11, I felt that my wife and I and, especially, our children needed the protection of citizenship, so we paid a lot of money to an immigration lawyer and became US citizens in 2004.  At Brown, my interests changed somewhat and, with the help of a grant from NSF, I started to work on various effects in two dimensional arrays of Josephson junctions such as disorder and in a magnetic field. These can be represented by a frustrated planar rotor model, which is quite different from the original 2*D*planar rotor model [4]. In this, I was greatly helped by a very good graduate student from Brazil, Enzo Granato. This system is an excellent system for the study of many variants of the original system of Kosterlitz and Thouless and is still under quite active theoretical and experimental investigation. We looked at some of the more elementary aspects of the system and slightly increased our understanding of it. These experimentally accessible variants of the model took us out of the realm of analytic work and my student and I turned to numerical simulations, which was the only way we could make any progress. This has turned into a more than twenty-year collaboration with Enzo at INPE in Brazil.  In 1985, I went on a sabbatical to France, bringing my family as I realized this might be the last opportunity for a family adventure. The children went to French schools and I spent six months at Saclay and Orsay with my Brown graduate students continuing the work on planar rotor models.  On return to Brown I became interested in numerical work with a couple of graduate students from Korea. Our projects were to study the kinetics of growth of a surface by random deposition. We studied the scaling of the interface width with time and evaluated the exponent to a high degree of accuracy. However, we could not compete with the massive simulations from a group in Germany. The other project was to investigate if it was possible to identify a weak first order transition by purely numerical methods [7] which method is still being used in 2016. Jooyoung has turned his talents to the protein folding problem and his group is now recognized as a leader in this field as they consistently score very highly in the CASP competitions. A Japanese graduate student, Nobuhiko Akino, has been very successful in his numerical work on randomness in superconductors and in *XY* spin glasses which have been longstanding intractable problems. We concluded that an *XY* spin glass exists in three dimensions and above, which result can also be obtained via massive simulations.  For reasons which are still unclear to me, I lost my NSF funding over this and have never been able to get it back. However, the problem never stopped to intrigue me and over the last ten years I have doggedly pursued the solution although it has proven to be somewhat elusive. I had a brilliant graduate student given to me by Brown who was invaluable help to me doing difficult numerical work, and together we managed to get a paper accepted in 2010.  I also have had a longstanding collaboration for the last twenty-five years with my colleague Tapio Ala-Nissila in Finland working on phase field models of growth. This is a surprisingly successful method for the numerical study of growth in fluids and in solids which we recently applied to the hydrodynamics of crystals [8]. This collaboration has also included my colleague, Martin Grant, at McGill in Montreal, Canada and Ken Elder at Oakland University in Michigan, USA as well as my Brown colleague See-Chen Jing. I also started a collaboration at the Korea Institute of Advanced Study in Seoul, Korea where I am now a Distinguished Professor visiting for two months every summer. Even at my advanced age of 73, physics still fascinates me because there are so many problems waiting for a solution that, despite my increasing incompetence, I would like to see understood before I retire. Perhaps in this respect, I am like my father who refused to give up working until he was over 90! On reflection having produced nearly sixty papers in my time at Brown is not bad, but nothing will ever compare to the exhilaration of our 1977 paper [5] when theory agreed quantitatively with experiment [9]. Each summer, Berit and I travel a lot, spending time in Brazil, Finland and Korea but always keep four or more weeks sacrosanct for our Swedish summer house where we can relax completely by watching the grass grow. The only disadvantage is that it always does grow and then needs cutting, which gives me about the only exercise I have during the year.  Last but by no means least, I am happy that I have managed to work since that dreadful day in September 1978 when I was diagnosed with MS. The twenty-five years have gone and, as predicted by the neurologist then, I now know the outcome. I was not a bad case. I had attacks every 18 months from age 35 to 55, some quite bad, some small relapses. When I was 55 my neurologist put me into a trial for a new MS drug. This was very successful and opened up a whole new field of pharmacological drugs for the easing of MS. Since then, I have been lucky in that I have never had another attack. I only battle the deadly fatigue that comes with the disease. I want to take this space to tell any budding scientist that, however bleak the future may seem due to illness or other problems, one cannot say you will not be successful.  More people than I can list here have contributed in vital ways to my success. Those that are probably the most important are David Thouless whose friendship, patience and collaboration are central to my career, Berit, my wife, for her patience and forbearance with my peculiarities and absences when I was either climbing mountains or working too hard and my children, Karin, Jonathan and Elisabeth for putting up with and loving their strange father who was absent too often and too long. I also acknowledge the support and friendship of my colleagues at Birmingham and Brown. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0026 = MK**  Michael Kosterlitz: Hello?  Adam Smith: Hello, my name is Adam Smith. I’m calling from Nobel Media, which is the media organisation of the Nobel Foundation in Stockholm. We run the official website for the Nobel Prize. Have you already heard the news of the announcement of the Physics…?  MK: No, I haven’t heard anything. I’m talking from an underground car park in Helsinki, Finland, right now so I can barely hear you.  AS: It has just been announced in Stockholm that you are one of the recipients of the 2016 Nobel Prize in Physics.  MK: Jesus. That’s incredible.  AS: [Laughs]  MK: That’s amazing.  AS: So the Prize is given to yourself, Duncan Haldane and David Thouless for theoretical discoveries of topological phase transitions and topological phases of matter*.*  MK: Oh that’s, yes, thank you, that’s, this is quite amazing. Thank you very much indeed.  AS: I must say you sound very calm.  MK: Er, yes. It just feels a little bit odd getting this news in an underground car park outside Helsinki.  AS: [Laughs] Actually, maybe remaining in the underground car park is a good option because you’ll be safe from the onslaught of press that will now descend.  MK: True, yes, but I guess I’ll have to face it eventually.  AS: I actually was given your number by your son at home, and I have to say he was absolutely elated.  MK: I’m sure that he is elated, but not half as elated as I am.  AS: [Laughs] That’s lovely. Oh, well, many congratulations from Nobelprize.org, and if you go to Nobelprize.org you will of course see the announcement of the news there.  MK: Thank you very much. |
| **Interview** |  |
| Q19 | **And you brought some artefacts for the museum, what did you bring?** |
|  | Michael Kosterlitz: I brought some things representing my two passions in life, or my two passions in life at the time. Which are one rock-climbing guide to big cliff in North Wales, and some old calculations on something, that unfortunately is not what I got the prize for, but one of the few hand read notes that we could find, after I had done a major clear-out. That was, what, 40 years ago or something, so of course, it’s had a few clean-outs since. And old scribbled notes which may be of some value, should I become famous. But at the time, the idea of becoming famous was just ridiculous, so it all went. |
| Q8 | **And the rock-climbing guide, you used to be a very avid rock climber?** |
|  | Michael Kosterlitz: Yes, I have two greater passions in life; well, probably in order my passions in life were first rock climbing, second physics and third family. |
| Q2 | **You are awarded the prize this year for absolute first research that you did, coming into the field that you now work in. Can you tell me a bit about how that came about?** |
|  | Michael Kosterlitz: I was doing high-energy physics and doing lots of elaborate calculations for no return. Then, I was a post-doc in Italy, and of course I needed another job, so the plan was to go to CERN in Geneva, but I failed to get the paperwork in on time, as is my standard procedure, and ended up at Birmingham university, because that was one of the few places from which I got an offer at the very late stage in the game. Birmingham was actually the last place I wanted to go, but it turned out to be the best move I ever made. Because it was there that I met [David Thouless](https://www.nobelprize.org/prizes/physics/2016/thouless/facts/) and we started working on this problem. Mind you at the time, as far as I was concerned, it was just an entertaining theoretical problem, no more. I had *no* conception that this could turn in to something big, no idea at all. I doubt that either of us had. It wasn’t until, probably about the 80ies at the earliest, that I realised that we had done something good. |
| Q16 | **So, in this time after you had done your research, until the time that you really realised what it had led to, did you think about changing fields again, or moving to other parts within physics?** |
|  | Michael Kosterlitz: Well, I did change fields; I started working on what is called non-equilibrium problems, in other words, on systems which aren’t in thermal equilibrium. And I was hoping to do something as good, when I realised that the work I had done, David and I had done, was actually good work. And I was trying to do something as good in another field but, never managed. |
| Q4 | **Have you moved back to this field again, is this what you are working on right now?** |
|  | Michael Kosterlitz: I haven’t moved back, because I haven’t really thought about the field for a long time. And there’s lots of other people work in the field, and are way ahead, you know, have developed it much further. And so, I am, I guess you could call it, one of the ‘grand old men’ of the field, who has trotted out from time to time to say some deep words about it. But beyond that I don’t really do much in the way of research in that field. |
| Q15 | **You work with topological changes and phase transitions. When the prize was presented, the Royal Academy brought out some different type of baked goods. Can you try to give us a bit of overview why this would be a useful image?** |
|  | Michael Kosterlitz: Ok, I’ll try. Let me start by saying that topology is a mathematical subject, which is concerned with the shapes of materials. Not the detailed shapes, because after all, as far as topology is concerned, a plane, you know, a flat surface, is equivalent to a sphere, which is equivalent to any shape you like. All that topology is interested in is the number of holes in the system. It’s a classification of shapes which can be continuously deformed into each other. |
| Q4 | **You can’t have a half of a hole?** |
|  | Michael Kosterlitz: Right. I mean, the rules are, you can’t get rid of a hole. Once a hole is there, you can’t get rid of it. Or you can’t make a new hole. Now the connection to what we did is a bit of a stretch, but the idea is that if you take your film of superfluid helium on a nice, flat surface – of course there are no holes in this surface. You say to yourself, what on earth has topology got to do with this? Well, in this context it doesn’t, but there are excitations in films of helium, where the fluid circles round and round a point. These are called vortices. And these excitations do exist, and it turns out they are quite important.  This is where topology comes in; because the surface of the manifold of which the topology is defined, is this layer of superfluid, not the actual thing that it is supported on. And so, if you got a vortex, where the fluid is spinning round and round, near the centre of the vortex, the velocity of the fluid has to divert; go to infinity. Which means that the material can’t be superfluid there, so that is going to hole. So, in other words, if you have a vortex produced, for whatever reason, the topology of the system changes. |
| Q2 | **Was that an intuitive leap, I mean, when you first thought of this idea? Because coming from the outside, it seems as two quite disparate things. Was it intuitive to you that this was a mathematical model that could be used?** |
|  | Michael Kosterlitz: Not directly. Because to me the physics was all in the various excitations that can occur. So, it is obvious that if you like, well we didn’t know it before we worked on this, that a vortex in superfluid helium, the centre of the vortex, was either empty, nothing there, or it was a normal fluid, not superfluid. So *that* part of it is simple. And so, I myself came from this point of view; I was only interested in the excitations, topology I didn’t know a thing about. Of course, I had the advantage of working with David Thouless, who seemed to know everything about everything. So, he realised that this was, you know, he used the word topology. And once he explained what topology meant, to me it suddenly became obvious. Call it topology or not, it didn’t really matter, but it sounded like a nice way to talk about it, so we called it ‘topological excitation’. |
| Q4 | **Are you surprised of how much this field has grown since you’ve worked in it?** |
|  | Michael Kosterlitz: I am amazed. Because there are so many …  The original papers are referred to so often it’s almost embarrassing. I knew that we both knew very well, that the same ideas could be applied to talking about two-dimensional crystals, at least the melting of two-dimensional crystals. Because the essential excitations that melted the crystal, you can call them dislocations if you wish, which are analogous to vortices in superfluid helium. That is as far as we went with the two-dimensional melting. You can work anything out, at least I did a calculation which didn’t go anywhere, because we made the assumption that the lattice structure didn’t matter. Then we also knew that in principle it could be applied to a superconducting film. So, given an estimate of what the critical temperature should be and so on.  But we never really took it seriously, because our argument was that you couldn’t have true superconductivity in thin films. Which is a correct argument, but we never thought about the question of how, what length scales superconductivity could exist. In turns out that experimentally, if you have a system of, let’s say, a centimetre, you know linear size a centimetre or so, which is big by experimental standards, then, as far as, this system should obey the standard vortex theory, and the cut off that is inherent in superconductivity is irrelevant, because it’s of the order of a centimetre as well. |
| Q15 | **So, are there any of these, I mean, there a number of proposed practical applications for this work. And sort of, moving on looking into the future, are there any applications that you are especially looking forward to seeing?** |
|  | Michael Kosterlitz: Oh yes! Oh yes yes yes. Because the hope is that the applications in quantum mechanical systems will eventually lead to this magic quantum computer. And I’ll be waiting for my desktop quantum computer. I hope to get one before I die, but I think that perhaps I shouldn’t hold my breath and wait and expect to get one. But anyway, with the developments in quantum mechanics, related to our ideas, it’s starting to look like a quantum computer may not be such a pie dream as I originally thought. |
| Q5 | **Looking at your career as a scientist, is there any person that really has inspired you, in your work or in your life?** |
|  | Michael Kosterlitz: Lots of people. My co-worker David Thouless. I first met him as a London graduate in Cambridge and Oxford around 1961, something like that. And he was teaching us, and it was ‘mathematics for scientists’ or something like that. And as soon as he started lecturing, I realised I am in presence of a mind that operates in a different level to mine, and probably most other people. So, of course, I was incredibly happy to collaborate with David, because collaborating with somebody with a mind like that, is just an amazing experience.  Then there are other people who have certainly influenced me greatly. There is Michael Fischer at Cornell, who taught me, I was a post-doc there, back in, when was it 1973-74, who taught me about phase transitions and critical phenomena and the importance of experimental work and how theories and experiments should collaborate and criticise each other. Then there was John Reppy, also Cornell, also a superb experimentalist, and who is responsible for the experimental verification of our theory. So, I guess there are all sorts of people who influenced my thinking and my career. But the most important ones happened early in my life. And the most important one is of course David Thouless. |
| Q2 | **I know you have been diagnosed with MS some time ago. How did you, what did you do? And how did you sort of overcome, and handle something like that?** |
|  | Michael Kosterlitz: I didn’t really manage to handle it very well at all, because at the time, as I said earlier, my major obsession, and a big part of my life, was mountain climbing. And that I had to quit because I couldn’t do it anymore. It is not easy to continue when half your life is just cut, you know you have no choice but to cut it out. I had a great deal of difficulty in coming to terms with my disability. However, fortunately, as my neurologist likes to say, I’m his star patient, so I did… My version of MS is at least one of these, going to the big remission where I come back almost to the level that I was before the attack. So eventually I managed to replace my passion for mountaineering with other things, and now I do work a lot and I travel a lot. And fortunately, I’ve got a very valuable wife who supports me whatever I feel like doing. And keeps on insisting ‘Look Michael, you can do it – its not as bad as you think, you can do it’. That is very important to me. |
| Q16 | **you’ve said earlier that coming into the field that you were awarded the prize for, one of the crucial things was your total ignorance of the details of that field. What do you mean by that, what do you mean when you say that?** |
|  | Michael Kosterlitz: Exactly what I said. Because, I was a high-energy physicist. And so, my graduate work at Oxford was all in high-energy physics, and I simply went to the required lectures and so on, and something called statistical mechanics, which I sort of ‘mm’ it was one of these model, rather difficult subjects, where it wasn’t part of my chosen research. I didn’t pay much attention to it. But statistical mechanics is the central tool of condensed-matter physics, so when I went to this problem with David Thouless, changed from high-energy to condensed-matter, then statistical mechanics became very important. |
| Q2 | **And was it important for you to sort of look at the problem with sort of unconventional eyes, or…?** |
|  | Michael Kosterlitz: Well sure, oh yes! Because, if you knew too much about it, if you were a normal person like me, you wouldn’t even go into the field because there are plenty of rigorous theorems, which were interpreted, is meaning that in that in systems like thin films of helium, two-dimensional crystals couldn’t exist. And there’s nothing wrong with the theorem, it’s just the interpretation of the theorem that was wrong. So David, who knew about these things, realised that it was just the interpretation that was wrong. Me, I was so stupid and ignorant that I said, I had no idea that this lack of long-range order was a serious problem. And so, I went ahead and basically looked at the problem in a different way, and it worked out. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0027** |
| **Biographical** | Iwas born on March 9, 1959, in Higashi-Matsuyama, a small city located about an hour’s train ride north of Tokyo. My house was located in the countryside, surrounded by rice fields on the north, east, and south. I grew up in such a peaceful environment.  I went to Kawagoe High School, a rather typical small-town school. This school had a tradition of allowing students to do whatever they liked rather freely. Therefore, I spent a lot of time practicing Kyudo (Japanese archery). I was not particularly good at Kyudo, but I liked it. During one’s time as a high school student, you have to decide what you intend to study as an undergraduate once you are admitted to a university. Since I was interested in physics as a high school student, my choice was rather clear: I decided to learn physics in the undergraduate course at Saitama University, a local university near Tokyo.  During my four years of undergraduate education at Saitama University, I continued to play Kyudo, even more seriously than during the high school. I regret that I should have learned more physics during my undergraduate studies, because these studies in undergraduate courses form the basis of everyday research. In any case, I found that physics was indeed interesting. So I decided to continue to studying physics at the graduate level.  I was particularly interested in experimental particle physics. Very fortunately, Professor [Masatoshi Koshiba](https://www.nobelprize.org/nobel_prizes/physics/laureates/2002/koshiba-facts.html) accepted me as a graduate course student in his group at the University of Tokyo. My life as a graduate course student began in April of 1981. Katsushi Arisaka was also a student in Prof. Koshiba’s group. He had just finished his Master’s thesis based on a Monte Carlo study of a nucleon decay experiment. This was the Kamioka Nucleon Delay Experiment (Kamiokande). He was the only student working on Kamiokande in early 1981. Just when I started my studies, production of newly developed photomultiplier tubes (PMTs) with a diameter of 50 centimeters began. Katsushi Arisaka convinced me that Kamiokande would be a very interesting experiment and asked me to work on it, which I started to do.  At that time the other main members of Kamiokande were Teruhiro Suda from the Institute for Cosmic Ray Research (ICRR) of the University of Tokyo, and Atsuto Suzuki and Kasuke Takahashi from the High Energy Accelerator Research Center (KEK). Soon after I joined the Kamiokande experiment, Yoji Totsuka returned from Deutsches Electron Synchroton (DESY) and started to help us. Soon he too joined Kamiokande. Kazumasa Miyano from Niigata University and Tadashi Kifune from ICRR joined during the preparation stage of the experiment. Also, Masayuki Nakahata, who was an undergraduate course student, worked with us.  I enjoyed the preparation work for Kamiokande. In early spring of 1983, we started the construction work on the Kamiokande proton decay detector in Kamioka. It took almost four months to finish building the detector. I liked the construction work, watching the detector being assembled slowly but steadily. After it was filled with water, data taking with the Kamiokande experiment began in early July of 1983.  Soon after the beginning of the data gathering, we came back to Tokyo to search for proton decays in the Kamiokande data. I enjoyed the data analysis as well. I decided to be a professional physicist as a result of experiencing the complete process of preparation, construction, and data analysis of an experiment through my work with Kamiokande.  I received my Ph.D. in March of 1986; my thesis was titled “Search for nucleon decays into anti-neutrino plus mesons.” No evidence for proton decay was observed. In the Japanese system at that time, one way to get a postdoctoral position was to be selected as a postdoctoral researcher by the Japan Society for the Promotion of Science (JSPS). I was not selected. Fortunately, Professor Koshiba offered me a position as a research associate at the International Center for Elementary Particle Physics (ICEPP) of the University of Tokyo for a fixed term of one year. However, I was unable to find a new position in a year, and so I stayed in this position for two years with the very kind understanding of the ICEPP people. I really appreciate their kind decision, because this two year period was the moment that I found the deficit of atmospheric *νμ* and carried out the initial studies.  In April of 1988, I moved to ICRR as a research associate working on the successor experiment to Kamiokande, the much larger Super-Kamiokande. From that point on I have been a member of ICRR. I was able to work on the atmospheric neutrinos and the preparation of Super-Kamiokande without worrying about finding a job. The construction of Super-Kamiokande was approved by the Japanese government in 1991. People from the USA, most of them from the IMB experiment, joined Super-Kamiokande in 1992. (IMB was a large water Cherenkov detector operated in the USA in the 1980s and in the early 1990s.) Since then, Super- Kamiokande has been an international collaboration. The onsite construction of the Super-Kamiokande detector started in April of 1995. I moved to Kamioka in March of 1995 for the construction of the detector and worked underground for a year. The construction was completed at the end of March of 1996.  After years of planning and construction, the Super-Kamiokande experiment started taking data precisely on schedule, at the stroke of midnight that began April 1, 1996. Since then, I have worked as a convener of the atmospheric neutrino analysis. In the initial stage of the Super-Kamiokande data analysis, analyses were carried out by two independent groups. After confirming that the analyses gave quite similar results, it was decided to merge the two groups into one. Since then, Ed Kearns from Boston University and I have led the atmospheric neutrino analysis. I stepped down from this role when I was appointed the director of ICRR in April 2008.  When I joined Kamiokande, underground experiments were just a very small sub-field of particle physics experiments. At present, after more than 30 years, these underground experiments have become some of the most promising, powerful, versatile, and efficient ways to explore both particle physics and the Universe itself. This research underground continues to stimulate my interest. I look forward to what new discoveries the future will hold. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0027= TK**  [Takaaki Kajita] Hello.  [Adam Smith] Oh hello, my name is Adam Smith, I’m calling from Nobelprize.org, the official website of the Nobel Prize. Congratulations on the award of this year’s Nobel Prize.  [TK] Thank you very much.  [AS] How did you hear the news?  [TK] Well, I just, well actually, when I received the phone call I was checking my e-mails.  [AS] In your office? Right. And what was your first reaction?  [TK] Well, that was really a surprise to me.  [AS] I imagine it is still sinking in.  [TK] Yes, yes, still kind of unbelievable.  [AS] You sound as if you are alone, are there not people around you yet?  [TK] Well actually I’m in a small room so no one around.  [AS] I’m sure that very soon you will be surrounded by people.  [TK] {Laughs] Thank you.  [AS] You’ve devoted your research career to the study of neutrinos and I just wanted to ask you what is it that makes neutrinos so fascinating for you?  [TK] Yeah, actually I started my career in the so-called Kamiokande experiment. That was a proton decay experiment, but after finishing my thesis I wanted to improve my study on proton decay and I need to study atmospheric neutrinos because it is a background for proton decay, and I noticed that there is something strange happening there. So that is the beginning of my research on neutrinos.  [AS] And what is the significance of your finding that neutrinos have mass?  [TK] Well I think the significance is, this is clearly the physics that is beyond the standard model of particle physics.  [AS] Yes, it’s extending the standard model. And perhaps it explains some of this missing mass in the universe.  [TK] Well, yes, the observed mass by neutrinos in this experiment could be a little bit too small to explain the majority of the masses in the universe.  [AS] Aha, but maybe explains a little bit?  [TK] Yes, yes.  [AS] One of the nice things about your work is that you get to play with such amazing equipment, these collectors deep underground. It must be exciting, wonderful.  [TK] Well, yes, yes, actually as an experimentalist I was always excited in working in super Kamiokande and also Kamiokande experiments.  [AS] Amazing, amazing pieces of equipment. So what do you think is going to happen to you now? In the next few minutes and hours?  [TK] No idea. [Laughs]  [AS] [Laughs] Have you ever dreamed of this moment before?  [TK] Well, of course, well, as really a dream, maybe years, but not serious dreaming so far.  [AS] Well, it will be exciting to hear how it goes for you. We very much look forward to welcoming you to Stockholm in December when you come to receive your Nobel Prize.  [TK] Yes, thank you very much.  [AS] Thank you. Well, many congratulations.  [TK] Thank you very much.  [AS] I think you should probably enjoy this quiet time because I imagine it’s the last quiet time you’re going to get for a very long time.  [TK] Hmm, OK.  [AS] OK, thank you very much for speaking to me.  [TK] Thank you very much. Goodbye.  [AS] Bye bye. |
| **Interview** |  |
| Q3 | **What’s your story? What brought you to science?** |
|  | Takaaki Kajita: When I was a high school student, I learnt basic physics and I started to get interested in physics. Then I decided to learn physics in undergraduate and I learned that the physics was really interesting and I decided to learn more and therefore I joined Professor [Koshiba](https://www.nobelprize.org/prizes/physics/2002/koshiba/facts/)’s group in the graduate course and I started the Kamiokande experiment. Actually, that was really the motivation for become a scientist. |
| Q5 | **Who was your most inspiring teacher?** |
|  | Takaaki Kajita: The most inspiring teacher to me was Professor Koshiba. He essentially told me that we always have to have a new idea about the new researches and also we always think about the really important researches. |
| Q4 | **Describe your Nobel Prize-awarded work in one minute.** |
|  | Takaaki Kajita: The work I was awarded to this Nobel Prize is based on studies of neutrinos produced by cosmic ray interactions in the atmosphere.  We observed that neutrinos passing through the earth are disappearing, well, half of them are disappearing, and this was concluded as the evidence for neutrino oscillations and therefore the evidence for the neutrino mass. |
| Q3 | **What motivated you to pursue your research?** |
|  | Takaaki Kajita: In 1986 I was analyzing the Kamiokande data, almost accidently I found that there is a significant deficiency in the newer neutron events observed in Kamiokande. That was really surprising, and this was the motivation for me to continue studies of atmospheric neutrinos in the later 20 years. |
| Q2 | **Have you had a eureka moment?** |
|  | Takaaki Kajita: Yes, again I think that eureka moment to me is the moment that we discovered the deficit of neutrino events in Kamiokande. |
| Q2 | **What’s the toughest challenge you’ve faced? How did you overcome it?** |
|  | Takaaki Kajita: The hardest time in my research work was when the Super-Kamiokande was badly broken in 2001. In the end we were able to recover from this accident, but the most important ingredient to get the Super-Kamiokande back was the good leadership and also the good teamwork. The photomultiplier tubes used in Super-Kamiokande was broken, more than half of them were broken in about ten seconds. Why these photomultiplier tubes were broken? Well, we think that one of the photomultiplier tubes in Super-Kamiokande was broken spontaneously, then it generated a shockwave and this shockwave broke the adjacent photomultiplier tubes and then these photomultiplier tubes generated another shockwave breaking the other photomultiplier tubes and this was what happened in 2001. After that accident Super-Kamiokande collaboration decided to rebuild the detector and we worked together to recover the original configuration. It took several years to recover to the full configuration. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0028** |
| **Biographical** | Iwas born in 1943 in Sydney, Nova Scotia, Canada, a city of about 30,000 people on Cape Breton Island. My mother’s and father’s families were Scottish and French settlers who had come to Atlantic Canada in the 1700s and early 1800s. My father was a Lieutenant in the Canadian Army and left for Europe when I was about a year old to participate in the battles related to the liberation of Holland. He received the Military Cross, one of the highest decorations for bravery and was wounded, returning to Canada in 1946. He and my mother were hard-working people who appreciated the value of a good education and encouraged me throughout my school days. I also had lots of support from my extended family and enjoyed my childhood, with a balanced mix of studies, fun, sports, family activities and work (I had a 104 house paper route that I remember as being almost all uphill, particularly in winter). My father was very active in the local community, serving as a City Councillor and together with my mother in many service and charitable organizations. I have one sister, ten years younger than I am, but we have a wonderful relationship in spite of the fact that I left home for university when she was just 7 years old.  Sydney was a great community in which to grow up, safe, social and supportive. I remember a positive educational environment in the  schools, with many helpful teachers. I and a number of my classmates in high school were particularly influenced by Mr. Bob Chafe, our math teacher, who went out of his way to engage us in the subject, including extra classes going beyond the normal curriculum. A number of my classmates went on to careers in academia, including several mathematics professors.  There was also a very healthy social environment in the community. I belonged to a service club for teenagers called HI-Y, associated with the local YMCA. This was the late 50s, Rock and Roll was King, and we had a dance for the boys and girls clubs on Friday nights after our meetings. Our clubs ran a community dance on Saturday night at the YMCA as a fund raiser for our service work. That was where I met my future wife, Janet, and I am very pleased to note that we will celebrate our 50th wedding anniversary in 2016. We have 4 children and 8 grandchildren who are a great joy to us. We have also loved to dance throughout our lives, undoubtedly because of our positive high school experience on the dance floor.  At the age of 17, I went to Dalhousie University in Halifax, 400 km away, to study Science, but with very little feeling for which area of Science I would like. I was strongly influenced again by my teachers, particularly Professor Ernie Guptill, my first year Physics teacher, who showed me how you could use mathematics to figure out how the world works in great detail. I also found that I was very good at solving physics problems and enjoyed doing so. These days when I am asked by young people how to choose what to follow as a career, I suggest that they try a number of areas to see which ones interest them and also to see which ones they are good at. The combination of those two features will enable them to have a successful career in an area where they are happy to go to work day after day. I feel that physics has been that way for me. I had my first experience of “engineering physics” by working in the summer for Prof. Ewart Blanchard, carefully measuring gravity on the roads of Nova Scotia, where we found an anomaly that was later developed into a profitable gypsum mine. I stayed at Dalhousie for my Master’s degree, working with Prof. Innes MacKenzie, studying the lifetimes of positrons in metals. A paper that arose from that work indicating the relationship of positron lifetimes to defects in materials is still one of my most highly cited papers. I spent one year obtaining a M.Sc. degree and then knew that I really wanted to be an experimental physicist.  As we were considering where to go for graduate school, my long-time friend, Peter Nicholson and I developed an idea that we put to the Department Chair at Dalhousie. We said that if he would sponsor a trip to potential graduate schools on the east coast of the US, we would take careful notes and provide a lecture and guide for other undergraduates considering where to apply. He agreed and we had a great time, visiting several of the Ivy League Schools among others. We then proceeded to apply to several of these schools for graduate study and were accepted but we were also accepted at CalTech and Stanford (Operations Research for Peter) and California just seemed like a great place to experience, so we accepted those offers.  CalTech was a marvellous experience. There was a Van de Graaff accelerator in the basement of the Kellogg Laboratory and we had as much beam time on that as we could want. My thesis supervisor, Prof. Charlie Barnes was an excellent mentor and very encouraging of the measurements that we wanted to pursue, using the nucleus to study fundamental symmetries and processes that could enable us to learn more about the basic laws of physics. I was very privileged to work with two close colleagues, Eric Adelberger and Hay Boon Mak. Eric went on to a very productive career in nuclear physics and in the study of the force of gravity at short distances with extremely beautiful and sophisticated experiments. Hay Boon became a Professor at Queen’s University, Kingston, Canada and made major contributions to parity violation measurements and the Sudbury Neutrino Observatory (SNO) project. At CalTech, we studied the properties of nuclear energy levels related to the symmetries of the Coulomb interaction, known as studies of Isospin Symmetry in nuclei.  Another interesting aspect of my time at CalTech (1965–69) was that I  was working in the Kellogg laboratory headed by Prof. [Willy Fowler](https://www.nobelprize.org/nobel_prizes/physics/laureates/1983/fowler-facts.html), who later received the Nobel Prize in Physics for the understanding of how the elements are produced in the sun and other stars. It was an exciting place to be a graduate student as the latest developments in physics were the subject of discussion every day. Willy had a very sunny personality and there was a very collegial atmosphere in the laboratory. The seminars were at 7:30 pm on a Friday night, often followed by a party at one of the Professor’s houses. An Indication of Willy’s personality is the subtitle of his Nobel Lecture: “Ad astra per aspera et per ludum” that he translated as “To the stars through hard work and fun.” This is a spirit that I have always respected for research work, education and collegiality.  John Bahcall was a junior faculty member at CalTech when I was there and [Ray Davis](https://www.nobelprize.org/nobel_prizes/physics/laureates/2002/davis-facts.html) visited for several periods. They were working on the theory and experimental design for the measurement of neutrinos from the sun with Davis’ proposed large tank of cleaning fluid. Some of my fellow graduate students were measuring nuclear reactions related to neutrino detection in chlorine and this experiment was a big topic of conversation in the lab. The discrepancy observed between Davis’ experiment and Bahcall’s theory became known as The Solar Neutrino Problem and ultimately was the impetus for the establishment of the Sudbury Neutrino Observatory almost 20 years later. Ray received the Nobel Prize in 2002 for his pioneering work in solar neutrino detection.  Following CalTech, I accepted a postdoctoral position at Atomic Energy of Canada (AECL) Chalk River Nuclear Laboratories, working on basic research at the accelerator facility there in Doug Milton’s Nuclear Physics Branch. This was another very productive period with an in-house Tandem Van de Graaff accelerator, lots of beam time and very skilled colleagues. A year after I arrived, I obtained a permanent position there because a vacancy opened up as Dr. George Ewan moved to Queen’s University. After completing a series of measurements providing detailed information on isospin-forbidden decays in light nuclei at Chalk River, Princeton, Washington and Michigan State, Eric Adelberger and  I published a paper concluding that the data was consistent with the effects expected from the basic Coulomb interaction with no evidence for any unusual distortions of this symmetry.  I then turned my interest to studies of parity violation in nuclei. One part of the Standard Electroweak Model associated with the exchange of neutral Z Bosons between quarks could only be probed effectively for up and down quarks, as the process was supressed for the other quarks. Therefore this process must be studied in weak interaction processes between quarks in nuclei where it is in competition with the strong interaction (about a million times stronger). The way to study the weak interaction in this case was to measure processes that violated parity symmetry. Basically we looked for the difference between a nuclear reaction and its mirror image. In most cases this would be expected to be a difference of only a part in a million, but in some nuclear processes it could be increased by factors of up to 1000 by the particular properties of nuclear levels.  During the 1970s I worked on a number of experiments of this nature with a number of collaborators, including a major experiment at Queen’s University involving many of the scientists who would eventually become colleagues on SNO. At Chalk River I concentrated finally on a measurement of parity violation in the disintegration of deuterium by circularly polarized gamma rays produced by a high intensity polarized electron beam. This experiment was carried out in collaboration with Dr. Davis Earle and formed the basis for our later long-time collaboration on SNO. The continuous-beam polarized electron source that we developed for this experiment was eventually transferred to the electron accelerator at MIT and used for further experiments by others there.  In 1982 I moved to Princeton University as a Full Professor and began work on polarized targets that were developed in collaboration with Prof. Will Happer, Prof. Frank Calaprice and Prof. Tim Chupp. The most interesting of these polarized targets with which I was associated was 3He polarized by spin transfer from optically polarized rubidium vapor. Through the development of samples at atmospheric pressure or greater, we extended the number of polarized nuclei by several orders of magnitude over previous techniques. This enabled a variety of experiments, including the use of polarized 3He to produce polarized epithermal neutrons for extensive measurements of parity violation in heavy nuclei at the Los Alamos spallation neutron facility. Following these initial developments, others used 3He polarized by this technique for a variety of nuclear and particle physics measurements and eventually for medical imaging.  In the summer of 1984, while at Chalk River finishing the analysis of our parity violation experiment, I became involved in the development of the SNO Collaboration, led by Herb Chen and George Ewan. Herb had an excellent idea for the resolution of the Solar Neutrino Problem if it was possible to borrow over 1000 tonnes of heavy water from Canada’s reserves. George had been seeking the best location for an underground laboratory as would be required for this measurement. Under their leadership, the original group of 16 collaboration members began work to develop a detector based on heavy water, to be sited underground and built with ultra-low radioactivity content.  It was a highly motivated group of scientists because we knew that the properties of deuterium in the heavy water could enable the simultaneous measurement of the electron flavor neutrinos produced in the sun and also the sum of all neutrino flavors. The comparison of these two measurements could provide a clear indication of whether any electron neutrinos had changed to another flavor before reaching our detector. However, the challenges associated with making these measurements were major, particularly with respect to restricting the radioactive gamma ray background that could break apart the deuterium leaving a free neutron, mimicking the second reaction that could be caused by any flavor of neutrino. The design had many challenges and required detailed simulation of the detector properties in order to be certain that it would be possible to observe both reactions.  I began work at Princeton on the measurement of radon gas emanated from materials and extracted from water. The tragedy of Herb’s death from leukemia in 1987 shocked and saddened us all, but we carried on, with George Ewan as Canadian spokesman and David Sinclair as UK Spokesman. I became the US Spokesman and was joined later in 1987 by Prof. Gene Beier of the University of Pennsylvania, an experienced neutrino physicist who had worked on the conversion of the Kamiokande detector to detect solar neutrinos. In 1988–89 I took a sabbatical year from Princeton at Queen’s University and worked with the international team on the development of a final design and detailed costing of the experiment for submission to the funding agencies in the three countries.  I was offered a faculty position at Queen’s University which I took up in the summer of 1989. Ironically, I was once again following in the footsteps of my long-time colleague and mentor, George Ewan, who was scheduled to retire several years later. I have often said that if you want a successful career in science, follow George Ewan’s lead. In December 1989 we received funding for the project from agencies in the three countries.  I became Director of the SNO Institute formed to take international responsibility for the project and also became Director of the international SNO Scientific Collaboration. By 1989, the collaboration had expanded to 14 institutions with a large number of experienced scientists and technical people with broad capabilities. The responsibility for various parts of the project had been accepted by groups within the collaboration, a number of our collaborators accepted responsibilities as Group Leaders and they carried out those responsibilities in a very dedicated way through the design, construction and operation of the project.  During the 1990s we followed our plans, overcame many obstacles by collaborative efforts and began data acquisition in 1999. Our first scientific results were published in 2001 and 2002. In those publications we demonstrated that we had in fact built the detector to meet the specifications that had been put forward in the 1980s. We had restricted the interfering radioactivity so that we could make accurate measurements of both of the reactions on deuterium that measured separately the number of electron neutrinos and the total number of all neutrino flavors reaching the detector from 8B decay in the sun. Our results showed that the calculations by Bahcall and others were very accurate for the initial rate of 8B electron neutrinos but that about two thirds of those neutrinos had changed into other active neutrino flavors (muon or tau) before reaching the detector. That conclusion was in agreement with the initial, less accurate results that we obtained by combining our results for electron neutrinos with the results of SuperKamiokande for solar neutrinos, where there is a small sensitivity for all neutrino flavors. We are honored to share the Nobel Prize with Prof. Kajita and the SuperKamiokande collaboration for their detailed measurements of atmospheric neutrinos and their observed decrease of the numbers of muon neutrinos while traversing the earth, explainable through the change of flavor of muon neutrinos.  These flavor changes for solar and atmospheric neutrinos could not occur unless the neutrinos have a non-zero mass. Those neutrino properties are outside the predictions of the Standard Model of Elementary particles and require extensions to that model. Finding those extensions that match the observed properties of neutrinos from these measurements and from the ever increasing number of new neutrino measurements will enable a fuller understanding of the laws of physics at a very fundamental level and an understanding of the many ways in which neutrino properties influence the evolution of our universe.  I was very fortunate to have such a dedicated and skilled group of colleagues, technical people and construction workers who worked wonders to build and operate a unique detector. When we obtained the data for neutrino interactions in our detector we were very satisfied to observe that the simulations that had been made back in 1987 were very accurate for the case where electron neutrinos were changing their flavor before reaching the earth. That meant that we had been able to accomplish the very stringent requirements that we had set for the project, including many aspects that had never been done before. It was a true team effort and I am very grateful to all members of the team and to their spouses and families that supported them throughout.  We were also very fortunate to receive strong international support from funding agencies, educational and research institutions, federal, provincial and local governments and the people of the Sudbury region who made us all feel at home there. We are also grateful to the management of AECL who arranged the loan of the heavy water and INCO/Vale who provided the underground location over many years, in what continues to be one of their most productive mines.  I truly enjoyed working with the SNO team and consider the very positive team effort on SNO to be a highlight of my many years of research in physics. Our results are significant for the basic understanding of neutrinos and that   is what we set out to do, for some of us almost twenty years earlier. I am very pleased with the very large number of young people who had the opportunity to have a “Eureka” moment with us and who have gone on to productive careers beyond SNO. This was a very significant scientific result and a very substantial educational experience for all of us, with which I am very satisfied. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0028 = AM**  [Adam Smith] Oh hello, my name’s Adam Smith, I’m calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm. Congratulations on the award of the Nobel Prize.  [AM] Thank you very much.  [AS] May I first ask, how did you hear the news?  [AM] Well, I received the phone call shortly after 5 O’clock my time, this morning, and was awoken to the wonderful news, and it was very surprising but very gratifying as well.  [AS] How extraordinary. What was your first reaction, what was the first think you did upon hearing the news?  [AM] I gave my wife a hug. [Laughs] She was awakened by the same phone call.  [AS] [Laughs] What is it about neutrinos which has kept you all so engaged all these years, what is it that makes them so very fascinating?  [AM] Well they are, along with electrons and quarks, fundamental particles that we do not know how to subdivide further, and therefore they make up a very fundamental part of the laws of physics at the most microscopic level, and therefore their properties are extremely important in terms of being able to understand our world in great detail. But they’re very difficult to detect and therefore their properties, for many years, were difficult to know, even the question of whether they have a mass that is greater than zero. We were presented with an opportunity to make a measurement which, if we were able to accomplish it, if we were able to reduce the radioactivity levels to a lowest level possible and build a detector the size of a ten story building two kilometres underground in a mine, that it was possible to make the measurement. And lo and behold, in 2002 we had an extremely clear result that showed that neutrinos do change from one type to the other.  [AS] What a lovely result, and it also must be enormous fun to play with such kit that you’ve described.  [AM] It’s fun, once you get there.  [AS] Yes.  [AM] Once you have the detector in operation and you’re sitting there observing a burst of light represented on your display screen you have the ability to observe particles that come directly from the core of the sun. They’re telling you what’s happing there right now in terms of the nuclear reactions that are powering it, you are able to make measurements of the fundamental properties of the neutrinos themselves and it’s a great experience to look at this data and realise that you are seeing things that are extremely fundamental in their nature.  [AS] That’s an extremely vivid and exciting image, yes, that neutrinos allow you to look into the core of the sun, which of course you can’t do with photons.  [AM] No that’s right, and it’s ironic that in order to observe the sun you have to go two kilometres underground. It’s not what you would expect, that that’s the best place to look into the core of the sun, but lo and behold it is. And the ability to understand how the sun burns is one other aspect of the measurements that is very interesting science as well and that we were able to contribute to in a basic way.  [AS] Wonderful, well, happily we’ll have the chance to speak about this more when you come to Stockholm in December to receive your Nobel Prize.  [AM] Thank you.  [AS] It’s been a real pleasure to speak to you, thank you, I hope you have a most marvellous day.  [AM] Thank you so much.  [AS] OK, bye bye.  [AM] Goodbye. |
| **Interview** |  |
| Q3 | **What’s your story? What brought you to science?** |
|  | Arthur B. McDonald: What brought me to science started back in high school when I had an interest in science, but really a more substantial interest in mathematics which was generated by a really excellent teacher who went out of his way to deal with us outside class and really get us fascinated by the whole subject of mathematics. When I went to university I was inspired by another physics teacher and I went to university thinking I wanted to do science and, with physics, the whole idea that you really could use mathematics to calculate how things work in detail intrigued me. I found out I was good at it, so what brought me to it was a combination of the fact that it was fascinating and that I seemed to be able to do it so I continued with physics through a masters degree and then a PhD and then my career. I have continued to feel exactly the same way, I am fascinated by it, I am still fascinated by the fact that you can do calculations that actually show how the universe works in great detail from the most microscopic to the largest scales. It is just great fun to have the opportunity to work in that sort of area. |
| Q4 | **Describe your Nobel Prize-awarded work in 1 minute.** |
|  | Arthur B. McDonald: What we received the prize for was determining brand new properties of neutrinos, fundamental particles that along with electrons and quarks we can’t subdivide any further and in the standard model of physics were thought to have zero mass and not change from one of the three types into another. We use neutrinos from the sun with a measurement using a material called heavy water that enabled us not only to measure the number of neutrinos of the type produced in the sun, the electron neutrinos, but also the sum of all neutrino types. If you compare those two you can tell whether or not there has been a change. We saw only one third surviving of the electron neutrino types showing that they change and that they therefore have a finite mass. |
| Q2 | **What’s the toughest challenge you’ve faced? How did you overcome it?** |
|  | Arthur B. McDonald: In 1989 I was presented with the challenge of being the director of an experiment to measure neutrinos from the sun using an enormous detector, the size of a 10 story building, two kilometers underground in an active nickel mine where they were simultaneously taking about five thousand tons of ore out of the other side of the same shaft. A very small shaft, about three meters by three meters by four meters into which we had to parcel all of the roughly one million parts of our detector. We had to do it in an ultra-clean environment because we were seeking to observe one neutrino an hour and we had to restrict all of the remaining parts to have radioactivity levels that did not interfere with this. We had created an ultra-clean environment, that environment was equivalent of what is created for producing semiconductor chips in a factory, in this enormously dusty environment. We also were able to borrow three hundred million dollars’ worth of a material called heavy water, which is about one in seven thousand water molecules have an extra neutron in the hydrogen nucleus, it was that neutron that made our experiment possible.  It was an enormous engineering task, you had to combine science and engineering to get the best of both in the overall process. You had hundreds of people working together in order for this to happen. I guess the biggest challenge was to try to get the tremendous creativity that each individual had working in a co-operative manner so that you were able to accomplish something that was truly unique and had not been done before in many of its different elements. So it was really a combination of making sure you didn’t compromise your scientific principles at any point, but practicing the art of the possible and dealing with the human element of such a large group of varied individuals and make sure that you get the best of all those talents applied to a successful project. So that was a challenge, and how did I accomplish it? I just had to deal with one day at a time and the questions that you came up with. But the biggest thing was having a tremendous group of very accomplished collaborators. |
| Q3 | **What motivated you to pursue your research?** |
|  | Arthur B. McDonald: I was a member of the original sixteen people who came together to start this project. We were inspired by a wonderful scientist named Herb Chen from the University of California at Irvine and a scientist from Canada named George Ewan. Herb Chen said You know if we had enough heavy water we could potentially solve a real problem for neutrinos from the sun in which it was observed that too few were being measured compared to what was calculated. One of the possible explanations of that was that neutrinos were changing from one type to the other and escaping previous experiments and that could be rectified by the use of heavy water. So it was a tremendous scientific concept that you could really answer once and for all a basic question in which if it were true you would be dealing with physics that goes beyond the very basic standard model and brings in new properties of neutrinos requiring a change to the most fundamental laws of physics.  We were all inspired by that idea that we could really go beyond the known because neutrinos have a very big influence in our universe. They affect how the universe evolves, the mass of neutrinos affects how stars and galaxies are formed. Neutrinos form a big part of supernova explosions where all of the elements greater than iron in mass are formed. We also knew that we could answer very fundamental questions about the sun if we were able to make these measurements. So the physics was very compelling, we as a group went on to add a number of other institutions, we ended up altogether with about 270 physics authors on our papers and that really indicates the number of people we were able to educate in the process and that was the other thing we thought was great. This a wonderful educational opportunity if we could possibly carry it off and so that is what motivated us. So, it was a very fundamental question to be answered. |
| Q14 | **What questions remain to be answered in your research?** |
|  | Arthur B. McDonald: There are number of very interesting questions remaining to be answered. We know now that neutrinos do change from one type to another and that means that they have a finite mass. We don’t know how large that mass is although we have some limits. Fortunately, we have an opportunity right next door in our laboratory in the same underground location with ultra clean environment, two kilometers underground to be able to address a number of these questions and we are doing so. One of the questions is this question of neutrino mass and also symmetries of the neutrino relating to the relationship between antimatter and matter, between neutrinos and antineutrinos. That may be an explanation for why it is in the early universe all of the antimatter decayed away leaving us with a matter dominated universe. Both of those questions, that and the absolute mass of neutrinos, can be answered by studying particular elements, in our case tellurium, that can undergo a process in which two neutrinos potentially could be emitted, it’s called double beta decay. By studying that very rare radioactivity in a reconstituted Sudbury Neutrino Observatory, with a so-called liquid scintillator in the middle with tellurium in, it we can address this question.  In other areas of our laboratory we are addressing another very big question which is what are the dark matter particles that in many ways behave similarly to neutrinos. They are weakly interacting, they pass through material very easily, but we know they have to be much more massive than the neutrinos that we measured from the sun or the atmosphere and those particles we think go well beyond our current knowledge of particles, they are a realm that we’d like to explore by observing the ones that are left over from the original Big Bang, because we feel that perhaps there was enough energy then to produce them and there is good evidence for them in our galaxy. People at CERN are attempting to produce them for the first time since the Big Bang so there is a lot of co-operative effort there. But we feel with about four or five different approaches to dark matter detection in our laboratory we have an excellent chance to perhaps observe them for the first time. |
| Q2 | **Where do you do your best thinking?** |
|  | Arthur B. McDonald: These days if I have a question that I really need to address in detail what I do is to sit with my computer which through google gives me access basically to the information that currently exists in the world. I used to do this in a library. In other words, I need to know what is already known on a topic that I am attempting to consider. I then write what I’m attempting to figure out in a very organised way. When I come to a given point in the problem I then look at what is already known about it and learn as much I can, attempt to work my way through the physics of it using my own knowledge of physics and my hands-on background in terms of how one does experiments. And in that way I simply organise myself over a period of hours, typically sitting by myself in a room in front of my computer attempting to come to an answer for the particular thing that I’m attempting to address. On the other hand, there are instances in which I wake up at three o’clock in the morning and all of a sudden I have the answer to something that I have been trying to figure out. That’s happened to me a number of times and I try to write it down and then get back to sleep. Sometimes I can’t get back to sleep and I end up sitting up and writing what has suddenly occurred to me. But I do find that every now and again there is an inspiration that comes in the middle of the night. |
| Q2 | **Have you had a eureka moment?** |
|  | Arthur B. McDonald: We are very fortunate in our experiment to have a very specific eureka moment because, and we have had several of them actually in our experiment, because we had several phases in which we approached the measurements that we were doing with different techniques. But in each case we worked out a way such that we were blinding, as we say, the people that are analysing the data. In other words, we didn’t want people to start with a preconceived notion of what the answer is for neutrinos where you are trying to make sure that you are seeing neutrinos in this very complex detector and not something else that is similar to them, but really is a radioactive background or something like that. So we introduced, we did several things, we only analysed part of the data in order to define all the parameters in which we were going to approach our final analysis. Or in some cases we actually added in extra data that looked like what we were looking for, but only one person in the collaboration knew how much of that had been added in. So that nobody really knew what the answer was until on one day all of a sudden, we removed the blindness.  Then everyone in the collaboration all at once knew and this happened particularly in 2002 for our major paper. Everyone all at once knew that we had an answer and not only was that an answer that said that neutrinos had changed from one type to another, but it had what we referred to as a five standard deviation uncertainty in it, meaning that there is less than a chance in ten million that this is not what you see as the result that neutrinos do change from one type to another and so yes, there was a collaboration-wide eureka moment. At any given point, I think on that particular paper there were 176 authors and so the majority of them had the opportunity to say eureka all at the same time. In some cases, students who had signed up for their PhD thesis on this and when you get a eureka moment like that as a part of your graduate education, it can be great fun. |
| Q1 | **What advice would you give yourself at 20 years old?** |
|  | Arthur B. McDonald: I guess there is partly a question whether I am meeting myself as a twenty-year-old student when I was actually twenty years old or whether I was presuming that today I am twenty years old and in the present circumstances I will give some advice. I will take the second of them and the advice I would give to that person is both general and it is specific. The specific will be to pursue the field in which I have worked because there’s still many things still to be learned. I will speak about that in a moment. The general would be train yourself at age twenty as broadly as you can, train yourself in the basic science that you are attempting to pursue. Read all those books, understand all those equations, work out all those examples and the problems that your professor gives you. That basic information is going to be essential for you to have overall the ability to be an accomplished researcher in this field. But in my case for experimental physics, learn how to be hands-on, learn how to work a lathe to build a piece of equipment, get into the machine shop and actually physically construct things, because then you will learn what is possible and what is not possible, everything from the most microscopic to the largest piece of equipment and this goes in to computers and electronics and so on.  Train yourself technically in order that you can be confident in all of these things. Don’t neglect the interpersonal elements of collaboration in science. Both in terms of the person next door and in terms of international collaborators. You have to develop a certain amount of what these days has come to be known as emotional intelligence as well as analytic intelligence in order for you to be able to gain the most benefit from an extremely skilled set of people out there in the world. If you can choose very good collaborators in your work, you are going to end up, all of you, in the collaboration able to accomplish very substantial things and so make sure you pay attention to how to get along with other people, how to understand what their motivation is for what they are doing and how to assess their skills such that you can have a complementary group of people accomplishing what you are trying to accomplish.  And I would say to you to consider the field in which I have been working, it is called particle astrophysics. It is a way of understanding particles that in many cases come from astronomical sources. It is a way in which we attempt to understand the most basic laws of physics. The ones that were defining things back in the very beginnings of the universe when the environment was very different than it is today. There are many very important questions still to be answered, we don’t know what dark matter is, we don’t know what dark energy is. We know it is there, it influences how our universe has evolved. Dark matter is accessible in a number of different ways, very important experiments going on to address it either by observing it directly or inferring its presence in both accelerator experiments and also astronomical measurements. It is a very big question because there’s five times as many dark matter particles as there are the particles that make us up in ordinary matter. And so a very fundamental question that’s I think addressable in a not too distant future so I would certainly recommend that type of study to people today. We have a universe that is becoming understood much more in the last twenty years or so than it ever has before. I would recommend that they become a part of that exercise.  Would I like to have obtained the advice I just stated when I was twenty years old? I think actually I did get that advice. I had the benefit of excellent teachers who approached things in just the way that I described. When I talk about learning how a machine works in the machine shop, actually all of us as graduate students at Caltech had to take a course in the machine shop from a truly excellent machinist. With respect to getting along with other people, I learned that from my parents when I was very young and also from my professors. In terms of studying hard and trying to understand things broadly that’s certainly also advice that I got at that time. So yes, I accepted all of that and I think I would accept it again |
| Q7 | **What does intelligence mean to you?** |
|  | Arthur B. McDonald: Intelligence, I think is the ability to size up a situation and attempt to understand what the essence of the thing you are dealing with is. Which in some instance requires you to be very analytical, requires you to go in to the scientific background that has been developed and in some cases be very creative in order to make that jump to the next level of synthesising all of the information that you have obtained both from past experience of yourself and others and also the information that you see in front of you. That analytic intelligence is something that the scientist can benefit from greatly.  I think there is another kind of intelligence as well which is tended to be called emotional intelligence these days, which is a way of assessing a situation that involves interpersonal relationships as well. And that is something if you are able to understand what’s happening in terms of other people’s approach to a topic, if you understand other people’s motivation, understand where they are coming from on a given situation and able to understand therefore how you and they can work together to obtain a solution to this particular problem that you are confronting or even simply to end up becoming friends and proceeding. That’s something I think is type of intelligence that the people who mastered well have quite a happy life, so I think there is a lot of different kinds of intelligence. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0029** |
| **Biographical** | **CURRICULUM VITAE**  **Education**  Mar. 52: B. Sc., Kyoto University.  Mar. 64: Dr. Eng. (Electronics), Nagoya University.  **Employment, Academic Appointments**  Apr. 52–Mar. 59: Research staff, Kobe Kogyo Corporation (now Fujitsu Ltd.).  Apr. 59–Mar. 64: Research Associate, Assistant Professor and Associate Professor, Department of Electronics, Nagoya University.  Apr. 64–Apr. 74: Head, Fundamental Research Laboratory-4, Matsushita Research Institute Tokyo, Inc.  May 74–Jul. 81: General Manager, Semiconductor Department at the same institute as above.  Aug. 81–Mar. 92: Professor, Department of Electronics, Nagoya University.  Mar. 87–Sep. 90: Project Leader, “JST’s1 Research and Development of GaN-Based Blue-Light-Emitting Diode.”  Apr. 92–present: Professor Emeritus, Nagoya University. Professor, Graduate School of Science and Technology, Meijo University.  Mar. 93–Sep. 99: Project Leader, JST’s “Research and Development of Short-Wavelength GaN-Based Semiconductor Laser Diode.”  Apr. 95–Mar. 96: Visiting Professor, Research Center for Interface Quantum Electronics, Hokkaido University.  Jul. 96–Mar. 01: Project Leader, JSPS’s2 “Research for the Future Program.”  Jul. 96–Mar. 04: Project Leader, High-Tech Research Center of Meijo University sponsored by MEXT.3  Nov. 98–present: Member of the Finnish Institute in Japan.  Apr. 01–present: Research Fellow at Nagoya University Akasaki Research Center.  Apr. 02–Mar. 04: Councilor of JST.  Apr. 03–Mar. 06: Chairman of R&D Strategic Committee on the Wireless Devices based on Nitride Semiconductors at the METI.4  Dec. 04–present: University Professor, Nagoya University.  Apr. 10–present: University Professor, Meijo University.  Apr. 11–present: Director of Research Center for Nitride Semiconductor Core Technologies, Meijo University.  **Honors and Prizes**  May. 91: The Chu-nichi Culture Prize from the Chu-nichi Press.  Aug. 95: The Heinrich Welker Medal from the International Symposium on Compound Semiconductors.  Nov. 96: The IEEE/LEOS Engineering Achievement Award from the IEEE/LEOS.5  Nov. 97: The Medal with Purple Ribbon from the Japanese Government.  Jul. 98: The Inoue Harushige Award jointly with Toyoda Gosei Co.Ltd. from JST.  Jul. 98: The Laudise Prize from the International Organization for Crystal Growth.  Nov. 98: The C&C Prize from the Foundation for C&C Promotion.  Dec. 98: The Jack A. Morton Award from the IEEE.  Dec. 98: The Rank Prize from the Rank Prize Funds.  Jan. 99: IEEE Fellow.  May 99: The Solid State Science &Technology Award from the Electrochemical Society.  Jul. 99: Honorary Citizen of Montpellier, France.  Nov. 99: *Honoris Causa*Doctorate from the University of Montpellier II, France.  Mar. 00: The Toray Science & Technology Prize from the Toray Science Foundation.  Jan. 01: The Asahi Prize from the Asahi Press.  May 01: *Honoris Causa*Doctorate from Linköping University, Sweden.  Mar. 02: The Outstanding Achievement Award from the Japan Society of Applied Physics.  Jun. 02: The Fujihara Prize from the Fujihara Foundation of Science.  Nov. 02: The Order of the Rising Sun, Gold Rays with Neck Ribbon from the Japanese Government.  Nov. 02: The Takeda Award from the Takeda Foundation.  Sept. 03: The SSDM Award from the International Conference on Solid State Devices & Materials.  Nov. 04: Person of Cultural Merit from the Japanese Government.  Mar. 06: The John Bardeen Award from the Minerals, Metals & Materials Society (TMS).  Oct. 08: Foreign Associate of the United States National Academy of Engineering (NAE).  Nov. 09: The Kyoto Prize from the Inamori Foundation.  Aug. 11: The Edison Medal from the IEEE.  Sept. 11: The Special Award for Intellectual Property Activities from JST.  Nov. 11: The Order of Culture conferred by the Emperor of Japan in person.  Dec. 12: Honorary Citizen of Minami-Kyushu.  Jan. 13: Life Fellow of the IEEE.  May 13: The Karl Ferdinand Braun Prize from the Society for Information Display.  May 14: The Okawa Publications Prize from the Okawa Foundation.  Jul. 14: The Imperial Prize and the Japan Academy Prize from the Japan Academy.  Dec. 14: The Nobel Prize in Physics.  Dec. 14: Member, The Japan Academy.  Feb. 15: The Charles Stark Draper Prize for Engineering from the NAE. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0029 = IA**  [Isamu Akasaki] Hello, this is Akasaki speaking.  [Adam Smith] Hello, this is Adam Smith calling. My congratulations on the award of the Nobel Prize.  [IA] Thank you very much.  [AS] How did you hear the news? How did you learn?  [IA] I was working at my office at the Meijo University. I was just arranging my papers and checking some documents.  [AS] Yes. And the telephone rang?  [IA] I heard from the Nobel Foundation about 6 pm. I felt rather surprised.  [AS] What was your reaction, your first reaction?  [IA] I was overwhelmed by so many interviewers and then messages of congratulations. So many congratulation messages. I feel this shows the authority and the greatness of the Nobel Prize.  [AS] Indeed, I imagine everybody wants to talk to you now.  [IA] Yes, yes.  [AS] Tell me, one of your co-Laureates, Hiroshi Amano, he was your graduate student?  [IA] Yes, I talked with him.  [AS] What is he saying?  [IA] Congratulations, he tells us. [Laughs]  [AS] How very nice. You were the pioneer of the blue LED.  [IA] Oh, I think so.  [AS] Yes, and how did you have the courage to try to develop it when nobody else could?  [IA] Ah, in the late 1960s, red and the low green LEDs and the infrared semiconductor lasers  had already been developed but there was no prospect of practical blue light emitters, even in the ’70s. So, very many researchers, including the front runners in gallium nitride, abandoned the development of gallium nitride-based devices. I have devoted myself to the study of crystal growth, aiming at the development of the p-n junction of gallium nitride. That is the short story.  [AS] It’s very very lovely to talk to you, thank you. We very much look forward to meeting you when you come to Stockholm in December.  [IA] December? Hi, hi.  [AS] Yes, to receive the Prize.  [IA] Yes, we can meet there.  [AS] And then we will speak further.  [IA] Sorry my poor English.  [AS] Sorry for my poor Japanese.  [IA] No, no. Japanese is a very strange foreign language for European people.  [AS] Yes, but you speak English beautifully so we are fine. It’s a great pleasure to speak to you. Once again congratulations and thank you.  [IA] Bye Bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0030** |
| **Biographical** | I was born in Hamamatsu, Shizuoka Prefecture, Japan, on September 11, 1960, to my father Tatsuji and mother Yoshiko, and I grew up with younger brother Takashi. Hamamatsu is famous as the birthplace of Professor Kenjiro Takayanagi, a Japanese pioneer in the development of television, who succeeded in transmitting the first Katakana character “**イ**” by wireless transfer using a Braun tube in 1926, and also Soichiro Honda, who established Honda Motor Co., Ltd., in 1946. In addition, several engineering and manufacturing companies, such as motorbike companies, musical instrument companies, and optoelectronics companies, are based in Hamamatsu. A possible reason why I considered a career in engineering may have been the influence of growing up in such an industrial city.  I spent elementary school, junior high school, and high school at Hamamatsu. I was a weak child who was often ill. My grandmother Ken always nursed me, during which she often told me of her wretched experiences during World War II, so I learned a lot about the war. At elementary school, I concentrated on sports such as baseball and football. During elementary school and junior high school, I did not enjoy studying because the only reason to study seems to be to pass the entrance examination for high school. At high school, I had the same mathematics teacher for three years who taught me the importance of logical thinking and how to approach difficult problems in mathematics. I found that I could solve difficult problems if I thought logically. Although I became very interested in solving mathematics problems, I still did not have a good reason for studying.  I moved to Nagoya in 1979 to enter Nagoya University as a student of the Department of Electrical Engineering. In the introductory class to engineering, I heard a very profound interpretation of the meaning of the Kanji character “**工**,” which means engineering. The lecturer explained that the meaning of “**工**” is the connection of people with people, which means that the ultimate goal of engineering is to enrich the lives of people. I was astonished with this explanation and felt that my view of study had suddenly opened through recognizing that the meaning of study is to benefit the people. As a result, I became interested in all fields of study offered by my department, particularly computer science. This was soon after Bill Gates and Paul Allen established Microsoft (1975) and Steve Jobs and Stephen Wozniak started Apple Computers (1976). After the establishment of these now giant companies, the development of personal computer (PC) systems proceeded rapidly, and I hoped to contribute to the further development of PC systems.  In 1982, when I was in my third year of university, I had to choose a dissertation research topic. Unfortunately, there were no topics concerning computer science, especially the design of central processing units. But when I found that GaN-based blue LEDs could be researched in Professor Isamu Akasaki’s laboratory, I decided to pursue this topic as my dissertation topic. At that time, Braun tubes were used as the monitors of PCs and also in television systems. Because Braun tubes were so large, I thought that if I could develop blue LEDs, I could change the world by improving people’s lives by providing the means to develop more smart PC and TV systems. At that time, I did not know how difficult it would be to develop blue LEDs.  At that time, funding of our laboratory was very limited. Therefore, the students at Nagoya University had to develop their own system to enable the growth of nitride crystals by metal-organic vapor phase epitaxy (MOVPE). Through the efforts of a master’s degree student followed by myself and Mr Koide, a student in the year above me, we succeeded in developing an MOVPE system. I then used our laboratory-built MOVPE system to try to grow high-quality GaN, and Mr. Koide focused on growing AlN and AlGaN. However, the growth of GaN on a foreign substrate such as sapphire was so difficult that I spent three years in vain trying to grow high-quality GaN.  In February 1985, almost at the end of my master’s course, I was still carrying out lonely fruitless experiments. When I compared my GaN and AlN grown by Mr Koide, I found that the surface morphology of his AlN was slightly better that that of my GaN, so I decided to deposit AlN just before growing GaN. I noticed that a very high temperature was necessary to grow AlN. However, the heating system of our MOVPE system was so old that it could not reach the required temperature. I then remembered a discussion with Dr. Sawaki, at that time Associate Professor at Akasaki Laboratory. He said that in the growth of boron phosphide (BP) on Si for which the lattice mismatch is 16%, almost the same as that of GaN on sapphire, the predeposition of P was effective for growing BP with a flat surface. He also mentioned that a P cluster should act as a nucleation center causing BP to grow laterally in the initial stage. I decided to deposit a very small amount of AlN at a low temperature, which I anticipated would act as a nucleation center. When I took the resulting sample out from the MOVPE reactor, I first thought that I had forgotten to supply the Ga source. But when I checked the surface morphology using an optical microscope, I finally recognized that I had succeeded in growing GaN with an atomically flat surface. It took almost one year for this result to be published in Applied Physics Letters because I had to check not only the surface morphology but also the crystalline quality, electrical properties and optical properties. I found that all the properties of the GaN film were far superior to those reported previously. This process is called low-temperature deposited buffer layer technology for growing GaN on a sapphire substrate by MOVPE.  Our next challenge for us was to realize p-type GaN. I unsuccessfully tried to grow p-type GaN using Zn as an acceptor dopant from 1985 to 1988. At NTT, where I spent the internship of my PhD, I found that blue luminescence increased irreversibly when Zn-doped GaN was irradiated with electrons. I called this low-energy electron beam irradiation (LEEBI) treatment. However, even after LEEBI treatment, the Zn-doped samples did not show p-type conduction. In 1989, I became a Research Associate of Akasaki laboratory before completing my PhD. When I read the book “Bonds and Bands in Semiconductors” written by J. C. Phillips, I found that Mg is a better acceptor impurity than Zn in GaP. Then, myself and Mr. Kito, a master’s student, started to investigate the Mg doping of GaN. We found that grown Mg-doped GaN showed high resistivity, but after LEEBI treatment, it showed distinct p-type conduction. We also succeeded in fabricating the world’s first pn-junction-type UV/blue LED. In 1991 Nichia Chemicals team led by Dr. Nakamura found that p-type GaN could be grown by simple thermal annealing, which became the de fact standard method for the growth of p-type GaN.  In 1992, I moved to the School of Science and Technology, Meijo University, to work with Professor Akasaki, where I became an Assistant Professor. In 1998 and 2002, I became an Associate Professor and a Professor, respectively. In 2010, I moved to the Engineering Department, Nagoya University, where I continued to work as a Professor. I have been Director of Akasaki Research Center, Nagoya University, since 2011.  I was awarded several honors such as the IEEE/LEOS Engineering Achievement Award in 1996; the Rank Prize, Rank Prize Foundation, UK, in 1998; the Marubun Academic Award, Marubun Research Promotion Foundation, Japan, in 2002; the Takeda  Award,  Takeda  Foundation, Japan, in 2002; the   Japanese  Association for Crystal Growth JACG Award, Japan, in 2008; the NISTEP Award, National Institute of Science and Technology Policy, Japan, in 2009; the Order of Culture from the Japanese Emperor in 2014; and the Nobel Prize in Physics, Nobel Foundation, Sweden, in 2014.  I have been a fellow of the Japan Society of Applied Physics (JSAP) since 2009 and a fellow of the Institute of Physics (IOP), UK, since 2011. I am also a member of several academic organizations.  Selected activities include Sub Chair of the Program Committee of the International Symposium on Compound Semiconductors in 2007, Program Committee Chair of the Second International Symposium on the Growth of Nitride semiconductors in 2010, Program Committee Chair of the Third International Symposium on Growth of Nitride Semiconductors in 2012, and Organizing Committee Chair of the International Workshop on Nitride Semiconductors in 2012. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0030 = HA**  [Hiroshi Amano] Hiroshi Amano speaking.  [Adam Smith] Hello. My name is Adam Smith from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  [HA] Oh, thank you for calling.  [AS] First of all, of course congratulations on the award of the Nobel Prize.  [HA] Thank you.  [AS] So, it must have been the most extraordinary week you’d been having?  [HA] Yes, I was so surprised. Yeah, it’s quite extraordinary for me.  [AS] Have you managed to find even a moment to yourself or have you been in the public eye the whole time?  [HA] Yeah, yeah, my university staff manage my schedule so I cannot manage by myself! [Laughs]  [AS] And you were in France when you heard the news, were you?  [HA] Mmm.  [AS] Were you travelling when the news broke? Or did you get a call? Or what happened?  [HA] Yeah, yeah. I saw in the PC. My PC, during the transit at Frankfurt airport, and I found many many email, and I still did not understand what happened. And when I arrived at Reims airport then I found I received as one of the winner. So I was so surprised. [Laughs]  [HA] It must have been a very confusing flight from Frankfurt while you wondered what was going on?  [HA] [Laughs] Yeah, yeah. It’s one of the most amazing travels for me.  [AS] And now that the news has had a few days to sink in, how do you feel about it?  [HA] Yeah, yeah, more than happy. But now I’m wondering how to act. Because many people are watching me so I should act very fortunate, responsible or something.  [AS] Suddenly you have to be very grown up, perhaps.  [HA] [Laughs] Yeah, yeah, sure.  [AS] The invention that the Prize has been awarded for is everywhere. Everywhere you look you see the blue LED. How does that make you feel to have been part of something that is now everywhere?  [HA] Yeah, yeah. For me it’s my great honour that many people use blue LEDs or LED lightings now. So, we can contribute to the energy savings for the humans, so I’m very very happy to contribute to the energy saving issues.  [AS] And it must be a great joy to be awarded together with your supervisor, Isamu Akasaki?  [HA] Yes.  [AS] What did he teach you?  [HA] So, he teached me that the nitride, gallium nitride, was very, is very promising material for the blue LED. So I could concentrate on the gallium nitride and not other materials like zinc selenide and silicon carbide.  [AS] It’s now the evening coming up in Japan. What do you have scheduled for the evening? Do you have a free evening for once? [Laughs]  [HA] Yes, I’m free.  [AS] That’s nice. So when did you come back to Japan?  [HA] October 10th, the day Friday morning.  [AS] Ah, there must have been an amazing welcome for you.  [HA] Yes, yes, there are many many interviewers at the airport so I was so so surprised. More than 80 peoples gather and say congratulations. [Laughs]  [AS] Anyway, I’m very happy to add my voice to those congratulations. So once again, thank you very much for speaking to us now.  [HA] Thank you so much.  [AS] Thank you very much indeed. Bye bye.  [HA] Bye bye. |
| **Interview** |  |
| Q9 | **What were you doing when you heard you had been awarded the Nobel Prize?** |
|  | When there were the announcements, I was on a plane from Japan to Frankfurt, that transit. When I took out from the plane, I saw many e-mails entitled “Congratulations, congratulations”, but I did not understand the reasons of all congratulations because I didn’t have time to open up the e-mails. So, I closed the PC and went to the final destination from Frankfurt to Rio. At the exit of the arrival gate, many Japanese journalists were there. They said: “Congratulations Amano-san, you have got the Nobel Prize.” That is the time when I knew I have received the Nobel Prize. |
| Q4 | **Could you please explain your Nobel Prize awarded work in simple terms?** |
|  | Maybe you know this place of portable games and there are also cellular phones or smartphones? In 1980s it was monochrome but now all the games and the smartphones are full colors. So in order to realise the full colors, blue light-emitting diodes it is necessary so we are achieving the blue light-emitting diodes. That is what we have done. |
| Q2 | **At what point did you realize your work was a breakthrough?** |
|  | One point is to realise bright blue LEDs, we need very high-quality crystals, that is gallium nitride. One thing is to realise very high-quality gallium nitride and the other one is to publicate LEDs, we need both N-type and B-type conductivity and for the N-type it is not so difficult, but B-type it was very, very difficult. So, we achieved the B-type conduction. Back in 1985, when I saw the samples, I thought that I forgot or missed something because the crystals were too perfect or too good, but when I checked by the microscope, I found that we have done it. That was a very amazing moment for me. |
| Q3 | **What brought you to science?** |
|  | The thing that brought me to the science it is, when I was a child I couldn’t understand why I should study, but when I entered the university some professor told us that the reason why we study is to contribute to the mankind or contribute to the human society or human life. So, I understood the reason why we should study and for me the science is the most nearest field which I can contribute to the people. That is why I was interested in the science. |
| Q5 | **Who is your role model, and why?** |
|  | Role model is of course professor Isamu Akasaki, he was my excellent supervisor. He was very much interested in the crystals and he have studied the semiconductor crystals for a long, long time; germanium, gallium arsenide, gallium phosphide and also the gallium nitride and he insisted in growing the high-quality crystals. So, he is my role model. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0031** |
| **Biographical** | Shuji Nakamura was born on May 22, 1954 in Oku, a tiny fishing village on the Pacific coast of Shikoku, the smallest of Japan’s four main islands. Farming is the principle occupation in Oku. Local farmers grow yams on steps cut into steep hillsides. Shuji’s maternal grandparents owned such a farm. To get to the nearest town, the villagers relied on a ferry. Inconvenient perhaps, but the village was an idyllic place to grow up.  Shuji’s father, Tomokichi, worked as a maintenance man for Shikoku Electric Power. From him, Shuji learned how to make wooden toys, like catapults and bamboo propellers. He liked making things and became good at it, a skill that would stand him in good stead.  At school Shuji was not academically gifted. His boyhood was typical. He fought constantly with his elder brother. Smaller than his sibling, Shuji always lost. Though physically defeated, mentally he would never give in. His mother was forever chiding her boys to do their homework. But for the most part, they ignored her admonishments.  Throughout primary and high school, Shuji’s passion was volleyball. There was no gym at the school, so his team had to practice outside in the mud. They tried hard, but rarely won. Fiercely competitive from an early age, Shuji always hated to lose. Volleyball left Shuji little time to study for his high school entrance exams. He was bad at rote learning, but good at math and science. Somehow, he managed to scrape into an academically-oriented school.  Here, too, volleyball remained his priority. His classroom teacher told him that to improve his scores, he would have to quit playing. It was time to concentrate on studying for the all-important university entrance exams. But he could not let down his team. Shuji was the only student in the A-stream to continue playing sports until graduation.  Shuji paid a price for his dedication to volleyball. His university entrance exam results were not good enough to win him a place at a prestigious school. Shuji’s dream was to become a theoretical physicist or a mathematician. But his teacher told him that he could not make a living from physics – he had better choose a course like engineering so that he could find a job.  Shuji picked electrical engineering because it seemed close to physics. In 1973, aged 19, he entered Tokushima University, a local state school. Many professors there were former high-school teachers. The textbooks were out of date.  His first two years consisted of general studies, including arts courses, which Shuji hated. He couldn’t understand why he had to take such irrelevant subjects. Soon he stopped attending classes. All day long he would read books, mostly on physics. But there was a limit to the amount he could absorb by reading alone. Finally, in his third year at Tokushima, Shuji attended a lecture on semiconductors. Fascinated by the physics of solid-state materials, he decided to stay on at university for a further two years and do a master’s degree under Professor Osamu Tada.  As his thesis topic, Shuji chose the conductivity mechanism of barium titanium oxide. His focus was theoretical. But Professor Tada was a dyed-in-the-wool experimentalist. He would catch his student reading papers and tell him that knowing theory was no use if he couldn’t make actual devices.  Tada’s lab was known as “the junk room.” It was crammed with broken televisions and old radios which could be cannibalized for spare parts. To build what they needed, students had to acquire manual skills – soldering, cutting and joining glass, beating and welding sheet metal, fashioning parts on a lathe.  Shuji remembered his days as a master’s student as like being a sheet-metal worker in a small factory. What he wanted was to study theory. But most of his time was taken up jury-rigging equipment for experiments. In fact, Shuji was gaining precisely the kind of skills that he would later need in his quest to develop a bright blue LED. As a corporate researcher, he would be forced to make or modify much of his own equipment. Ultimately it would largely be this technical mastery that would give him the edge on his rivals.  As a 25-year-old graduate student with a master’s degree in electrical engineering, Shuji expected one of Japan’s consumer appliance manufacturers would hire him. But the likes of Sony tend not to recruit graduates from local universities.  In his interview at Matsushita, Shuji made the mistake of discussing the theoretical aspects of his thesis research. We don’t need theoreticians, the firm’s recruiters told him. At Kyocera, he did better, emphasizing the practical applications of his work. The company offered him a job. But as the day on which Shuji was due to report for work in Kyoto neared, he had second thoughts. Prior to his job-hunting trips, he had rarely left Shikoku.  At the same time, he really wanted to work in a proper research laboratory at a major company. Torn between two paths, Shuji asked Professor Tada what he should do. Tada pointed out that in Tokushima, there were no jobs for electrical engineers. If Shuji elected to stay on the island, he would have to give up a career in his chosen field. Eventually Shuji decided to remain in Tokushima.  His professor introduced Shuji to Nobuo Ogawa, the founder-president of an obscure local chemical firm called Nichia. The company was initially reluctant to hire him, but Shuji refused to take no for an answer.  When Shuji joined in April 1979, Nichia had fewer than 200 employees. The firm made phosphors for color televisions and fluorescent lamps. These were mature markets. If Nichia was to grow, it needed new products.  Shuji was assigned to the company’s two-man development section. His first job was to refine high-purity gallium metal. This turned out to be a dead end. The company ordered him to produce gallium phosphide, a material used to make red and green LEDs. Knowing next to nothing about LED materials, Shuji had to start from scratch.  There was no budget for equipment. He had to scavenge, fixing broken parts by hand. To build his reactor, Shuji scrounged heat-proof bricks, cables, a vacuum pump, and an old electric furnace. He had to order quartz tubes. To seal the open-ended tubes so they could be evacuated, he had to learn how to weld quartz.  To make gallium phosphide you heat phosphorus in a tube. If the tube gets too hot, the phosphorus vapor expands, causing the quartz to crack. This lets in oxygen, which reacts with the phosphorus causing an explosion. Such explosions became a feature of Shuji’s time at Nichia. His lab would fill with white smoke. Ignited phosphorus would fly everywhere, along with shards of broken quartz. Shuji would run around pouring water over the burning phosphorus, desperately trying to douse the flames.  The blasts happened several times a month, often in the evening. The shock wave would hit his fellow-workers as they were heading for their cars in the parking lot. The first few times it happened, they dashed into his lab to see if he was alright. By the fifth or sixth time, however, they had become so used to the bangs they no longer came to check.  Eventually, Shuji succeeded in developing commercial-grade gallium phosphide. More satisfying than producing the material was how he felt when the company’s salesmen told him they had made a sale. It gave him pleasure to think that he had finally managed to contribute to the company’s bottom line. But only a little: the market for gallium phosphide was already crowded. A late entrant, Nichia was only able to win a sliver of the pie.  Shuji’s next assignment was to produce gallium arsenide, which is also used to make LEDs, typically infrared ones such as those found in television remote controls. But GaAs also has other applications, like the semiconductor lasers used in optical fiber communications. Thus, the potential market for the material was much larger.  Happily, unlike phosphorus, arsenic is not inflammable. Unhappily, the material is poisonous, releasing lethal arsenic oxide gas every time the furnace blew up. Shuji had to wear a home-made “space-suit” and breathe through a respirator. Miraculously he was never adversely affected by having to work in such a toxic environment.  By 1985, Shuji was producing gallium arsenide in bulk. But when it came to selling the product, the market’s response was the same. There were plenty of existing suppliers, so why buy from an untried latecomer like Nichia? The next idea the salesmen brought back was, instead of making the starting materials for LEDs, why not make the devices themselves? To fabricate a simple LED required mastering a technique known as liquid phase epitaxy.  Shuji performed countless experiments. Small differences in thickness, he discovered, could make a big difference in brightness and lifetime. As usual, the company pressured him to produce a saleable product quickly; as usual, there was no budget for equipment. Eventually he managed to fabricate some prototype LEDs. Samples were delivered to a client for evaluation. Not having his own measuring equipment meant Shuji was dependent on such external evaluations. He had to wait months to get data back before he could start making improvements.  Shuji felt strongly that if the company was going to enter the LED business, then he should be able to conduct his own evaluations. He presented the case to his boss, but was told no budget, so not possible. Previously, Shuji would have accepted this answer and given up. By now, he had realized that Nichia was run on the say-so of its president and founder, Nobuo Ogawa. He went directly to Ogawa to ask for the equipment he needed. To his surprise, the old man immediately agreed to his request.  Shuji had made many friends among Nichia’s employees. When work finished they would often ask him to make up the numbers for a game of softball. Afterwards, they would drop by a local bar. There, his workmates would implore him to develop products that would make the company grow. Knowing that he had yet to produce anything that had a significant impact on the company’s bottom line, Shuji would hang his head. Others, especially older employees, were critical. They asked him what he had been doing for the past five years. In their opinion, he was just wasting the company’s money.  The only way a corporate researcher can contribute directly to the bottom line is through patent royalties. But fearful of losing trade secrets, Nichia did not permit patent applications. Thus Shuji’s apparent sales were zero. In ten years, Shuji had not published a single scientific paper, because of Nichia’s policy of keeping its technical know-how secret. From a professional point of view, he had no achievements.  Finally, in desperation, he approached the president with an audacious proposal: to develop the world’s first bright blue LED. To do this he would need around five hundred million yen (then worth about US $4 million). This was equivalent to two percent of the company’s sales that year, an unbelievably large amount. Nonetheless, Ogawa gave Shuji his blessing.  Two thirds of the money would go to equipment, together with the laboratory and clean-room facilities to house it. Of the remaining third, the largest item was mastering metal-organic chemical vapor deposition, the crystal growth technology needed to make bright blue LEDs.  Shuji selected MOCVD because it could be applied to the factory floor. Shiro Sakai, an expert on the technique, was an old acquaintance from Tokushima University. Now a professor at Tokushima, Sakai was on sabbatical at the University of Florida. Shuji invited him to visit Nichia. There he outlined the significance of MOCVD. Blue LEDs were not mentioned. Sakai recommended that Nichia should send Shuji to Florida for a year to learn the technique.  In March 1988 Shuji flew to Gainesville. It was the first time the country boy had boarded an airplane. Like many first-time fliers, he feared it might fall from the sky. It was also his first trip abroad. He worried that his rudimentary English would not enable him to communicate with Americans.  Shuji was 34 years old, rather long in the tooth for a student. His fellow researchers at the University of Florida were mostly in their mid-twenties. All of them were PhD students. Shuji’s status was ambiguous. Since he was not studying for a degree, he was obviously not a student. Nor, since he did not have a PhD, could he be a post-doctoral fellow. As a compromise, he was designated a “guest research associate.”  Initially, his fellows treated Shuji as an equal or even, because he was older, as a senior. However, once they discovered that he only had a master’s degree and, worse, that he had not published a single paper, their attitude changed. Henceforth they looked down on Shuji, treating him as little more than a technician. It was particularly galling because, from his perspective, these PhDs were mere novices whereas he had years of hands-on experience. They could not do the simplest experiment. Something would go wrong and they would come running to him for help. Their condescending attitude provided Shuji with further motivation. “I feel resentful when people look down on me,” he wrote. “At that time, I developed more fighting spirit – I would not allow myself to be beaten by such people.”  When Shuji arrived at Gainsville, the MOCVD system at the lab to which he had been assigned had not yet been built. He had to spend ten months of his precious year in the US with his sleeves rolled up, connecting pipes and welding quartz, just like back at Nichia. Here again, adversity in the short term would turn out to be priceless experience in his quest to develop the first bright blue LEDs. He gained an intimate familiarity with the workings of MOCVD equipment. Having managed to assemble the system, Shuji was only able to do a few device-growing runs. Then it was time to go home.  Shuji returned to Nichia in March 1989. While in the US he had ordered his own MOCVD equipment, keeping his goal a secret from the supplier. The reactor had arrived. The question was, what material to grow in it? There were three candidates. One, silicon carbide, despite the fact that it was in limited commercial production, he had already rejected. Silicon carbide had an indirect band-gap, meaning that the material would never be able to emit bright blue.  The other two materials, zinc selenide and gallium nitride, both suffered from the same deficiencies. One was that, to make a proper LED, you need to fabricate both negative- and positive-type material. Thus far, however, it had proved impossible to produce either p-type zinc selenide or p-type gallium nitride. When making his choice Shuji could not have known it, but this was about to change: in 1989, researchers would succeed in fabricating p-type gallium nitride; the following year would see the first p-type zinc selenide.  The second, more serious, drawback was the lack of a suitable base material on which to fabricate an LED. Gallium arsenide LEDs could be grown on gallium arsenide wafers. But nobody had been able to grow bulk zinc selenide or gallium nitride. That meant employing wafers of some “foreign” material as the substrate, which in turn mean a mismatch between substrate and light emitting layers. The result was defects, which are undesirable because they cause LEDs to dissipate energy in the form of heat instead of light.  With zinc selenide, a soft material, the problem seemed much less severe. You could grow ZnSe on a gallium arsenide substrate and the mismatch was only 0.3 percent, not far off the ideal value of 0.01 percent. This translated into a defect density of around one thousand per square centimeter. With gallium nitride, a rock-hard material, the best available substrate was sapphire. But even sapphire produced a huge mismatch, of sixteen percent. That translated into in a defect density of ten billion per square centimeter. It was plausible to imagine that imperfections in crystal ZnSe could be reduced by an order of magnitude. But *ten billion*defects? It seemed unlikely that that figure was going to be significantly reduced during any researcher’s working lifetime.  Gallium nitride had been thoroughly investigated by RCA, Bell Labs, and Matsushita. It was almost universally perceived to be a dead end. Few groups were still active in the GaN field. The overwhelming consensus was that zinc selenide was the way to go. Yet zinc selenide devices tended to fall apart when zapped with current. ZnSe simply wasn’t strong enough to cope with the stress of giving birth to photons. What nobody could have foreseen in 1989 was that gallium nitride would turn out to behave very differently than previous light emitting materials. Any other semiconductor with that density of defects simply would not function. Much to everyone’s surprise, however, with gallium nitride, defects just didn’t seem to matter.  Having arrived at what he described as this “fateful fork in the road,” Shuji chose gallium nitride. His reason for placing this apparently reckless bet was not because he was confident that he could do what no-one else had done. Rather, it was because he had repeatedly had the bitter experience of developing products only to find that his company could not sell them. If he chose zinc selenide, since big companies had several years’ head start, history would likely repeat itself. With gallium nitride, in the unlikely event that he did succeed, there would be no competition, because no other companies were working on GaN.  Another motivation, following his unhappy experience at the University of Florida, was that Shuji wanted to get a PhD. In Japan it was possible to obtain a PhD by publishing a minimum of five scientific papers. If he had selected zinc selenide, it would have been difficult to publish papers because a huge number of papers on ZnSe had already been published. If he selected gallium nitride, it would be easier to publish because only a few papers had been published.  Shuji was able to make this seemingly foolhardy decision by himself without reference to Nichia’s senior management because none of them knew anything about semiconductors. All they knew was that his goal was to develop a bright blue LED. The choice of methodology to adopt and material to work on was his alone. Had he been working at a large company, his proposal to work on a known-loser material would undoubtedly have been shot down. But as he himself would later say, “breakthroughs are born out of unusual circumstances.”  The quest began. Shuji had no colleagues with whom he could discuss his work. Other than New Year’s Day, he never took time off. His solitary routine seldom varied. He would get into work around 7AM, leaving around 7PM. He would go home, eat dinner with his family, have a bath, then go to bed. All the while he would be musing about his work.  Modifying the MOCVD equipment was the key to his success. Shuji took the reactor apart, then put it back together exactly how he wanted. He bent the steel pipe, changing the height and the angle at which it was attached to the reaction chamber. He welded quartz tube, cut high-purity carbon, re-did the wiring. He even altered the shape of the gas nozzles.  Shuji’s motto was “remodel in the morning, experiment in the afternoon.” Such urgency was not because he was worried that other researchers might overtake him. Rather, impatient by nature, he was eager to see the results of the changes he had made.  To grow high-quality films of gallium nitride, one major problem had to be solved. Nitrides are vulnerable to parasitic reactions. The gases react with each other spontaneously to form an adduct – in this case, a white powder that researchers call “snow.” Flakes of snow fall on the wafer, ruining the film. Much ingenuity therefore goes into designing reactors so that the gases are injected separately, keeping them apart as they flow down to the wafer. Shuji conceived a novel way of doing this, which he dubbed “two-flow” MOCVD.  Still, failure followed failure. Nichia kept demanding to know when he could develop a product. But as time went by and no results emerged, his boss stopped bothering him. Even Shuji’s friends at the company left him alone. Then, one winter’s day, the clouds finally lifted.  Everything was as usual: Shuji arrived at work and grew a thin film of gallium nitride crystal. He hooked up his sample to measure its electron mobility. The figure was surprisingly high: the best result to that point, achieved at Nagoya University by [fellow Nobel Laureates] Professor Isamu Akasaki and his student Hiroshi Amano, was less than half as much. Shuji had succeeded in making the world’s best gallium nitride. “It was the most exciting day of my life,” he recalled.  Further breakthroughs followed. Akasaki and Amano had blazed the trail, with their buffer layer (1986) and positive-type gallium nitride (1989). The buffer layer was necessary to mitigate the effect of the mismatch between the sapphire substrate and the gallium nitride layers deposited on top. Interposing a buffer enabled the growth of smoother films. For their buffer layer Akasaki and Amano had used aluminum nitride. Shuji was determined not to copy his rivals, knowing patent problems would result if he did. He used gallium nitride as the material for his buffer layer. He was able to achieve a smooth, mirror-like  surface that had better electrical characteristics than aluminum nitride.  But to build a blue LED, Shuji needed first to make positive-type gallium nitride. His rivals had produced p-type GaN by irradiating the material with an electron beam. This was a wonderful scientific discovery, but impractical technologically because the method was too slow for LED manufacturing.  Akasaki and Amano announced their discovery at a conference in 1989, just after Shuji got back from Florida. He asked them what the hole concentration of their material was. The answer told him the quality of their material was not high. But at least they had demonstrated it was possible to make p-type GaN. Shuji would thus start his research on GaN just as the hitherto most intractable problem in the field had been shown solvable. It was an incredible stroke of luck. In July 1991, armed with both negativeand positive-type materials, Shuji was able to proceed to the next stage, making a simple LED. The device lit up with a violet-blue light. Though not very bright, it was fifty percent brighter than conventional, silicon carbide LEDs. The outstanding question was longevity. How long would a fragile thin film with ten billion defects per square centimeter continue to emit light? He went home that night, leaving his LED switched on. Next morning, he returned to the lab, his heart thumping, to find that … it was still lit! He measured the output and was elated to discover that it had barely dropped. In fact, on testing, the lifetime turned out longer than 1,000 hours.  Next, he focused on making high-quality p-type gallium nitride. His rivals had not figured out why e-beams caused the transformation. Shuji speculated that it was merely heat that turned the material p-type. In December 1991, he tried annealing magnesium-doped films. The resultant material was p-type. Thermal annealing was simpler and much faster than the e-beams, hence applicable to the production line. It also produced much better quality gallium nitride. E-beam penetration was very shallow, with only a very thin surface layer of the material becoming p-type. Thermal annealing converted the material to p-type all the way through. This was a major breakthrough.  Shuji also clarified the mechanism of hole compensation, which had been a mystery for twenty years. Atomic hydrogen produced from the dissociation of ammonia gas forms Mg-H complexes. This formation prevents magnesium acceptors from behaving as acceptors. Using thermal annealing removes atomic hydrogen from the Mg-H complexes activating the Mg acceptors. The material then becomes p-type gallium nitride.  The world’s first conference on nitrides was held in St Louis in 1992. Shuji gave a talk on his prototype blue LED. He revealed that its lifetime was more than 1,000 hours. The audience reacted by giving him a standing ovation. Encouraged by the response, on his return to Japan, Shuji embarked on the final stage of what he called his “climb to the summit of Mount Fuji.”  To make a bright blue LED, he had to take two further steps. First, to make the light bright, he had to build a more complex device, called a  double heterostructure. Second, in order to make the light pure blue, as opposed to violet-blue, he had to prepare alloys that incorporated indium, whose slightly narrower bandwidth would produce longer-wavelength light.  Thus far, no one had been able to make indium gallium nitride of sufficiently high quality for practical use. The difficulty was that the InGaN layer has to be grown at a much lower temperature than the confining layers of GaN. The bonds between indium and nitrogen are weak. Increase the temperature too quickly and the indium atoms disassociate themselves from their nitrogen neighbors. How to move on to grow the next layer, upping the temperature without destroying the thin layer of InGaN in the process? It was at this final hurdle that Akasaki and Amano fell.  Shuji solved the disassociation problem in two ways. First, by brute force, turning the indium tap on his system all the way open, using ten times as much indium as would turn out to be needed, attempting to get least some of the stuff to stick. Second, by guile, adding an extra “blocking” layer to cap the InGaN layer, preventing the material from disassociating.  In September 1992, he succeeded in fabricating a double heterostructure LED. Its wavelength was still too short to qualify as true blue. By the end of the year, he had adjusted the growth program, increasing the amount of indium and reducing the thickness of the active layer. This time, there was no doubting the result. Thus far, the output of blue LEDs had been given in milli-candelas, or thousandths of a candle. Now, for the first time, Shuji’s device crossed into the candela class. It shone with a dazzling sky-blue light, a hundred times brighter than silicon carbide blue LEDs, bright enough to be clearly seen in broad daylight. Shuji felt like he had reached the top of Mount Fuji.  On November 29, 1993, at a press conference in Tokyo, Nichia announced the world’s first bright blue LED. The initial reaction was incredulity. Once the disbelief subsided, however, orders for Nichia’s LEDs started pouring in.  Shuji continued making breakthroughs. In May 1994, he demonstrated blue and blue-green LEDs capable of emitting two candelas, double the brightness of his original devices. Next year Nichia commercialized bright emerald green light emitters, the first true green LEDs. In September 1995, Shuji announced the first quantum-well-based blue and green LEDs. These featured a brightness of up to ten candelas. Also in 1995, at Shuji’s suggestion, the company developed white LEDs. They worked by placing a yellow phosphor in front of a bright blue LED, converting its light to white. Wavelength conversion opened up huge new markets, in particular general illumination.  Perhaps Shuji’s biggest coup was developing a blue laser diode. Many people thought such devices would be impossible given that GaN crystal was  riddled with micro-cracks. To amplify light, a laser needs a more complicated structure than an LED; it also has to be pumped with more current. The structural defects in the material should have scattered the light, preventing optical amplification. Under high current, the defect-ridden layers should have caused instantaneous catastrophic failure.  In the mid 1990s, blue lasers were seen as more significant than blue LEDs. The reason is that, whereas it was hard to imagine all the applications that would emerge for bright blue LEDs, it was clear what the big application for a blue laser would be: data storage. That was why consumer electronics and disk drive companies were pouring resources into blue laser development. In 1996 Shuji unveiled a prototype violet-blue laser at a conference in Berlin, using the laser as a pointer in his presentation. By the end of the year, he and his group at Nichia announced an improved blue laser that operated for 1,000 hours.  In December 1999, Shuji left Nichia to join the University of California at Santa Barbara (UCSB) as a professor of materials and electrical & computer engineering. Since then he has continued to push the boundaries in solid-state lighting and associated crystal growth methods with his colleagues Professors James Speck, Umesh Mishra and Steven DenBaars.  As the Research Director of the Solid State Lighting & Energy Electronics Center (SSLEEC) and the Cree Chair in Solid State Lighting & Displays, he is overseeing the research enabling the next generation opto-electronic devices. Of the various research topics, he is a strong advocate of developing and using native gallium nitride substrates, which offer significant improvements towards efficient operation at high current. Furthermore, it opens the door to investigating the use of laser based solid-state lighting due to the superior performance of lasers over LEDs at very high current densities, and hence light output. To enable this future, he is heavily invested in pursuing bulk single crystal growth of GaN boules using the ammonothermal method – a method which grows single crystals from a supercritical ammonia solution under extreme conditions (thousands of atmospheres pressure and hundreds of degrees Celsius).  Shuji currently holds more than 200 US patents, over 300 Japanese patents and has published more than 550 papers in his field. Since coming to UCSB, Shuji has become a fellow of the National Academy of Engineering (NAE) and the National Academy of Inventors (NAI) and has won numerous awards. They include the Charles Stark Draper Prize (2015), the Order of Culture Award (2014), the Inventor of the Year Award from the Silicon Valley Intellectual Property Law Association (2012), the Technical and Engineering Emmy Award (2011), the Harvey Prize from Technion, the Israel Institute of Technology (2009), the Japan Science of Applied Physics Outstanding Paper Award (2008), the Prince of Asturias Award for Technical Scientific Research (2008), the Czochralski Award (2007), the Santa Barbara Region Chamber of Commerce Innovator of the Year Award (2007), Finland’s Millennium Technology Prize (2006) and the Global Leader Award, Optical Media Global Industry Awards (2006). |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0031 = SN**  [Shuji Nakamura] Hello, I’m Shuji Nakamura.  [Adam Smith] Oh, Professor Nakamura. Thank you very much for speaking to us and congratulations on the award.  [SN] OK. Thank you, thank you.  [AS] So can you tell me, where were you when you heard the news?  [SN] At my home. I was sleeping.  [AS] You were sleeping. [Laughs] A nice way to be woken up!  [SN] Yeah. [Laughs]  [AS] What was your initial reaction on hearing the news?  [SN] Yeah, I was so surprised because I’m not too sure whether I could win a Nobel Prize, you know, because basically physics, it means that usually people was awarded for the invention of the basic  theory. But in my case, not a basic theory, in my case just making the device, you know. So I’m not sure whether I could win a Nobel Prize or not, but the Nobel Committee called me and “You  got the Nobel Prize”. So, I was so, so happy, and I was so surprised.  [AS] That’s very nice and very humble of you. But your device is everywhere now. One cannot move but for finding LEDs everywhere. So how do you feel now that your invention is ubiquitous?  [SN] Yeah, of course I am very, very happy, because my device is everywhere. It is everywhere to save energy. So I am very happy every time I see my LED and laser diode. So I am very happy.  [AS] Did you ever imagine it would become so important?  [SN] No, no! When I started on my research I never expected I could invent the LED and laser diode. So I never…  I was so lucky  [AS] But people said that it would be very, very hard to make a blue LED. What gave you the courage to continue and make it happen?  [SN] Oh, because basically I like research because research is like to solve the quiz, you know. Always there is a problem and I have to solve the problem. So I like those patterns. It’s almost like research is sort of in a quiz. So always, problems happen and I wanted to solve the problem. And so that’s why I continued for research using those styles. So basically I like research, so that is the reason.  [AS] And what advice do you have for students who are just starting out?  [SN] I think the most important thing is students have to find out is what you like, what I like, you know. And almost it’s like a dream. And then, just work hard for your dream. I think that’s the best way, you know, to a good invention.  [AS] Now it’s the middle of the night there in California where you are.  [SN] Yes!  [AS] Will you get any sleep at all tonight?  [SN] No, basically no, because the Nobel Committee called me at 2 am and since that time all mass media is coming. A lot of Japanese … [unclear]. So, no time for sleep and no time for rest. Yeah, it’s amazing.  [AS] Are you enjoying yourself, yes?  [SN] Yes, I’m enjoying. [Laughs] Yeah. But too many mass media! Yes, I’m enjoying. It’s a totally different experience!  [AS] Good. Well for now let me just wish you all congratulations on your Prize.  [SN] OK, thanks very much.  [AS] And good luck with everything that’s going to happen tonight.  [SN] OK, thank you, thanks a lot.  [AS] Thank you very much for speaking to us.  [SN] Thank you, bye. |
| **Interview** |  |
| Q2 | **What has been the most significant breakthrough in your career?** |
|  | Shuji Nakamura: I think, because in 1993 I could develop the very high efficient blue LEDs that is almost 100 times brighter than all the blue LED. All the blue LEDs in long time ago, already in 1970’s already, some people already developed blue LEDs, but that is not the p-n junction blue LED but that is very dim. My invention is 100 times brighter so this blue LEDs can be used in all kinds of applications – it is bright enough for all kinds of applications. So that is when I realised that my invention is great, that is in 1993. Of course, I achieved this big breaks in 1993 I had already achieved several breakthrough like p-type gallium nitride, indium gallium nitride and all they all maybe were similar breakthroughs but this in 1993 the final was a breakthrough for the high efficient blue LED as a product. |
| Q3 | **What brought you to science?** |
|  | Shuji Nakamura: Math is the most important because during my childhood using math, I expect the using you know, I could understand what was happening in the nature because using the equations I tried to explain what was happening in the nature using one equation. So that is my feeling of the maths. Also, in the same time math is used to explain the nature, all kind of things happening. Most important math plus also people has to show the interest of the nature. To understand the nature, we need maths and also physics – maths and physics. The most important, children have to play outside to understand what is happening in nature and also to sort of understand nature we need some tool like maths – the tool is maths. And also gradually maths become physics and physics also become the tool to explain the nature, so that combination is very important for childhood. Play outside to understand nature, study math and physics. |
| Q5 | **Who is your role model, and why?** |
|  | Shuji Nakamura: Sorry, I don’t have, because I have no role model at all because I was born in a very remote city where I worked at study in the city and my university also located in a very remote city in Japan and no famous professors were there. I joined a small company and no famous people worked there. In my years I had to do everything myself from scratch – everything, so no role model in my case. Many people are asking about my role model, and I say no, nobody. I just had to do everything myself. Most important is that people have to believe in themselves because people have to try new things and greater expectations and greater confidence – that is the most important. Just be yourself. |
| Q9 | **What were you doing when you heard you had been awarded the Nobel Prize?** |
|  | Shuji Nakamura: I was sleeping because I live in California, Santa Barbara. The Nobel Foundation called me just around midnight, or almost 2 a.m. California time so I was sleeping and then suddenly I got the phone call and they said “Congratulations, you have got the Nobel Prize” and I was surprised. So I was sleeping, yeah. |
| Q9 | **Did your life change after the award?** |
|  | Basically no change. Lot of mass media came to me for interviews but a lot of noise happening caused by mass media, but my studies and research work is no change. Already thinking my ideas and studies – so basically no change but a lot of noise and mass media is contacting me. But basically no change. The final reward is I don’t know, we changed it so the overall /spread/ is around 50 percent and our goal is to cross 100 percent, so we have to work very hard to increase and change our third /- – -/. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0032** |
| **Biographical** | Iwas born in Belgium on 6 November 1932. I am married to Mira Nikomarow and have five children: Michèle, Anne, Georges from a first marriage with Esther Dujardin and Sarah, Hélène from a second one with Danielle Vindal.  My parents emigrated from Poland in 1924 with my brother, who was a few months old. They were from a simple family of Polish Jews. They were looking, I suppose, for a better economic life and were escaping from an anti-Semitic environment. They worked hard, set up a textile shop and managed to reach a rather decent life when in May 1940, Nazi Germany invaded Belgium, less than one year after the invasion of Poland. I was seven years old and quite aware of the situation. Persecution came gradually. After two years of relatively normal life, I was compelled to wear, as all Jews who could not hide their identity, the distinctive Star of David. A few months later, the Nazis started the deportation of Jews to concentration camps where they were murdered.  My parents, my brother and I survived the war in Belgium. We were helped and hidden by people who did not even know us, people who in those times of darkness took the great risk of displaying generosity, humanity and courage.  My parents were hiding in a place unknown to me. Separated from them to increase my chances of survival, I was taken care of by Camille and Louise Jourdan, the owners of a cafe-restaurant in Lustin, a village in the Ardennes. I want to pay tribute to their memory and of that of their daughter Yvonne, whose tenderness in initiating me to music and piano was like a glimmer of hope in a world of desperation. And to the memory of priest Warnon of Annevoie, an other small village in the Ardennes, where our family, fleeing from Lustin after a denunciation, reunited and stayed to the end of the war; he presented us to the village inhabitants as Christians; he went as far as baptising me so that I could attend the Catholic College “Notre-Dame de Bellevue” in Dinant as an ordinary student; he enrolled my brother, dressed in a fake soutane, at a seminarist school as if preparing him for priesthood. And also to the memory of many others who helped us. Without these wonderful people we could not have escaped the persecution and I would not be here to tell about it.  I have always present in my mind the courage of my parents, their perspicacity in finding the right move for escape at many critical moments, and most of all their love: in confronting the barbarity of the German Nazis and their many complicities, they always put the life of their children before the preservation of their own. Their survival was tragically marred by the complete disappearance of their Polish family, murdered in Poland by the German Nazis.  After the war, we attempted and largely succeeded in resuming a normal life. I went to secondary school and, while memories of past years still haunted the nightmares of my sleep, I functioned well at school. I developed an interest in literature, music and mathematics. My teacher of mathematics, whom I deeply appreciated, recommended studies in polytechnics for its extended program in mathematics at the University. My parents also pushed me in the same direction, out of concern about my future well-being. So in 1955 I got my degree in electrical-mechanical engineering. I realised however that my interest was less in practical applications than in the understanding of the underlying theoretical structure and I decided to learn physics. As an assistant in the polytechnic department, I was able to finance new studies and got my Physics Masters Degree in 1958 and my PhD in 1959.  I had discovered a passion for research and I was thrilled when the same year, based on recommendations and a few previous publications in *Condensed Matter Physics*, I was offered a two-year position in the United States at Cornell University, Ithaca (NY), as Research Associate for the young Professor Robert Brout. I immediately accepted and left for Ithaca.  Our first contact was unexpectedly warm. During my stay the convergence of our vision of science and life laid the groundwork for lasting collaboration and a lifelong friendship. In Ithaca, we worked together in condensed matter physics and in the statistical theory of phase transitions, mainly on ferromagnetism and superconductivity. We realised the importance of spontaneous symmetry breaking in phase transitions and we were extremely impressed when [Yoichiro Nambu](https://www.nobelprize.org/nobel_prizes/physics/laureates/2008/nambu-facts.html) showed how this notion could be transferred to elementary particle physics to explain the small pion mass on the hadron scale. This work and his beautiful analysis of superconductivity in field theoretic terms drove us later to study the fate of spontaneous symmetry breaking in the context of gauge theory.  In fall 1961, I was scheduled to return to Belgium. By that time our collaboration and our friendship had become deeply rooted. I was offered a University Professorship at Cornell but I was missing Europe very much. I decided not to accept it and to return to Belgium. Robert and his wife Martine had a similar attraction for the Old Continent; Robert got a Guggenheim fellowship and they joined me with their children in Belgium. After a few months, the social life there and our personal relationship persuaded Robert to resign from his professorship at Cornell University and to settle permanently at the Université Libre de Bruxelles in Brussels. He eventually acquired Belgian nationality and together we directed the theoretical physics group at the ULB.  In Brussels, we resumed our analysis on spontaneous symmetry breaking, both in the statistical theory of phase transition and, inspired mainly by Nambu’s work, in field theory. This is how we discovered the mass generating mechanism that may now indeed be viewed as a phase transition from a high temperature phase in the early Universe, where elementary particle were massless, to the present low temperature phase where their mass arises from a generalisation of spontaneous symmetry breaking to Yang-Mills fields, namely the BEH mechanism.  At the ULB, Brout and I initiated a research group in fundamental interactions, that is, in the search for the general laws of nature. Joined by brilliant students, many of them becoming world renowned physicists, our group contributed to the many fields at the frontier of the challenges facing contemporary physics. While the mechanism discovered in 1964 was developed all over the world to encode the nature of weak interactions in a “Standard Model,” our group contributed to the understanding of strong interactions and quark confinement, general relativity and cosmology. There we introduced the idea of a primordial exponential expansion of the universe, later called inflation, which we related to the origin of the universe itself, a scenario, which I still think may possibly be conceptually the correct one. During these developments, our group extended our contacts with other Belgian universities and got involved in many international collaborations.  With our group and many other collaborators I analysed fractal structures, supergravity, string theory, infinite Kac-Moody algebras and more generally all tentative approaches to what I consider as the most important problem in fundamental interactions: the solution to the conflict between the classical Einsteinian theory of gravitation, namely general relativity, and the framework of our present understanding of the world, quantum theory. Although this conflict appears experimentally to affect known results only at very tiny scales of the order of 10–33*cm*, transcending it would amount to overcoming a conceptual mistake. As such, a solution of this conflict might affect our understanding of the laws of nature at all scales and is crucial for attempting to reach a rational understanding of the origin of the Universe.  Robert was less interested in these new developments and concentrated more on cosmology. Our collaboration became less frequent but our friendship was unaffected. He passed away on May 3, 2011 after a prolonged illness and missed the remarkable discovery of the Standard Model scalar boson at CERN and the awarding of the Nobel Prize. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0032 = SN**  [SN] Are you there with us Professor Englert?  [FE] Yes, I am on the phone.  [SN] Good day and congratulations, how do you feel right now?  [FE] Well, thank you very much. I feel very well of course … I thought first I had to make a low festivity because I thought that I didn’t see any announcements that I didn’t have it. But now I’m very happy.  [SN] (laughs) Yes. So I’m sitting here in the Session Hall at the Royal Swedish Academy of Sciences and I have a large group of people from the media (FE interrupts: Yes) and the international press. And are you ready to take some questions from the press Professor Englert?  [FE] Yes please, I will try to do what I can.  [SN] Okay, I have a question there.  [MGA] Yes hello Professor Englert. My name is Maria Gunther Axelsson and I’m writing for the Swedish newspaper *Dagens Nyheter* and you and I met this summer in Stockholm if you remember. And congratulations to the prize.  [FE] Thank you very much.  [MGA] Yes and I have a question for you. (FE: Yes) Now, when the Standard Model is complete, what is the biggest question …  [FE] Sorry, I didn’t hear well, what did you say?  [MGA] Now that the Standard Model is complete (FE: Yes), which in your opinion is the biggest question remaining to be solved in Physics?  [FE] Well, there are a few big questions, right. First the question which is still not solved is whether there is broken supersymmetry which would manifest itself at energies which have not yet been reached. This is the critical point for what will happen. But of course, there are other problems, some of them might be directly related, some other indirectly or maybe not related, which is the problem of dark matter is probably somehow hopefully related to particle physics. The problem of dark energy is a more tricky problem, which, one way or another, leads us to what is, in my opinion, the most and the fundamental problem which is not solved today, despite some progress, which is the problem of quantum gravity, of the quantization of gravity.  [SN] Do we have some other question?  [K] Hi, um. Congratulations. My name is Kounteya and I’m from the *Times* of India. Very interestingly, the press release here starts with the line “Here at last!” Professor, how true is that?  [FE] I’m sorry I didn’t hear what you said.  [K] I was saying that the press release here talks about … the first line in the release says “Here at last!” How true is that?  [FE] Huh. I’m afraid … maybe it’s my phone or maybe it’s my ear, but I don’t understand exactly what you say.  [SN] We have a question? Yes, do you have a microphone?  [JR] Hello. Congratulations, my name is Joanna Rose, I work for a Swedish popular science magazine *Forskning och Framsteg*, and I would like to ask you, in the late sixties, when you worked on theory, did you ever think about the discovery of the Higgs Boson?  [FE] Oh yes, but the whole thing at the time … well first the late sixties was not really when the thing was done, it was the beginning of the sixties and in 64 it was published after a lot of thought. At that time, we saw that we were going to solve, this way, the problem of short-edge forces, which was completely unsolved at that time and which obviously is related to the problem of the origin of mass. So the boson itself is something that is the experimental test of the existence of the whole mechanism, and one had to wait. Certainly we had to wait first before the theory itself was applied to something which is a Standard Model, which took some time. It took some time to first prove the consistency of our theory, which was up to beginning of the seventies. And during the seventies, the Standard Model was built up. And only after that could one look for a test, because the Standard Model was wonderfully verified except for the missing element which was that boson, whose condensation is what gives the mass to particles and the short-range forces.  [SN] Okay, do we have another question please?  [M] Yes, hi, congratulations. This is Malin from the Associated Press (FE: Thank you). Of course this was highly-anticipated by all of us. But how did you feel when you found out about the award and what are you planning to do with the prize money?  [FE] Ah, well I’ll answer first the second question, I don’t know. That’s not my concern now (laughs). The first part of the question was what? About?  [M] Just, how does it feel to have won a Nobel Prize? I mean …  [FE] Well, you may imagine that this is not very unpleasant of course. (laughter) I’m very, very happy to have that recognition of that extraordinary reward … so I am very happy of it. What can I say more?  [M] Okay, thank you.  [SN] Okay, I think we have a final question there.  [DL] Hell this is David Landiss with *The Local* here in Stockholm. Congratulations Professor Englert. A question about your co-winner. I’m wondering when you last spoke to Professor Higgs and what you plan to say to him when you see him next?  [FE] Ah, well I saw him, actually … the first time I saw him was in the 4th July conference at CERN, we never met before, but since then we met, and we met in particularly the EPS conference (2013 Europhysics conference on High Energy Physics), and what I am going to say to him of course I’m going to congratulate him (laughter), because I think he did very important and excellent work.  [SN] Okay, thank you very much. And thank you very much, I think this was the last question from the press here, and thank you Professor Englert. And once again, warm congratulations, and we look forward to see you in Stockholm in December for the Nobel Prize Ceremony.  [FE] Yes, thank you.  [SN] Thank you, bye.  [FE] Goodbye. |
| **Interview** |  |
| Q4 | **Could you describe your Nobel Prize awarded work for young students?** |
|  | François Englert: First I will tell them or remind them if they know a little bit that all the matter that we have around us is composed of atoms. The atoms are kind of mini solar system, not quite, but nevertheless it’s an image, and in the middle there is an nucleus which is round. And for a long time this was supposed, the whole the atom, to be not … that it was not possible to cut it and to separate it, but now we know that it is. Inside the atom there are particles which are called quarks and other particles of all kind which of the constituent fundamental of matter which are what called elementary particles and there are also other elementary particles which are constituent of things which are transmitted from one object to another like photons which are constituents of light and there are other ones. Some of these particles have no mass and some have mass. Photons have no mass and the characteristic of mass is normally when you push it, it’s very difficult, if it’s a high mass it’s very difficult to push it.  But the more detailed meaning of what it means is that those particles which have no mass travel, whatever you do, with the same velocity which is the velocity of light. Particles which have mass never reach the velocity of light, so that’s the big distinction between the two. The problem was that one did not succeed to make a theory which could predict what one could do with this elementary particle. In particular we know what we can do of them, everything that is around us, whether it’s that glass, whether it’s the door, whether it’s this television apparatus whatever. We can do all of that, but the recipe for doing that was not really well known and one particular difficulty is that one could imagine constructing a recipe which would work if all particles which have no mass. If all particles which have no mass, then one could indeed form a formula which would predict everything but that would result in predicting nothing because after all that is not the world that we live in.  So we were obliged to invent how particles which have no mass can acquire mass, some of them at least, and some not. For this we imagined at that moment some particles, bosons, scale bosons, whatever the name is on it and that condense to give kind of a sea, but a sea that exists all over the universe so that every particle has to go through it, including ourselves. So the elementary particle that goes through them can acquire a mass if they are sensitive to this, that is more complicated to understand, but the theory predicts which are the particle which will get a mass and those which will not get a mass. That essentially permitted to construct the recipe for doing the thing and the important thing of course is that from this condensation it is possible to extract one particle and test if the theory is correct and that is what has been done at CERN. I don’t know if a 13-14 years old will get something, but I hope they will at least get a little bit interested. |
| Q3 | **Why did you go into science?** |
|  | François Englert: When I was in secondary school I was actually interested very much in literature but I was also very much interested in mathematics. My teacher in mathematics, I liked him very much, I think it’s about the only one I liked when I was in the secondary school. I had a very good relation with him. I was not a bad student, so he pushed me to enter the polytechnic school for engineering. This was also partly because engineering in Belgium requires an entrance examination which is very hard, actually in three places if you wish the diploma of secondary school. You don’t have to do secondary school if you succeed that exam. He prepared me for that exam, so I was an engineer. Being an engineer, I realized that I don’t want to be an engineer, but I was an engineer so that gave me … I realized by being an engineer that what interested me is not so much the application of things but the way they really function. That is why I got interested in physics and research and fundamental physics. After having been engineer I got sufficient money because I had a job at the university, and I studied physics afterwards. Then I had my PhD and then I went to the United States and met Robert Brout with whom I collaborated a lot and that’s in a short hand the story. |
| Q5 | **Who is your role model, and why?** |
|  | François Englert: There is one person which I consider as an extraordinary physicist at that time I knew him. He is a man of extraordinary humanity, that is [Nambu](https://www.nobelprize.org/prizes/physics/2008/nambu/facts/), in fact who got the Nobel Prize by the way. And he was the one who introduced the essential notion which was essential for doing the model which was taken from the theory of phase transition in solid … statistical theory of solid objects and to field theory, which I liked. I was interested by many people who did it, but I think I generally wanted … and Robert Brout was famous like me, we wanted to work things by ourselves. But Nambu played an important role, that is for sure. |
| Q2 | **At what point did you realize your work was a breakthrough?** |
|  | François Englert: I think we realized it nearly immediately, I can tell you something about that. To be clear our work was kind of a general theory, we were not sure to what it would apply. Actually, we thought first it would apply to the strong forces which was a mistake, but we then realized it applied to electroweak forces. But we didn’t make the theory, that was won by [Weinberg, Salam and Glashow](https://www.nobelprize.org/prizes/physics/1979/summary/) who had the Nobel Prize for that and there have been a lot of Nobel Prize connected to this theory. First the Nobel Prize for the electroweak theory, the Nobel Prize for the fact – which was very important and which we were completely aware of from the beginning – which is the fact that it’s valid quantum mechanically which was proven in a beautiful way by Dr. [Veltman](https://www.nobelprize.org/prizes/physics/1999/veltman/facts/), they got the Nobel Prize. There was a discovery by particles at CERN the W and the Z boson which transmute … which have mass and transmit the weak interaction and that was also a Nobel Prize.  All of these were essentially the base to particles to what we have constructed as a theory but of course there is a lot of things to be added to get all of that and it took time. The point is that after let’s say -83, 1983 when world discovered the W and the Z we were totally convinced that the theory was two, but there was still an alternative. Either it was mediated by a condensate of objects, the sea, which was not the boson, or it was the bosons, which were condensate. We in our original paper put both hypotheses and didn’t know which was the right one and at that moment in- 83 it was still untrue, which was the right one and it is the discovery at CERN that would select the right one which is one of the two hypotheses we put in -64.  So I have a bit deviated from your question which is when did we realize, of course we didn’t realize all of that, that’s obvious, but we realized that I think it was a break-through, because we saw that it was the first time there is a way to understand how interaction, fundamental interaction which acts between objects which are very close to them and are not felt at large distances – that is called short-range interaction – that that could work, and so I remember with Robert Brout, who was my friend, and we worked together after that also for years, that we went to a café that I still see very well and we ordered a bottle of champagne and we said … and that was I think a few months after having had the article, so that we had the impression. We were a little bit sad that people didn’t seem to realize that or to pay attention, let’s say. There were good reason not to pay attention at that time, because there were lots of things. But I think we had the right. We were convinced by the way we did it, we didn’t have a proof at that time that it was consistent quantum mechanically, but the way we constructed it – which is very peculiar at that time – it’s based on what’s called field theory and we had the impression that that at the end would work and so we celebrated, we might have celebrated wrongly, but at least we got the champagne and that was fine. |
| Q54 | **Could you explain the standard model?** |
|  | François Englert: The standard model is precisely what classifies all these particles from which we can construct the whole world. Actually, the standard model contains essentially all these quarks, these electrons that constitute the atoms, all the other elementary particles that one can construct out of them, or then out of collisions, and it contains … describes the force which are /---/ them and that is essentially the recipe because the forces are also transmitted by some elementary particles some which have mass, some which don’t have mass, photons have no mass, those which give the so-called weak interaction give mass. Essentially there are three kinds of interaction which are included in the standard model: the electromagnetic interaction, the weak interaction, which are responsible in particular for the disintegration, radioactive decay, and these two were united in a theory that make a very deep use of our theory which was then in a quite general frame. And the standard model contains in addition strong interaction which also are interaction between objects which are inside the nucleus, but which are responsible for essentially the cohesion of these nuclei. These are the strong interactions which are not explained by our theory but by something different, which actually is essentially related to our theory by something which is called, let’s say it’s the opposite. The opposite of something is not necessarily very distinct, that it is.  These are the three types of interaction which form these particles that we know today that are hold together and getting a mass with this boson that was discovered or rather by the sea of them which gives this sea which permeates the universe and there is another interaction which we feel every day, which is gravitation  which is what … we let an object fall and it falls and why the planets turn around the earth,and why everything like that works and that is also a theory, but for the moment it is not included in the standard model. It is something apart, we know it very well, it is the first thing that was known, it was redeveloped by [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) and generalized by Einstein, it’s called – a bad name, but it is called general relativity. That was in the beginning of the 20th century and, so, that is very well known, but it has its difficulties and not, I don’t know if you want to enter into that, that’s more complicated. |
| Q9 | **What were you doing when you got the message of being awarded the Nobel Prize?** |
|  | François Englert: First it took some time to hear it because there was some delay. When I was there, three of my daughters were with me and my wife, because after all it was quite possible, it was not totally unexpected that I would get it, let’s say, the Nobel Prize. So we were waiting and then we had decided that for some reason either they did not give it for the theory this year or whatever, my daughter were not quite nice with what they said about the Nobel Committee at that time because they thought that we don’t get it, but then the call came and so I was of course extremely happy and so were my daughters and my wife**.** |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0033** |
| **Biographical** | Peter Higgs was born on 29 May 1929 in the Elswick district of Newcastle upon Tyne, UK. He graduated with First Class Honours in Physics from King’s College, University of London, in 1950. A year later, he was awarded an MSc and started research, initially under the supervision of Charles Coulson and, subsequently, Christopher Longuet-Higgins. In 1954, he was awarded a PhD for a thesis entitled ‘Some Problems in the Theory of Molecular Vibrations’, work which signalled the start of his life-long interest in the application of the ideas of symmetry to physical systems.  In 1954, Peter Higgs moved to the University of Edinburgh for his second year as a Royal Commission for the Exhibition of 1851 Senior Student, and remained for a further year as a Senior Research Fellow. He returned to London in 1956 to take up an ICI Research Fellowship, spending a year at University College and a little over a year at Imperial College, before taking up an appointment as Temporary Lecturer in Mathematics at University College. In October 1960 Peter Higgs returned to Edinburgh, taking up a lectureship in Mathematical Physics at the Tait Institute. He was promoted to Reader in 1970, became a Fellow of the Royal Society of Edinburgh in 1974 and was promoted to a Personal Chair of Theoretical Physics in 1980. He was elected Fellow of the Royal Society in 1983 and Fellow of the Institute of Physics in 1991. He retired in 1996, becoming Professor Emeritus at the University of Edinburgh. He was awarded Fellowship of the University of Swansea in 2008, Honorary Membership of the Saltire Society and Fellowships of the Royal Scottish Society of the Arts and the Science Museum London in 2013.  Peter Higgs’ contribution to physics has been recognised by numerous academic honours: the Hughes Medal of the Royal Society (1981, shared with Tom Kibble), the Rutherford Medal of the Institute of Physics (1984, also shared with Tom Kibble), the Saltire Society & Royal Bank of Scotland Scottish Science Award (1990), the Royal Society of Edinburgh James Scott Prize Lectureship (1993), the Paul Dirac Medal and Prize of the Institute of Physics (1997), and the High Energy and Particle Physics Prize of the European Physical Society (1997, shared with Robert Brout and François Englert), the Royal Medal of the Royal Society of Edinburgh (2000), the Wolf Prize in Physics (2004, shared with Robert Brout and François Englert), the Stockholm Academy of Sciences Oskar Klein Memorial Lecture and Medal (2009) and the American Physical Society J. J. Sakurai Prize (2010), shared with Robert Brout, François Englert, Gerry Guralnik, Carl Hagen and Tom Kibble. He received a unique personal Higgs medal from the Royal Society of Edinburgh on 1 October 2012 and the 2013 Nonino Prize ‘Man of Our Time’. He shared the award of the 2013 Edinburgh International Science Festival Edinburgh Medal with CERN and the 2013 Prince of Asturias Award for Technical and Scientific Research with François Englert and CERN.  He has received honorary degrees from the Universities of Bristol (1997), Edinburgh (1998), Glasgow (2002), King’s College London (2009), University College London (2010), Cambridge (2012), Heriot-Watt (2012), Manchester, (2013), Durham (2013), La Scuola Internazionale Superiore di Studi Avantzi di Trieste (2013), St. Andrews (2014) and the Université Libre de Bruxelles (2014).  In 2011 he was awarded the Edinburgh Award for his outstanding contribution to the city. In the 2013 New Year Honours List he was appointed a Companion of Honour. In 2013 he was granted the Freedom of the City of Bristol. In 2014 he was awarded the Freedom of the City of Newcastle, his birthplace and the Freedom of the City of Edinburgh. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** |  |
| **Interview** |  |
| Q1 | **Could you describe your Nobel Prize awarded work for young students?** |
|  | Peter Higgs: Imagine a snowfield, and that is an analogous to this background field throughout all the universe, this affects the way that people crossing it in different ways depending on whether they wear skis or snowshoes or just boots. The analogy is then that the people with skis relatively unaffected and untroubled with high high speed, people with snowshoes do not quite so well and the people who just wear boots go very slowly and that analogy is with the effect on some kind of particles which continue to travel this speed of light and the massless and what happens to particles which are heavier, but to me that contains less of the physics than my more roundabout explanation. |
| Q4 | **Can you explain the standard model?** |
|  | Peter Higgs: The work which was done in 1964 led to the so called electroweak theory, the unification of weak and electromagnetic interactions in elementary particles which was done in 1967 by [Weinberg](https://www.nobelprize.org/prizes/physics/1979/weinberg/facts/) and [Salam](https://www.nobelprize.org/prizes/physics/1979/salam/facts/) by taking a theory, which had the right kind of unification but couldn’t produce good calculations due to [Glashow](https://www.nobelprize.org/prizes/physics/1979/glashow/facts/), and combining it with the kind of models of symmetry breaking which we had discussed in -64. That was the beginning of the standard model, because once that theory was shown to be mathematically sound, that you could really calculate with it, people started to study other kinds of so-called gauge theory in relation to the other forces in particle physics and those investigations led on to a theory of the strong forces, called quantum chromodynamics. So the work in -64 was the beginning of the return of this kind of so-called quantum field theory in particle physics. It had previously been successful in the 50s, quantum electrodynamics, and then became neglected in particles that didn’t seem to work and what we did was a step on the way to making it work. |
| Q2 | **At what point did you realize your work was a breakthrough?** |
|  | Peter Higgs: It wasn’t a precise moment. The way in which I came to this realisation was that I was trying to evade a theorem which had been proved about this way of breaking symmetries in particle physics which implied that there would exist massless particles without spin. This theorem made this kind of theory unacceptable because such particles were not known. The theory in which these occurred had been formulated four years earlier by [Yoichiro Nambu](https://www.nobelprize.org/prizes/physics/2008/nambu/facts/) who got a share of the 2008 prize and Jeffrey Goldstone and it was really for me a matter of realizing that the theorem as proved had a flaw in it, there were certain mathematical axioms which you need to prove any kind of theorem which certain kinds of theory of fields didn’t obey. And the prime example of the kind of field which didn’t obey these axioms was Maxwell’s electromagnetic field as it occurs in quantum electrodynamics.  Quantum electrodynamics is a theory which doesn’t involve this phenomenon of symmetry breaking but once it was clear that there were fields of the maximal type which didn’t obey the axioms, then the way was opened to introducing these fields of this type into the kind of theory which Nambu started of symmetry breaking and that’s essentially what happened to me over a weekend during which I gradually realised that I knew two things which had to be brought together. It was related to the fact that I had read papers not long before by [Julian Schwinger](https://www.nobelprize.org/prizes/physics/1965/schwinger/facts/) who was one of the people who shared the quantum electrodynamics prize and that was -65 I think. He had a way of formulating that theory which was a little bit different from what most people preferred. It resulted in some equations which were explicitly  violating, apparently violating the rules of [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) relativity theory but the physics was not affected by this, it was just a peculiarity of the formal mathematics and so it was my recollection of equations written by Julian Schwinger which made me see that this was what had to be done. But it was a process which wasn’t sudden, it was during the weekend and I had to go back to my office on the Monday and check that I hadn’t made a mistake about this. |
| Q5 | **Who is your role model, and why?** |
|  | Peter Higgs: In my high school days I didn’t find the physics that I was taught very interesting. I was better at mathematics and chemistry, my scientific subjects, and I was quite enthusiastic about chemistry, understanding the structure of matter of the molecular level. Gradually I came to know that there were deeper levels and that these were classified as physics and there were interesting things to do in theoretical physics there. One of the influences on me which probably a former pupil at the same school of about a quarter of a century earlier, [Paul Dirac](https://www.nobelprize.org/prizes/physics/1933/dirac/facts/), who was one of the founding fathers of quantum mechanics in the mid-1920’s, and I was curious about what he had done because his name appeared frequently on the roll call of the achievements of former pupils. I was curious and that led me to read about atomic physics and quantum theory before I was ever taught them.  The only other thing perhaps to add about influences in my days at high school was that at the end of my time there, it was very soon after the end of the war and the dropping of the bombs on Japan. I went to some public lectures in the University of Bristol organized by the two professors of physics, one theoretician and one experimentalist. These were lectures for the public to tell them what was the background of the development of these bombs and it was a great success series of lectures. The experimental physicist was [Cecil Powell](https://www.nobelprize.org/prizes/physics/1950/powell/facts/) who worked in experimental particle physics, in those days it involved sending packages of photographic emulsion up into the upper atmosphere with the help of balloons, and he decided to give some lectures about his own work which I then went to when I learned from him a lot about the current state of experiment in particle physics and that helped me to see what I wanted to do. |
| Q9 | **What were you doing when you got the message of being awarded the Nobel Prize?** |
|  | Peter Higgs: When the prize was announced I wasn’t at home and that was deliberate, I wasn’t trying to avoid the people from the Nobel Foundation or the Academy, but I was trying to avoid media attention which I expected would follow very rapidly. So I made sure to go out at 11 in the morning with the expectation that the announcement would be maybe 11.30 or something like that and I also went down to the harbor area Edinburgh for a lunch, went to an art exhibition and came back home at about 3 o’clock and I was told the news first by a former neighbor who stopped her car which overtook me as I was walking home and came across the street and said “Congratulations, my daughter phoned me from London to tell me about the award” and I said “what award?”. She gave me the answer I expected so I then went home and listened to my phone messages. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0034** |
| **Biographical** | I was born on September 11th 1944 in Casablanca, Morocco, in a Jewish family with mixed Sephardic and Ashkenazi origins. My father’s parents were teachers at the Alliance Israëlite universelle (AIU), which operated a network of schools dedicated to the dissemination of French language and culture throughout Jewish communities in North African and Mid-Eastern countries. My paternal grandfather had been one of the first students of the AIU school in the town of Marrakesh, in the last years of the nineteenth century. My paternal grandmother had studied at the same time in Tetouan, in the part of northern Morocco under Spanish influence, in the first AIU school, which had been founded there in 1863. After completing their school years, they both decided to become teachers and, unbeknownst to each other, came to France at the very beginning of the 20th century, to get their degree from the AIU teacher training school in Paris. Although they were both there at the same time, they did not meet then since the boys’ and girls’ schools were separated.  I found a few years ago in one of my grandfather’s notebooks a handwritten recollection of his souvenirs as a young teenager suddenly immersed in 1900 Belle Epoque Paris, seeing for the first time automobiles and trains, enjoying theater shows and silent movies, experiencing with bewilderment a modernity contrasting so much with the medieval atmosphere of Marrakesh at that time. My paternal grandparents met shortly after graduating and returning to Morocco, at the time this country became a French protectorate. They soon married and had seven children, while moving from school to school on successive assignments, ending as headmaster and headmistress of the boys and girl’s AIU schools in the town of Salé, near Rabat. My father, born in 1920, was raised there and studied law in Rabat, becoming a lawyer at the beginning of the Second World War.  My mother’s family emigrated from Russia in the 1920s, in the years following the Bolshevik revolution. Her parents were physicians and chose to settle in Morocco, a country where their Russian medical degrees were recognized. My mother, born in Odessa in 1921, was raised in Casablanca and met my father at the university in Rabat, just before the war, while she was studying to become a teacher in French and German literature. In my early childhood, I was raised in Russian as well as in French, and I remained bilingual until I went to elementary school at the age of six, unfortunately then very quickly losing my fluency in Russian. The community of Russian Jewish immigrants in Casablanca had kept strong links and I remember meeting many of them in my grandparents’ house. Among them was an architect, Casimir Zeligson, who has built several of the Art Nouveau building and villas which still give a special character to the town of Casablanca. The Zeligsons had a daughter, Claudine, with whom my younger brother Joël and I used to play. After a long separation, I met her again in Paris fifteen years later, but here I am anticipating.  I have kept strong memories of this time in Casablanca, of the warm Mediterranean weather, the long rainless summers and mild winters with their flowering bougainvillea and hibiscuses, bathing in the cold Atlantic Ocean whose waves can be so strong and treacherous, especially to a young child. Some remembrances are particularly vivid, like the recurring sirocco storms covering everything with a blanket of sand coming from the Sahara, or the locust invasions, which on several occasions have plagued the fields in the countryside around the city, leaving them barren after they had gone. During the last years of my life there, I witnessed the events leading to the independence of Morocco from the French, understanding that, amid the convulsions and sometime violent events, history was in the making.  When independence finally came in 1956, my parents decided, like many Jews, to leave Morocco and settle in France with their sons Joël, Gilles and me (a fourth son, Michel, was born in Paris in 1959). This was essentially a cultural choice. They had received and given to their children a French education and thought that France was the natural place for the family to go. The first years in Paris were difficult, requiring a painful adjustment, especially during the damp and dark winters, contrasting so much with the vivid colors of the Moroccan weather. Learning at school was a consolation, though. I was a very good student, immediately at the head of my class in the Lycée Carnot where I studied until the “Baccalauréat,” the final degree of the French high school education.  I was indiscriminately interested in literature, history, mathematics and physical sciences. In humanities, I could share my tastes with my parents, who raised my three brothers and me to love reading, going to museums and discussing all kind of issues ranging from ancient history to modern politics. In scientific matters, I was on my own, however, the first in the family to wander in a domain which required a mathematics background. I remember how, early on, I was fascinated by astronomy and by calculus, the notion of derivatives and simple differential equations which describe so directly and so well the laws of dynamics obeyed by moving bodies. This was the time of the first artificial satellites, the sputniks which orbited the earth and launched the American-Soviet race to the moon.  I marveled at the fact that I was able, with the elementary calculus I knew, to compute the escape velocity of rockets, the periods of satellites on their orbits and the gravitational field at the surface of all the planets … I understood then that nature obeys mathematical laws, a fact that did not cease to astonish me. I knew, from that time on, that I wanted to be a scientist. For that, I embarked in the strenuous and demanding “*classes préparatoires*” of the famed Lycée Louis-Le-Grand, one of the preparatory schools which train the best French students for the contest examinations leading to the “*Grandes Ecoles*.” They are the engineering and academic schools, which since the French Revolution, have formed the scientific elite of France. These were two years of intensive study where I learned a lot of math and of classical physics. I eventually was admitted in 1963 to the Ecole Polytechnique (ranking first in the national examination,  to  the great pride of my parents) and at the Ecole Normale Supérieure (ENS). I chose to enter the latter because, at that time, it offered a much better opportunity to embark in a scientist career.  The years as a student at ENS (1963–1967) have left me wonderful memories, contrasting sharply with the strenuous training of the preparatory school. Here, in the middle of the Latin Quarter, I was free to organize my time as I wished, to meet and discuss with students working in all kinds of fields in science or humanities and to enjoy all the distractions and cultural activities Paris has to offer. And I was paid for that, since the “Normaliens” as the ENS students are called, are considered civil servants and receive a generous stipend! These were my formative years as a scientist. Coming so to speak from the physics of the 19th century which was taught in the *classes préparatoires*, I was immediately thrown into modern physics and the quantum world by the classes of exceptional teachers. [Alfred Kastler](https://www.nobelprize.org/nobel_prizes/physics/laureates/1966/kastler-facts.html) gave us a lyrical description of the dance of atomic kinetic moments, and gave atoms and photons a near poetic existence. Jean Brossel brought us back to Earth by describing the great experiments thanks to which quantum concepts were established, instilling in us the austere passion for precision. And [Claude Cohen-Tannoudji](https://www.nobelprize.org/nobel_prizes/physics/laureates/1997/cohen-tannoudji-facts.html) revealed the theory’s formalism to us with extraordinary depth and clarity. I still remember three books I read avidly at the time: *Quantum Mechanics*by Albert Messiah,  where  I  truly  understood the depth and beauty of the quantum theory; *Principlesof Nuclear Magnetism*by Anatole Abragam, who introduced me to the subtle world of atomic magnetic moments; and Feynman’s *Lectureson Physics*, which was a revelation.  But physics was only one side of that time’s story. In the spring of 1964, I met Claudine Zeligson again by chance in a Latin Quarter café. She had followed her own path from Casablanca to Paris, where she was studying English, psychology and sociology at La Sorbonne. We resumed a relationship which had been interrupted by a fifteen year latency period and married in 1965, at the age of 21. She later embarked in her own career as a scholar, doing research in sociology and anthropology at the French National Center for Scientific Research (CNRS). We have been together since then, sharing all aspects of life, including a common love for music and painting, movies and travels. Without her love, intellectual stimulation and constant support, nothing would have been possible.  Enthralled by the mysterious beauty of the quantum world, it did not take me long to decide that I wanted to become a quantum physicist. That was the time when various optical methods for  the  manipulation  of  atoms  were  being invented in  the  *Laboratoirede Spectroscopy Hertzienne*of  ENS,  which  was to be called later the *Kastler Brossel*laboratory. I remember the day in the fall of 1966 when, as a young student, I witnessed the joyous turmoil following the announcement of Kastler’s Nobel Prize, attributed to the invention of optical pumping methods. I measured my luck to have just started working in a field which was getting such a worldwide recognition. The Kastler lectures I had attended were immediately illustrated by everyday reality at the ENS laboratory, where in 1967 I started my thesis under the enthusiastic supervision of Claude Cohen-Tannoudji.  I have described in my Nobel Lecture how my interests in physics have naturally evolved from that time on, motivated by the challenge to probe and control the atom-photon interactions processes at the most fundamental level. This adventure started with my PhD work with Claude on the “dressed atom” formalism, analyzing atoms irradiated by radiofrequency fields as being dressed by a cloud of photons surrounding them. We learned how to describe the atoms coupled to photons as a combined entity, whose energy level structure revealed, in a synthetic way, all the properties of the system. At that time, it was unusual to describe the field, especially in the radiofrequency or microwave spectral range, in terms of photons. Some famous physicists even believed that all phenomena in atomic physics, including spontaneous emission, could be explained classically and that the photon was a superfluous concept. They were wrong, as we now know, but it is true that the classical picture viewing the field as a time-varying electromagnetic wave is sufficient to account for all effects involving huge amounts of light quanta.  Claude and I, though, were persuaded that the quantum description of the dressed atom formalism went deeper, giving a more satisfactory interpretation of the phenomena and a better insight into novel effects which were more difficult to predict within the classical approach. Since that time, Claude and I have kept using the dressed atom point of view, along with many other scientists who have adopted it. My main interest has been the interaction of atoms with invisible microwave photons, the kind of photons with which I became familiar during my PhD studies. Claude, with the students he trained after me, extended the formalism to deal with the interaction of atoms with optical fields and used it to explain in a particularly illuminating way the subtle effects involved in the cooling and trapping of atoms with laser light. This eventually led him to the famous studies which were recognized by his 1997 Nobel prize.  The experiments I performed to illustrate the “dressed atom” formalism during my thesis work were done with classical spectral lamps. The laser sources which underwent a spectacular development during the 1960s promised to open new perspectives in atomic physics and I realized that I needed to learn how to use these new tools. After completing my PhD, I chose to become a postdoctoral fellow at Stanford University, in the laboratory of [Arthur Schawlow](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/schawlow-facts.html), one of the inventors of the laser. Our son Julien, born in 1970, was a young toddler and our daughter Judith a new born baby when Claudine and I arrived with them in the San Francisco Bay Area in September 1972. The year we spent there has left us wonderful memories. The California weather with its balmy winters reminded us of our childhood in Morocco. We enjoyed a way of life which was very different from the one in Paris, living in a large house on the Stanford campus, close to the natural wilderness of the Pacific coastal mountain range and the ocean beaches, as well as to the sophistication of San Francisco and Los Angeles with their theatres, opera houses and gourmet restaurants. It was an ideal place to raise young children.  In Art Schawlow’s lab, I had a lot of fun with the marvelous toys that were the first tuneable lasers sent as prototypes to California laboratories by the commercial companies of what was to become Silicon Valley. Art’s enthusiasm was contagious. Every day, a new idea would spring up, sometimes wacky, sometimes brilliant. There came the first “edible” laser the day he had the idea of turning those ghastly food jellies of garish colors that he loved to eat into laser amplifying mediums, but also many demonstrations of clever spectroscopic methods, every time pushing the limits of the precision and sensitivity of measurements further. These new spectroscopic methods led to the Nobel Prize awarded to Schawlow in 1981. A young associate professor, [Theodor Hänsch](https://www.nobelprize.org/nobel_prizes/physics/laureates/2005/hansch-facts.html), had joined Art’s lab a few months before my arrival there and he was the driving force in the group, always finding new ways to exploit these marvelous laser sources to probe deeper and deeper into atomic spectra.  Art had a great sense of humor, which I believe is essential to maintaining a healthy atmosphere in a laboratory. “To succeed in research,” he often said, “one doesn’t need to know everything about everything, it’s enough to just know a few things that others don’t.” This sentence, pronounced with his contagious kindness and laughter, went a long way in relieving us of the intimidating weight of universal knowledge, which so often inhibits one, whether it is discouraging, or leads to an overly skeptical attitude about the world and the discoveries still to make. The hospitality of Art and Aurelia Schawlow in their campus home was memorable and we have kept strong connections with them long after Claudine and I had left California. It is during one party at the Schawlows that Claudine and I had a chance to meet [Felix Bloch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1952/bloch-facts.html), who had been the first to describe the quantum behavior of electrons in solids and had invented magnetic resonance, the phenomenon I had exploited during my PhD work a couple of years earlier. For the young postdoc I was then, it was an awesome experience to interact with a scientist of this stature, a man who had worked with the founding fathers of the quantum theory. This kind of encounter contributed greatly to the excitement of our life in California.  After a few weeks in Stanford, Art gave me a lab room and a pulsed dye laser and told me it was up to me to find something interesting to do with it. With one of his graduate students, Jeffrey Paisner, I decided to try to study atomic quantum beats. They manifest themselves as time modulations in the fluorescence of atoms following a pulsed optical excitation. Such modulations had been observed previously in experiments realized with spectral lamps, but I suspected that with the increased power and sharper pulses produced by lasers,  much faster and stronger beat signals should be obtained. When the experiment worked, almost on the first trial, I was exhilarated. For the first time, a project I had conceived and pursued without supervision was working and yielding interesting results. I presented them in the first International Conference on Laser Spectroscopy which took place in Vail, Colorado, in June 1973. The year spent in California was also an opportunity to meet scientists who have left a strong impression on me, and to start a longstanding friendship with some. This has been the case with Ted Hänsch, who has been a friend since then and whom Claudine and I have met on many occasions in the US first, then in Germany when he settled back there in the 1980s. It is also during the Vail meeting that Claudine and I got to know Daniel Kleppner of MIT and his wife Beatrice, who have become lifelong friends.  During my postdoctoral year, I had been thinking about a project to study Rydberg states, very excited atomic levels whose energies are close to the atom’s ionization limit. These states, of gigantic dimensions on the atomic scale, had been already observed in outer space in radio astronomy, but their preparation and study in the laboratory have had to await the development of lasers. My quantum beat experiments in Stanford had persuaded me that these laser sources were indeed very promising for investigating atomic states previously inaccessible to classical spectroscopy. I was fascinated by the prospect of being able to prepare and manipulate atomic matter under such exotic conditions, which should have properties so different from ordinary atoms. I wrote this proposal in an informal letter to Jean Brossel, who was the director of the ENS physics department. By return mail, he immediately offered me a position at ENS and, when I returned to Paris in the fall of 1973, he gave me a laboratory space and the start-up money required to buy the laser equipment for these experiments.  A bright student from ENS, Michel Gross, joined me and, within a few months, we built a new laboratory and started the investigation of these Rydberg atoms, which have been the workhorses of nearly all the experiments I have done since. Brossel had also secured me a research position of “maître de recherche” in the Centre National de la Recherche Scientifique (CNRS), which allowed me to spend all my time doing research, without any mandatory teaching duties. I had always liked the idea of teaching, though, finding it challenging and stimulating to explain scientific ideas and to communicate about science and research. So when in 1975 the opportunity arose, I applied to a position at Paris VI University and was appointed there as a professor. I started teaching basic physics to premedical students and a course about energy to physics undergraduates. At that time, the teaching load of professors was not as heavy as it has since become and I enjoyed splitting my activities between research and teaching.  Brossel had maintained in his laboratory an atmosphere  of  freedom  and trust which  seemed to me  natural. In retrospect,  I now measure  how lucky I have been to work in such a favorable environment, without having to write incessant proposals and to submit to constant evaluations. In this context, I have been able to develop my research in the long term, justifying it only by the papers I published and the recognition they received. Time and trust are, I think, worth even more than money. They are two essential ingredients required for good basic science to flourish. I have been lucky to enjoy plenty of them in my early career as an independent scientist. When the conditions changed in the 1980s and research in France became more and more subject to the rules that the global market started to impose, I had to learn how to write proposals and how to answer repeated evaluation requests. My reputation as a scientist was made, though, and I did not have too much trouble adjusting and receiving the grants required for my group to operate in good conditions.  Time, trust and money are certainly necessary, but the most important factor for the success of my research has been the quality of the people I have had the luck to work with. In 1976, Michel Gross was joined by Claude Fabre who started his PhD work with me. Soon thereafter Jean-Michel Raimond completed the group of bright and dedicated “normalien” students working with me, inventing new ways to explore the radiative properties of Rydberg states. For that, we needed good microwave sources, operating at frequencies in the tens of GHz range. In another laboratory of the ENS physics department, a solid state physicist, Philippe Goy, was precisely developing and using such sources for his electron cyclotron resonance experiments. I got him interested in our project and he soon joined us for the exploration of the microwave spectra of alkali atom Rydberg states.  This was a very exciting time in the laboratory, when we started to develop the techniques which have allowed us to control the Rydberg atoms and perform the Cavity Quantum Electrodynamics (Cavity QED) experiments described in my Nobel lecture. Michel Gross and Claude Fabre went on to their own research careers after graduating, while Jean-Michel decided to stay in my group. With Philippe Goy, we supervised several PhD students during the 1980s and received many foreign visitors in our laboratory, including Daniel Kleppner and Luigi Moi, a former student of Adriano Gozzini, a friend and colleague of Kastler, working at the Scuole Normale of Pisa, the Italian sister school of the ENS. Moi became a close friend who pursued later his career as a professor at the University of Siena.  In the mid 1980s we were joined by another exceptionally gifted student, Michel Brune, who also stayed with us after his PhD thesis. At that time, Philippe Goy started a small microwave equipment company, AB Millimetre, which has built custom-made microwave sources spectrum network analyzers for customers all over the world. He has no longer worked directly with us since, although we still consult him when we have to solve an arduous technical problem. Jean-Michel, Michel and I have worked together up to now, training generations of students and postdocs in our lab. All the achievements which have led to the Nobel recognition are theirs as well as mine. Beyond the exhilaration of obtaining interesting results, we have shared the pleasure of exchanging ideas in an atmosphere of trust and friendship. We do take the research seriously, but we like to joke about ourselves. Claudine, who visits us often in the lab, likes to define the spirit in our group as a special mixture of self-confidence and derision. Of course we recognize the value of what we are doing, but at the same time, we try not to take ourselves too seriously.  Teaching was an important part of my activities. Since the early 1980s, Paris VI University and ENS had an agreement under which I could do my teaching at ENS to “normaliens”and to a group of selected students accepted in the ENS curriculum. To graduate students, I taught courses in atomic physics and quantum optics, and  to undergraduates,  I lectured  about electromagnetism and  quantum mechanics. At the same time, I had a part-time position as a lecturer at the Ecole Polytechnique, where I had also the opportunity to teach very bright students. I found all this teaching very rewarding, especially since I could always do it on topics which had a more or less direct connection with my own research, finding ways to illustrate the lectures with the description of modern atomic physics or laser experiments.  The 1980s were also years when America called. Harvard had offered me a full time professor position in 1981, but I was not ready to leave Paris and the group I was working so well with. Having loved our year at Stanford, I was however tempted to try another experience as a scientist in America. So when Yale, at the initiative of Vernon Hugues, allowed me to come and do research in New Haven for one term each year, while retaining my Paris position, I accepted the challenge to carry out experiments on both sides of the Atlantic. For a few years, I was able to successfully perform atomic physics experiments at both places. In Paris, it was relatively easy since my group was well organized there and Jean-Michel had the maturity to lead the group activities during the time I was away. In New Haven, I collaborated with Edward Hinds who was a professor at Yale, and with Dieter Meschede, a former student of a Munich colleague, Herbert Walther. Dieter had accepted the postdoc position I had offered him to help me start a research program at Yale.  We did some very good work during that time and we had some bright students who have had very successful careers thereafter. I also loved the opportunity to teach physics in an environment very different from the one I had at ENS or at the Ecole Polytechnique. American undergraduate students are somewhat younger and less mature than the “normaliens” I was used to training in Paris. They know less mathematics. Paradoxically, that makes them less inhibited than the French students and more prone to ask questions, sometimes naïve and sometimes deep. I enjoyed the interaction with them very much. I stayed there during the fall term, with frequent round trips to Paris. Claudine and the children visited often, while staying most of the time in France where they worked and studied. Julien and Judith had turned into teenagers who loved to visit the US for a while, but wanted to attend school in France, where they had their friends and their habits. Working in both places was thus a kind of state superposition that was hard to maintain coherently over a long period of time. In the early 1990s, I decided it was time to stop this experiment and to come back full time to my group in Paris, which had an ambitious research project to carry out. It is indeed at that time that the ideas about manipulating and observing photons non-destructively were developed, as well as those about preparing Schrödinger cat states of light.  These ideas sprang from long-term collaboration we had with Brazilian colleagues. I had had the opportunity to visit Brazil for the first time in 1983 when I participated in one of the first French-Brazilian workshop in quantum optics, held in Rio de Janeiro. Claudine and I were immediately seduced by the beauty of the country, its relaxed atmosphere and the gentleness of the Brazilian people. Since then, we have been to Brazil at least once a year and often more, visiting many parts of this huge country, with such diverse people and natural beauty. The physicists I met there in 1983 were enthusiastic about the possibilities opened for science in a country which was emerging from long years of dictatorship. Most of them had studied and obtained their doctorate in the US, where they had fled during the dark times. Some became good personal friends whom we enjoy to visit in Brazil or to welcome in Paris. Luiz Davidovich and Nicim Zagury, two quantum optics theorists from Rio, became soon familiar with the Paris Cavity QED setup and worked on the theoretical aspects of our experiments. Luiz was with us in Paris in 1987 when we operated our two photon Rydberg atom maser, the topic of Michel Brune’s doctoral thesis.  Jean-Michel Raimond and Michel Brune were naturally involved in this friendly collaboration. It was during a visit that Jean-Michel made to Rio in 1989 that the ideas about the non-destructive counting of photons first emerged. I remember the excitement we shared while communicating by phone calls and mail between Rio, Paris and New Haven, exchanging ideas and discussing the results of the first computer simulations of photon counting that Jean-Michel was doing with Nicim in Rio. A couple of years later, while Luiz was again in Paris for a sabbatical, we realized that the setup we had in mind to count photons non-destructively could be used to generate Schrödinger cat states of light and to study the phenomenon of decoherence. A 1991 paper in *PhysicsToday*by Wojciech Zurek, which very clearly described superpositions of harmonic oscillator states and their decoherence, played an important role in this context. When reading this paper, we realized that we were in  a  position  to  observe these effects in the laboratory. With our Brazilian colleagues, we wrote a long article in *PhysicalReview*describing in detail the experiments we were planning. For this, we needed to develop new experimental methods for preparing and manipulating circular Rydberg states and new cavities able to trap photons for a very long time. It took us fifteen years to get a setup allowing us to observe the effects we had predicted in the early 1990s. I have described this adventure in my Nobel Lecture.  The years after my return from Yale were very busy. I resumed my teaching at ENS, under an arrangement which was made possible by the creation of a new structure in the French university system, called the “Institut Universitaire de France” (IUF). Being appointed in the IUF means that, while staying at the University which employs you, for a period of time ranging from 5 to 10 years you get a reduced teaching load and some money to travel or to spend on your research. In 1991 I was lucky to secure one of the first IUF positions and could thus spend more time on research at a time when, in the laboratory, we were struggling to build the setup we were dreaming about in order to manipulate photons “in vivo.” Soon after the appointment to IUF, though, the time I could devote to science was reduced by administrative duties. In 1994 I accepted an appointment as chairman of the ENS physics department. This turned out to be a very demanding task, which I assumed for six years. It is also around that time that I was elected  a member  of the  French Academy of Sciences. With great pride, my parents attended my induction in the Academy, under the  famed dome of the Institut de France. My father was already very sick at that time. They lived a few more years and passed away in 1998, a few months apart, after having shared the good and the bad times for more than sixty years.  The 1990s were also the years when ideas about quantum computing and quantum information processing with isolated quantum systems started to become popular and competition with other groups around  the  world  became very strong. With Luiz Davidovich and Nicim Zagury we studied various ways to exploit our Cavity QED setup to perform demonstrations of simple quantum information steps. Some of these ideas have remained theoretical. Others have led to actual experiments. We recognized that our system, in which we were trapping photons was complementary to the one that David Wineland and his Boulder group were working on, in which they were trapping atoms. Some of the experiments we were performing in Paris and in Boulder were very close in spirit. We published back to back two physical Review Letters describing the observation of similar [Rabi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1944/rabi-facts.html) oscillations in a cavity QED and an ion trap. In 1996 we also prepared “[Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/schrodinger-facts.html) cat states” of harmonic oscillators whose features looked alike and we studied their decoherence in experiments bearing strong similarities. It is at that time that David and I became good friends. Claudine and I convinced David and his wife Sedna to join us on a short vacation trip to Italy in the summer of 1996. Since then we have often met in Boulder or in Paris.  In 1999, Claude Cohen-Tannoudji and [Pierre-Gilles de Gennes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1991/gennes-facts.html) approached me to find out whether I would be interested in a position at the Collège de France, a very famous institute to which they both belonged. The Collège de France, founded in the sixteenth century, is a unique institution in the French academic system. I had attended many of Claude’s lectures there and I was impressed by the spirit and the values of this institution, in which the professors give public lectures which have to be renewed each year on the topic of their research. No enrollment is required to attend the lectures and there is no final exam. In other words, to prepare a course at the College de France, there is neither a recipe nor a captive audience whose assiduity might be encouraged by the pursuit of a degree. There are only free listeners, who share the same interest and expectation. Each professor tackles this task with their own personality, shaped by their encounters and singular experiences. It makes for a very diverse institution, gathering a broad range of specialists in physical, natural sciences and humanities. The faculty meets three times a year to decide the opening of new positions and to share their views on matters of learning and culture in a unique atmosphere.  The list of former professors at the Collège de France throughout history is impressive. In physics alone, it includes such celebrities as André-Marie Ampère, Leon Brillouin, Paul Langevin, [Frederic Joliot](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1935/joliot-fred-facts.html), Anatole Abragam, Pierre-Gilles de Gennes and, of course, Claude Cohen-Tannoudji. To be asked to join this club was overwhelming, but I accepted the challenge and paid the traditional pre-election visit to all the professors, fifty in total, to explain them why it was timely to create a chair in the Collège de France on quantum physics. These visits were a unique  opportunity  to meet  colleagues  working in fields  very  far from  my own, which turned out to be an enriching experience. I was elected a Professor at the Collège de France in June 2000 and gave the inaugural lecture of my chair in December 2001. I have since given a new course each year, on various topics dealing with quantum information science. Preparing these lectures has been very challenging, and also stimulating for my research. Having to present subtle phenomena as clearly as possible has more than once led me to conceive new experiments to illustrate some physics concepts.  After a few years of teaching, it seemed timely to collect the material from these courses in a book. With Jean-Michel Raimond, who at that time was teaching quantum information at ENS, we embarked in the task of writing a comprehensive volume describing the physics of atoms in cavities, making connections with related problems in quantum optics. This turned out to be a multi-year project, which resulted in the publication of the book “*Exploring the quantum: Atoms,cavitiesand photons*” in  the  summer  of  2006.  It  included  a  detailed  theoretical analysis of the coupling of atoms with quantized fields in cavities, along with the experiments we had performed up to then. It also described the related experiments that ion trappers, including David Wineland and his team, had realized and also made comparisons with the physics of cold atoms in optical lattices.  The book appeared exactly at the time when our laboratory got an exceptionally good cavity which finally made possible the experiments we had been dreaming about over the previous fifteen years. In the months and years which followed, with Stefan Kuhr and Igor Dotsenko − two exceptionally gifted postdoctoral fellows both coming from the laboratory of  Dieter  Meschede, now working in Bonn, Germany − we were able to observe many of the effects we had predicted in theoretical papers and announced in the book. The no-destructive counting of photons trapped in the cavity, the observation of field quantum jumps, the preparation and reconstruction of Schrödinger cat states of the field in the cavity and the direct observation of their decoherence were published in a fast succession of papers. These results were the topics of the PhD work of a succession of very bright and dedicated graduate students, one of whom, Sébastien Gleyzes, joined us a few years later to become a permanent member of our team.  Jean-Michel and I have often reflected upon the fact that our book was completed just before we could include in it the description of the most demonstrative experiments we have performed, which are only analyzed there as proposals. This calls for writing a revised edition incorporating all these results. But the events of the last few years are, for the time being, distracting us from this task. One of these events has been the CNRS Gold Medal, which I received in June 2009. This distinction is accompanied in France by a lot of media attention, which gave me a foretaste of what was going to happen in 2012. During that year I reached my 65th birthday, which was celebrated by a symposium at Collège de France during which I had the pleasure to meet many friends and colleagues coming from all the world for the event. It culminated with a dinner at the Musée Jacquemard André in Paris, before which we could visit a wonderful exhibition of Renaissance European painting. Reaching this symbolic milestone has been preceded by a sad family event. My youngest brother Michel had died in January 2009, before reaching his fiftieth birthday.  During the last three years, I have been busy with the project of starting experimental physics research at the Collège de France. The Collège laboratory buildings dating from the 1930s have been fully remodeled and we intend to move our research labs there from ENS in the coming months, along with those of Jean Dalibard, a colleague, former student of Claude Cohen-Tannoudji who has just been appointed to the Collège de France. Another professor, Antoine Georges, who is a condensed matter theorist, will join us to make up the Physics Institute of the Collège de France. We hope to attract research teams of junior scientists who will be able to start their independent research career in a favorable environment. It will not be an easy task to build, in the prevailing economy, a laboratory nurturing the kind of values that have allowed my research to develop and thrive when I was young. To succeed, the new institute will maintain a strong link with ENS and the Laboratoire Kastler Brossel, which have kept alive the “time and trust” legacy of Jean Brossel. I find the challenge of building a new research unit on these values to be especially stimulating.  In the fall of 2012, two events occurring a month apart have suddenly made my life, and that of Claudine, more hectic. First, I accepted the post of administrator of the Collège de France. Having received so much from this institution, I considered that it was my turn to take charge of its administration and management, at a time when it was expanding into new directions, especially with the opening of its buildings to new labs, in physics, but also in biology and chemistry. And then, a month later, the announcement of the Nobel Prize in Physics has exposed me to a worldwide media  attention  that  the  CNRS  Gold  medal had hardly prepared me for. I have learned during the last few months that it is essential to be able to say “no” to a lot of solicitations in order to keep some coherence in my life as a scientist and as a person.  Reflecting over the events of the last fifty years, I feel very privileged. On the professional side, I have had the luck to embark in a field – atomic physics and quantum optics – which has undergone  fantastic  developments  over this period of time, improving by many orders of magnitudes the sensitivity of experiments and the precision of measurements. Thanks to advances in laser technology, new domains have been explored, in ultra-low temperature physics or in the study of ultrafast phenomena for instance, that we could not even imagine at the time I was working for my PhD. I did not work myself in many of these fields, but I witnessed these developments as a member of a very active and imaginative community of physicists, sharing the excitement and the bewilderment brought about by all these spectacular advances. And in my own research area of Cavity Quantum Electrodynamics, new developments extending the studies to artificial atoms and to a variety of electromagnetic resonators have kept the subject alive and thriving, with many promising developments to expect in the near future.  But above all, I feel privileged in my personal life. Thanks to Claudine and our children, my interests have extended to many areas beyond physics. By pursuing her own intellectual interests and keeping a very active professional career of her own, Claudine has maintained in our lives a balance between science, arts and humanities which has been very enriching to both of us. Since the beginning of my career as a scientist, as often as possible we have traveled together to conferences and meetings, discovering new places and new people and sharing our impressions about them. We have also taken time for vacation away from physics, visiting the natural and man-made wonders of the world, from Egypt to South Africa, from Patagonia to the Gulf of Saint Laurent, from Angkor to the Maya country, from Machu Picchu to the Galapagos islands. Julien and Judith joined us on many of these trips as children or teenagers and we keep wonderful memories of these special times with them. They are now pursuing their own full and interesting lives in Paris, Julien as a medical doctor doing clinical research in internal medicine and Judith as a lawyer. We are enjoying the pleasure and wonderment of witnessing the development and awakening to the world of Judith’s three children, her twin girls Elsa and Rachel (born in 2005) and her boy, Samuel (born in 2009). I know that Claudine, with her love of privacy and her keen judgment, will help keeping our life in the future not so much different from what it has been up to now. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0034 = SH**  [Serge Haroche] Hello?  [Adam Smith] Oh hello, Professor Haroche?  [SH] Yes?  [AS] Ah, hello, this is Adam Smith from Nobelprize.org, in Stockholm.  [SH] Yes?  [AS] We have this tradition of interviewing new Laureates for just a very few minutes, so may we speak?… Thank you. Congratulations on the award of the Nobel Prize.  [SH] Thank you very much.  [AS] What were you doing when the call came, when you received the call from Stockholm?  [SH] I was walking in the street with my wife. I get ready to get back home and I was just caught by the phone – by the call on my cellular phone.  [AS] What was your first thought on receiving the call?  [SH] My first thought was amazement, you know. I realize think I had this thought, even before I got the phone, because I saw the code ’46’ for Sweden, so I knew the prize was giving today so it’s … I could not believe it! That was my first reaction: I was really amazed and of course it’s wonderful. And, then, I learned about it and it’s starting to sink in. But, it’s very … it takes some time because immediately after that I was swamped with phone calls from all over the world and I’m still trying to recuperate from that and it’s not finished!  [AS] Ha! No, I think it will go on for some time. But, yes, your heart rate must have increased very rapidly when you saw that ’46’.  [SH] Yes, yes! And, I was really overwhelmed by this. Very! And I was also very glad to share it with Dave Wineland, when I heard about the news, because he’s fine and I admire his work very much. We have been in contact with each other for many, many years and so I’m very glad to share the prize with him.  [AS] And, in some ways, there’s this lovely symmetry about it that you trap photons and he traps atoms …  [SH] Exactly, and I use atoms to study the photons and he uses photons to study atoms. So, it’s really symmetrical and, at some point during our work, we published papers back-to-back. Just by chance, it happened that we are doing similar things on his atoms and my photons. And, there are many similarities between our group and his. He’s also working on a very long range project, as I’m doing in Paris with a large team of postdocs, visitors and students. And, I’m sure he shares my feeling by saying that this prize is as much the prize of all of our colleagues who are working with us in our groups than ours. We know the rule of the Nobel Prize – it has to be given to never more than three people so it’s very difficult to share it. But, I want to stress that this is the work of two teams which have been working very hard, with a lot of very bright people in both teams.  [AS] Again, perfect symmetry, because we spoke to him earlier and he did express exactly the same feeling, that it was …  [SH] Sure, and I did not hear his! My assistant [name inaudible] told me that you had already interviewed him but I did not have a chance to hear his interview.  [AS] And, it does seem quite amazing to think that in your laboratory you arrange meetings between single photons and single atoms …  [SH] Yes, we are doing that kind of experiments and for a very long time people were working with huge collections of atoms and photons, and when you work with big ensembles the quantum properties are, so to speak, veiled. They are hidden because of statistical effects. And, if you work with single particles, as we do, then you can reveal the quantum effects in a very dramatic way, and you can learn about all these quantum processes. And, that’s what we are trying to do.  [AS] How long did it take to build the apparatus to allow you to do this?  [SH] Oh, it took a long time! The ideas which gave rise to the prize started about, I would say, more than twenty years ago in our lab. And, I’m sure the same in Dave Wineland’s lab. And, it took us many years to build the apparatus and to improve it to the point that we were able to trap the photons for such a long time. So, it’s a very long term project and I am glad that I was working in an environment which allows for this long range project to be able to mature and to flourish. I was working at the École Normale, in Paris, in a lab where we have very good students from École Normale and from other grandes écoles in France and a lot of postdocs and visitors from all over Europe and all over the world and it’s a very fantastic atmosphere to be able to develop these. And now, I have just changed. I have moved to Collège de France, which is very close to l’École Normale, in Paris, and become the administrator of Collège de France which is a very important task. And, that’s why I do not realize very well what’s happening because I will have to manage my administrative tasks with all the tasks which will come from the Nobel Prize. And, I have to organize my life. For the time being I don’t know exactly what will happen.  [AS] Let’s hope you have a good assistant administrator!  [SH] Yes, and everybody at École Normale and Collège de France are very happy and very helpful and … I hope everything will go well!  [AS] It’s nice that everyone shares in your joy. Just one last question because there is so much emphasis on application, application. But, really the work you do, it focuses on understanding the fundamentals, on understanding this frontier between classical and quantum mechanics.  [SH] Yes, exactly. And, if you were to ask me what was the application, I would tell you I don’t know. And I would just tell you that I think there will be some applications. But, whether these applications will be for the general public or applications which will help to improve some devices which will be used by scientists, it’s not clear. To take an example about the work that Dave Wineland is doing, one part of Dave’s work is to work on atomic clocks and he’s using this single ability to control single particles to develop clocks which are fantastically accurate. And, this precision could be used to develop ways to detect very small effects, like small gravitational shifts, for instance. So, this is one application. In my work I am also using atomic clocks but in a quite different context. I use clocks which are so sensitive to light that they can be used to detect single photons. So, again, there is some kind of connection between his work and mine. But, what will be the use to be able to detect these photons without destroying them, I don’t know. I hope that there will be some applications, but I cannot tell which.  [AS] Ah, but the basic research has to come first anyway, yes. Okay, well, when you come to Stockholm, in December, to receive your prize, happily we have the chance to interview both you and David Wineland together.  [SH] Yes, with pleasure. I’m looking forward to that, yes.  [AS] We are too. So again many congratulations and thank you for speaking …  [SH] Thank you very much, thank you, goodbye.  [AS] Goodbye, thank you. |
| **Interview** |  |
|  | The Physics Laureates talk about the months following their Nobel Prize announcements in October 2012 |
|  |  |
|  | what inspired them as children to become scientists (2:40); |
|  |  |
|  | the importance of mentorship (5:40) |
|  |  |
|  | how to deal with failure (14:10); |
|  |  |
|  | what quantum states are (19:00); |
|  |  |
|  | Schrödinger’s cat (22:30); quantum physics (25:00); |
|  |  |
|  | creating more accurate clocks and quantum computers (27:00); |
|  |  |
|  | curiousity-driven research (33:00); |
|  |  |
|  | long and short-term funding for scientific research (34:00). |
|  |  |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0035** |
| **Biography** | I was born in Wauwatosa, Wisconsin – just outside Milwaukee – in February 1944, two years after my sister Judy. My parents were born in the U.S. in 1903 and 1905; they both had independent careers until my sister was born; then my mother stayed at home to be with my sister and me. My father received an engineering degree from Cal Tech in 1931. He and my mother moved around quite often during the war; at that time my father was working for Allis Chalmers on jet engines. At the end of the war, we moved to Denver, where he returned to a job with the Bureau of Reclamation as a civil engineer. Shortly after that, in 1947 we moved to Sacramento, California where my sister and I grew up and my father spent the remainder of his career.  Growing up, I was always fascinated by mechanical things, particularly anything that had an engine in it. As a kid, this took the form of model airplanes, stimulated by the fact that we lived very near a major Air Force base that was filled with many airplanes from the war, which my friends and I could explore with little restriction. I also liked math from an early age; my father would play simple games like adding, multiplying, etc. in rapid succession – something I could beat my sister at! My parents were definitely products of the great depression; they emphasized the importance of frugality and getting a good education, which could enhance the prospects of finding a good job later on. So, from an early age I knew I was destined to go to college and I always kept up my grades to be able to do that. As a senior in high school, I took my first physics class. I immediately liked the idea that relatively simple mathematics could explain many of the things we see in the world. But at that time I didn’t really jump in; my passion then was cars and motorcycles. With the permission of my father, I bought my first car at 14, a year and a half before I could have a driver’s license, but it gave me time to take it apart and fix it up. Of course, this was partly social; my buddies and I all loved cars and motorcycles and having those close friends with a common interest was a great part of growing up.  After high school, in 1961, I attended the University of California, Davis, in part because of the prestige of the University of California and because Davis is fairly near to Sacramento. I started as a math major and was a bit unsure how I would measure up, but about half way through the first semester I realized that if I worked hard I could be near the top of my class. I now regret that I was too much motivated by achieving high grades, which meant I spent a disproportionate amount of time on subjects in the humanities that were difficult for me, rather than just enjoying those classes and getting B’s. The math and physics came much more naturally.  At that time, Davis was still emerging from being primarily an agricultural school, so in my junior year I transferred to the Berkeley campus as a physics major – to be at the big time! Berkeley was a frightening, impersonal place to me at the time, but I loved it and the challenge it presented. Out of fear of not making good grades, I didn’t become involved in research, so when I graduated, I still had no real idea of what experimental physics was like. My classical mechanics teacher at Berkeley, Fredrick Byron, had a great influence on me. His class was probably the most difficult one I took at Berkeley, but he had the knack of making us want to do the work. Because I respected him so much, I asked him about places to apply for graduate school. He recommended Harvard, so I applied there (and to Berkeley as insurance, because that would have been fine and I was pretty sure I would be accepted there).  In 1965, I started graduate school in physics at Harvard in a class of 26. Because of Harvard’s reputation, I remember thinking uh-oh, 25 geniuses and me! In the end I think there were maybe a couple of geniuses, but the rest were like me, pretty ordinary, but motivated to do well. As much as I still liked how mathematics and physics combined so well together, I was beginning to see that theoretical physics was not for me, so I started to think about experiments. At that time, it appeared to me that the real excitement was in particle physics; it seemed that a new particle was being discovered every other week. The excitement was palpable and it was natural to look in that direction. What frightened me was the prospect of getting buried in a large group and not finding a way to distinguish myself – good or bad. From looking at the low energy experimental groups, it was evident that I would probably be able to have my own experiment if I went that direction. There were several very good low energy experimental groups at Harvard, but in the end I asked to join [Norman Ramsey](https://www.nobelprize.org/nobel_prizes/physics/laureates/1989/ramsey-facts.html)‘s group. Ramsey, with his close colleague Dan Kleppner and student Mark Goldenberg had recently invented and demonstrated the first hydrogen masers. I was attracted to the idea of precision measurements and a not-insignificant factor was that Ramsey had the reputation of being a very nice person. After the initial results on hydrogen, Ramsey wanted to make precise measurements of the hyperfine frequencies of all three isotopes of hydrogen, so I chose to work on deuterium. The experiment was relatively straightforward, complicated a bit by the relatively long wavelength (~ 92 cm) of deuterium’s hyperfine transition relative to that of hydrogen (~ 21 cm).  During that time, Ramsey served as president of the University Research Association, which managed the construction and operation of Fermilab in Batavia, Illinois. Therefore, he was away from Harvard much of the time. As a consequence, most of us graduate students would help each other along and I liked that experience very much. However, since we were left alone, it sometimes took a while for us to figure out when we made a wrong turn in our projects. In spite of Ramsey’s commitments at Fermilab, he would nearly always make a point to return to Harvard for our weekly group meetings. Moreover, whether or not it was deserved, he always supported us and offered encouragement. Ramsey had a very intuitive approach to physics, and was able to make simple pictures that explained the essence of the physics without getting bogged down in mathematical complexity; a lot of the physics we explored could be explained by mapping the problem onto that of a spin-1/2 magnetic dipole in a magnetic field. By the time I finished graduate school, I was hooked on highresolution spectroscopy and precision measurements. However, I still wasn’t completely immersed in physics and took many Sundays in graduate school and as a postdoc to realize a long-term dream of racing motorcycles. I had a lot of fun with it but it was clear it was not a way of the future for me.  During the time I was a graduate student, I read about the precision 3He+ hyperfine spectroscopy and electron g-factor measurements of [Hans Dehmelt](https://www.nobelprize.org/nobel_prizes/physics/laureates/1989/dehmelt-facts.html) and his colleagues at the University of Washington in Seattle. I was very attracted to ion spectroscopy, but I joined the group to work on Dehmelt’s electron magnetic moment experiment. Fred Walls had started the electron experiment as a graduate student with Dehmelt and would later become a colleague at the National Bureau of Standards (NBS). After a while working on the experiment, it became clear that systematic shifts would be smallest for a single trapped electron, so obtaining that goal was my primary project while at Seattle. Dehmelt also had great intuition and was able to reduce all problems to simple semi-classical pictures. Similar to Ramsey, it seemed he could map nearly all problems onto a tuned circuit. I’ve tried my best to copy their intuitive approach and many times it has served me well. During my tenure at the University of Washington, I met and married my wife, Sedna. Her father, George Quimby, was an anthropologist and director of the Burke Museum, so she was used to and understood long hours spent at work. Her support of this has always been extremely important to me. We have two sons, Charles and Michael; neither are physicists – probably one in the family is enough! In Seattle, I also became good friends with fellow postdoc Bob Van Dyck, who later took a faculty position there and was the key person on the electron magnetic moment experiment. When I began looking for a permanent job, I had my heart set on finding an academic position. However, after about a year of looking, it became clear that the academic positions offered to me would not be able to provide sufficient funds to start a viable experimental program.  Fortunately, a position opened in the Time and Frequency Division of NBS in Boulder, Colorado, and with my background and interest in clocks, I was more than pleased to find a home there. When I arrived, NBS did not have an operating accurate Cesium beam clock, so there was some urgency to achieve this. David Glaze from the division had built the newest version of the Cesium beam clock, NBS-6, and he and I worked to calibrate the device and produce the unit of time, the second. This took about a year and a half. At that time, the Time and Frequency Division had very little research activity. Fortunately my boss, Helmut Hellwig, had a vision that the Division should be doing more basic research and prior to my being hired, he had hired Fred Walls, who had spent a postdoc at JILA, to start research on hydrogen masers. After the Cesium frequency standard was made operational, Helmut was able to get some internal support to start an experiment on laser cooling. Bob Drullinger, who had experience with dye lasers, joined in the project, and with my and Fred Walls’ experience with ion traps, we were off and running. This was a very exciting time for me, because it was a project of our own choosing, and it would be great to realize an earlier idea of laser cooling that I had developed with Dehmelt. We started with an empty lab in the summer of 1977, but by the spring of 1978 we had our first results. I knew that we had competition because I was aware that Dehmelt had taken a sabbatical to work in Peter Toschek’s lab in Heidelberg, with the same goal of demonstrating cooling. I didn’t know at what stage they were at in their experiment and I don’t believe they were aware of our experiment. Our paper was published a bit earlier than theirs, but in a near coincidence – even with no contact between the groups – our papers were received at Physical Review Letters within a day of each other (the Toschek group beat us by one day!).  Bob Drullinger and I were subsequently joined by Wayne Itano and Jim Bergquist on these projects and few years later by John Bollinger. We have spent nearly our entire careers working together or on closely related projects. This has been a great experience for me because they have been such great colleagues as well as friends. One of the primary and continuing goals our work has been to make better atomic clocks, and laser cooling has been important to suppress relativistic time dilation in these experiments. In 1985 we demonstrated the first clock that employed laser cooling, which was based on a hyperfine transition in 9Be+ ions. In 2006, Jim Bergquist and colleagues demonstrated the first atomic clock whose systematic errors were smaller than those of Cesium. This clock was based on an optical transition in a single trapped 199Hg+ ion, perhaps signaling the advent of high precision optical clocks.  Chris Monroe (now at the University of Maryland) was a very important part of our group from 1992 to 2000, during the time we were building up our experiments on quantum information. After Ignacio Cirac and Peter Zoller’s 1995 proposal for a quantum processor using trapped ions, we were able to quickly make a demonstration of the two-qubit logic gate described in their proposal. Didi Leibfried joined our group shortly after that; first as a postdoc and later as a permanent staff member. Didi and I continue to work together to explore the use of atomic ions in quantum information processing. Till Rosenband, who joined the group as a permanent staff member in 2006, has masterfully developed an optical clock based on 27Al+ ions; it is currently the clock with the lowest systematic error of around 1 part in 1017. At this level it is possible to observe relativistic time dilation for ions moving at the speed of a fast runner.  In 1988 NBS became NIST, the National Institute of Standards and Technology. In my 38 years at these institutions, our group has always had great support from our immediate supervisors Helmut Hellwig, Sam Stein, Don Sullivan and Tom O’Brian. We are also indebted to our laboratory director, Katharine Gebbie, for her support and encouragement. One measure of her success is that I am the fourth NIST person, after Bill Phillips, Eric Cornell, and Jan Hall, to receive a Nobel Prize during her tenure as lab director. Perhaps a good example of how this support has paid off is laser cooling, which started as a basic research project, but is now employed in all accurate atomic clocks.  It hasn’t been all work with my lab-mates in Boulder. I’ve always liked outdoor activities and for example, Jim Bergquist and I played together on volleyball and softball teams and have ridden bicycles together since the mid 1980s. I still like things with motors on them; unfortunately I don’t really have time for cars and motorcycles but I can still sometimes find an hour or so in the evening to build free-flight model airplanes.  Since receiving the Nobel Prize, I’ve often been asked for advice to give to young students. Of course there’s no one answer that fits all, but for me, because of my upbringing, it’s been pretty simple. I would suggest finding something interesting (even if you change your mind) and give it your best possible effort. That means hard work, and although not everybody above you will appreciate it, most of them will recognize it and support you. And, as nice as it is to be recognized for accomplishments, I think the biggest reward for me has been just to have the opportunity to explore new ideas. The physics has never been a job; it’s more like a hobby – and just the process of doing research is extremely interesting and rewarding. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0035 = DW**  [Adam Smith] Oh hello, I’m sorry to call so very early, may I speak to Professor David Wineland please?  [David Wineland] Yes, this is he.  [AS] Oh good morning, my name is Adam Smith, from the Nobel Prize website in Stockholm. We have a tradition of recording extremely short interviews with new Laureates. Would you be able to speak for just a very few minutes?  [DW] [Laughs] Sure. Okay.  [AS] Thank you very much indeed. First of all, of course, our sincere congratulations on the award of the Nobel Prize.  [DW] Oh, thank you.  [AS] I know it’s extremely early in the morning, in fact the middle of the night there. What were you doing when the call from Stockholm came?  [DW] Well, I was sleeping, and my wife got the call and woke me up.  [AS] [Laughs] Do you recall your initial reaction?  [DW] Well, I mean a wonderful surprise, of course. Yes, just amazing, sure.  [AS] I imagine it’s thrown the house into some kind of disruption there.  [DW] Well, we probably won’t go back to sleep for a while [Laughs]. Yeah.  [AS] I guess there’s the normal business of life to run along side handling the press that are about to descend on you.  [DW] That’s right, yeah.  [AS] Your main interest, I gather, is developing far more accurate clocks and that’s what you’ve been devoted to in your career.  [DW] That’s been the main theme. Yes, but there’s been many spin-offs from that, including the work on single atoms.  [AS] And the trapping of single atoms, this allows you to observe the superposition of quantum states?  [DW] That’s right.  [AS] This is basically observing, if you like, the frontier between the classical world where the states don’t superimpose and quantum states where you can have multiple states at the same time.  [DW] Well, you might say that. That’s right, yes.  [AS] How do you trap the atoms?  [DW] Well, in our case the atoms are ions, charged atoms. So we use electric fields to hold them in one place.  [AS] And then you use laser beams to manipulate them, is that correct?  [DW] That’s right.  [AS] Tell me more, please, about why need more accurate clocks.  [DW] Well, I think historically it’s always been true that when we’ve made better clocks there’s always been an application. The main use throughout history for the last many centuries is that clocks are used in navigation and the better clocks we have the better navigation we can do. So that theme has carried through for many centuries. As we make better clocks that’s still been the primary application. These days also the timing, the precise timing, you know, by good clocks is also used in communication. But historically the main use has been and continues to be in navigation.  [AS] And how accurate are our most accurate clocks now? You have this mercury ion clock.  [DW] Currently the most accurate one is also in our lab. It’s based on aluminium. And accuracy meaning, you know, how well we can control the environmental effects and so on, is at about one part in 10 to the 17.  [AS] [Laughs] And how long can you keep it running for? Is it indefinite?  [DW] Well, so far these are laboratory devices. So they do not work continuously, but they can work many hours and days to produce these results.  [AS] So the other application that is often talked about is quantum computing.  [DW] Right.  [AS] And does your work take us a step closer to quantum computing?  [DW] Well, I think you might say that. But in the same breath, you have to say that it’s a long way before we have a useful quantum computer. But I think most of us feel that even though that is a long, you know, long way off before we can realise such a computer, many of us feel it will eventually happen. It’s primarily a matter of controlling these systems better and better. Both Serge Haroche and I work on atoms. There’s many other platforms and condensed matter where this might happen. But wherever it happens I think we believe that in the long run we should be able to [inaudible] well enough to realise such a device.  [AS] May I ask you, you work at the National Institute of Standards and Technology and it seems to be a hotbed for the production of new inventions and, indeed, Nobel Laureates. What is it that’s so special about this place?  [DW] I think that, one of the things, you know, certainly is the people, my management. People above me have been very supportive of these things. You know, it couldn’t happen without that. I think supportive management, and it helps being around very good people. That’s made the difference.  [AS] Must be a very exciting place to be. And, indeed, it must be extraordinarily exciting looking at this new frontier, being the first to observe this new world of quantum states, which haven’t been previously observable.  [DW] Well, I wouldn’t say … I wouldn’t put myself in the first, you know, maybe we’re among the first. But there’s many good people working on these things though. It’s certainly a big enterprise by now and many people are working on this in this area.  [AS] Sure, but there must a constant thrill of excitement, of feeling you’re on uncharted territory.  [DW] Well, yeah, that’s been really true in science, to be near the leading edge, I suppose. Yeah, it’s always been great, really exciting to be in this field.  [AS] Thank you so much for talking to us. Again, apologies for calling in the very middle of the night …[both laugh] … when you come to Stockholm in December we have the chance to interview you at a greater length, which we very much look forward to. But for now, I wish you the very best of luck with what will surely be an exciting day.  [DW] Well, thanks very much. All right, thank you.  [AS] Thank you. Nice to speak to you. Bye bye.  [DW] Bye bye. |
| **Interview** |  |
| Q4 | **Could you describe your Nobel Prize awarded work for young students?** |
|  | Peter Higgs: Imagine a snowfield, and that is an analogous to this background field throughout all the universe, this affects the way that people crossing it in different ways depending on whether they wear skis or snowshoes or just boots. The analogy is then that the people with skis relatively unaffected and untroubled with high high speed, people with snowshoes do not quite so well and the people who just wear boots go very slowly and that analogy is with the effect on some kind of particles which continue to travel this speed of light and the massless and what happens to particles which are heavier, but to me that contains less of the physics than my more roundabout explanation. |
| Q4 | **Can you explain the standard model?** |
|  | Peter Higgs: The work which was done in 1964 led to the so called electroweak theory, the unification of weak and electromagnetic interactions in elementary particles which was done in 1967 by [Weinberg](https://www.nobelprize.org/prizes/physics/1979/weinberg/facts/) and [Salam](https://www.nobelprize.org/prizes/physics/1979/salam/facts/) by taking a theory, which had the right kind of unification but couldn’t produce good calculations due to [Glashow](https://www.nobelprize.org/prizes/physics/1979/glashow/facts/), and combining it with the kind of models of symmetry breaking which we had discussed in -64. That was the beginning of the standard model, because once that theory was shown to be mathematically sound, that you could really calculate with it, people started to study other kinds of so-called gauge theory in relation to the other forces in particle physics and those investigations led on to a theory of the strong forces, called quantum chromodynamics. So the work in -64 was the beginning of the return of this kind of so-called quantum field theory in particle physics. It had previously been successful in the 50s, quantum electrodynamics, and then became neglected in particles that didn’t seem to work and what we did was a step on the way to making it work. |
| Q2 | **At what point did you realize your work was a breakthrough?** |
|  | Peter Higgs: It wasn’t a precise moment. The way in which I came to this realisation was that I was trying to evade a theorem which had been proved about this way of breaking symmetries in particle physics which implied that there would exist massless particles without spin. This theorem made this kind of theory unacceptable because such particles were not known. The theory in which these occurred had been formulated four years earlier by [Yoichiro Nambu](https://www.nobelprize.org/prizes/physics/2008/nambu/facts/) who got a share of the 2008 prize and Jeffrey Goldstone and it was really for me a matter of realizing that the theorem as proved had a flaw in it, there were certain mathematical axioms which you need to prove any kind of theorem which certain kinds of theory of fields didn’t obey. And the prime example of the kind of field which didn’t obey these axioms was Maxwell’s electromagnetic field as it occurs in quantum electrodynamics.  Quantum electrodynamics is a theory which doesn’t involve this phenomenon of symmetry breaking but once it was clear that there were fields of the maximal type which didn’t obey the axioms, then the way was opened to introducing these fields of this type into the kind of theory which Nambu started of symmetry breaking and that’s essentially what happened to me over a weekend during which I gradually realised that I knew two things which had to be brought together. It was related to the fact that I had read papers not long before by [Julian Schwinger](https://www.nobelprize.org/prizes/physics/1965/schwinger/facts/) who was one of the people who shared the quantum electrodynamics prize and that was -65 I think. He had a way of formulating that theory which was a little bit different from what most people preferred. It resulted in some equations which were explicitly  violating, apparently violating the rules of [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) relativity theory but the physics was not affected by this, it was just a peculiarity of the formal mathematics and so it was my recollection of equations written by Julian Schwinger which made me see that this was what had to be done. But it was a process which wasn’t sudden, it was during the weekend and I had to go back to my office on the Monday and check that I hadn’t made a mistake about this. |
| Q5 | **Who is your role model, and why?** |
|  | Peter Higgs: In my high school days I didn’t find the physics that I was taught very interesting. I was better at mathematics and chemistry, my scientific subjects, and I was quite enthusiastic about chemistry, understanding the structure of matter of the molecular level. Gradually I came to know that there were deeper levels and that these were classified as physics and there were interesting things to do in theoretical physics there. One of the influences on me which probably a former pupil at the same school of about a quarter of a century earlier, [Paul Dirac](https://www.nobelprize.org/prizes/physics/1933/dirac/facts/), who was one of the founding fathers of quantum mechanics in the mid-1920’s, and I was curious about what he had done because his name appeared frequently on the roll call of the achievements of former pupils. I was curious and that led me to read about atomic physics and quantum theory before I was ever taught them.  The only other thing perhaps to add about influences in my days at high school was that at the end of my time there, it was very soon after the end of the war and the dropping of the bombs on Japan. I went to some public lectures in the University of Bristol organized by the two professors of physics, one theoretician and one experimentalist. These were lectures for the public to tell them what was the background of the development of these bombs and it was a great success series of lectures. The experimental physicist was [Cecil Powell](https://www.nobelprize.org/prizes/physics/1950/powell/facts/) who worked in experimental particle physics, in those days it involved sending packages of photographic emulsion up into the upper atmosphere with the help of balloons, and he decided to give some lectures about his own work which I then went to when I learned from him a lot about the current state of experiment in particle physics and that helped me to see what I wanted to do. |
| Q9 | **What were you doing when you got the message of being awarded the Nobel Prize?** |
|  | Peter Higgs: When the prize was announced I wasn’t at home and that was deliberate, I wasn’t trying to avoid the people from the Nobel Foundation or the Academy, but I was trying to avoid media attention which I expected would follow very rapidly. So I made sure to go out at 11 in the morning with the expectation that the announcement would be maybe 11.30 or something like that and I also went down to the harbor area Edinburgh for a lunch, went to an art exhibition and came back home at about 3 o’clock and I was told the news first by a former neighbor who stopped her car which overtook me as I was walking home and came across the street and said “Congratulations, my daughter phoned me from London to tell me about the award” and I said “what award?”. She gave me the answer I expected so I then went home and listened to my phone messages. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0036** |
| **Biographical** | My four grandparents all immigrated as young adults to the United States from Eastern European Jewish towns and villages early in the twentieth century. This was a generation of poor but optimistic intellectuals, who expected that the newly rationalist world would use education and creativity to leave behind boundaries, borders, and religion to build a better world. (My mother’s father, for one, although he earned a living welding in the shipyards during the Second World War and later ran a sandwich stand, was by avocation a self-educated scholar and teacher of Yiddish literature and history.) It is perhaps not surprising then that their only children, my parents, both became professors, my mother, Felice Davidson Perlmutter, in Social Work and Social Administration, and my father, Daniel D. Perlmutter, in Chemical Engineering.  My mother’s work life was full of research collaborations that she organized and enjoyed professionally and personally. The social work field appeared full of warm, friendly people who took great pleasure in working with each other. My father’s work involved careful, accurate calculations with slide rule and graphs (both attractive to me as a child), and experiments with lab equipment – and graduate students whom he taught patiently and mentored. On weekends our home was full of my parents’ friends, discussing politics and movies, books and arts late into the night. Concerns about social and political conditions of the world were a constant theme.  In this atmosphere, I grew up wanting to know all the “languages” – music, literature, math, science, symbols, architecture, psychology – particularly those that seemed most universal. I thought *everybody* needed to know what was in the missing “Owner’s Manual” of the Universe, since we lived here and “used” it. The Quaker schools my two sisters, Shira and Tova, and I attended encouraged creativity, critical thought, and social concerns; there were excellent teachers in the basic math and science courses but (at the time) not much beyond the fundamentals to encourage further creativity in these areas. Instead, I was excited to learn to write clearly and expressively, and to become familiar with the non-science approaches to the world.  As with many scientists, music was an important part of my life. I don’t know if there is a documented relationship between musical and scientific thinking, but there certainly seem to be shared elements, perhaps starting off with the delayed gratification inherent in practicing an instrument. In my case, my violin teacher Frances Duthie (the only teacher who was a constant throughout my school years) certainly taught a strong ethic of perfectionism, but in the service of a warm, humanistic tradition of interpersonal communication, and the chamber music ideal of collaborative, shared listening and contributing. The pleasure of group music-making continued though string-quartet playing and group singing throughout my life – and my favorite science group experiences share some of the same feeling. (Many years later, as a professor at Berkeley, I was glad to have the opportunity to design a “Physics & Music” course for undergraduates.)  By the time I reached college, I thought I would follow my fascination with the big Philosophical Questions. As I saw it, the two big mysteries are: How does the world work? and How does the mind work? (The latter was also relevant to the former, since we only perceive the world through our minds.) So starting Harvard, in 1977, I considered double majoring in Philosophy and Physics – until I realized that if I did this there would be no time for courses in Humanities and Social Sciences. I decided to major in Physics first, since it seemed more likely that I would later come back to Philosophy than the other way around. (We’ll see.)  College then became a chance to take courses in many fields. I discovered a strong interest in the then relatively-new area of Cognitive Sciences. I found out that Biology was not the boring field full of memorization that I had taken it to be, but in fact new exciting concepts were being discovered every day. I learned that Physics is a very social activity, with groups of friends and roommates meeting every week to solve the math and physics problems sets. I also found that there is barely any time to digest the implications of the physics that you learn as an undergraduate – and when I graduated in 1981 I was curious to go on to graduate school and have a chance finally to focus on the physics.  I chose to go to the University of California, Berkeley, for graduate school because I wanted a wide variety of strong experimental research groups to choose from, and this was something that Berkeley appeared to offer well beyond the other top physics departments. When I started grad school, my goal was to find a research project with real data (not just theory) that would address a deep philosophical question. The most likely route for this was a particle physics accelerator experiment, and I expected to end up doing this; but first I thought I should look to see if I could learn on a more small-group project. In my second year was lucky enough to find an unusual, dynamic, eclectic research group led by Professor Richard Muller.  Rich followed – perhaps outdid – the tradition of his mentor [Luis Alvarez](https://www.nobelprize.org/prizes/physics/1968/alvarez/facts/) to pursue interesting research topics wherever they arose, with a can-do experimentalist’s attitude – and rigorous skepticism of all claims. When I joined the group, the group members were reporting at the weekly meeting on their work: (1) an idea for a new fundamental physics experiment, measuring the gravitational deflection of starlight by Jupiter; (2) a robotic-telescope supernova search to measure the Hubble constant; (3) a Raman scattering measurement of atmospheric carbon to study the carbon cycle; (4) a table-top cyclotron for radioisotope dating; (5) cosmic microwave background measurements searching for anisotropy; and (6) sundry topics, including implications of the impact theory that Luis had recently developed with his son to explain the extinction of the dinosaurs. This was the tradition of physicists at their best: playing with ideas, building toys. (Caution: physicists at play!)  I soon focused on the robotic-telescope supernova project, since it seemed to offer the possibility of a fundamental measurement, the Hubble constant. I ended up developing the software (and a little hardware) that made it possible to automatically identify the supernovae in the images, and rule out sources of confusion such as asteroids and cosmic rays. I had always enjoyed playing with computers, and the latest computers were just getting fast enough and with enough memory to make possible the near-real-time image analysis that triggered the follow-up of the supernovae. By the time I graduated with my PhD in 1986 the automated supernova search was successfully up and running, and I was asked to stay on as a postdoc to get the results from this project. (Along the way, I had of course been drawn into the myriad topics of Rich’s group, and my thesis actually used the same robotic telescope and image analysis techniques to search for a possible companion-star of our Sun, which had been proposed to explain *periodic* mass extinctions on the earth every 26 million years – the extinction of the dinosaurs was just one of these.)  In 1980 when Rich had begun the Hubble constant project, it looked likely that the so-called Type II supernovae would be used as a distance indicator for the measurement, with a calibration based on the Baade-Wesselink expanding photosphere method (later studied by my co-Laureate [Brian Schmidt](https://www.nobelprize.org/prizes/physics/2011/schmidt/facts/)). But by the time the project was up and running in 1986, there was evidence (presented particularly strongly by Gustav Tammann and his student Bruno Leibundgut) that the new sub-classification of Type “Ia” supernovae could be used as an alternative (perhaps better) distance indicator. This news prompted Dr. Carl Pennypacker (a more senior researcher in Rich’s group) and me to think about possible implications for new projects. The Type Ia supernovae were significantly brighter than the average Type II, so they could in principle be studied at much further distances. Ever since the first supernovae were studied in the 1930’s there had been the hope that they could someday be used to measure the deceleration of the universe’s expansion – now the uniformity of the new Type Ia subclassification opened up the possibility that this idea could be revisited. And now we also had several new tools at our disposal to exploit the Type Ia supernovae: for the robotic supernova search we had worked with the very first generation of “charge-couple-device” (CCD) detectors to be used for ultra-sensitive astronomical imaging, and we had developed the image analysis tools that could sift through the large amounts of digital data that the CCD’s produced.  Late in 1987 Carl and I thus proposed a new project: we would design and build a new wide-field camera, the widest ever with a CCD on a 4-meter class telescope, and develop the software to search through tens of thousands of galaxies in one night. Unlike our previous robotic nearby supernova search, which studied one galaxy in each image, this approach would allow us to look at thousands of much more distant galaxies at a time. We estimated that in several years we could in this way discover sufficient numbers of supernovae at redshifts as high as ~0.3 that we could make an excellent measurement of the deceleration parameter. The project got off the ground in 1988 with the support of our skeptical mentor, Rich, and was one of the founding projects of Berkeley’s new Center for Particle Astrophysics.  The project began slowly with only two-and-a-half nights of good weather out of more than a dozen nights scheduled over almost two years at the Anglo-Australian 4-m Telescope in Siding Springs, Australia. Still, by 1992, when I was asked to take over from Rich as leader of the supernova research group, we had found a Type Ia supernova at z = 0.45 – doubling the world’s high-redshift sample. This was at the time the highest redshift supernova known. (The other high-redshift type Ia SNe was the one found in a several year search by a Danish team led by Norgaard-Nielson.) In the next two years, we parlayed this success into access to other large telescopes (with somewhat more reliable weather).  Two key problems stood in our way: relating brightnesses of high- and low-redshift supernovae (measured in different filters); and guaranteeing distant supernova discoveries in advance – and in time to measure their peak brightness. Without such a guarantee, one could not obtain time on the large telescopes needed to study them. By 1994, we had solved these problems and we were able to guarantee entire “batches” of multiple high-redshift supernovae, all still brightening, and all found on a pre-selected date, perfect for scheduling the measurements of brightness and spectrum. Such “guarantees” led us to propose a novel use of the Hubble Space Telescope: precision measurements of distant supernovae, particularly important for the ultra-far z~1 supernovae that collaborator Ariel Goobar (then a postdoc in Berkeley, now a Professor at Stockholm University) and I had shown could be used to distinguish among cosmological theories.  Meanwhile, between 1990 and 1993 several approaches had been developed to further calibrate the Type Ia standard candle: David Branch showed that selecting by color could give a standardized set, while Mark Phillips showed that a relationship could be established between the peak luminosity and the timescale of the brightening and fading lightcurve. A beautiful dataset of low-redshift supernovae had been found in the Calan/Tololo supernova search (led by Mario Hamuy, Jose Maza, Mark Phillips, and Nick Suntzeff) that allowed these improved calibrations to be made. So, by late 1994, after establishing that we had an effective approach with our batch discovery and multi-band follow-up of high-redshift supernovae, our now-international team of scientists was working together round-the-clock, collecting new batches of high-redshift supernova data using the best telescopes in the world for the purpose. And so was a new, competing team of experienced supernova researchers, organized by co-Laureate Brian Schmidt.  Finally, in 1997, we were analyzing our haul of 42 Type Ia supernovae at redshifts about *z*~0.5 and finding an odd result: the universe’s expansion was apparently dominated by a cosmological constant, or more generically a “dark energy” pervading all space, so it was actually *speeding up*– this didn’t fit with known models of physics! We announced this startling evidence for a cosmological constant at the American Astronomical Society January 1998 meeting**.** Because both our team and Brian’s team -including co-Laureate [Adam Riess](https://www.nobelprize.org/prizes/physics/2011/riess/facts/) – independently announced matching results at conferences in the beginning of the year, by the end of the year most of the scientific community had accepted the extraordinary findings.  When we started the project we thought that whatever answer we found would be exciting: if the universe were decelerating enough we would know that the universe is finite and is coming to an end in a Big Crunch; if not then we could establish that the universe is likely infinite in space and time, and the inflation theory would have a successful prediction. We could not have hoped for the actual outcome, a surprise that presents a new puzzle to fundamental physics. This is the sort of conclusion to a project that in turn initiates many new projects. We now have the fun of trying to figure out what it is that causes the universe to accelerate. Since 1999 I have been working with colleagues on such new projects, including the development of a new space telescope that can make a much more precise measurement of the expansion history of the universe.  Perhaps when my wife, Laura Nelson, and I send our now eight-year-old daughter, Noa, to college, science will have the next installment of answers – or, better yet, new surprising questions about our world. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0036 = SP**  [Adam Smith] Hello, this is Adam Smith.  [Saul Perlmutter] Hi, this is Saul Perlmutter returning your call.  [AS] How very kind of you to call. Thank you, and congratulations on the award of the Nobel Prize.  [SP] Thank you, thank you. It’s a real honour.  [AS] So, as I think has been explained to you we record these very brief interviews for Nobelprize.org, so could we talk for just a few minutes?  [SP] Okay, surely!  [AS] Perfect, thank you. So, it’s very early morning in Berkeley. What were you doing when the news came?  [SP] [Laughs] We were sound asleep! And we got the first call from a reporter from Sweden, actually, who asked me how I felt. And I said “How do I feel about what?” And he told me that we’d won the Prize and my wife, of course, rushed to the computer to check to see whether this was a hoax!  [AS] And happily it wasn’t, yes! [Laughs]  [SP] Exactly!  [AS] So, taking you back to another moment of surprise, in 1998 you came up with this amazing result that the expansion of the universe was accelerating. Was there a sudden moment you realised that you’d come upon this, or was it a slower “Aha!” moment?  [SP] Yes, as I’ve been saying to people, I think this has to be the slowest “Aha!” in the history of science, in that nowadays, when you’re looking at data, it’s a very complex task to interpret it very carefully, and calibrate very carefully. And the first job we had when we saw the data coming in was to say “Ah, well, it looks like good data, and this should be nice, but right now it doesn’t make any sense. You know, the plots all look a little bit strange. I’m sure that once we finish all the calibration it’ll go away – it’ll all seem logical.” And of course, the more we calibrated it, the more the surprise sharpened up, and so by 4 months in, you really started to believe that this is a big deal. It’s a very different result than anything we ever expected. And, yes, by then, of course, you’ve been staring at the data for 4 months, so it’s hard to call it the same kind of surprise that people sometimes imagine.  [AS] Yes, yes. But there must have been a tension between wanting to be sure and wanting to be first, because you were in competition with the other team?  [SP] Yes, I think we still thought that we were very far ahead, in that we had many, many more supernovae that we had measured at that point. And so we didn’t – I don’t think we quite realised how close they were to seeing the same results. Because they were just working on this small data set at that stage. But we also felt very strongly the tension between wanting to announce an important answer, but also wanting to make sure that whatever you announced, people would believe it and that you would believe it, and that you had done all the homework necessary to make it make sense.  [AS] And did people believe it as soon as you announced it?  [SP] Well, I think that the first response would have been that it was harder to believe, but within 6 weeks the other team had also announced the same basic results. And so at that point, I think people started to believe it, because they knew that the two teams were fierce rivals and would have been very glad to prove each other wrong if it had been possible!  [AS] Yes. And so this accelerating expansion means that there has to be something, which has basically been termed dark energy, that’s pushing the universe apart. Is there any idea what that is yet, do you think?  [SP] No. Not only do we not know what dark energy might be, that would be making the universe expand faster and faster, we don’t even know whether really the answer will turn out to be a new energy in the universe. It’s possible that we’ve just discovered an extra wrinkle in [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/)‘s Theory of Relativity, and that that would be the real final result. But at this point, the job is really back in our court again as observers, and we have to come up with more data that will help narrow in on what the answer is.  [AS] Because the theoretical people have been proposing solutions at a great rate, but more data is needed to know which if any of these proposals is correct.  [SP] Exactly. I think the papers have been coming out on the order of one per day for the last 10 years, from the theorists. And of course, all these theories can’t be right! But the other thing is that the theorists themselves I think would tell you that they don’t believe any one of their theories. They’re just trying to expand the range of possibilities. And if it turns out that, you know … What they’re really looking for is for us – the observers and the experimentalists – to come up with some more data to help home in, help narrow in, on which range of possibilities could be right.  [AS] It seems to be one of the lovely things about physics; that sometimes it’s the time of the experimentalists, and then it goes back to the theorists, and then back to the experimentalists again.  [SP] No, no, it makes for a great tennis game of science!  [AS] [Laughs] And so what do the experimentalists have to do next? What is the next big challenge?  [SP] So our job now is to develop ways to measure this very subtle effect. It seems like a big deal, something that can power the expansion of the universe, but it only makes a difference over the timescale of a billion years, and billions of light years. So now we have maybe three different techniques that we could use to get at what’s going on. And each of them is a very challenging experiment that will require building new instruments, and perhaps even a new space telescope.  [AS] So in looking for these subtle effects, do we have to look at the fringes of the universe, or can we build experiments here on earth that will allow us to see it?  [SP] So far, nobody’s figured out any way that we could detect it locally. So far, as far as we know, it takes very large distances and billions of light years to be able to see enough of the effect.  But, who knows, it may turn out that there’ll be new ideas that we haven’t invented yet.  [AS] Splendid! When you come to Stockholm in December to receive your Nobel Prize, we happily have the chance to interview you at greater length, and we can talk more about this, so just a last question: when do you think you’ll be free to celebrate? I imagine you’re in the grip of the press now.  [SP] Exactly. I’m hoping that things will calm down by the end of the day, and maybe we can have a quiet evening and invite friends over to sit around and enjoy it!  [AS] Okay [Laughs]. And look at the stars perhaps!  [SP] Exactly! [Laughs] We’re having one of our first rains of the season, so we might not get many stars tonight.  [AS] [Laughs] Okay. Well anyway, congratulations once again, and thank you so much for speaking to us.  [SP] Well thank you, it’s a pleasure. Bye bye.  [AS] Bye bye. |
| **Interview** |  |
| Q2 | **I suppose that one could make some complicated joke about the pace of the recession and all these lights and the astronomers, but we won’t go there. You are here because you made this remarkable discovery about 15 years ago, that the expansion of the universe is accelerating and up until that point people had thought that the expansion of the universe was either preceding at a steady rate or might be slowing down. And yet this remarkable discovery was welcomed by the committee, people didn’t say ”No, this can’t be” but rather they said ”Yes”. Why? Why did they accept it?** |
|  | Saul Perlmutter: I think it was in this particular case there were two things going for us, and I will say one of the fastest acceptances of a breakthrough question, breakthrough issue in science topic. But the two things where one is that there were two teams that were gaining these results so the fact that both Brian’s, Adam’s team and my team were saying the same things and we were known to be in a very tough competition with each other and that we would have been very happy to call each other out, make a mistake. That, I think, was one of these cases where they got an immediate confirmation instead of having to wait for the years it would often take for another team to make a confirmation. That was one of the practical aspects of it. There was also a part; it certainly made a lot of other problems fit, so suddenly things fell into shape. I am sure we had all been hearing many complaints and worries about how is it that the universe is younger than the oldest star and these other issues that were floating around and suddenly those went away.  Brian P. Schmidt: There was a very strong theoretical view of how the universe should be. As an observer I just said ”They have it wrong” but it turns out this theoretical view of what the universe should be like was sort of confirmed by the observations of the accelerating universe. That section of the community I think were very happy to see it. Whereas the observational side, the people who actually go out and make experiments like to have things be as simple as possible. I think it was more skepticism on that side but the joint teams finding the same thing I think helped erode a little bit of that skepticism from that side of the community.  Adam G. Riess: Just to follow up on what Brian was saying with this theoretical preference. The preference was that there would be a certain amount of energy or matter in the universe and so we kept looking for the matter. After a while of looking we could only find about 30% of it, there was a hook that if we looked further out, we would finally find it. We didn’t but what we found was the other 70%. It was in a funny form, this funny kind of energy that we call dark energy. As difficult as it was to understand some theorists were kind of happy that they said “Well, anyway you complete the picture, we are happy to get to the 100%”.  Saul Perlmutter: I mean, it is funny because I think there was one group of theorists that I think were telling us ”Why are you bothering to do this measurement when you already know what the answer is going to be” whereas most of the observers in the world at that time would say ”Well, you can’t trust the theorists, they have been telling us all sorts of things that turned out to be wrong once we actually did the measurement.” |
| Q2 | **You have described it before as the slowest ‘Aha’ moment in history perhaps, but you were coming to this realization that you were going to have a result that you were not expecting. How did you individually deal with it because the world welcomed it when it came out but as you were analyzing the data and beginning to see that this result … were you doubtful, were you thinking this is not what we should be seeing, there is something wrong?** |
|  | Adam G. Riess: I just thought it was wrong, but just it was going to be wrong for a simple or done reason. We would say a math error or any sophistical analysis that usually means a bug in a computer program. But that it would be wrong for that reason and as it evolved I started thinking maybe at least it is wrong for an interesting reason, something you know we won’t kick ourselves and say ”We should have known that”. I am pretty amazed that it survived to this day at face value of what we thought it was.  Saul Perlmutter: I guess there is the other element which is that there is a chain of analyses that we all have to do and many steps in that chain you have to calibrate. In the very first days of the measurement when you first put the chain together you often put the chain together and put the point in the plot just to make sure, to show that the chain all holds together and works. And then you plan to go back and fix, tune each of the different calibration steps along the way to make sure that you got all your i’s dotted and your t’s crossed. We also, I think, in the very early stages thought that, as we started to put back all the calibrations, the plot would home in to something more sensible and of course as we did those checks, it just homed in more nonsensible.  Adam G. Riess: I think the flavor of our analysis was different. I think we had our tool kit and we were going to take the data through that and see the answer at the end. And when we saw it, we were kind of stuck only with ”Maybe we made a mistake” …  Saul Perlmutter: So you had pre-calibrated everything?  Brian P. Schmidt: We had pre-calibrated everything. We really just wanted ‘dun, dun, dun’ and then ‘katching’ and you were like ”Oh, that is not so good, let’s go through each step again and what went wrong?”. So that was me, I just said ”Ahh, alright where did we mess up?”  Saul Perlmutter: But you should say that most what a scientist does, I think, is look for your own mistakes. That is not unusual, that you are spending a lot of your time trying to figure out ‘Is what I have done today correct? Is what I did last month correct?’ Sometimes this notion that you get asked the question What did you set out to prove? I don’t think any of us set out to prove anything in these things. We were just going out to make a measurement and making measurements are hard.  Brian P. Schmidt: We were trying to prove was the universe slowing down a little or was it slowing down a lot. You had gone through the idea of testing this, we knew we could do that but I kind of figured that that was a irrelevant question. Indeed, I was a referee of a paper he did where he asked this question and one of my comments as a referee was ”Fine, but it is not” … There was a figure where if the universe was not accelerating then we couldn’t really show too much about how much of this dark energy there was. What I had ignored was if the universe really was accelerating you could actually tell that. So, I went in with such a strong bias that I said ”This is an irrelevant experiment cause it would have been. If the universe wasn’t accelerating, we wouldn’t have been able to say anything interesting”. But I had failed to grasp that if the universe really was weird the experiment could show that. I was a bad referee. |
| Q4 | **Yes, indeed. Okay, so we do apparently live in a universe that is speeding away from itself faster and faster. Does that mean that that will never stop, can we say that yet? Can we say that it is just going to carry on accelerating or do we not know?** |
|  | Saul Perlmutter: Until you understand why it is accelerating today, you can’t really make good predictions about what it will do in the future. Particular if one of the answers is true, if it turns out to just be a cosmological consonant as [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) had put it in the equations then you would expect it to go accelerating forever. But if it turns out to be something more like the clause of that very first fraction of the second acceleration in an inflationary universe, we know that decay away. And then it would turn into a deceleration after that and in which case this one could perfectly well decay away and we could be left with deceleration in the future. Until we have evidence that will point us to which explanation is right, I think it is going to be very hard for us to make our, what we would thought was going to be very solvable prediction for the fate of the universe when we are all set out for this.  Brian P. Schmidt: Although that being said, that’s being ultra-cautious. Okay, it is an ultra-cautious statement. The simplest part of what we have measured is that it is not decaying away right now, so it is behaving very much like Einstein’s cosmological constant which is energy fixed with space. Although a theorists could make the universe do a right turn and turn on a very small piece of ground, that is not a simple explanation. The simplest explanation is yes, that the universe is going to keep doing this for long enough that it will sort of make the universe disappear from us. That is the simplest explanation. It is not iron cloud but I would probably wind up bet some money on it, I would certainly wind up to bet my hamster on it. |
| Q8 | **Do you have a hamster?** |
|  | Brian P. Schmidt: No, I don’t.  Adam G. Riess: If he loses the bet, he will have to get one. |
| Q4 | **So how close are we to saying anything truly meaningful about dark energy and what it is, if dark energy is the all-embracing term for what it is that is pushing the universe apart? How close are we to knowing?** |
|  | Adam G. Riess: There is sort of an observational and theoretical answer to that question. I think what we all are hoping is that a deep theoretical idea comes along, sort of like when Einstein revitalised gravity with his general relativity. That people say ”Wow, oh my gosh, that has to be right and that explains everything.” And I think we can learn at this point far more about dark energy by that path if it happens, on the other hand you can wait around forever and the next Einstein couldn’t come along. Then we are collecting these observational clues and the biggest clue is, as Brian was describing, if we could test and actually rule out that it is Einstein’s cosmological constant. If we do see a changing with time another possibility is that we saw the behavior of dark energy changing on scale. Changing when you look on the scale of clusters of galaxies versus the whole universe. If you saw different behavior then you might expect that there is a scale, we say scale dependence, which would mean general relativity which has no scale dependence, might not be the right theory. |
| Q14 | **Unless you design your next experiments, how much interplay is it there with the theorists? Are the theorists dictating where you are going or are you basically just …?** |
|  | Saul Perlmutter: They are not giving enough direction where to go at this stage, so far they are just telling us “Just give us something to go on” and they have been laying ideas all over the terrain in case we happen to go in that direction with our data.  Brian P. Schmidt: But they are haven’t really given us a lot of space to go through brilliant new ideas: ”Try this!” We really are kind of feeling our way through the dark at this point and it has been a little, it is a bit frustrating I guess , it is frustrating for them too because they just keep on beating their heads against this brick wall, it is like ”Why is this stuff there and no one really has got an insight?”  Adam G. Riess: In fact, I would even go further, some theorists have actually reached an explanation or point where they would say it is beyond our ability to measure. Many theorists now subscribe to this point of view that there is a multiverse – there are many disconnecting universes. Each with their own value of this dark energy and that rather than being able to understand a deep reason why ours is as it is, nature rolls the dice every time one of these universes is born. We got a value that is reasonable enough to allow life to form and the disappointing thing to us experimentalists is there is no prediction. There is no experiment, there is nothing to say “Well, if that is right then we ought to go look for something” so that is almost the opposite of helpful.  Brian P. Schmidt: Well, it is a metaphysical argument.  Saul Perlmutter: Some say that we probably won’t really accept that theory, or that set of theories until they do provide some prediction that we can go test as a …  Brian P. Schmidt: One would hope that idea of there being millions or billions of universes out there, that there would ultimately be some prediction in our own universe that we could make.  Saul Perlmutter: Or else it is just philosophy here.  Adam G. Riess: Some say they are satisfied enough that it is Einstein’s cosmological constant, that we should stop, we will never find evidence. I think our view as observers is any time we can think of an experiment to look at the universe in a way it hasn’t been looked at before or to a precision that it hasn’t been looked at before: it behoves us to do that because after all, in 1998 we were pretty surprised. We didn’t listen to people who said …  Brian P. Schmidt: “We know the answer” that’s right. |
| Q14 | **I wanted to ask about the climate for doing your research because in these days when there is a certain amount of cutback on space research, is it the same for astronomy or is astronomy in a flowering time?** |
|  | Brian P. Schmidt: I think it depends on where you are at. For example, United States and Australia are at very different spaces right now. Australia is spending lots of money on astronomical research fundamentally looking at the square kilometer ray, which is a next generation radio telescope which the government is very excited about so they have put quite a bit of effort in. Maybe you guys want to explain the US situation.  Saul Perlmutter: Of course. I think the US world at this moment happens to be a pretty difficult political scene to try to build anything new and in general of course people are mostly focusing on cutting back at this stage, but it is also true that during times of cutback often you need to try to figure out what are our prime targets? Is there some area where we should make sure we don’t lose that field and I think maybe people are optimistic that things like this Nobel Prize and actually there have been a number of very influential decades of survey style reports will help focus funding so that it will at least preserve some of the fields that are looking very brightly at the moment.  Adam G. Riess: I think the key word is focus for the US portfolio now. I mean we are finishing building the James Webspace telescope which is going to be a big large flagship like the Hubble space telescope but that may be the last of its line. And everything after that is going to have to be focused and smaller scale and targeted, and I think we are hoping that dark energy falls into that camp. It was the number one recommendation of the decadal survey to build a focus mission on dark energy. The Europeans are also planning a mission like that as well. |
| Q6 | **You mentioned frequent flyer miles before we started. Does travelling around the world take up a lot of your time?** |
|  | Brian P. Schmidt: Specially in Australia, it is a problem. We have all to go to observatories and things, I mean Adam is more of a space telescope guy, but he does go observe as well. Saul and I will know tripsing back and forth to observatories and to meet your colleagues. It is a very international field so, but all of our teams had people on I think five continents between the two of us so you can only do certain much on the phone. At some point you got to get meet people in the flesh and get stuff done.  Saul Perlmutter: Besides this amazingly interactive social human activity you really have to talk together and work together. It is funny that still video conferencing isn’t good enough. |
| Q18 | **Does you work lend itself to this cloud approach where you just put it out to humanity not just the specialists, but you allow other people to get involved?** |
|  | Saul Perlmutter: It hasn’t happened that much yet except in one particular domain which is that the average astronomers have been quite successful over the years in supernova work. They were the ones originally who found the supernova for us. Today, there are these things as galaxy zoos where they distribute images and then they let the world go loose on trying to classify things, so that particular area happens to be very successful. Some supernova research has been done that way.  Brian P. Schmidt: I am actually with one of my new surveys going to be using that because I don’t have the capacity to find things really quickly and confirm them, so we are going to be crowdsourcing, as they say, so people will go through and for example when we are looking at nearby galaxies and we have a supernova go off that is really young like hours old. Rather than me wait up in the morning and see if it is real before I start telling everyone the crowdsourcing can do that for me so it is our hope that we can use that. |
| Q3 | **And on this subject of engagement, I was interested what made you become astronomers in the first place? Because everybody on the planet spends time looking up at the stars wondering, few make that their lives. But I would think lots of people would think it was a very appealing thing to do. What made you do it?** |
|  | Brian P. Schmidt: In my case I always knew that I was going to be a scientist – my father was a scientist and I loved science. What made me go into astronomy itself – well, I didn’t really know what to do when I finished high school, I had dabbled in astronomy, I hadn’t done a lot of it, I dabbled in it. And I suddenly realized that I had to figure out what I needed to do at university. I said “Jeez, I don’t know what I want to do! What would I do for free? Hm, I would do astronomy for free”, so I figured I would start my astronomy career figuring I would get trained up and I would ultimately do something else, so it was really quite a holding paddle for me.  Saul Perlmutter: You already had a backyard telescope experience as a child?  Brian P. Schmidt: I had a little telescope as a child, I was interested in it, but I wasn’t obsessed. You know, some of my friends were really obsessed, knew every constellation, everything. Naw, it was something I did a bit of, but you know very part time. |
| Q1 | **So unfortunately we are running short of time but I just wanted to ask before we stop; what do you say to young students coming in to the lab or to the telescope and saying: What should I be doing in the future? What are the good questions? What advice do you give?** |
|  | Adam G. Riess: Students who are first coming, they don’t know exactly what the best areas to research are so first you do start in the big picture, the high level, you say: You know we found the universe is accelerating but we don’t understand why. There might be dark energy we don’t understand, something about gravity, this is the big question, but I have an idea for you, and it is to investigate this new technique or this way of doing it. I might also ask the student what their skills are, if they are interested in observing, if they are interested in theory or computer programming. But usually it is a marriage of the big picture and an idea I had in the shower last night on a technique and then you know we will see where it goes. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0037** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0037 = BS**  [Adam Smith] Hello, Adam Smith.  [Brian Schmidt] Hello, this is Brian.  [AS] How very kind of you to call. Thank you so much. Let me please start by congratulating you, adding to the many congratulations you must already have received!  [BS] Thank you.  [AS] And, unlike your co-Laureates who were in America, it was evening when you received the call, so what were you doing when it came?  [BS] I was preparing dinner, which I sadly have not yet eaten!  [AS] [Laughs] Nobody has thought to bring you food during this time!  [BS] Well, I really haven’t had a chance between … I’ve had TV crews and picture crews and lots of phone calls, so I really wouldn’t have had a chance to eat I’m afraid. But we’re getting towards the place where I can go to bed here!  [AS] Yes, it’s coming up to half-past midnight, is that right, with you now?  [BS] That’s right.  [AS] I guess you can soon escape. But it’ll all start again in the morning.  [BS] Yes, I’m teed up for a 6:45am interview on all the TV shows here in Australia.  [AS] Yes, one does become big news.  [BS] Yes, which is great.  [AS] [Laughs] If we turn to the work. The accelerating expansion of the universe when it was announced in 1998 was a huge surprise to everybody. Do you remember the surprise you felt?  [BS] Ah yes. Certainly the first surprise was when Adam and I were talking about the first results that he was coming out with, and we could see the results and the data. And so, I have to admit at that point I just figured that a mistake had been made. But that mistake never really went away. And so, after about 6 weeks, I think the surprise of what was in the data had worn off, but then I think we had to face the realities that we were going to have to tell the world about it. And I wasn’t so convinced that they would be terribly kind in response to our findings, as it seemed just crazy.  [AS] And were they kind? Was the reception good?  [BS] Surprisingly so. I think that the idea of the accelerating universe, indicating that there was some other big thing in the universe, other than things that have normal gravity, meant that a lot of the problems that existed in cosmology back in 1998 were suddenly solved if this stuff existed. So there was a lot of people, especially theorists, who wanted the universe to be geometrically flat, which means it had to have a lot of stuff in it that we just didn’t know was there. And this stuff solved that problem. It gave the extra matter in the universe that needed to be flat.  [AS] But leaving of course an enormous number of questions unsolved. And it seems that cosmologists are comfortable with a large degree of uncertainty.  [BS] So, we have an uncertainty of what the dark energy actually is. That is, why does it exist, and what is its precise form. But the model of dark energy, dark matter, normal atoms, really explains in exquisite detail the observations we make of the universe. So, on one side we have a very precise model of the universe now, that we can test, and every time we test it we keep on getting the same answers. But the fundamental understanding of “What is this dark matter?” and “What is this dark energy?” remain. And so I think that’s the more fundamental question.  [AS] And is that a question that you can look into yourself, or is that over to others?  [BS] Well, we’re trying to test the model as hard as we can. We’re trying to push the model to see if we can break it, to see whether or not [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)‘s cosmological constant – that’s the explanation of the acceleration – whether or not we can show that that’s wrong. At this point we haven’t been able to do that, but if we were able to show that it was wrong, then I think we would get some insight into what was going on. If we cannot show … If it really does look like Einstein’s cosmological constant, then I think we really need to have some brilliant mind, an Einstein-like figure, come along, and from a theoretical point-of-view, sort of shine some light on what’s going on.  [AS] In a conversation with Saul Perlmutter just about an hour ago, I was asking about the relationship between the theoretical and the experimental physicists. The ball seems to be sort of batted back and forth between the two groups.  [BS] Yes, that’s often the way things go. The theorists come up with a great idea, we try to test it; most of the time we expect we’ll be able to prove it wrong, but occasionally we show it’s right, or it seems  to be right, it doesn’t seem to be wrong. And so it’s a great interplay, when you can have theory and observations continually playing off each other. That means you make progress really quickly. If it’s all theory, or it’s all observation, then the progress is much slower.  [AS] And as an experimentalist, what’s the new excitement? Is it technology that’s improving to allow you to look deeper into space?  [BS] Yes, so the technology; these big telescopes, better detectors. Those types of things really enable us to do better and better experiments. So those are a huge driver. But I guess also just good ideas. Astronomy is not like a physics lab. You can’t design an experiment. You need to go through and look up into the heavens, and sort of figure out what the cosmos has given you, and make an experiment out of that.  So it’s a very different process. And so there’s always the chance of having a really good idea of how to put together things in space to do the experiment that you want to do.  [AS] That’s a beautiful description of astronomy.  [BS] Yes, it is a very different thing than experimental physics that way.  [AS] And is the environment for it supportive? Is there lots of money around to help you with the new ideas?  [BS] So astronomy is really going through a heyday right now. That is, it’s very well supported compared to what it was 30 or 40 years ago. But I think one thing to remind ourselves is that great ideas don’t need billions of dollars, they need moderate support, and you need to have a lot of people with moderate support to get the good ideas. There are also needs for great big experiments and those are expensive, but they tend not to be so imaginative. They’re sort of the brute force way of solving problems. And so, I’m a very strong believer that you want to try both things, but you want to make sure you keep a bunch of smaller groups with innovative ideas, at the same time as maybe a couple of brute force big experiments.  [AS] Do you think the award of a Nobel Prize to the field will help promote … will help encourage youngsters to join it?  [BS] I certainly hope so. I hope that it will remind people of how exciting astronomy is, and how trying to understand the universe is something that really helps us understand our place in the universe. And it’s certainly my hope that children in Australia but also around the world will go through and say “Ah, I’d kind of like to figure out how to do that myself”. So yes, that is one of my big hopes.  [AS] Well it’s a wonderfully compelling story. And when you come to Stockholm in December to receive your Nobel Prize, we happily have the chance to talk more about it.  [BS] Yes, that would be great.  [AS] For now I wish you luck in finding some dinner! [Laughs]  [BS] Yes, I think at this point I’m just going to go to bed. [Laughs]  [AS] Okay. [Laughs] Well, sleep well and good luck with the next few days. Thank you so much for calling.  [BS] Okay, cheers. Thank you so much.  [AS] Thank you. Bye bye.  [BS] Yes, goodnight. |
| Q2 | **I suppose that one could make some complicated joke about the pace of the recession and all these lights and the astronomers, but we won’t go there. You are here because you made this remarkable discovery about 15 years ago, that the expansion of the universe is accelerating and up until that point people had thought that the expansion of the universe was either preceding at a steady rate or might be slowing down. And yet this remarkable discovery was welcomed by the committee, people didn’t say ”No, this can’t be” but rather they said ”Yes”. Why? Why did they accept it?** |
|  | Saul Perlmutter: I think it was in this particular case there were two things going for us, and I will say one of the fastest acceptances of a breakthrough question, breakthrough issue in science topic. But the two things where one is that there were two teams that were gaining these results so the fact that both Brian’s, Adam’s team and my team were saying the same things and we were known to be in a very tough competition with each other and that we would have been very happy to call each other out, make a mistake. That, I think, was one of these cases where they got an immediate confirmation instead of having to wait for the years it would often take for another team to make a confirmation. That was one of the practical aspects of it. There was also a part; it certainly made a lot of other problems fit, so suddenly things fell into shape. I am sure we had all been hearing many complaints and worries about how is it that the universe is younger than the oldest star and these other issues that were floating around and suddenly those went away.  Brian P. Schmidt: There was a very strong theoretical view of how the universe should be. As an observer I just said ”They have it wrong” but it turns out this theoretical view of what the universe should be like was sort of confirmed by the observations of the accelerating universe. That section of the community I think were very happy to see it. Whereas the observational side, the people who actually go out and make experiments like to have things be as simple as possible. I think it was more skepticism on that side but the joint teams finding the same thing I think helped erode a little bit of that skepticism from that side of the community.  Adam G. Riess: Just to follow up on what Brian was saying with this theoretical preference. The preference was that there would be a certain amount of energy or matter in the universe and so we kept looking for the matter. After a while of looking we could only find about 30% of it, there was a hook that if we looked further out, we would finally find it. We didn’t but what we found was the other 70%. It was in a funny form, this funny kind of energy that we call dark energy. As difficult as it was to understand some theorists were kind of happy that they said “Well, anyway you complete the picture, we are happy to get to the 100%”.  Saul Perlmutter: I mean, it is funny because I think there was one group of theorists that I think were telling us ”Why are you bothering to do this measurement when you already know what the answer is going to be” whereas most of the observers in the world at that time would say ”Well, you can’t trust the theorists, they have been telling us all sorts of things that turned out to be wrong once we actually did the measurement.” |
| **Interview** |  |
| Q2 | **You have described it before as the slowest ‘Aha’ moment in history perhaps, but you were coming to this realization that you were going to have a result that you were not expecting. How did you individually deal with it because the world welcomed it when it came out but as you were analyzing the data and beginning to see that this result … were you doubtful, were you thinking this is not what we should be seeing, there is something wrong?** |
|  | Adam G. Riess: I just thought it was wrong, but just it was going to be wrong for a simple or done reason. We would say a math error or any sophistical analysis that usually means a bug in a computer program. But that it would be wrong for that reason and as it evolved I started thinking maybe at least it is wrong for an interesting reason, something you know we won’t kick ourselves and say ”We should have known that”. I am pretty amazed that it survived to this day at face value of what we thought it was.  Saul Perlmutter: I guess there is the other element which is that there is a chain of analyses that we all have to do and many steps in that chain you have to calibrate. In the very first days of the measurement when you first put the chain together you often put the chain together and put the point in the plot just to make sure, to show that the chain all holds together and works. And then you plan to go back and fix, tune each of the different calibration steps along the way to make sure that you got all your i’s dotted and your t’s crossed. We also, I think, in the very early stages thought that, as we started to put back all the calibrations, the plot would home in to something more sensible and of course as we did those checks, it just homed in more nonsensible.  Adam G. Riess: I think the flavor of our analysis was different. I think we had our tool kit and we were going to take the data through that and see the answer at the end. And when we saw it, we were kind of stuck only with ”Maybe we made a mistake” …  Saul Perlmutter: So you had pre-calibrated everything?  Brian P. Schmidt: We had pre-calibrated everything. We really just wanted ‘dun, dun, dun’ and then ‘katching’ and you were like ”Oh, that is not so good, let’s go through each step again and what went wrong?”. So that was me, I just said ”Ahh, alright where did we mess up?”  Saul Perlmutter: But you should say that most what a scientist does, I think, is look for your own mistakes. That is not unusual, that you are spending a lot of your time trying to figure out ‘Is what I have done today correct? Is what I did last month correct?’ Sometimes this notion that you get asked the question What did you set out to prove? I don’t think any of us set out to prove anything in these things. We were just going out to make a measurement and making measurements are hard.  Brian P. Schmidt: We were trying to prove was the universe slowing down a little or was it slowing down a lot. You had gone through the idea of testing this, we knew we could do that but I kind of figured that that was a irrelevant question. Indeed, I was a referee of a paper he did where he asked this question and one of my comments as a referee was ”Fine, but it is not” … There was a figure where if the universe was not accelerating then we couldn’t really show too much about how much of this dark energy there was. What I had ignored was if the universe really was accelerating you could actually tell that. So, I went in with such a strong bias that I said ”This is an irrelevant experiment cause it would have been. If the universe wasn’t accelerating, we wouldn’t have been able to say anything interesting”. But I had failed to grasp that if the universe really was weird the experiment could show that. I was a bad referee. |
| Q4 | **Yes, indeed. Okay, so we do apparently live in a universe that is speeding away from itself faster and faster. Does that mean that that will never stop, can we say that yet? Can we say that it is just going to carry on accelerating or do we not know?** |
|  | Saul Perlmutter: Until you understand why it is accelerating today, you can’t really make good predictions about what it will do in the future. Particular if one of the answers is true, if it turns out to just be a cosmological consonant as [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) had put it in the equations then you would expect it to go accelerating forever. But if it turns out to be something more like the clause of that very first fraction of the second acceleration in an inflationary universe, we know that decay away. And then it would turn into a deceleration after that and in which case this one could perfectly well decay away and we could be left with deceleration in the future. Until we have evidence that will point us to which explanation is right, I think it is going to be very hard for us to make our, what we would thought was going to be very solvable prediction for the fate of the universe when we are all set out for this.  Brian P. Schmidt: Although that being said, that’s being ultra-cautious. Okay, it is an ultra-cautious statement. The simplest part of what we have measured is that it is not decaying away right now, so it is behaving very much like Einstein’s cosmological constant which is energy fixed with space. Although a theorists could make the universe do a right turn and turn on a very small piece of ground, that is not a simple explanation. The simplest explanation is yes, that the universe is going to keep doing this for long enough that it will sort of make the universe disappear from us. That is the simplest explanation. It is not iron cloud but I would probably wind up bet some money on it, I would certainly wind up to bet my hamster on it. |
| Q8 | **Do you have a hamster?** |
|  | Brian P. Schmidt: No, I don’t.  Adam G. Riess: If he loses the bet, he will have to get one. |
| Q4 | **So how close are we to saying anything truly meaningful about dark energy and what it is, if dark energy is the all-embracing term for what it is that is pushing the universe apart? How close are we to knowing?** |
|  | Adam G. Riess: There is sort of an observational and theoretical answer to that question. I think what we all are hoping is that a deep theoretical idea comes along, sort of like when Einstein revitalised gravity with his general relativity. That people say ”Wow, oh my gosh, that has to be right and that explains everything.” And I think we can learn at this point far more about dark energy by that path if it happens, on the other hand you can wait around forever and the next Einstein couldn’t come along. Then we are collecting these observational clues and the biggest clue is, as Brian was describing, if we could test and actually rule out that it is Einstein’s cosmological constant. If we do see a changing with time another possibility is that we saw the behavior of dark energy changing on scale. Changing when you look on the scale of clusters of galaxies versus the whole universe. If you saw different behavior then you might expect that there is a scale, we say scale dependence, which would mean general relativity which has no scale dependence, might not be the right theory. |
| Q14 | **Unless you design your next experiments, how much interplay is it there with the theorists? Are the theorists dictating where you are going or are you basically just …?** |
|  | Saul Perlmutter: They are not giving enough direction where to go at this stage, so far they are just telling us “Just give us something to go on” and they have been laying ideas all over the terrain in case we happen to go in that direction with our data.  Brian P. Schmidt: But they are haven’t really given us a lot of space to go through brilliant new ideas: ”Try this!” We really are kind of feeling our way through the dark at this point and it has been a little, it is a bit frustrating I guess , it is frustrating for them too because they just keep on beating their heads against this brick wall, it is like ”Why is this stuff there and no one really has got an insight?”  Adam G. Riess: In fact, I would even go further, some theorists have actually reached an explanation or point where they would say it is beyond our ability to measure. Many theorists now subscribe to this point of view that there is a multiverse – there are many disconnecting universes. Each with their own value of this dark energy and that rather than being able to understand a deep reason why ours is as it is, nature rolls the dice every time one of these universes is born. We got a value that is reasonable enough to allow life to form and the disappointing thing to us experimentalists is there is no prediction. There is no experiment, there is nothing to say “Well, if that is right then we ought to go look for something” so that is almost the opposite of helpful.  Brian P. Schmidt: Well, it is a metaphysical argument.  Saul Perlmutter: Some say that we probably won’t really accept that theory, or that set of theories until they do provide some prediction that we can go test as a …  Brian P. Schmidt: One would hope that idea of there being millions or billions of universes out there, that there would ultimately be some prediction in our own universe that we could make.  Saul Perlmutter: Or else it is just philosophy here.  Adam G. Riess: Some say they are satisfied enough that it is Einstein’s cosmological constant, that we should stop, we will never find evidence. I think our view as observers is any time we can think of an experiment to look at the universe in a way it hasn’t been looked at before or to a precision that it hasn’t been looked at before: it behoves us to do that because after all, in 1998 we were pretty surprised. We didn’t listen to people who said …  Brian P. Schmidt: “We know the answer” that’s right. |
| Q14 | **I wanted to ask about the climate for doing your research because in these days when there is a certain amount of cutback on space research, is it the same for astronomy or is astronomy in a flowering time?** |
|  | Brian P. Schmidt: I think it depends on where you are at. For example, United States and Australia are at very different spaces right now. Australia is spending lots of money on astronomical research fundamentally looking at the square kilometer ray, which is a next generation radio telescope which the government is very excited about so they have put quite a bit of effort in. Maybe you guys want to explain the US situation.  Saul Perlmutter: Of course. I think the US world at this moment happens to be a pretty difficult political scene to try to build anything new and in general of course people are mostly focusing on cutting back at this stage, but it is also true that during times of cutback often you need to try to figure out what are our prime targets? Is there some area where we should make sure we don’t lose that field and I think maybe people are optimistic that things like this Nobel Prize and actually there have been a number of very influential decades of survey style reports will help focus funding so that it will at least preserve some of the fields that are looking very brightly at the moment.  Adam G. Riess: I think the key word is focus for the US portfolio now. I mean we are finishing building the James Webspace telescope which is going to be a big large flagship like the Hubble space telescope but that may be the last of its line. And everything after that is going to have to be focused and smaller scale and targeted, and I think we are hoping that dark energy falls into that camp. It was the number one recommendation of the decadal survey to build a focus mission on dark energy. The Europeans are also planning a mission like that as well. |
| Q6 | **You mentioned frequent flyer miles before we started. Does travelling around the world take up a lot of your time?** |
|  | Brian P. Schmidt: Specially in Australia, it is a problem. We have all to go to observatories and things, I mean Adam is more of a space telescope guy, but he does go observe as well. Saul and I will know tripsing back and forth to observatories and to meet your colleagues. It is a very international field so, but all of our teams had people on I think five continents between the two of us so you can only do certain much on the phone. At some point you got to get meet people in the flesh and get stuff done.  Saul Perlmutter: Besides this amazingly interactive social human activity you really have to talk together and work together. It is funny that still video conferencing isn’t good enough. |
| Q18 | **Does you work lend itself to this cloud approach where you just put it out to humanity not just the specialists, but you allow other people to get involved?** |
|  | Saul Perlmutter: It hasn’t happened that much yet except in one particular domain which is that the average astronomers have been quite successful over the years in supernova work. They were the ones originally who found the supernova for us. Today, there are these things as galaxy zoos where they distribute images and then they let the world go loose on trying to classify things, so that particular area happens to be very successful. Some supernova research has been done that way.  Brian P. Schmidt: I am actually with one of my new surveys going to be using that because I don’t have the capacity to find things really quickly and confirm them, so we are going to be crowdsourcing, as they say, so people will go through and for example when we are looking at nearby galaxies and we have a supernova go off that is really young like hours old. Rather than me wait up in the morning and see if it is real before I start telling everyone the crowdsourcing can do that for me so it is our hope that we can use that. |
| Q3 | **And on this subject of engagement, I was interested what made you become astronomers in the first place? Because everybody on the planet spends time looking up at the stars wondering, few make that their lives. But I would think lots of people would think it was a very appealing thing to do. What made you do it?** |
|  | Brian P. Schmidt: In my case I always knew that I was going to be a scientist – my father was a scientist and I loved science. What made me go into astronomy itself – well, I didn’t really know what to do when I finished high school, I had dabbled in astronomy, I hadn’t done a lot of it, I dabbled in it. And I suddenly realized that I had to figure out what I needed to do at university. I said “Jeez, I don’t know what I want to do! What would I do for free? Hm, I would do astronomy for free”, so I figured I would start my astronomy career figuring I would get trained up and I would ultimately do something else, so it was really quite a holding paddle for me.  Saul Perlmutter: You already had a backyard telescope experience as a child?  Brian P. Schmidt: I had a little telescope as a child, I was interested in it, but I wasn’t obsessed. You know, some of my friends were really obsessed, knew every constellation, everything. Naw, it was something I did a bit of, but you know very part time. |
| Q37 | **So unfortunately we are running short of time but I just wanted to ask before we stop; what do you say to young students coming in to the lab or to the telescope and saying: What should I be doing in the future? What are the good questions? What advice do you give?** |
|  | Adam G. Riess: Students who are first coming, they don’t know exactly what the best areas to research are so first you do start in the big picture, the high level, you say: You know we found the universe is accelerating but we don’t understand why. There might be dark energy we don’t understand, something about gravity, this is the big question, but I have an idea for you, and it is to investigate this new technique or this way of doing it. I might also ask the student what their skills are, if they are interested in observing, if they are interested in theory or computer programming. But usually it is a marriage of the big picture and an idea I had in the shower last night on a technique and then you know we will see where it goes. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0038** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0038 = AR**  [Adam Riess] Hello?  [Adam Smith] Hello, may I speak to Adam Riess please?  [AR] This is he.  [AS] Oh hello, very nice to speak to you.  [AR] Yes.  [AS] This is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  [AR] Yes?  [AS] Many congratulations on the news that you’ve just been awarded the Nobel Prize.  [AR] Thank you very much.  [AS] We have a tradition here at the website of recording extremely short telephone interviews with new Laureates; would you be able to speak for just a few minutes?  [AR] Sure.  [AS] Thank you very much indeed. I know it’s early there in Baltimore. May I ask what you were doing when you heard the news from the committee?  [AR] I was sleeping and listening to my 10-month old son only sort of sleeping, and sort of crying and sleeping! [Laughs]  [AS] As they do, yes!  [AR] Yes.  [AS] So you were awake for the news?  [AR] I was.  [AS] And, you’ve been awarded the Prize for this immensely surprising discovery that the expansion of the universe is accelerating …  [AR] Yes.  [AS] … Can you remember the moment that you realised just what you’d found?  [AR] I do. I remembered going through the analysis of the data to the end. I remember seeing that the sign of the answer was, I would have said, wrong [Both Laugh]. And I remember thinking “Uh, I’ve made a terrible mistake, and I have to find this mistake”, and then spending weeks looking for it, and only after that starting to allow the possibility that the sign could be real, and then that the universe could be accelerating.  [AS] Yes. And you were working in competition with the team of Saul Perlmutter …  [AR] That’s true.  [AS] Did that add to the sort of sense of excitement as you were gathering the data?  [AR] Oh absolutely! Absolutely. It, you know, it lent a sense of urgency, and when I did find out that they were seeing the same thing, it went from “Oh, this is a terrible mistake” to “Oh my God, this might be the right answer!” So, it was very exciting.  [AS] And you made the measurements – both teams made the measurements – by looking at these far off supernovae …  [AR] That’s right.  [AS] How far away are we talking?  [AR] We are talking about 5-billion light years.  [AS] [Laughs] Okay. Mind-boggling distances. To explain the acceleration, you had to propose some sort of force pushing galaxies apart, and that’s what we refer to as “dark energy”.  [AR] Right. We actually did not have to propose that, seeing that there was, I would say, off-the-shelf and ready, a model from [Albert Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/), something he referred to as the cosmological constant, which would neatly do the trick. And so all we did was to say that that seemed like at that point the simplest hypothesis.  [AS] But Einstein had felt that he’d been wrong in proposing it, I gather.  [AR] That’s right, so maybe he should be getting the Nobel Prize again! [Both laugh]  [AS] And do we have any idea at all what dark energy is yet?  [AR] No.  I wish we did! [Both Laugh] They didn’t give us the Nobel Prize for that!  [AS] [Laughs] What’s our best hope of finding out, do you think?  [AR] More experiments, and hopefully somebody else with some great ideas. But more experiments.  [AS] Okay.  [AR] Probably some space satellite, you know, the use of our current facilities, taking a lot more data.  [AS] Okay. Last question.  [AR] Yes.  [AS] When you’re tracking the infinite recesses of space in this way, does it not get a little bit scary thinking at such distances?  [AR] Is it scary? No, I would say I find it very calming. It just feels so big. [Pauses] So, I need to go, but thank you.  [AS] Of course, of course. We look forward to meeting you in Stockholm when you come to receive the Nobel Prize in December. Congratulations again.  [AR] All right.  [AS] Thank you. Bye bye.  [AR] Bye. |
| **Interview** |  |
| Q2 | **I suppose that one could make some complicated joke about the pace of the recession and all these lights and the astronomers, but we won’t go there. You are here because you made this remarkable discovery about 15 years ago, that the expansion of the universe is accelerating and up until that point people had thought that the expansion of the universe was either preceding at a steady rate or might be slowing down. And yet this remarkable discovery was welcomed by the committee, people didn’t say ”No, this can’t be” but rather they said ”Yes”. Why? Why did they accept it?** |
|  | Saul Perlmutter: I think it was in this particular case there were two things going for us, and I will say one of the fastest acceptances of a breakthrough question, breakthrough issue in science topic. But the two things where one is that there were two teams that were gaining these results so the fact that both Brian’s, Adam’s team and my team were saying the same things and we were known to be in a very tough competition with each other and that we would have been very happy to call each other out, make a mistake. That, I think, was one of these cases where they got an immediate confirmation instead of having to wait for the years it would often take for another team to make a confirmation. That was one of the practical aspects of it. There was also a part; it certainly made a lot of other problems fit, so suddenly things fell into shape. I am sure we had all been hearing many complaints and worries about how is it that the universe is younger than the oldest star and these other issues that were floating around and suddenly those went away.  Brian P. Schmidt: There was a very strong theoretical view of how the universe should be. As an observer I just said ”They have it wrong” but it turns out this theoretical view of what the universe should be like was sort of confirmed by the observations of the accelerating universe. That section of the community I think were very happy to see it. Whereas the observational side, the people who actually go out and make experiments like to have things be as simple as possible. I think it was more skepticism on that side but the joint teams finding the same thing I think helped erode a little bit of that skepticism from that side of the community.  Adam G. Riess: Just to follow up on what Brian was saying with this theoretical preference. The preference was that there would be a certain amount of energy or matter in the universe and so we kept looking for the matter. After a while of looking we could only find about 30% of it, there was a hook that if we looked further out, we would finally find it. We didn’t but what we found was the other 70%. It was in a funny form, this funny kind of energy that we call dark energy. As difficult as it was to understand some theorists were kind of happy that they said “Well, anyway you complete the picture, we are happy to get to the 100%”.  Saul Perlmutter: I mean, it is funny because I think there was one group of theorists that I think were telling us ”Why are you bothering to do this measurement when you already know what the answer is going to be” whereas most of the observers in the world at that time would say ”Well, you can’t trust the theorists, they have been telling us all sorts of things that turned out to be wrong once we actually did the measurement.” |
| Q2 | **You have described it before as the slowest ‘Aha’ moment in history perhaps, but you were coming to this realization that you were going to have a result that you were not expecting. How did you individually deal with it because the world welcomed it when it came out but as you were analyzing the data and beginning to see that this result … were you doubtful, were you thinking this is not what we should be seeing, there is something wrong?** |
|  | Adam G. Riess: I just thought it was wrong, but just it was going to be wrong for a simple or done reason. We would say a math error or any sophistical analysis that usually means a bug in a computer program. But that it would be wrong for that reason and as it evolved I started thinking maybe at least it is wrong for an interesting reason, something you know we won’t kick ourselves and say ”We should have known that”. I am pretty amazed that it survived to this day at face value of what we thought it was.  Saul Perlmutter: I guess there is the other element which is that there is a chain of analyses that we all have to do and many steps in that chain you have to calibrate. In the very first days of the measurement when you first put the chain together you often put the chain together and put the point in the plot just to make sure, to show that the chain all holds together and works. And then you plan to go back and fix, tune each of the different calibration steps along the way to make sure that you got all your i’s dotted and your t’s crossed. We also, I think, in the very early stages thought that, as we started to put back all the calibrations, the plot would home in to something more sensible and of course as we did those checks, it just homed in more nonsensible.  Adam G. Riess: I think the flavor of our analysis was different. I think we had our tool kit and we were going to take the data through that and see the answer at the end. And when we saw it, we were kind of stuck only with ”Maybe we made a mistake” …  Saul Perlmutter: So you had pre-calibrated everything?  Brian P. Schmidt: We had pre-calibrated everything. We really just wanted ‘dun, dun, dun’ and then ‘katching’ and you were like ”Oh, that is not so good, let’s go through each step again and what went wrong?”. So that was me, I just said ”Ahh, alright where did we mess up?”  Saul Perlmutter: But you should say that most what a scientist does, I think, is look for your own mistakes. That is not unusual, that you are spending a lot of your time trying to figure out ‘Is what I have done today correct? Is what I did last month correct?’ Sometimes this notion that you get asked the question What did you set out to prove? I don’t think any of us set out to prove anything in these things. We were just going out to make a measurement and making measurements are hard.  Brian P. Schmidt: We were trying to prove was the universe slowing down a little or was it slowing down a lot. You had gone through the idea of testing this, we knew we could do that but I kind of figured that that was a irrelevant question. Indeed, I was a referee of a paper he did where he asked this question and one of my comments as a referee was ”Fine, but it is not” … There was a figure where if the universe was not accelerating then we couldn’t really show too much about how much of this dark energy there was. What I had ignored was if the universe really was accelerating you could actually tell that. So, I went in with such a strong bias that I said ”This is an irrelevant experiment cause it would have been. If the universe wasn’t accelerating, we wouldn’t have been able to say anything interesting”. But I had failed to grasp that if the universe really was weird the experiment could show that. I was a bad referee. |
| Q4 | **Yes, indeed. Okay, so we do apparently live in a universe that is speeding away from itself faster and faster. Does that mean that that will never stop, can we say that yet? Can we say that it is just going to carry on accelerating or do we not know?** |
|  | Saul Perlmutter: Until you understand why it is accelerating today, you can’t really make good predictions about what it will do in the future. Particular if one of the answers is true, if it turns out to just be a cosmological consonant as [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) had put it in the equations then you would expect it to go accelerating forever. But if it turns out to be something more like the clause of that very first fraction of the second acceleration in an inflationary universe, we know that decay away. And then it would turn into a deceleration after that and in which case this one could perfectly well decay away and we could be left with deceleration in the future. Until we have evidence that will point us to which explanation is right, I think it is going to be very hard for us to make our, what we would thought was going to be very solvable prediction for the fate of the universe when we are all set out for this.  Brian P. Schmidt: Although that being said, that’s being ultra-cautious. Okay, it is an ultra-cautious statement. The simplest part of what we have measured is that it is not decaying away right now, so it is behaving very much like Einstein’s cosmological constant which is energy fixed with space. Although a theorists could make the universe do a right turn and turn on a very small piece of ground, that is not a simple explanation. The simplest explanation is yes, that the universe is going to keep doing this for long enough that it will sort of make the universe disappear from us. That is the simplest explanation. It is not iron cloud but I would probably wind up bet some money on it, I would certainly wind up to bet my hamster on it. |
| Q8 | **Do you have a hamster?** |
|  | Brian P. Schmidt: No, I don’t.  Adam G. Riess: If he loses the bet, he will have to get one. |
| Q4 | **So how close are we to saying anything truly meaningful about dark energy and what it is, if dark energy is the all-embracing term for what it is that is pushing the universe apart? How close are we to knowing?** |
|  | Adam G. Riess: There is sort of an observational and theoretical answer to that question. I think what we all are hoping is that a deep theoretical idea comes along, sort of like when Einstein revitalised gravity with his general relativity. That people say ”Wow, oh my gosh, that has to be right and that explains everything.” And I think we can learn at this point far more about dark energy by that path if it happens, on the other hand you can wait around forever and the next Einstein couldn’t come along. Then we are collecting these observational clues and the biggest clue is, as Brian was describing, if we could test and actually rule out that it is Einstein’s cosmological constant. If we do see a changing with time another possibility is that we saw the behavior of dark energy changing on scale. Changing when you look on the scale of clusters of galaxies versus the whole universe. If you saw different behavior then you might expect that there is a scale, we say scale dependence, which would mean general relativity which has no scale dependence, might not be the right theory. |
| Q14 | **Unless you design your next experiments, how much interplay is it there with the theorists? Are the theorists dictating where you are going or are you basically just …?** |
|  | Saul Perlmutter: They are not giving enough direction where to go at this stage, so far they are just telling us “Just give us something to go on” and they have been laying ideas all over the terrain in case we happen to go in that direction with our data.  Brian P. Schmidt: But they are haven’t really given us a lot of space to go through brilliant new ideas: ”Try this!” We really are kind of feeling our way through the dark at this point and it has been a little, it is a bit frustrating I guess , it is frustrating for them too because they just keep on beating their heads against this brick wall, it is like ”Why is this stuff there and no one really has got an insight?”  Adam G. Riess: In fact, I would even go further, some theorists have actually reached an explanation or point where they would say it is beyond our ability to measure. Many theorists now subscribe to this point of view that there is a multiverse – there are many disconnecting universes. Each with their own value of this dark energy and that rather than being able to understand a deep reason why ours is as it is, nature rolls the dice every time one of these universes is born. We got a value that is reasonable enough to allow life to form and the disappointing thing to us experimentalists is there is no prediction. There is no experiment, there is nothing to say “Well, if that is right then we ought to go look for something” so that is almost the opposite of helpful.  Brian P. Schmidt: Well, it is a metaphysical argument.  Saul Perlmutter: Some say that we probably won’t really accept that theory, or that set of theories until they do provide some prediction that we can go test as a …  Brian P. Schmidt: One would hope that idea of there being millions or billions of universes out there, that there would ultimately be some prediction in our own universe that we could make.  Saul Perlmutter: Or else it is just philosophy here.  Adam G. Riess: Some say they are satisfied enough that it is Einstein’s cosmological constant, that we should stop, we will never find evidence. I think our view as observers is any time we can think of an experiment to look at the universe in a way it hasn’t been looked at before or to a precision that it hasn’t been looked at before: it behoves us to do that because after all, in 1998 we were pretty surprised. We didn’t listen to people who said …  Brian P. Schmidt: “We know the answer” that’s right. |
| Q14 | **I wanted to ask about the climate for doing your research because in these days when there is a certain amount of cutback on space research, is it the same for astronomy or is astronomy in a flowering time?** |
|  | Brian P. Schmidt: I think it depends on where you are at. For example, United States and Australia are at very different spaces right now. Australia is spending lots of money on astronomical research fundamentally looking at the square kilometer ray, which is a next generation radio telescope which the government is very excited about so they have put quite a bit of effort in. Maybe you guys want to explain the US situation.  Saul Perlmutter: Of course. I think the US world at this moment happens to be a pretty difficult political scene to try to build anything new and in general of course people are mostly focusing on cutting back at this stage, but it is also true that during times of cutback often you need to try to figure out what are our prime targets? Is there some area where we should make sure we don’t lose that field and I think maybe people are optimistic that things like this Nobel Prize and actually there have been a number of very influential decades of survey style reports will help focus funding so that it will at least preserve some of the fields that are looking very brightly at the moment.  Adam G. Riess: I think the key word is focus for the US portfolio now. I mean we are finishing building the James Webspace telescope which is going to be a big large flagship like the Hubble space telescope but that may be the last of its line. And everything after that is going to have to be focused and smaller scale and targeted, and I think we are hoping that dark energy falls into that camp. It was the number one recommendation of the decadal survey to build a focus mission on dark energy. The Europeans are also planning a mission like that as well. |
| Q6 | **You mentioned frequent flyer miles before we started. Does travelling around the world take up a lot of your time?** |
|  | Brian P. Schmidt: Specially in Australia, it is a problem. We have all to go to observatories and things, I mean Adam is more of a space telescope guy, but he does go observe as well. Saul and I will know tripsing back and forth to observatories and to meet your colleagues. It is a very international field so, but all of our teams had people on I think five continents between the two of us so you can only do certain much on the phone. At some point you got to get meet people in the flesh and get stuff done.  Saul Perlmutter: Besides this amazingly interactive social human activity you really have to talk together and work together. It is funny that still video conferencing isn’t good enough. |
| Q18 | **Does you work lend itself to this cloud approach where you just put it out to humanity not just the specialists, but you allow other people to get involved?** |
|  | Saul Perlmutter: It hasn’t happened that much yet except in one particular domain which is that the average astronomers have been quite successful over the years in supernova work. They were the ones originally who found the supernova for us. Today, there are these things as galaxy zoos where they distribute images and then they let the world go loose on trying to classify things, so that particular area happens to be very successful. Some supernova research has been done that way.  Brian P. Schmidt: I am actually with one of my new surveys going to be using that because I don’t have the capacity to find things really quickly and confirm them, so we are going to be crowdsourcing, as they say, so people will go through and for example when we are looking at nearby galaxies and we have a supernova go off that is really young like hours old. Rather than me wait up in the morning and see if it is real before I start telling everyone the crowdsourcing can do that for me so it is our hope that we can use that. |
| Q3 | **And on this subject of engagement, I was interested what made you become astronomers in the first place? Because everybody on the planet spends time looking up at the stars wondering, few make that their lives. But I would think lots of people would think it was a very appealing thing to do. What made you do it?** |
|  | Brian P. Schmidt: In my case I always knew that I was going to be a scientist – my father was a scientist and I loved science. What made me go into astronomy itself – well, I didn’t really know what to do when I finished high school, I had dabbled in astronomy, I hadn’t done a lot of it, I dabbled in it. And I suddenly realized that I had to figure out what I needed to do at university. I said “Jeez, I don’t know what I want to do! What would I do for free? Hm, I would do astronomy for free”, so I figured I would start my astronomy career figuring I would get trained up and I would ultimately do something else, so it was really quite a holding paddle for me.  Saul Perlmutter: You already had a backyard telescope experience as a child?  Brian P. Schmidt: I had a little telescope as a child, I was interested in it, but I wasn’t obsessed. You know, some of my friends were really obsessed, knew every constellation, everything. Naw, it was something I did a bit of, but you know very part time. |
| Q1 | **So unfortunately we are running short of time but I just wanted to ask before we stop; what do you say to young students coming in to the lab or to the telescope and saying: What should I be doing in the future? What are the good questions? What advice do you give?** |
|  | Adam G. Riess: Students who are first coming, they don’t know exactly what the best areas to research are so first you do start in the big picture, the high level, you say: You know we found the universe is accelerating but we don’t understand why. There might be dark energy we don’t understand, something about gravity, this is the big question, but I have an idea for you, and it is to investigate this new technique or this way of doing it. I might also ask the student what their skills are, if they are interested in observing, if they are interested in theory or computer programming. But usually it is a marriage of the big picture and an idea I had in the shower last night on a technique and then you know we will see where it goes. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0039** |
| **Biographical** | Several years ago I was on a trekking trip in the Jordanian desert with a large group of Brits. We were camping and, as usual, there was not much to do in the evenings, so we filled the hours by sitting around a campfire, playing the popular British game ‘Call My Bluff’. In it a player makes several statements only one of which is true, and the rest of the group have to guess which one it is. All other statements are called ‘bluffs’. I teased my fellow hikers with statements like ‘I was born in a Mediterranean climate’, ‘I was a lieutenant in the Red Army’, ‘I have won an Ig Nobel prize’, ‘I climbed several five kilometre high mountains’, ‘I fell down a 100 m deep crevasse without a rope’, ‘I was called ‘Russian’ for the first time at the age of 32′, ‘At my university I studied intercontinental ballistic missiles’, ‘I was a bricklayer north of the Arctic Circle’, ‘I knew Mikhail Gorbachev personally’ and so on. What surprised me was that all but the last statement were dismissed by most of the group as ‘bluffs’, while people found it easy to believe that it is typical for any Russian to know their political leaders personally. I won every single game because the truth was a complete opposite: Apart from knowing Gorbachev (whom I only ever saw on TV) all the other statements were true. This made me think for the first time that, perhaps, my life had not been as trivial as I thought.  Still, with reference to the epigraph, I am not dead yet. I think it is too early for me to write an autobiography, as doing so somehow implies that one’s life story is finished. I am only fifty-two and plan to actively continue my research work. However, I am a law-abiding citizen (of course!) and, according to the rules of the Nobel Foundation, I must provide an autobiography. So, below I have conceded a sort of one, a literary exercise. Although I do not expand on any of the non-bluff statements above, the reader is still likely to find my life path atypical. I do not know whether this somehow infiuenced my way of doing things or it is just a separate story, having little in common with my research career.  The timeline of this autobiography ends in 1987 when I received a PhD. After that point, my scientific biography is given in the[Nobel lecture](https://www.nobelprize.org/nobel_prizes/physics/laureates/2010/geim-lecture.html) ‘Random walk to graphene’.  **Soviet Taxonomy** I was born on October 21, 1958 in the small Black Sea resort of Sochi, the second son of Nina Bayer and Konstantin Geim. The first seven years of my life I spent there with my grandmother Maria Ziegler and grandfather Nikolai Bayer. I remember little of my grandfather because he died when I was only six, but my grandmother was my best friend and an important part of my life until the university years, when I left home. At the age of seven, it was time to go to school and, reluctantly, I had to leave Sochi and go to live with my parents and my elder brother Vladislav in the city of Nalchik where they worked. Nalchik is the capital of a small Republic of Kabardino-Balkaria in the foothills of the Caucasus Mountains and can be found on the world map as a host to the Europe’s highest peak Elbrus and in proximity to the infamous Chechnya. For the next ten years I spent my school time there but returned to Sochi every year to stay with my grandmother during the summer months.  At this point, it is probably right to mention my ethnic origins, because for certain groups of people in the Soviet Union ethnicity was a very important factor and often defined their life choices and eventually their life path. I belonged to one such group. Despite the great ethnic diversity of the Soviet population (the official census of 1989 listed over 100 ethnicities), the authorities managed to keep track of each and every one of them by having a special line in the Soviet passport (‘line 5: nationality’). In my passport this line stated ‘German’. This is because my father came from the so-called Volga Germans, descendants of colonists from Germany who settled on the Volga River banks in the 18th century. My mother’s bloodline was also mostly German. I have long believed that my maternal grandmother Maria was Jewish, but according to my brother’s recent research into family history, her father was also German. Therefore, to the best of my knowledge, the only Jew in the family was my great-grandmother, with the rest on both sides being German.  A note is needed here to explain why I devote so much space to explaining my ethnicity. Firstly, of course, the word ‘German’ in my Soviet passport had a very real effect on my life, as the reader will find out below. Secondly, the issue of my ethnicity unexpectedly surfaced again after the announcement of the Nobel Prize – suddenly there have been a lot of discussions whether this prize is British, Dutch, Russian, German or Jewish. To me these discussions seem silly. Having lived and worked in several European countries, I consider myself European and do not believe that any further taxonomy is necessary, especially in such a fluid world as the world of science.  **Skeletons in the Old Chest** My knowledge of our family history is rather sketchy and, for a Western person, it is perhaps difficult to understand why. The reason goes back to well before I was born. In Stalin’s time, family history was a dangerous subject to discuss, and stories were not passed from generation to generation because parents deliberately concealed their history from the children in order to protect them. A telling example can be found in the many documents that I had to fill when applying to university, for a job and so on. Among such documents there was always a questionnaire asking whether you had relatives abroad, whether any of your relatives were prisoners in forced labour camps (the infamous Gulag) or were prisoners of war. I always answered ‘No’ to all those questions, in good faith, believing this answer to be true. It was only in the late 1980s that I learned that nearly everyone in my family, including my father and grandfather, spent many years in the Gulag, that some of the family were prisoners in German concentration camps, and that I had an uncle living in Bavaria. This was deliberately and successfully concealed from me during my first 30 years of life.  Below is what I have learned since then from my few living relatives. My grandfather Nikolai Bayer was a professor at Kharkov University who specialised in aerial cartography. In 1946, documents were found by the Soviet Army in post-war Poland which revealed that after the First World War, he was a junior minister in Petliura’s short-lived Ukrainian nationalist government. This anti-Bolshevik past, together with his German ethnicity and the fact that at the time he was compiling maps of Eastern Siberia, was apparently enough reason to accuse him of passing state secrets to the Japanese and send him to a northern Gulag camp near Vorkuta. He was released only in 1953, after Stalin’s death.  When I was born, my father was forty-eight years old and already had quite a long and difficult history behind him as well, which I managed to learn from him bit by bit over many years. Until his last years, he avoided discussing it, even when I asked, and those bits came out mostly accidentally. Before the Second World War he was a young professor at Saratov State University, lecturing in physics and maths. However, when the war broke out in Europe, being an ethnic German became a political crime and he was sent to a Gulag camp in Siberia, where he spent many years building a hydroelectric power station and a railway. In 1949 he was allowed to join his family, who in the meantime had been deported to Novosibirsk.  An episode I vividly remember from my early years is finding a box of old medals at the bottom of an old chest hidden in my grandparents’ garden shed in Sochi. One of them was the Cross of St George, an award of high military distinction in the Russian Empire (before the revolution). I showed my findings to my grandmother. Being confronted, she explained that the Cross belonged to her father, who served as an army surgeon in World War I, whereas other decorations were related to the nobility status of her grandfather, a descendant of German aristocrats. In the 19th century her family lived in Poland (then a part of the Russian Empire), where they took part in the 1863 uprising and consequently were deported to Siberia, which was to become such a familiar place to my forebears a century later. The next time I tried to find those medals, they were long gone. It was only many years later that I found that my grandma Maria threw them all away immediately after the episode. Incomprehensible as it sounds to us today, this kind of behaviour became imprinted in the DNA of people who lived through the Stalinist terror. She was afraid I would talk about the medals to my friends and, if the story got around, the whole family would be in trouble. This happened in Khrushchev’s times, when the terror receded, but ‘bourgeois’ reminders were still deemed unacceptable by ‘the proletariat’ until the 1990s.  By the time I went to school, the mentality of Stalin’s time was largely gone from the Soviet system. Except for some remnants, such as the ‘nationality’ line and all those family questionnaires, young people like me were largely unaware of the recent terror. The only time I really suffered because of my ethnicity was when trying to get into a top university, as described below. Otherwise, it was just being occasionally called ‘fascist’ in the playground, or a ‘bloody Jew’ (‘ЖИД’ or ‘Zhid’), because a foreign name was often associated with being a Jew (in Russian, the word ЖИД sounds very offensive). Maybe because of the latter I am particularly keen to emphasise that some small portion of my blood is likely Jewish.  **Schooling as Usual** Despite this sombre family history, I myself was lucky enough to be born late and had a happy childhood. My best childhood memories are associated with my birthplace, Sochi. My grandma Maria was a meteorologist and I spent my first years of life on the beach, around the weather station where she worked. My mother was a head of quality control and my father chief engineer at a very large vacuum-electronics factory (chief engineer would be equivalent to a CEO in the West). After two decades, many people in Nalchik still remember him as a hard-working and influential person. Perseverance and hard work are the qualities I probably inherited from him. My parents’ occupations placed our family in the top layer of technocrats in the Soviet Union. They were not within the communist party elite who enjoyed all the perks of the Soviet system and, as ethnic Germans, they could not possibly be. Nevertheless their status allowed the family a relatively comfortable existence.  My school in Nalchik was called a specialist English language school and considered to be the best in town. Despite its name, the teaching of English was not its strongest point. Looking back and comparing how we were taught English then and how I was taught Dutch 30 years later, the notion of English specialisation in my old school seems nothing but laughable. On the other hand, mathematics was taught at an extremely high level, especially in senior forms, thanks mostly to our maths teacher, Valenida Sedneva. I may not have realised this at the time but, when I looked at my old exam papers several years later and was already a student at an elite university, I was amazed at how tough and challenging those papers were. Some of them required not only powers of recall but also imaginative and non-standard thinking. Physics and chemistry were taught at a good level, too. I once won a regional chemistry Olympiad, which however was not so much due to my love of the subject as to the fact that in a couple of days I managed to memorise a whole chemistry dictionary some 1,000 pages long (happily forgotten in the following few days).  I also fondly remember Olga Peshkova, our teacher of Russian and Literature. Despite getting excellent marks in these two subjects, I did not excel in either of them. Still, I like to think that her lessons were helpful in learning – eventually – how to write research papers in a clear and concise way. There is nothing else particularly remarkable to mention about my schooling, except for the brain-washing Soviet propaganda that penetrated every aspect of our lives at that time. As a counterbalance, schoolchildren often listened to the Voice of America and similar radio stations, and this small rebellion helped us to develop a healthy scepticism about many things (albeit not all) that the propaganda told us. Of course, like everyone around me, I played my due role of a disciplined Soviet pupil.  **Failing the First Hurdle** At the age of 16, I graduated from school with a gold medal, a distinction given to those who achieved the perfect score in all subjects (typically, the top 5%). My parents encouraged me to go to the best possible university and my sights were set on a couple of elite universities in Moscow. At school I was doing well in all exact sciences, including physics and chemistry, but my strongest subject was maths. However, my parents persuaded me that pure maths would not offer good career prospects. Hence, my decision was to study physics. The very top university for physics in Russia was (and still is) the Moscow Institute of Physics and Technology (Phystech). However, the entrance examinations to Phystech were famously competitive and extremely tough and, as I grew up in a provincial town, I believed they were beyond my ability. So, I chose to go to another leading university, Moscow Engineering and Physics Institute (MIFI). In the way of preparation I solved problems from sample MIFI and Phystech exam papers and felt ready, even if still not very confident. Little did I know that the main obstacle for me would turn out to be my ethnicity.  The first exam in MIFI was written maths, and I was pretty confident that I solved all the problems correctly and would get an ‘excellent’ (the marking system in Russian schools and universities consists of four grades: ‘excellent’, ‘good’, ‘satisfactory’ and ‘fail’). However, I then found it was only a ‘satisfactory’ and, even worse, my mark for the oral maths was a ‘fail’. I attributed this failure to poor preparation and my inexperience in sitting real tests: problems at my oral exam seemed a lot harder than those from the sample MIFI papers that I did at home. So I decided to go home, continue to study and take my chances a year later.  That gap year turned out to be very important for me. My parents were supportive and found a job for me at the factory where they worked, as a technician responsible for calibration of measurement equipment, and also paid for tutoring in maths, physics and Russian literature (these were standard entrance exams at my chosen universities). After a couple of weeks I found that I knew maths better than my tutor (who was considered to be the best in the town), so these tutorials stopped. On the other hand, my physics tutorials were the best I could wish for. My tutor was a physics professor from Nalchik’s University, Valery Petrosian. I thoroughly enjoyed every lesson. We solved many problems from old exam papers either from Phystech or, even harder, from international Olympiads. But even more helpful was the way he taught me to deal with physics problems: it is much easier to solve a problem if you first guess possible answers. Most problems at Phystech level require understanding of more than one area of physics and usually involve several logical steps. For example, in the case of a five-step solution, the possibilities for dealing with the problem quickly diverge and it may take many attempts before you get to the final answer. If, however, you try to solve the same problem from both ends, guessing two or three plausible answers, the space of possibilities and logical steps is much reduced. This is the way I learned to think then and I am still using it in my research every day, trying to build all the logical steps between what I have and what I think may be the end result of a particular project. After a couple of months, my tutor no longer asked me to write up a solution. Instead, I just explained verbally the way I would solve a particular problem – all the logical steps required to get to its end without describing routine details. This allowed us to go through the problems at lightning speed.  I also learned an important lesson from my tutorials in Russian literature. My tutor said that what I was writing was good but it was clear from my essays that I tried to recall and repeat the thoughts of famous writers and literature critics, not trusting my own judgement, afraid that my own thoughts were not interesting, important or correct enough. Her advice was to try and explain my own opinions and ideas and to use those authoritative phrases only occasionally, to support and strengthen my writing. This simple advice was crucial for me – it changed the way I wrote. Years later I noticed that I was better at explaining my thoughts in writing than my fellow students.  **Enemy of the State** After this year of intensive preparations I felt I knew enough and was much more confident than the previous year and ready for MIFI. I easily solved all the problems in the written maths exam (which again was first), polished the presentation and expected an ‘excellent’ mark. However, at the next exam (oral maths) I was told that the mark was only ‘good’, and the examiner refused to explain what was wrong or to show me the script, even though it was right there in front of him. He gave me three further mat hs problems, the hardest I had ever seen. I managed to solve one, partially solved the second one, with a minor mistake, and provided the correct answer to the third one. However, I could not explain how I came up with this answer. It just appeared in my head and I still remember it now: the answer was 998. The mark I got for these efforts was ‘satisfactory’, which was clearly not enough to be admitted to the university. In addition to the rather harsh treatment from the examiner, I noticed more odd things about the exam – apart from me, not one single person in the same room (about 20 candidates) managed to get even a ‘satisfactory’ mark; they all failed. Even more curiously, the names of all the candidates were either Jewish- or foreign-sounding. I went to look at the lists of people in other examination rooms and most of the names sounded Russian, with a very few exceptions.  Even for someone as naïve as I was at 17, it was clear that there was a policy in place to fail certain ethnic minorities. In hindsight this can be easily explained because this particular university specialised in nuclear physics and, at that time, if you were a Jew or a German, you were assumed to be a potential emigrant who would learn ‘state secrets’ and then go abroad. That was always considered a threat in the Soviet Union. So in a sense it was clearly a policy, and even an understandable policy, but not much advertised. Several years later I found that there were a few Jewish people who attended and successfully graduated from MIFI. To achieve this, their parents had to go to KGB representatives at MIFI (they were present in every Soviet organisation at the so-called First Departments) and persuade them that their children were reliable Soviet citizens and had no intention of leaving the country. Apparently, these tactics did work but neither I nor probably my parents even suspected that it was needed. Or, maybe my parents were too aware of the true lies in my family questionnaires.  **Accidental Physicist** This was the first time I experienced discrimination at an official level and it was quite a shock. Fortunately, there was still a week left to try my luck at another university. I said to myself ‘what the hell’ and applied to Phystech. The way I was treated there was a shocking experience in itself, as it was so different from MIFI. The examiners were friendly and even helpful, the exam problems interesting and the whole environment welcoming. I felt as if by mistake someone put me in a wrong room, away from a firing squad of examiners. Perhaps, this was the case.  My examination marks were comfortably above the threshold required for admission, even though I got only one ‘excellent’ mark out of four exams, with the rest ‘good’. I felt that I could have done better but my MIFI experience was still fresh, and the memories of those failed exams kept coming back, affecting my concentration and sometimes my judgement of the difficulty of the problems. This was especially apparent in my oral physics exam, which I still remember well. The first problem given to me seemed easy and I quickly solved it, but the examiner said ‘It’s a wrong answer’. I tried to protest, and it took us a few minutes to understand that I had solved a much harder problem than the one he gave me; even though the answer to the problem I actually solved was correct, it was still wrong. Incredibly, the same story happened with the second problem. So, when giving me the third one, the examiner repeatedly asked whether I was sure that I understood what was being asked.  The last hurdle at Phystech was an admissions interview and I was scared that the question of my ethnicity would arise again and they might not accept me despite my good marks. It was well known that, on the basis of the interview, sometimes candidates with marks just below the threshold were accepted and those with marks above rejected. The ethnic question did arise in the form of ‘How is your German?’ I answered ‘Barely’ and started thinking what else to add. One of the panel members (Seva Gantmakher, as in the Gantmakher effect) quickly interjected saying ‘Then he is not a real German’. As it turned out, this remark, as well as his following interventions, influenced all of my further life by putting me on the path of solid state physics.  Like many would-be students of that age, I dreamed of doing astrophysics or particle physics and aspired to solve ‘the greatest mysteries of the universe’. But there was a rumour among Phystech candidates that saying so was considered to be very naïve by interviewers. I remembered that but did not want to cheat. So, when asked about my aspirations, I said that I wanted to study neutron stars (true) because I wanted to understand how matter behaved at extremely high densities (an excuse, not to sound so naïve). A prompt reply from Seva was ‘Good, you can then study high-pressure physics at our Institute [of Solid State Physics].’  Another memory of that interview is being asked to estimate the weight of the earth’s atmosphere (it was customary to give candidates some tricky mental problems to solve). I spent most of my three minutes multiplying the numbers in my head (atmospheric pressure multiplied by the surface area of the earth divided by gravity, all in SI units) and when I gave an answer in trillions of trillions of kg, everyone was surprised because I was only expected to give a general answer, not a specific number.  This is how I entered Phystech. In the end, my rejection from MIFI turned out to be a blessing in disguise because Phystech was a two-notch higher level university. The only reason I did not go there first was because I did not believe I was up to it. Basically, circumstances forced my first choice on me rather than my second one!  **Mother of All Grillin** Phystech is quite an exceptional university, not only by Russian standards, where it is considered crème de la crème, but also with respect to any other university I know. The only reason that it is not found in any world league tables is that it is a purely teaching university. (Teaching and research are traditionally separated in Russia – research is done mainly at the Academy of Sciences and teaching at universities). In addition to the very rigorous student selection, a well-known reason for Phystech being so good was that, unlike other Soviet universities, all specialist and some general courses were taught by practising scientists from the Academy institutes from all over the Moscow region. Of course, in the West it is a standard to have active researchers giving undergraduate courses, but in Russia it is an exception.  Even more importantly, as Phystech students we were forced to think and find logic in everything we studied, as opposed to just memorising facts and formulas. This was largely due to Phystech’s examination style: when it came to specialised subjects, many of the exams we took every year were open-book. This meant that there was no need to remember formulas, as long as one knew where to find them. Instead, the problems were challenging, requiring combinations of different subject areas and thus teaching us to really understand science rather than merely to memorise it.  From the moment of its establishment, Phystech was led by prominent Soviet scientists such as [Kapitsa](https://www.nobelprize.org/nobel_prizes/physics/laureates/1978/), [Landau](https://www.nobelprize.org/nobel_prizes/physics/laureates/1962/) and many others. Among my own lecturers and examiners were many eminent scientists such as Emmanuel Rashba, Vladimir Pokrovski, Viktor Lidskii, Spartak Belyaev, Lev Pitaevskii, Isaak Khalatnikov and Lev Gorkov, to name but a few. I have to admit that their names did not tell me much at the time, which was helped by the fact that I was not very good at attending lectures. I rediscovered some of the names only recently, when I saw their signatures in my old exam certificates, which Phystech put on the web after the Nobel Prize announcement.  The workload at Phystech was heavy and the courses extremely challenging. It is probably enough to say that our standard textbooks for quantum mechanics, statistical physics, electrodynamics and classical mechanics were from the Landau-Lifshitz Theoretical Physics Course. Perhaps they are not the best textbooks for undergraduate students, but they are a good indication of the expected level of achievement. Not all students managed to cope with the psychological pressure imposed by this teaching style and some dropped out not only because of bad marks but, more often, because of nervous breakdowns. I personally knew several students who developed suicidal tendencies and psychiatric problems. My own sanity was perhaps saved by the amount of alcohol that I and some of my friends consumed after each exam to release the accumulated stress.  The first two and a half years of foundation courses were particularly tough. After that the pressure subsided as we moved on to specialist courses. From year three we started attending lectures at the so-called base institutes of the Academy of Sciences. In my case it was the Institute of Solid State Physics in Chernogolovka, chosen at the above-mentioned interview due to my love for high-density neutron stars. From year five, we also started working in research labs – not on some specially designed undergraduate projects but on real ongoing projects, where we worked as part of an academic research team. Year six was a Master’s year and 100 % research based. After that, the normal route (if you wanted to stay in academia) was two years of research probation and, if you were successful, you were eligible for a PhD student-ship, which lasted another 3 years. It was an 11-year long process to get a PhD – 6 years at Phystech plus 5 years leading to a ‘viva’, or oral defence of one’s thesis.  For me personally, only the first half year at Phystech was a struggle. I came from a provincial town, while some of my classmates were graduates of elite Moscow schools specialising in physics and maths. Quite a few were winners of international Olympiads in physics or mathematics. The first few months were essentially designed to bring everyone to the level of those guys; they were nearly a year ahead of the rest of us in formal topics, especially maths. Only after I got all the highest marks in the first set of mid-year exams did I start feeling confident enough in this wunderkind environment and was able to relax somewhat. Despite all the pressure and grilling, every single one of us who managed to graduate from Phystech have great memories of those hard years and are most proud of our alma mater.  **Go With the Top Flow** I graduated from Phystech with a so-called ‘red diploma’, which meant within the top 5 to 10% of my class. Out of 50 or so final exam marks, I got only two ‘good’. One of them was for a course on “political economy of socialism”, which I attributed without much shame to my inability to find any logic in the subject. By contrast, I got ‘excellent’ for the political economy of capitalism and to this day have fond memories of reading *Das Kapital* by Karl Marx, whom I occasionally quote to tease, or perhaps shock, my Western colleagues. My second ‘good’ was for the course on superconductivity taught by Lev Gorkov himself, who also was my examiner. Oddly for Phystech, he did not allow us to use textbooks during the exam (shame on him), and I made a mistake in one of the derivations. This is funny because in the 1990s, when I was already a professor in the Netherlands, superconductivity became my research subject.  Despite my exam success, I do not believe I particularly stood out among the students in my class. In my year there were one or two students with only ‘excellent’ marks, and some were digging deeper and understood the courses better than I did. At that time I did not really try my best; I worked just hard enough to guarantee myself maximum marks and stay at the top of the class. I was successful at that but it did not take all of my time or effort. In fact, in my university years I was not at all an exemplary student. With excellent marks, I normally was entitled to a scholarship awarded every half a year, but it was quite regularly (four or five times) withdrawn as a punishment for missing some mandatory lectures, being late from holiday breaks, organising those after-exam parties that sometimes saw some people end up in a hospital and similar misbehaviour. Missing lectures was generally allowed (unless it was a political subject) and I managed to miss most of them. I learned from textbooks and attended group tutorials, unless I disliked particular tutors. I would not recommend this style of learning to aspiring students as a recipe for success, but it may well suit some people as it suited me and a few other students in my class.  My attitude of doing alright to reach a goal but not doing my utmost persisted through all the university and PhD years. I only started to really enjoy physics and do my absolute best, for its own sake, much later when I became an independent researcher.  **From the Sublime to the Ridiculous** The topic of my Master’s project was electronic properties of metals, which I studied by exciting electromagnetic waves (so-called helicons) in spherical samples of ultrapure indium. From the helicon resonances I could extract information about the resistivity of those samples. The competitive edge of this research was the extreme purity of the indium I was working with, such that at low temperatures electrons could shoot over distances comparable with the sample diameter (~1 cm). After graduating I started working towards my PhD in the same laboratory, as was customary for many Phystech graduates. Looking back, those five years of doing PhD seem remarkably uneventful in terms of the science I was doing.  My first year as a PhD student was signified by an event that was to become a rather regular perturbation in my life: moving from one institute to another. This was when my PhD supervisor, Victor Petrashov, moved from the Institute of Solid State Physics to the newly established Institute of Microelectronics Technology. Although the two were only 200 m apart, it meant a serious disruption of work, losing some equipment and setting everything up again. Initially, I did the metal physics research with some enthusiasm but it gradually faded away as I realised that no one, except perhaps my supervisor, was interested in what I was doing. Nevertheless, educationally, those years were very important for developing experimental skills and making my fingers ‘green’. This experience played a crucial role in my further research career, including the graphene story. In this respect I owe a lot to Victor, whom I count as one of the most skilful experimentalists I ever met. With the help of a shoestring and sealing wax he could do amazing things, and a shoestring and sealing wax was what, in those days, we typically had in research labs in Chernogolovka.  I meet quite a few people who feel nostalgia for the ‘golden era’ of Soviet science, but I myself never saw those times even in Chernogolovka, which was a rather elitist academic place. My recollection is that the arrival of almost any material important for research, be it copper wire or GE varnish, was a cause for celebration, almost on a par with the arrival of a multimillion piece of equipment in the West. Once Victor was lucky to borrow a US-made lock-in amplifier to do some measurements, which we usually had to do using a Soviet equivalent (the word ‘equivalent’ does not describe the entirety of the difference). In just a couple of weeks I was able to get results that I could not dream of with the ‘equivalent’. The availability of resources (or the lack of them) essentially dictated what I could possibly do. I believe experimentalists who claim to have witnessed ‘the greatness of Soviet science’ either belonged to the select few who had benefactors among the top academicians or more likely fool themselves, choosing to believe that the skies were bluer in the old days.  Having said this, it is true that in the Soviet Union there was a huge difference between being an experimentalist and being a theorist. The theory school was extremely strong, especially what people referred to as ‘Landau theory school’. Those guys did things at the highest possible level. The roots of this strength were partly in education but also in the way Soviet theorists worked. I witnessed it by attending many research seminars. A lot of time was spent in discussions and heated debates, where there were no questions that could not be asked and no authority that could not be questioned. In the West, this style is still remembered well by those who ‘experienced’ Soviet scientists in the 1980s and 1990s. It could be a dreadful experience for the participants, but sometimes I really miss this style. The nostalgia usually appears after coming across certain papers in today’s scientific literature: If they were to be first presented at such seminars, even the authors would not dare to put them in print. Those debates were very influential and allowed people to learn more quickly and to develop a broad and informed view of many areas of physics. I myself benefited greatly from such seminars and consider them the second most important part of my education in Chernogolovka. Many of the seminars I attended were organised by Seva Gantmakher. His care for detail and breadth of experimental knowledge were a great example for me and my fellow students.  Despite the great atmosphere in theory departments, even theorists suffered from the state of Soviet science and in the late 1980s many of the best of them moved to the West. I do not think that better living conditions were the only reason for this brain drain: Theoretical ideas do not come out of vacuum; they are often born in interaction with experimentalists, as experimental results serve as a trigger for new ideas. This was completely lacking in Chernogolovka, because new results were hard – if at all possible – to get with the existing equipment. By the time of my PhD, Soviet experimental science had decayed to the point where it was considered that the most appropriate route to reach the top of fame and glory for an experimentalist was to confirm a theory produced by an eminent Soviet theoretician. Indeed, many experimentalists in Chernogolovka were doing just that.  This was my scientific life. Parallel to that, there was another life, busy with events. Chernogolovka is a nice Moscow suburb, quiet and peaceful, surrounded by forest. Life was generally pleasant, even though my living conditions were austere to the extreme – for most of my years there I lived in a residence hall, sharing a room with two other young researchers. One of my roommates was Sergey Dubonos who over the years became my regular coauthor and also played an important role in the graphene paper recognised by the Nobel award. In addition to research, my other hobbies were mountaineering and white-water canoeing. Every year I spent more than a month in the mountains and on the rivers in different corners of the Soviet Union, from the Caucasus to Central Asia, sometimes managing to fit in as many as four trips in a year. Those travel experiences were often shared with Max Maximenko and Phystech friend Stas Ionov. It was at this time that I met my wife, Irina Grigorieva, who was also working towards a PhD at the neighbouring Institute of Solid State Physics. She later became my collaborator and significantly contributed to the graphene work.  In a way Chernogolovka offered ideal conditions for scientists – there were hardly any distractions, which allowed us to concentrate on research. Except for queuing for hours for sausages and cheese (which had become a regular scene in the 1980s), most of our time was spent in the labs. Even without much enthusiasm, my research advanced at a steady pace, with a few papers published and due progress made. But it was only when I became an independent researcher, and especially after moving to the West in 1990, that I started to do my real best and the pace of my life changed dramatically, as described in my Nobel lecture ‘Random walk to graphene’. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0039 = AR**  [Andre Geim] Andre Geim.  [Adam Smith] Oh, hello. Professor Geim, this is Adam Smith calling from the Nobel Foundation website, in Stockholm. Congratulations on the Prize. We have a tradition of recording very short telephone interviews with new Laureates. Would you be willing to record a few minutes interview with me?  [AG] Yeah, carry on.  [AS] Thank you very much. So I gathered that from your press conference at the KVA that the news interrupted other plans that you had for the day?  [AG] Yes, Sir, that’s correct, ok, yeah. I was planning to write a paper and answer hundreds of emails I usually get from my collaborators. So, it’s work, work and work. And, yeah, Nobel Prize interrupted my work. I’m not sure that it’s a useful interruption, ok, certainly it’s a pleasant one.  [AS] You sound like somebody who might be able to handle the interruption though.  [AG] Yeah, I used to answer enquiries of journalists and so on. So it was not a new experience in my life.  [AS] I’d like to start by asking you about your research style. It seems that play is a very important part of your research.  [AG] Yeah, there is a sort of, you know … there are different sort of scientists. Some of them that work on a particular subject usually induced by their supervisor at early PhD years. And, then they continue through the whole life studying essentially the same subject. Not that I’m despising this style. I understand that different people are different, and they’re allowed to do whatever they want.  But, for me, it’s very boring to deal with the same subject year after year. So, whenever we are doing any particular research, at the same time I’m looking what else can be done. Using the facilities and knowledge we have at hand … And, yeah, this is essentially the case. And, ok, whatever I do, even if I’m doing graphene all the time, I’m thinking, “Ok, what else? What can be used to?” And, usually, it comes out something like late evening or Friday night experiments where you try something very elementary and try to go into one or another direction. And, ninety nine times out of hundred, you do not succeed, but, sometimes there are very simple experiments and very simple discoveries to be made using what is at hand. OK, example, some popular … Not popular but educational experiments, like levitating everything what is around in the lab, which was eventually to emphasize the importance of diamagnatism. We levitated a frog and, later, we did experiments with gecko tape, making the first realistic example of a tape which uses the same mechanism which geckos use to climb walls. And, graphene was within this series of very many failures, but this was a successful one!  But, those three examples are only something which you can expect the general public and people who do not have a specific knowledge … OK, similar experiments were on much more boring, if you wish, much more Boffin-like topics, before and after.  [AS] It’s not bad to get a Nobel Prize for a subject that you describe as being ‘non-Boffin-like’. That’s nice!  [AG] Yeah, it’s, it’s very non-Boffin-like. A big part of it, it’s straightforward, OK. Do you want me to explain which part is non-Boffin-like?  [AS] No, no, it’s ok. I mean, for a start, the isolation of graphene using Scotch tape seems beautifully non-Boffin-like and wonderfully accessible. It gives hope to all.  [AG] Yeah, it’s a great educational experiment. In a sense not that it’s isolation of graphene: it shows people that, in fact, you don’t need to be in a Harvard or Cambridge, in one of the universities which collect the smartest people and the best equipment. You can be in the second or even third rated universities in terms of facilities and, whatever, prestige, but you still can do something amazing and something which, I hope, this is an example, which brings more enthusiasm to young generation of inspiring scientists, that they can do something without being at the best place at the best time.  [AS] Hmm, hmm, that’s a nice message. The trick in having this approach of playing with new things while finishing off old things must be getting the balance right. You have to learn to find new areas while not neglecting the one’s you’re working on.  [AG] Yeah, balance is important. And, putting long hours because nothing comes for free. If you … It’s extremely hard, it’s extremely hard. First of all, not all the experiments I mentioned – levitating frog, gecko tape, graphene – were originally funded by anyone, ok. And, only graphene later got some research grants to continue this work on another level. But, essentially, you have your work for which you are paid and, yeah, you have not to neglect this work. So, at the same time, you want to start a new subject and, it requires a lot of hours to find the previous literature because, if you are not an expert, you have to look through the literature not to invent the wheel again. And, this is the hardest one.  And, in addition, OK, balance is not as important as courage. Because … Courage is really important because you stumble on something, ok, which you are still not confident. You feel, ok, sort of you feel secure within your own research area and what you are doing. If you are doing something new, you always can be considered as a fool, inventing the wheel, as I said. Or, you can just be wasting your time. So, the courage is not social courage. The courage is about, ok, investing your time into something which might turn out like a blip.  [AS] I suppose that, there again, play comes in because if work is being play, partly, then it’s easier to put the time in.  [AG] Yeah, absolutely. So, yeah, my work is my hobby. Some people would call me a workaholic. I don’t consider this time: I just love my work so much, so it’s my real hobby, ok. And, yeah, getting some play during working hours for which you are paid is the best job I can recommend for anyone around!  [AS] Much of course, now, is made of the possibility for graphene’s applications. But, am I right in thinking that you’re more fundamentally interested in the basic science that can be accessed through graphene?  [AG] Yes … By my background, my knowledge and my experience, ok, just pushed me into the direction of studying fundamental properties of this research. I guess the Nobel Prize is mostly for discovering the fundamental properties despite it’s so short in citation. It’s probably for fundamental properties. On the other hand, you see the huge amounts of possible applications and you can’t just stop yourself playing again, trying to demonstrate proof of concept devices.  During the last five years, I compared myself with those Jack London books where people go in Alaska through a mountain pass trying to carry big rucksacks full of stakes in their rucksacks. And, after this mountain pass, they try to put those stakes in the ground to cover their area. So, I many times compared myself with these gold miners who are trying to put a lot of stakes in the ground. So, many of those stakes, I put during the last five years were on proof of concept devices, whether they were LCD devices, liquid crystal displays using graphene, or ultra sensitive chemical sensors, or transistors.  Having such a new and amazing material with so many potential applications, it just forces to think about applications. You consider, as you said, it’s a part of play making something that’s maybe not that non-Boffin but a little bit Boffin-like, but still new.  [AS] If one extends the gold miner analogy a little bit, I suppose those gold miners suffered tremendous competition from each other. How do you view the competition in the graphene field now because it’s so huge?  [AG] It’s enormous competition, OK. So we are more or less free from competition for the first three years, which allowed us to put a lot of stakes in the ground. It’s essentially because this routine which most professionals use in the field when they find something interesting worth pursuing, they employ a post doc or PhD student and it typically takes two, three years to produce a result. On the other hand, my style is just, ok, switch off from one direction and switch on another direction. So, we are … in this style we were a little bit ahead of the competition. But, yes, competition is enormous with thousands of researchers involved. I have, I tried, I’m very proud when I scoop someone, ok. I scoop quite a few people in the area, but I have been scooped on a regular basis myself, ok. So, I’m sorry about myself and sorry about many other people, like Phillip Kim for example, a researcher from Columbia whom I’ve certainly scooped quite a few times but he scooped me back!  [AS] So, do you see yourself migrating away from the graphene field in the future?  [AG] Ah, I can accurately predict only the past. Yeah, future is, really hard to predict. At the moment, each time I’m tempted to leave the area for something which is less crowded … In effect I have been doing this for the last three years, trying to migrate away. But, graphene is such a huge research area that it’s possible to make a switch in your research direction as large as we did a couple of times, which from physics into chemistry, and it’s always in graphene. So, in a sense, I jumped already using this play, or game, analogy a couple of times – but it was within the graphene research area. So, yes, I’m trying to find a subarea of the research and it can be as far from – as far in *knowledge* as say a flying frog from graphene. So, it’s a lot of work and a lot of knowledge was acquired during the five years time, although these days everything comes under the umbrella of graphene, if it’s even very far within the same area.  [AS] Right. May I just ask you about your relationship with your co-Laureate, Kostya? You’ve worked together for some time and it’s obviously …  [AG] Ah, yeah, he came as a PhD student. Although I was not officially his supervisor, non officially he was my PhD student. I invited him from Russia when I was working in Nijmegan. And, you see how good he was! So, the first thing when I left the Netherlands was to invite him as a post doctoral researcher although he didn’t get his PhD at that time. So, we were working for the last, I would, say twelve years together. I do not make my … with people whom I respect, it’s Kostya and quite a few others, I don’t make much distinction between whether you’re a PhD student or whether you’re a full professor. As long as you work hard and you work efficiently, all people are colleagues. So, that was appreciated. So, it’s one of those cases where usually people fall out, especially if there is something big to share. We didn’t fall out. We will never.. we’re OK. And, ah, yeah, he’s one of the two, three, five colleagues with whom I’m really happy to spend my working hours.  [AS] Thank you. I should just finish by asking, since you’re the first Laureate, to my knowledge, to have been awarded an IgNobel Prize and a Nobel Prize, do you plan to …  [AG] To my knowledge too!  [AS] Do you plan to display them together in the office?  [AG] Ah. Unfortunately, the IgNobel Prize … I have somewhere the IgNobel Prize in my office. But, it’s not really something, ok, visually attractive. OK, but speaking seriously, I’m actually quite proud of my IgNobel Prize – not because of IgNobel Prize – it’s a more subtle association. Essentially, yeah, IgNobel Prize is given for something which forces people to smile. And, that was always the idea behind the flying frog, which we shared with another distinguished scientist, Professor Sir Michael Berry, known for his ‘Berry phase’. And, the second thing which with Nobel Prize, it’s quite obvious that, if you are offered, I’m not aware about anyone who rejected an offer of Nobel Prize, ok. Although history might be long.  [AS] Well, there’s [Jean Paul Sartre](https://www.nobelprize.org/nobel_prizes/literature/laureates/1964/) but, otherwise, you’re pretty much right, yes.  [AG] Yeah, ok, so, so … But, with IgNobel Prize they ask you a few weeks in advance whether you are ready to accept this prize or not. So we considered hard and, at the end, we had courage to accept this IgNobel Prize, yeah. So, I’m proud that I had enough courage to do that and I do not regret accepting it.  [AS] It certainly doesn’t seem to have done you any harm. On the contrary, I suspect.  [AG] Yeah, with the benefit of hingsight! Ha!  [AS] Ok, well thank you very much …  [AG] Ah, ok, bye.  [AS] A great pleasure to speak to you, thank you. Bye, bye.  [AG] Bye! |
| **Interview** |  |
| Q2 | **I think it gets busier as it goes on. I want to start by discussing the subject of play. People often view science as a very serious exploit but it’s really quite playful and you in particular keep play at the forefront of your research activities. Can you tell me how play figures in your research?** |
|  | Konstantin Novoselov: I came to do my PhD with Andre in 1999 and at first for me it was just an opportunity to work in some other lab, but I was so fascinated with the style of work that Andre imposed on the lab that I worked with him in the same lab since then. And it is really important that you Andre impose the style which promote the freedom of mind, you are just allowed to do whatever you want as long as it’s not boring, and for me I’m not sure if, even if we are not doing science in the eyes of other people, it’s nice, it’s enjoyable and that’s what I like. |
| Q2 | **So, with the isolation of graphene which was done in this beautifully simple way just with sticky tape on graphite we are told. Was that the end of a long series of attempts to isolate graphene where this was just the last thing that one might try?** |
|  | Andre Geim: No, it was the beginning of a long series of serious experiments. Yeah so, it was very first try which turned out working and then it was okay a few years to hard work to clear it up.  Konstantin Novoselov: Andre is a little bit modest, in fact it was his idea first to create the metallic transistor and we thought about several possible ways and it was one of the possible ways and it did work. It was, although it sounds playful, it was quite a serious series and it ended up nicely.  Andre Geim: It’s probably doesn’t give the sounds okay what’s adventure and courage and so on, probably. What is important is, that when you are working within your specific direction you know all minor details of the area, you are aware about or control etcetera. The biggest adventure in doing different experiments, is to move into area where you are not an expert. It requires a lot of work to accumulate additional knowledge, read hundreds and hundreds of different papers and be courageous enough to enter in the area where you have not been before so, for me for example, in mid-nineties, I switched subject from semi-conductor physics to superconductivity. I went to those conferences as a beginner with having a couple of already prestigious papers being an associate professor and people looked at me and something like that, who is this mature post-doc, what his teaching with us. Because I came from a completely different community. It requires a lot of work and this sort of moving away, it’s not secure. You are moving in the unknown waters which is not only scientifically unknown but in terms of psychology, it’s unknown community. This requires a bit of adventure.  Konstantin Novoselov: It’s a great pleasure as well, if you are young like a student again because you were for a very short period of time, you got to learn so much that people will learn over decades and that’s a great pleasure. |
| Q4 | **In this case it seems to be a successful melding. Let’s talk about graphene a little bit. It’s a single atomic carbon in a hexagonal lattice just one atom thick and its forming exists in this single atom sheets. It’s supposed to be the strongest material on earth is that correct?** |
|  | Konstantin Novoselov: Well, let’s start from the beginning. Over the last seven years, people like to talk about how strong it is and how conductive it is and how probably useful it’s going to be for the electronic industry, but for me it’s just the very existence of it something which is only one atom thick can exist on millimetre or even inch size and be so perfect and it’s practically resolve any defect that you can actually pick it up and hand it in your fingers put it back and still it will work as a perfect transist. Just the very existence of this material is still very fascinating for me and then in addition to it, all the other properties that can come on top is the most, is the strongest material known to us, it’s probably the most conductive and we are so lucky with finding so many unusual electronic properties in it, on which we really specialize. |
| Q14 | **Lovely, it’s great depth there to discover then. And it acts as a laboratory as well as a material which can be applied, and I understand you can do some very fundamental physics using graphene. So, it holds potential both for the expiration of quantum physics and for applications such as touch screens and alike. Are you interested in the entire spectrum of those things or do you tend towards one or other end of the spectrum?** |
|  | Konstantin Novoselov: Whe’re generally interested in what is interesting whether it is, if it is applications which are interesting at this particular moment for us, then we do some particular part of the applications. Mostly, it just happened that physics is so fascinating in this material indeed as you said, electrons behave so specially in this material, they mimic relativistic particles, so you can produce experiments which were not possible with real elementary particles in the real world, and it allows so much freedom and so many different types of experiments that honestly, one can be lost. Myself and Andre, we often complained that these Friday evening experiments, which you were mentioning, are no longer active but in fact, when you think about this material it’s not true, we broaden the spectrum of our work in the lab so dramatically we now study, not only transfer properties that we are used to do, we also study mechanical properties and optical properties and chemistry and the interplay between those. That’s the overall, the spectrum of the experiments is probably even wilder than from the levitating frog to superconductivity. |
| Q20 | **Is it difficult to choose what experiment to do, given all these possibilities?** |
|  | Andre Geim: Well, circumstances force us to move in one area or another area, so you have a limited freedom of choice. Circumstances sometimes dictate, just for simple reasons, one area even was incorrect. /- – -/ research becomes so crowded, another area is still not exploited, you have certain possibilities with your experimental facilities, so, it dictates what you can chose and within this already limited choice of options you have to think what to do and then you poke in the right direction similar to this Friday night experiments. If the poke is unsuccessful after few days, few weeks, so even a few months, I usually say, let’s cut losses like on a stock market and do something else within this area. So, it’s always hard but at the same time we are forced into. |
| Q3 | **So is it that you like to be first in an area, you like to seed in an area?** |
|  | Andre Geim: First is a waste when to many people working in the same area, secondly, possibilities are wider when you are in a new area and you are not afraid of being scooped because when you are in some area you have this feeling that you have to rush. When you are in some other area you can exploit in a better way and think about what you have found rather than try to publish in front of other people, so that’s much better. Feels much better.  Konstantin Novoselov: As in this sense, we were so lucky with our first paper on graphene, we had this privilege of working on it for more than a year. Our days it’s so competitive, we have to work much faster, and publish results within half a year probably. |
| Q10 | **It’s interesting, people often talk about science is this great collaborative enterprise where there’s the enjoy partly is the give and take between all the participants in a particular research field and conferences and learning from each other and it’s a balance between that pleasure and having more space to do your thing in a less crowded field. So how does that work for you? How much of the collaborative enterprise do you enjoy?** |
|  | Konstantin Novoselov: It is indeed a very fine balance and I’m still proud that Andre made this, I’m proud, that Andre made this decision when the very first paper was coming out that we didn’t try to patent this method or graphene or anything, we just invited as many people from all over the world with the task to joining us in this situation. People used this opportunity which increased this field dramatically and since then we enjoyed our collaboration and I hope that was the moment we set up the mood in the community because the community is incredibly friendly. We have so many very good collaborators, tough collaborators, but they are really fair collaborators and many of them are going to be here this week. |
| Q21 | **I wanted to ask you about that decision not to patent, was that difficult** |
|  | Andre Geim: We actually prepared … it’s a well-known story these days, we actually prepared a patent on the basis of this first paper, our first paper, on graphene and it was with patent lawyers arranging a proper manner of speech and so on. And at that time when it was still with lawyers, I was at a conference, speaking with a representative of a very large multinational company, billions and billions of dollars, so you can guess which company this could be yourself. So, I told, we have this patent concerning graphene, it’s very expensive and troublesome to patent a whole area and possible sideway. Outcomes of this research, would be you interested in collaborating with us, we provide sort of ideas, you keep this patent running and also patent it whatever further comes, we are not very much interested in royalties, very tiny fractions of percent would be good if it ever comes to this one. And the guy told me, You know, we are aware about graphene, it’s a deep promising material, but we don’t think anything will come within the next ten years. If after, year eleven, we find out that this material is as promising as it is, I will put 100 lawyers on this project, each of them will file hundred patents per day and you’ll spend the rest of your life and the GDP of your little island, direct quote, “to try to sue us”. It is very arrogant and so on, but actually it was very useful to know this opinion because then I realised that there is no point of patent whole areas or visionary ideas, graphene electronics, graphene that, graphene this, you have to pinpoint some particular application and then you have a chance to do something otherwise its patent is just a general knowledge and there is no point in patent a general knowledge, so it was a useful comment despite of his arrogance.  Konstantin Novoselov: We should say, since then for several years, when ever came back to the idea of patenting anything so just, until recently. We were enjoying our scientific work only. |
| Q19 | **And then the application you chose to patent in, were what?** |
|  | Andre Geim: It’s about flourographene, two dimensional teflons, so I think it’s a bit narrow enough that it may stand a chance to bring some application in foreseeable future which might bring money to this project.  Konstantin Novoselov: We just loved the name two dimensional teflons. |
| Q4 | **So there has been some debate about whether graphene is two dimensional material or not. Do you think it is a two-dimensional material? Or is this debate in unlearned circles?** |
|  | Konstantin Novoselov: I am not aware of these debates. Of course you can argue whether it is two dimensional or has some thickness or if you band it or fold it would acquire some three-dimension properties. When we call it two-dimensional, we call it two-dimensional in electronic terms, that electrons behave as being restricted to two dimensions only. Of course, for certain applications or problems, like the mechanical stability, flexural phonons, the repulse on the schiffer of graphene or in graphene, you might consider it as a three dimensional. |
| Q4 | **So two dimensional teflon is to do what?** |
|  | Andre Geim: It’s fluorinated graphene to each cover atoms you have attached a fluorine atom, so that’s essentially the same graphene sheet with attached fluorine so it’s two-dimensional teflon. It’s an analogous molecule so a molecule of teflon is just a chain of carbons with the fluorine atoms attached, in this case we have a whole sheet of carbon which is graphene which is /- – -/ metric manner attached fluorine atom to each carbon.  Konstantin Novoselov: But we also seen as the demonstration of another idea, you can consider graphene as a two dimensional material, a two dimensional crystal, but you can also consider it as a giant organic molecule and then you can modify by chemistry and here you can for example attach fluorine to it and use it as an insulator for some applications if you want, but we hope that the idea goes even further. You will be able to modify it by other means as well and acquire some additional or some other properties.  Andre Geim: It’s a new type of chemistry we invented on the basis of graphene, it’s a giant giga molecule which you can modify. Previously, graphene was ‘peppered’ with different chemical molecules or dopes and so on, and in this case just like in chemistry, you take one molecule and you change it, in a very specific manner into something else, and in this case graphene was changed in very specific manner to something else, rather than peppered something else. |
| Q15 | **So which applications are you most hopeful of seeing in use? From graphene.** |
|  | Konstantin Novoselov: Myself, the most hopeful of seeing applications which I haven’t even thought about, because up to now what we had, we just take this material, we figure out which particular properties are slightly better than in other materials like, for example mobility is a little bit better than in silicon, so we can use it for transistor application or transparencies, that would be better than in other material and we can use it for mechanic application. But for me the best one would be when you invent an application for this particular material, because it has this unique combination of very unusual properties; it’s being the strongest, the most elastic, the most conductive, the most transparent, so you just try to invent something for yourself. Now there is bendable electronics, quite fashionable and probably stretchable, electronics or I don’t know, but something like that which doesn’t exist yet, where we don’t use graphene as a replacement for another material for an application which is specifically created for graphene, that is what I am hoping for.  Andre Geim: Let us be clear, usually it takes decades for a material to go from an academic lab into industrial lab. It’s a very long way, involving many, many people before something originated from an academic lab comes to a consumer product. The Nobel Prize, at least as I perceive it, was not given for application, it was given, in my view, for bringing this material to the attention of a wide range of community, not even a single community of physicists, so it’s like a philosopher’s stone stand out to be of material whatever properties this material you touch, whatever properties you touch was this material, it turns out to be magic and applications is just an extra bonus to this project and we never expected that after five years, people would start talking about applications. It did happen and at the moment it’s a promise, a very strong promise for example, recently I was shown a graphene roadmap from Samsung and on this roadmap, there are, I don’t know, twenty, thirty, fifty different points. Each point means a specific application, which happens in a year, say 2012, 2025, and another axe it’s marked capitalisation in billions of dollars per year, so there are many applications, but let’s see, let’s wait for another 3 to 5 years before we will see first gadgets in a wider use. |
| Q3 | **I wanted to just go back to your scientific beginnings and ask you both why you chose to become physicists? What attracted you in the first place? Andre would you like to start?** |
|  | Andre Geim: It’s always very simple, you are at school, you are doing one or another subject and I was much better in maths and physics than, say, in literature or in English. Okey, I got all high smart, but I was much better in physics and maths in particular. And then it is natural progression. |
| Q6 | **And you both, now you both work, at the University of Manchester, there was a sojourn in the Netherlands and indeed you are the Dutch citizen. What drew you away from Russia and towards Western Europe?** |
|  | Konstantin Novoselov: Before I start answering this, I should say probably that, it’s in principle quite natural for scientists to go from one place to another. There are of course circumstances as well, but it is natural, and it should be natural that you do your masters here, you go for your PhD elsewhere and go for a post-doc somewhere else. And that is how I moved from my university, Chernogolovka, to Holland and then in Manchester and I’m a little bit ashamed that I spend already, what, nine years now in Manchester and, it’s probably, I won’t say it now, that it is a good time to move and I’m a little bit ashamed of that. It’s a natural process, it helps in exchanging techniques, knowledge procedures. |
| Q5 | **Ok, thank you. To close, I’d just like to ask you both about each other. Maybe I should do when you are not sitting next to each other, but nevertheless, this is the opportunity. So, first of all, Konstantin, what is it that, you obviously have a strong partnership that works very well, what is it that you admire most about Andre’s work?** |
|  | Konstantin Novoselov: Andre is, was my supervisor, but supervisor is some cold term, so he’s my teacher. They teach you a lot about physics in the university, but they don’t teach you how to do science. You have to learn by yourself how to do science and it’s just, you only learn about it from someone. Andre got a very specific way of doing science and I’m really glad that I learnt exactly that way that you, first of all you are honest with yourself and you honestly consider the results of your experiments, the very pragmatic way of doing experiments and that has been since my PhD years and then through post-doc years, so I hope … We were always colleagues, that’s how he set up things in the lab, but I learned every day from him, also from other people in the lab of course, but I still learn a lot from him.  Andre Geim: I quite often hear from my colleagues who say, my PhD student, my post-doc, my lab and etcetera. There is this style, if you ever hear my interview I never say ‘my’, I always say ‘our’, sometimes it’s misinterpreted when I say our lab interpreted as Kostya’s lab and my lab, in fact it’s our lab is a community of many people, PhD students, post-docs and staff members and so on. I do not distinguish between PhD student and staff member if they contribute to common work, to their best possible extent, if there is a new PhD student, he is coming and he becomes my colleague rather than anything else. All people are colleagues, but if, ok, of course there is a draw back from this as well, because if students doesn’t contribute, ok, I immediately ignore that. He can continue his PhD studies, but he is no longer a member of our group, because if you like my time to be contributed to your work, to your PhD, etcetera, you have to contribute back. It’s a small society if you wish and it’s very important to treat younger colleagues not like your property or your feudal property, like many of my colleagues still do, but consider them as younger colleagues. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0040** |
| **Biographical** | I was born in 1974 in Nizhnii Tagil, a middle-sized industrial city in the Ural Mountains in Russia. My mother, Tatiana Novoselova, was an English teacher at my school (though, in spite of all her efforts, I only started to speak, not even proper, but *any* English after I moved to the Netherlands), and my father, Sergey Novoselov, was an engineer at the local Factory.  The Factory – a huge enterprise the size of the city itself – was central to our life, even at the most basic level: every morning there would be a whistle loud enough to wake people several miles away at 7.00 am, two at 7.30 am to get people out of their homes, three at 8.00 am as a signal to start working and another at 4.30 pm when the workers could go home. It produced railway carriages and tanks, including the legendary T-34 (it was moved from the occupation zone of Kharkov during the Second World War), a fact I was very proud of despite the trouble it brought to our family (my granddad Gleb Komarov, a tank test-driver who was evacuated from Kharkov with the factory, lost his legs in an accident in his tank in 1944).  Having such high-technology industry in the vicinity meant there were large numbers of highly qualified engineers and specialist technicians around, and inevitably, our hobbies were rather technical as well. So, along with cross-country skiing, I was seriously into carting, mainly due to my father, who was himself into auto sports, and many parts of the cars were produced or modified by our own hands. Through this hobby, I learned bits of lathing, milling and welding, skills which I also put to use during summer placements at the factory.  I had always been quite technical. When I was eight, my father gave me a German model railway, and the part I used most was its variable DC power source, which came in handy in experiments from electrolysis to building electromagnets. With my parents working full time, and my seven-years-younger sister Elena in the nursery, I had a few hours after school each day to do ‘research’ such as looking for gunpowder recipes or casting metals and then cleaning up the kitchen afterwards.  The load on our kitchen was significantly reduced when I reached the higher grades, and my passion for such experiments was supported by my physics teacher, Ljudmila Rastorgueva, who allowed me free rein with the equipment in our school physics laboratory. She also, together with my maths teachers Valentina Filippova and Ljudmila Bashmakova, introduced me to the Distance Learning School of the Moscow Institute of Physics and Technology (Phystech), as well as pushing me to participate in physics and maths Olympiads at various levels. Other great sources of information and encouragement at this time were the monthly journal *Quant*, a series of fantastic books by the same publisher and translated texts by Martin Gardner. But, it would be wrong to suggest that I limited myself to physics and maths literature; quite a keen reader, my school-time favourites included [Pasternak](https://www.nobelprize.org/nobel_prizes/literature/laureates/1958/), Pushkin, Jack London, H.G. Wells, Jerome K. Jerome, Lewis Carroll and Mark Twain (though my tastes changed dramatically over time).  My participation in the Distance Learning School and Olympiads made entering Phystech in 1991 fairly straightforward. I chose the Faculty of Physical and Quantum Electronics and experienced an amazing and bizarre combination of the highest standards of education and rather tough living conditions. The curriculum was also quite intense, especially during the third year when one could easily spend ten straight hours a day in the lectures, tutorials and research labs. But with our courses given by the leading actively-working scientists, we felt privileged and extremely proud to study there.  The Phystech students formed a very close and friendly community, and these connections helped us survive the turbulent times of 1991–95 in Russia. I remember, during one of the blackouts which were unfortunately very regular (especially during winter), Sasha Zhuromskii reading something from Tolkien using the last candle we could find and a good dozen people hanging around on the double-decker beds in our hostel room, which was small even for the four of us who lived there. Another source of entertainment, despite a continuous shortage of money on all of our parts, were regular visits to the Bolshoi Theatre, where we traded work as *claqueurs* (paid applauders) for a chance to see the performances.  Of course, there were quite a few temptations outside science and many alternative paths to take. In 1993, I participated in the October Putsch in Moscow, and it was quite an experience. I still feel lucky that they refused to give me a gun, despite my strong insistence. As a consequence, I decided that my revolutions would be in physics, definitely not politics – in fact I decided to stay as far away as possible from any politics at all. My romance with business was somewhat longer lasting: for about three years I was heavily involved with a construction company in parallel with my study (luckily the work was mostly during my summer breaks) and for some time it was good fun to learn another profession, meet new people and earn good money. But, after a while, I got bored, and when the question of science or business arose, I chose science; it is absolutely impossible to do ‘part-time science’, and I feel lucky that I realised that quite early in my career.  Phystech is rather different from other Russian universities. Science in the country is traditionally concentrated in research institutes and Phystech uses these as so-called ‘bases’, where students can follow specialised courses and get involved in research projects. Typically students spend about a day a week on a ‘base’ during their third year, with the proportion reaching 100% by their sixth year. My first base was Astrophysica, the State Research Centre originally focused on research into powerful laser systems and their use in military applications, but within a year I had decided that it was not what I wanted and moved to Chernogolovka’s Institute of Microelectronics Technology.  Chernogolovka is a very small town in the middle of a forest about sixty km east of Moscow, with 20,000 people and a dozen research institutes. I loved everything about it: the place itself looked amazing, especially during the winter (I would have to walk through the forest for a good half hour each day to get to the institute), the people were enthusiastic and passionate about science, and the range of courses we were offered was excellent. In addition, the lectures were given by the leading scientists at the Institute of Solid State Physics, the [Landau](https://www.nobelprize.org/nobel_prizes/physics/laureates/1962/) Institute for Theoretical Physics and the Institute of Microelectronics Technology: Vsevolod Gantmakher, Vladislav Timofeev and Mikhail Trunin to name just three.  At Chernogolovka, I started to learn microelectronics technology (now it would be happily called ‘nanotechnology’) from Sergey Dubonos and worked with Zhenia Vdovin, Yura Khanin and Sergey Morozov on tunnelling spectroscopy [[1]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not1) in the laboratory of the late Yura Dubrovskii. I learned so much from these people, from basic human communication skills to the most complicated experimental techniques. I remember that I so envied the skills with which Yura Khanin and Zhenia Vdovin handled the most miniscule samples that I asked a good friend of mine, Marina Dvinianina, to get me a cut-throat razor to develop the steadiness of my hands when shaving – it was quite a painful and bloody experience, but I soon achieved a less dangerous level of expertise.  In 1997, I was doing my PhD in the same lab [[2]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not2) (and still shaving with the same razor, which I use to this day), when I got an opportunity to go to Nijmegen in the Netherlands to work with Andre Geim. Andre already had a reputation for being an innovative and creative experimentalist, so I didn’t think twice. During the spring of 1999, I spent a couple of months in Nijmegen as a probation period, where I did everything possible to disappoint Andre, once forgetting to close the lid on the helium dewar (which has never happened to me before or since) and using a ‘u’ instead of an ‘a’ in the phrase “*last* opportunity” when Andre asked me to write to a journalist for him from his e-mail account. Yet, despite all my “efforts” to sabotage my chances, I started my PhD with Andre in Professor Jan Kees Maan’s high magnetic field laboratory in August 1999.  This was quite a different experience for me. The laboratory was large, international, had a huge variety of projects running [[3–4]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not3) simultaneously, and always had visitors coming in for measurements on the high magnetic field installation. It certainly broadened my horizons in terms of science but unfortunately not with regards to Dutch: with our community being so international (my closest friends were Igor Shkliarevskii, Fabio Pulizzi and Cecilia Possanzini) we spoke quite a weird dialect of English, with a smattering of Italian, French, Dutch and Russian words and grammar lifted from Guy Ritchie’s movies*Lock, Stock …* and *Snatch* (kindly and patiently explained to us by A. Keen and A. Quinn).  In 2001, as many people finished their PhDs and postdocs, the community started to break up. Andre himself moved to Manchester early that year, and I didn’t hesitate for a moment when he invited me to join him, even though it meant leaving my PhD unfinished for the second time in a row. When I arrived in Manchester, it was to an empty room with one lock-in (still working), a turbo-pump (still there) and Sergey Morozov (still around), measuring ‘magnetic water’ while on a short visit to Andre. It was my third lab in less than three years and a different experience again: everything had to be built from scratch both in there and in the clean rooms (though I had less involvement with the latter), but this did allow for plenty of fun as everything was bespoke to our specific requirements.  Despite the fact that the lab then included only Andre, Irina Grigorieva, myself and a couple of other postdocs and visitors, the number of projects (in comparison to what I was exposed to in Nijmegen) hasn’t dropped. Probably even otherwise, besides mainstream projects like Irina’s cryogenic Bitter decoration [[5]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not5) and mine and Andre’s domain wall motion in garnets [[6]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not6), we were all involved in several others, including the mentioned ‘magnetic water’, mesoscopic superconductivity [[7]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not7), gecko tape [[8]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not8), scanning tunnelling microscopy (STM) with a gate electrode … the list was endless. In between the projects, I also managed to convince Irina Barbolina to marry me (over the phone, she was in Nijmegen at the time), and, with a PhD in microbiology, she joined the group for a few months, helping with biorelated experiments. We all enjoyed watching the turbulent life of yeast and other micro-organisms under a microscope during a project we dubbed “the last fart of a living cell” [[9]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not9).  One of our projects, initiated by Andre, was an attempt to make a metallic field effect transistor. The choice of material, quite naturally, fell to graphite, mostly due to its low carrier concentration. I will skip giving a detailed description of the first stage of the project as Andre Geim describes the process in his [lecture](https://www.nobelprize.org/nobel_prizes/physics/laureates/2010/geim-lecture.html), but I thought I would just mention that we thought we would have to drop it altogether when Andre’s PhD student Da Jiang enthusiastically polished a piece of very expensive graphite into dust. The unexpected solution to the problem came from the scanning tunnelling microscopy project, which was led by Oleg Shkliarevskii.  At that time, I was doing very long measurements on domain walls, with magnetic field sweeps easily taking a day or more, so I was often hanging around the cold STM. Oleg was doing the first scans and showed me the way he cleaned graphite, by cleaving it with Scotch tape. Using Scotch tape (with residual flakes on it) taken literally from the dustbin, it took me less than an hour to produce a device which immediately demonstrated some miserable field effect; but, however small the effect – it was clear we had stumbled upon something very big (though I doubt at that time I realised how far it would go). We got onto it, and within a few months we had our first graphene device (sample ZYH-K51) [[10]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not10).  The results we were getting were quite puzzling though, and I admit we got a great deal of help from theorists. I was organiszing our group seminars at the time and invited Dima Khmelnitsky from Cambridge to visit us. The seminar was due to start at 3 pm and Dima arrived at 7 in the evening, arguing that he calculated it should have taken him 3.5 hours to reach Manchester. Obviously, the seminar had to be cancelled, but we were able to spend the rest of the evening chatting about our recent results. When he heard about graphite, Dima immediately told us about the linear spectrum and pointed out (off the top of his head) that the Landau level quantisation for such a spectrum was considered in the problem to the paragraph on “[Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/) equation for an electron in external field”, on page 148 of *Quantum Electrodynamics* (book four of the Russian edition of the Landau and Lifshitz course) – information which came just in time. Another piece of useful information came from Dima the very next morning, when he called me to confirm that his theory was correct and that he had reached Cambridge within 3.5 hours after having left Manchester at 4 am.  By 2004, my first postdoc was coming to an end, and I was actively searching for funding to allow me to continue in Manchester. I was granted a fellowship from the Leverhulme Trust, but they pointed out something unfortunate in the fine print: the recipient must have a PhD. I started running around like a headless chicken, searching for a body which would give me a degree within three months – with my project finishing soon and close to being kicked out of the country. I was extremely close to buying a so called ‘life experience’ PhD on the internet, but things came together, and I was awarded a PhD from Nijmegen – though even then things were touch-and-go as the company I was flying with went bust and my passport got stuck at the Foreign Office. Finally, after months of havoc, I was able to proudly phone the Leverhulme Trust to ask where to send my certificate. “We don’t want to see it,” they replied, “If you say you’ve got it, you’ve got it.” I was absolutely charmed with this kind of attitude.  In 2009, Irina’s and my bio-mechanical experiments paid off, and we produced a pair of amazingly good-looking samples: Sophia and Victoria. I sincerely hope that they will continue to work for many, many, many years.  Finally, the promised moral. Four of us (Andre Geim, Volodia Falko, Boris Altshuler and I) were sitting in a seminar room in Lancaster discussing our recent experiments on weak localisation in graphene. Volodia was telling us that it was unlikely that there was no weak localisation at all, and that we should measure better, and we were insisting that those were the facts and encouraging everyone to discuss the real physical situation and try to understand it. The truth, as usual, appeared to be somewhere in the middle, but the discussion became rather heated and even personal (as it often does between Andre and Volodia). Eventually Boris jumped up, ran away and brought back a poorly copied paper by Stark. It is really a bizarre reading on “The Pragmatic and the Dogmatic Spirit in Physics”[[11]](https://www.nobelprize.org/prizes/physics/2010/novoselov/biographical/#not11). It starts with, probably, the most concise description of how science should be done, and you are ready to sign under every single word until you turn the page, where … Well, let’s say Volodia turned out to be a bad guy (in the illustrious company of [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/), [Schrodinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/) and [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/)).  The moral is that it is impossible to learn the spirit of science from a textbook or article. They may be able to teach us physics and chemistry and many other disciplines at university, but it is up to us to develop a gut feeling for how best to ‘do science’. I am extremely lucky to have worked with and learned from Andre Geim, who is highly innovative and broad in his perspective but, at the same time, very truthful and critical of himself, with manic attention to details. It is so easy to lose sight of the bigger picture underpinning the details or get carried away with your ‘beautiful theory’ and stop paying attention to the facts; Andre is a master of finding the narrow path between these extremes, and, if there is one thing I am proud of in my life, it is that I have learned a little of this style. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0040 = KN**  [Konstantin Novoselov] Hello.  [Adam Smith] Hello, may I speak to Konstantin Novoselov please?  [KN] Yes speaking.  [AS] Hello this is Adam Smith calling from the Nobel Foundation’s web site in Stockholm.  [KN] Right.  [AS] Congratulations on the news of the award of the Nobel Prize in Physics.  [KN] Thank you.  [AS] We have a tradition of recording very short interviews for the Nobel Prize web site with new Laureates. Would you mind if we recorded a few minutes’ interview?  [KN] You mean right now?  [AS] Right now, yes.  [KN] (Laughs) OK right, yeah. Can you give me just a minute, just a second …  [AS] Yes of course.  [KN] Sorry I was just in the middle of the measurements there and … sorry about that …  [AS] I’m sorry, we’ve interrupted an experiment then.  [KN] (Laughs) Well, that’s a pleasant [interruption] … I mean … Should we do it now?  [AS] Having done this for a few years, I do know from experience that the world descends upon you, so that’s why we call you right now.  [KN] So you’re basically saying that I should stop my experiments now, that there will be no chance …  [AS] I’m afraid that if I had to give advice to new Nobel Laureates, it would be don’t try and do anything once this news breaks, and switch off your telephone.  [KN] OK, let’s try to do it now.  [AS] OK, I’m very grateful. Thank you very much indeed. May I first ask you about your collaboration with Andre Geim. He has been your mentor for many years now.  [KN] Yes, sure.  [AS] What is it about your partnership that works so well?  [KN] Well, Andre is just an amazing physicist and honestly I learned a lot from him. I’d say [01:49 mobile phone rings] almost everything I knew from … about physics, I learned from him so …  [AS] How would you characterize the style of the lab that you work in?  [KN] So the style … OK, looks like things are going crazy now …  [AS] Yes.  [KN] That’s the most important thing because we have … When you think that we’re doing physics, we’re not, we’re actually doing science and this means that our interests are much, much broader than any particular field of physics or just physics by itself, so we just try to be curious in everything and most important is to have fun. So Andre introduced this habit of Friday evening experiments which … where you do just crazy things and then some of them sometimes come out, sometimes not. And basically graphene was one of those as well.  [AS] Yes, so this attempt to isolate graphene, which had been known about but never isolated and thought impossible to isolate, was a Friday evening experiment. You did it in the end using Scotch tape?  [KN] Yes actually we did. So … and … things are becoming crazy now … You, you, you were right.  [AS] (Laughs)  [KN] So we did, and that’s exactly the spirit of this Friday evening experiment. You are not allowed to use any complicated machinery or anything. You just do something on your knees with your bare hands and if it works, it works. It was one of those things, you know, that did work.  [AS] It’s lovely – it’s a very hopeful message that experimental physics doesn’t necessarily require complicated things that you can’t afford; you can do experiments simply. And graphene opens up new worlds of fundamental physics and potential applications. Which is it that drives you more?  [KN] I think it is … it’s fundamental physics which drives me more but it is actually the type of things which you could never think about doing before. I really enjoyed doing with this material something which you couldn’t do before and also you can do it really on your knees with your bare hands. That’s what … you really get involved with it temporarily, but really personally, and that’s what I really like.  [AS] Andre has a notorious habit of changing fields quite regularly but he’s been with this field for six years. Do you think …  [KN] Yeah, that’s a little bit unfortunate. I’m trying to escape as well.  [AS] With the explosion of interest it must be hard to escape, but you plan to? You plan to jump at some point?  [KN] Yes.  [AS] OK. Have you yet thought about where you might go next?  [KN] I have a few things in mind but I would reserve it for Friday evening experiments which might work out, might not.  [AS] So you’re one of … among the younger Nobel Laureates that have ever been made. Have you ever thought about how this might affect your future research?  [KA] No. [05:14 mobile phone rings] I never … I try not to think about this … because the moment you start thinking it’s just … I think … it’s not healthy.  [AS] Is this entirely unexpected?  [KN] It is, yes. It is.  [AS] I certainly can detect in your voice a [05:35 mobile phone rings] somewhat surprised attitude.  [KN] Oh yeah. I had quite a bit of plans for today. Yeah. It looks like they’re all ruined now.  [AS] I think other plans may take over, yes, as you say. (Laughs) Maybe it’s best just to relax and let the day take over. It’s been an enormous pleasure to speak with you. I am happy to say that we have a chance to interview you in more depth and more sort of gentle surroundings when you come to Stockholm to receive the Nobel Prize in December.  [KN] Right. Thanks.  [AS] So thank you for speaking to us and congratulations. |
| **Interview** |  |
| Q2 | **I think it gets busier as it goes on. I want to start by discussing the subject of play. People often view science as a very serious exploit but it’s really quite playful and you in particular keep play at the forefront of your research activities. Can you tell me how play figures in your research?** |
|  | Konstantin Novoselov: I came to do my PhD with Andre in 1999 and at first for me it was just an opportunity to work in some other lab, but I was so fascinated with the style of work that Andre imposed on the lab that I worked with him in the same lab since then. And it is really important that you Andre impose the style which promote the freedom of mind, you are just allowed to do whatever you want as long as it’s not boring, and for me I’m not sure if, even if we are not doing science in the eyes of other people, it’s nice, it’s enjoyable and that’s what I like. |
| Q2 | **So, with the isolation of graphene which was done in this beautifully simple way just with sticky tape on graphite we are told. Was that the end of a long series of attempts to isolate graphene where this was just the last thing that one might try?** |
|  | Andre Geim: No, it was the beginning of a long series of serious experiments. Yeah so, it was very first try which turned out working and then it was okay a few years to hard work to clear it up.  Konstantin Novoselov: Andre is a little bit modest, in fact it was his idea first to create the metallic transistor and we thought about several possible ways and it was one of the possible ways and it did work. It was, although it sounds playful, it was quite a serious series and it ended up nicely.  Andre Geim: It’s probably doesn’t give the sounds okay what’s adventure and courage and so on, probably. What is important is, that when you are working within your specific direction you know all minor details of the area, you are aware about or control etcetera. The biggest adventure in doing different experiments, is to move into area where you are not an expert. It requires a lot of work to accumulate additional knowledge, read hundreds and hundreds of different papers and be courageous enough to enter in the area where you have not been before so, for me for example, in mid-nineties, I switched subject from semi-conductor physics to superconductivity. I went to those conferences as a beginner with having a couple of already prestigious papers being an associate professor and people looked at me and something like that, who is this mature post-doc, what his teaching with us. Because I came from a completely different community. It requires a lot of work and this sort of moving away, it’s not secure. You are moving in the unknown waters which is not only scientifically unknown but in terms of psychology, it’s unknown community. This requires a bit of adventure.  Konstantin Novoselov: It’s a great pleasure as well, if you are young like a student again because you were for a very short period of time, you got to learn so much that people will learn over decades and that’s a great pleasure. |
| Q4 | **In this case it seems to be a successful melding. Let’s talk about graphene a little bit. It’s a single atomic carbon in a hexagonal lattice just one atom thick and its forming exists in this single atom sheets. It’s supposed to be the strongest material on earth is that correct?** |
|  | Konstantin Novoselov: Well, let’s start from the beginning. Over the last seven years, people like to talk about how strong it is and how conductive it is and how probably useful it’s going to be for the electronic industry, but for me it’s just the very existence of it something which is only one atom thick can exist on millimetre or even inch size and be so perfect and it’s practically resolve any defect that you can actually pick it up and hand it in your fingers put it back and still it will work as a perfect transist. Just the very existence of this material is still very fascinating for me and then in addition to it, all the other properties that can come on top is the most, is the strongest material known to us, it’s probably the most conductive and we are so lucky with finding so many unusual electronic properties in it, on which we really specialize. |
| Q14 | **Lovely, it’s great depth there to discover then. And it acts as a laboratory as well as a material which can be applied, and I understand you can do some very fundamental physics using graphene. So, it holds potential both for the expiration of quantum physics and for applications such as touch screens and alike. Are you interested in the entire spectrum of those things or do you tend towards one or other end of the spectrum?** |
|  | Konstantin Novoselov: Whe’re generally interested in what is interesting whether it is, if it is applications which are interesting at this particular moment for us, then we do some particular part of the applications. Mostly, it just happened that physics is so fascinating in this material indeed as you said, electrons behave so specially in this material, they mimic relativistic particles, so you can produce experiments which were not possible with real elementary particles in the real world, and it allows so much freedom and so many different types of experiments that honestly, one can be lost. Myself and Andre, we often complained that these Friday evening experiments, which you were mentioning, are no longer active but in fact, when you think about this material it’s not true, we broaden the spectrum of our work in the lab so dramatically we now study, not only transfer properties that we are used to do, we also study mechanical properties and optical properties and chemistry and the interplay between those. That’s the overall, the spectrum of the experiments is probably even wilder than from the levitating frog to superconductivity. |
| Q20 | **Is it difficult to choose what experiment to do, given all these possibilities?** |
|  | Andre Geim: Well, circumstances force us to move in one area or another area, so you have a limited freedom of choice. Circumstances sometimes dictate, just for simple reasons, one area even was incorrect. /- – -/ research becomes so crowded, another area is still not exploited, you have certain possibilities with your experimental facilities, so, it dictates what you can chose and within this already limited choice of options you have to think what to do and then you poke in the right direction similar to this Friday night experiments. If the poke is unsuccessful after few days, few weeks, so even a few months, I usually say, let’s cut losses like on a stock market and do something else within this area. So, it’s always hard but at the same time we are forced into. |
| Q3 | **So is it that you like to be first in an area, you like to seed in an area?** |
|  | Andre Geim: First is a waste when to many people working in the same area, secondly, possibilities are wider when you are in a new area and you are not afraid of being scooped because when you are in some area you have this feeling that you have to rush. When you are in some other area you can exploit in a better way and think about what you have found rather than try to publish in front of other people, so that’s much better. Feels much better.  Konstantin Novoselov: As in this sense, we were so lucky with our first paper on graphene, we had this privilege of working on it for more than a year. Our days it’s so competitive, we have to work much faster, and publish results within half a year probably. |
| Q10 | **It’s interesting, people often talk about science is this great collaborative enterprise where there’s the enjoy partly is the give and take between all the participants in a particular research field and conferences and learning from each other and it’s a balance between that pleasure and having more space to do your thing in a less crowded field. So how does that work for you? How much of the collaborative enterprise do you enjoy?** |
|  | Konstantin Novoselov: It is indeed a very fine balance and I’m still proud that Andre made this, I’m proud, that Andre made this decision when the very first paper was coming out that we didn’t try to patent this method or graphene or anything, we just invited as many people from all over the world with the task to joining us in this situation. People used this opportunity which increased this field dramatically and since then we enjoyed our collaboration and I hope that was the moment we set up the mood in the community because the community is incredibly friendly. We have so many very good collaborators, tough collaborators, but they are really fair collaborators and many of them are going to be here this week. |
| Q21 | **I wanted to ask you about that decision not to patent, was that difficult** |
|  | Andre Geim: We actually prepared … it’s a well-known story these days, we actually prepared a patent on the basis of this first paper, our first paper, on graphene and it was with patent lawyers arranging a proper manner of speech and so on. And at that time when it was still with lawyers, I was at a conference, speaking with a representative of a very large multinational company, billions and billions of dollars, so you can guess which company this could be yourself. So, I told, we have this patent concerning graphene, it’s very expensive and troublesome to patent a whole area and possible sideway. Outcomes of this research, would be you interested in collaborating with us, we provide sort of ideas, you keep this patent running and also patent it whatever further comes, we are not very much interested in royalties, very tiny fractions of percent would be good if it ever comes to this one. And the guy told me, You know, we are aware about graphene, it’s a deep promising material, but we don’t think anything will come within the next ten years. If after, year eleven, we find out that this material is as promising as it is, I will put 100 lawyers on this project, each of them will file hundred patents per day and you’ll spend the rest of your life and the GDP of your little island, direct quote, “to try to sue us”. It is very arrogant and so on, but actually it was very useful to know this opinion because then I realised that there is no point of patent whole areas or visionary ideas, graphene electronics, graphene that, graphene this, you have to pinpoint some particular application and then you have a chance to do something otherwise its patent is just a general knowledge and there is no point in patent a general knowledge, so it was a useful comment despite of his arrogance.  Konstantin Novoselov: We should say, since then for several years, when ever came back to the idea of patenting anything so just, until recently. We were enjoying our scientific work only. |
| Q19 | **And then the application you chose to patent in, were what?** |
|  | Andre Geim: It’s about flourographene, two dimensional teflons, so I think it’s a bit narrow enough that it may stand a chance to bring some application in foreseeable future which might bring money to this project.  Konstantin Novoselov: We just loved the name two dimensional teflons. |
| Q4 | **So there has been some debate about whether graphene is two dimensional material or not. Do you think it is a two-dimensional material? Or is this debate in unlearned circles?** |
|  | Konstantin Novoselov: I am not aware of these debates. Of course you can argue whether it is two dimensional or has some thickness or if you band it or fold it would acquire some three-dimension properties. When we call it two-dimensional, we call it two-dimensional in electronic terms, that electrons behave as being restricted to two dimensions only. Of course, for certain applications or problems, like the mechanical stability, flexural phonons, the repulse on the schiffer of graphene or in graphene, you might consider it as a three dimensional. |
| Q4 | **So two dimensional teflon is to do what?** |
|  | Andre Geim: It’s fluorinated graphene to each cover atoms you have attached a fluorine atom, so that’s essentially the same graphene sheet with attached fluorine so it’s two-dimensional teflon. It’s an analogous molecule so a molecule of teflon is just a chain of carbons with the fluorine atoms attached, in this case we have a whole sheet of carbon which is graphene which is /- – -/ metric manner attached fluorine atom to each carbon.  Konstantin Novoselov: But we also seen as the demonstration of another idea, you can consider graphene as a two dimensional material, a two dimensional crystal, but you can also consider it as a giant organic molecule and then you can modify by chemistry and here you can for example attach fluorine to it and use it as an insulator for some applications if you want, but we hope that the idea goes even further. You will be able to modify it by other means as well and acquire some additional or some other properties.  Andre Geim: It’s a new type of chemistry we invented on the basis of graphene, it’s a giant giga molecule which you can modify. Previously, graphene was ‘peppered’ with different chemical molecules or dopes and so on, and in this case just like in chemistry, you take one molecule and you change it, in a very specific manner into something else, and in this case graphene was changed in very specific manner to something else, rather than peppered something else. |
| Q15 | **So which applications are you most hopeful of seeing in use? From graphene.** |
|  | Konstantin Novoselov: Myself, the most hopeful of seeing applications which I haven’t even thought about, because up to now what we had, we just take this material, we figure out which particular properties are slightly better than in other materials like, for example mobility is a little bit better than in silicon, so we can use it for transistor application or transparencies, that would be better than in other material and we can use it for mechanic application. But for me the best one would be when you invent an application for this particular material, because it has this unique combination of very unusual properties; it’s being the strongest, the most elastic, the most conductive, the most transparent, so you just try to invent something for yourself. Now there is bendable electronics, quite fashionable and probably stretchable, electronics or I don’t know, but something like that which doesn’t exist yet, where we don’t use graphene as a replacement for another material for an application which is specifically created for graphene, that is what I am hoping for.  Andre Geim: Let us be clear, usually it takes decades for a material to go from an academic lab into industrial lab. It’s a very long way, involving many, many people before something originated from an academic lab comes to a consumer product. The Nobel Prize, at least as I perceive it, was not given for application, it was given, in my view, for bringing this material to the attention of a wide range of community, not even a single community of physicists, so it’s like a philosopher’s stone stand out to be of material whatever properties this material you touch, whatever properties you touch was this material, it turns out to be magic and applications is just an extra bonus to this project and we never expected that after five years, people would start talking about applications. It did happen and at the moment it’s a promise, a very strong promise for example, recently I was shown a graphene roadmap from Samsung and on this roadmap, there are, I don’t know, twenty, thirty, fifty different points. Each point means a specific application, which happens in a year, say 2012, 2025, and another axe it’s marked capitalisation in billions of dollars per year, so there are many applications, but let’s see, let’s wait for another 3 to 5 years before we will see first gadgets in a wider use. |
| Q3 | **I wanted to just go back to your scientific beginnings and ask you both why you chose to become physicists? What attracted you in the first place? Andre would you like to start?** |
|  | Andre Geim: It’s always very simple, you are at school, you are doing one or another subject and I was much better in maths and physics than, say, in literature or in English. Okey, I got all high smart, but I was much better in physics and maths in particular. And then it is natural progression. |
| Q6 | **And you both, now you both work, at the University of Manchester, there was a sojourn in the Netherlands and indeed you are the Dutch citizen. What drew you away from Russia and towards Western Europe?** |
|  | Konstantin Novoselov: Before I start answering this, I should say probably that, it’s in principle quite natural for scientists to go from one place to another. There are of course circumstances as well, but it is natural, and it should be natural that you do your masters here, you go for your PhD elsewhere and go for a post-doc somewhere else. And that is how I moved from my university, Chernogolovka, to Holland and then in Manchester and I’m a little bit ashamed that I spend already, what, nine years now in Manchester and, it’s probably, I won’t say it now, that it is a good time to move and I’m a little bit ashamed of that. It’s a natural process, it helps in exchanging techniques, knowledge procedures. |
| Q5 | **Ok, thank you. To close, I’d just like to ask you both about each other. Maybe I should do when you are not sitting next to each other, but nevertheless, this is the opportunity. So, first of all, Konstantin, what is it that, you obviously have a strong partnership that works very well, what is it that you admire most about Andre’s work?** |
|  | Konstantin Novoselov: Andre is, was my supervisor, but supervisor is some cold term, so he’s my teacher. They teach you a lot about physics in the university, but they don’t teach you how to do science. You have to learn by yourself how to do science and it’s just, you only learn about it from someone. Andre got a very specific way of doing science and I’m really glad that I learnt exactly that way that you, first of all you are honest with yourself and you honestly consider the results of your experiments, the very pragmatic way of doing experiments and that has been since my PhD years and then through post-doc years, so I hope … We were always colleagues, that’s how he set up things in the lab, but I learned every day from him, also from other people in the lab of course, but I still learn a lot from him.  Andre Geim: I quite often hear from my colleagues who say, my PhD student, my post-doc, my lab and etcetera. There is this style, if you ever hear my interview I never say ‘my’, I always say ‘our’, sometimes it’s misinterpreted when I say our lab interpreted as Kostya’s lab and my lab, in fact it’s our lab is a community of many people, PhD students, post-docs and staff members and so on. I do not distinguish between PhD student and staff member if they contribute to common work, to their best possible extent, if there is a new PhD student, he is coming and he becomes my colleague rather than anything else. All people are colleagues, but if, ok, of course there is a draw back from this as well, because if students doesn’t contribute, ok, I immediately ignore that. He can continue his PhD studies, but he is no longer a member of our group, because if you like my time to be contributed to your work, to your PhD, etcetera, you have to contribute back. It’s a small society if you wish and it’s very important to treat younger colleagues not like your property or your feudal property, like many of my colleagues still do, but consider them as younger colleagues. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0041** |
| **Biographical** | **Family Background** The Kao family comes from a township called Zhangyan in the Jinshan district near Shanghai, China[1](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not1). As landowners, the family would have been considered wealthy. The sons of each generation would be well educated in the style of the times. My knowledge of the family’s genealogy goes back only to Grandfather.  Grandfather Kao Hsieh[2](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not2) also went by the courtesy name Kao Ch’ui Wan[3](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not3). He was a literary man, famous for his beautiful poems, which he would render in Chinese calligraphy; the combination of poetry with calligraphy is an art form of the East. As a Confucian scholar, he was a collector of books and also a prominent member of the *Nan She*(Southern Society). Other family members were also active in the Society; its aim during the 1911 Chinese Revolution was to help bring down the ruling Qing dynasty. A museum now erected in the town exhibits his work and also gives a history of the political activities the man participated in. He was of a liberal bent.  Grandfather had four sons and two daughters. My father, Kao Chun Hsin, was not the eldest of the sons. The eldest would have stayed in the town to look after the family properties and affairs; that is where those duties always fell. He was the third son. These sons were growing up as modern times were coming to China. After a good education in Shanghai, my father went to study for a year at the Michigan Law School. Before he left for the U.S.A., a marriage was arranged to a petite lady from one of the families in the social circle. She was left behind to wait for the return of the adventuring husband. Coming from an equally modern and intellectually accomplished family, the new wife was well educated and a poet herself.  Those were the years when scions of the few wealthier middle-class families in China ventured out to Paris, to London, to New York, to further their experiences and studies. When they returned to China, they were welcomed with great enthusiasm.  On his return from the U.S.A., Kao Chun Hsin, a young man then only in his mid-twenties, was appointed to sit as the Chinese judge in the Court for International Law. He shared the bench with established judges from Western countries. With this prestigious appointment, he and his young wife moved to Shanghai, where they joined the social life of the city.  The first child born to the couple was a daughter, followed two years later by a son. Misfortune struck. In a measles epidemic, both children were stricken, and both died from complications, the elder child ten years old, the younger eight. The mother was small boned and of a delicate, fragile appearance. Childbirth would not have been easy. In the years after the tragedy, she had miscarriages one after another. Finally in 1933, I was born, for my parents happily a healthy child. My younger brother Timothy was added to the family four years later.   |  | | --- | |  | |  |   Because of the earlier loss of the two elder siblings, my brother and I lived a very pampered and protected life. Nursemaids kept constant watch. With my parents busy at dinner parties and social events, we only met them as if for a daily royal audience. Later, home tutors came to give us lessons. The basic lesson plans were readings from the well-known classics, the *Four Books*[4](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not4), which we learned to recite by memory. A second tutor taught English.  Finally, when I was ten, I was sent off to school. The driver dropped me off inside the school playground, telling me to wait and someone would tell me where to go. I had never seen so many kids running about so wildly in a crowd and stood wide-eyed.  A bell rang and soon the playground was empty, but for one solitary child. A kindly looking lady appeared and took me to my class. Maybe it was the home tutoring, or the late start to formal schooling, or an overly cautious and protective upbringing, but in any case I never became a talkative person. As an adult I am not always comfortable in social gatherings with small talk. I must have inherited my father’s gentle nature.  The primary school I attended in Shanghai was a very liberal one, established by scholars who had return from an education in France. The children of leading families were enrolled there, including the son of a well-known man, believed to be a top gangster of the underworld!  By now the family had a home that was situated inside the French Concession. The International Settlements were in an area within the city where various Western powers held authority. These areas were generally kept free from the rough and tumble of the metropolis, providing a haven from the human poverty of China. The locals nicknamed this area *shili yangchang*(a ten-mile strip of foreign spectacles). So when the Japanese invaded China in the 1930s, these areas were off limits for the invaders.  The family was shielded from the horrors that occurred. With the chaos of war, the law courts were suspended, and social gatherings dried up. My parents stayed home more during this period, and we built a closer relationship with them. Together we played bridge and card games to pass the hours.  The cessation of war with Japan did not bring peace. Soon the Red Army, who had been fighting the Nationalist Government, was at the city gates. My father made the decision to leave. The topic had been discussed often over the family games of bridge and the various options mulled over. To move to Chungking[5](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not5), or to join relatives in Taiwan, or perhaps there were relatives who lived in Hong Kong too?  In 1948, gathering a few belongings, the family boarded a ship that sailed out of Shanghai on a grey dismal wet day. The famous waterfront at the Bund slowly receded out of sight and that was the last we saw of our home city for many years.  A short sojourn in Taipei, the capital of Taiwan, persuaded my father that Hong Kong would be a better haven. With help from maternal relatives in Hong Kong, the family found a small apartment and my brother and I were enrolled into St. Joseph’s College where our cousins also studied. As English was the preferred medium of instruction, the local dialect of Cantonese was not a requirement and I never felt the need to learn the tongue.  At 48, my father felt he was too old to study for the law exams that would qualify him to practice in Hong Kong. Instead he took on a job as legal adviser to a company and also taught a few Chinese law courses at local colleges. He became known in Hong Kong as the expert interpreter of the old Chinese laws. There were still some cases of family inheritance and such matters that would need expert advice of this type.  I did well academically, but did not apply myself much to athletics or sports during my five years at St. Joseph’s College. My ex-classmates, in later life, reported that they remember me as a quiet person who did not join in the rough and tumble of boy play. I managed almost straight A’s in the school matriculation examinations, which qualified me to apply for entry to the University of Hong Kong. However the University was still in some disarray after the war and not all faculties were functioning. I wished to study electrical engineering. Britain beckoned and the British Council in Hong Kong was helpful. In 1953 I boarded a P & O liner bound for England.  I enrolled at Woolwich Polytechnic in London, to sit for the A-level exams which I passed easily. I was so much at home at Woolwich Polytechnic that instead of applying for entry to the other more prestigious Colleges of London University, I continued on to study for my bachelor’s degree there. I graduated in 1957 with a B.Sc. in Electrical Engineering. In those days the degrees were awarded as a First, Second, Pass or Fail. As I spent more time on the tennis court than with my books, my degree was a Second.  It was necessary to immediately find a job. Financing my studies had been a heavy burden for my father. I joined Standard Telephones & Cables (STC), a British subsidiary of International Telephone & Telegraph Co (ITT) in North Woolwich, a factory located on the opposite bank of the Thames. As a trainee, I was rotated through different sections for a year before I decided that I liked the work in the microwave division. After two more years, I felt it was time to move on, and applied for a lectureship at Loughborough Polytechnic.  During my three years at STC, I met and married Gwen, a fellow engineer who worked in the lab one floor above my work bench. So we drove to Loughborough for the interview, I got the job, and we immediately set about to look for a home. Finding newly built homes, we put down a deposit on one and drove back to hand in our notices to STC.  **However, It Was Not To Be** Someone in senior management had noticed how I had worked on the microwave projects and felt the promise I showed should not be lost to the company. The offer came immediately to allow me to transfer to the research lab, STL, in Harlow. To add incentive for me to stay, a job would be found for Gwen too at the new location. Loughborough had to be pacified and the lawyers got to work on that, and to get the house deposit refunded. The offer was too good to pass up, and my future was destined to take on this new course. I stayed with ITT Corp, working for the next thirty years at various locations, in the U.K., the U.S.A. and Europe.  During these years, in 1967 my parents emigrated to join us in the U.K.. They were then both in their late 60s and elderly, but still able to live independently. They were happy to see a grandson, born in 1961, and a granddaughter, born in 1963.  In 1970, The Chinese University of Hong Kong, CUHK, came calling. Would I come to the institution to set up an electronics department? STL agreed to give a two-year leave of absence, which then became four years. I was able to see the first batch of students graduate, and to establish a graduate program as well. During this time, 1970–1974, annual summer leaves were taken to return to STL to keep abreast of developments in the research field of optical fibers. It was also an opportunity to see my parents, who had remained in Harlow.  By 1974, the project I had seeded, with the now famous 1966 paper published in the *IEE Proceedings*, had progressed to the pre-production development stage. An industry had grown up around it and it was full speed ahead to revolutionize the telecommunications systems around the world. ITT wanted me back to be part of the team for this endeavor.  So the young family was uprooted again and the move this time was to the ITT plant in Roanoke, Virginia, in the U.S.A.. I was promoted to Chief Scientist, then Vice-President and Director of Engineering in charge of the electro-optical products division at ITT. During my years in the U.S.A., I continued to travel to other research labs around the world, to discuss progress, to encourage work, to keep abreast of the leading edge of the developments. It was a busy time. I always dropped by to visit my parents in the U.K..  In 1976, my mother died at the age of 76 years. My widowed father, who visited Roanoke once or twice, preferred his life in the U.K. and this situation continued until finally, in his eighth decade, he moved in to live with us, when we were back in Hong Kong again.  By the 1980s optical fibers were being laid across the world in vast quantities, and the industry had evolved into a giant. The capacity for communications had grown exponentially. In 1982 I was appointed Executive Scientist at ITT in charge of all research and development activities and I moved to the Advanced Technology Center in Connecticut, U.S.A.. This post was specially created and allowed me the freedom to do anything I considered important for ITT. In order to chart the waters on the ultimate limits of optical communication technology, I pioneered the Terabit Optoelectronics Technology Project to explore the technologies that could reach terabits per second of transmission capacity. The project involved a consortium of ten universities and institutions, and aimed at a goal that was three orders of magnitude higher than the then state-of-the-art. In 1985 I was appointed the Director of Corporate Research at ITT. During this time, many innovations by creative minds were evolving to make use of the increased capacity for sending information over the system. The internet was born.  In 1986, CUHK again came calling. This time the request was for me to accept the position as President of the University, the contract to begin in 1987. In British terminology, this position is known as the Vice-Chancellor[6](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not6) of the University.  The second move to Hong Kong was made as ITT Corp began the process of selling off all its technical divisions in the U.S.A. to Alcatel, a French business. After thirty years it was a sad moment to part company with colleagues of long standing.  I spent nine years as the third Vice-Chancellor of CUHK. It was an interesting and challenging time, both for Hong Kong as it prepared for the resumption of sovereignty by China, and for the higher education sector, which saw a massive expansion and improvements in quality. During those years I sought to establish research more prominently as part of the normal work of the faculty, set up contacts with many leading institutions in the U.S.A. and the U.K., and set the University on a path to become a more well-known institution, competitive with the best institutions. I was especially pleased with the establishment of the Faculty of Engineering, in which information technology played a major role. A Faculty of Education was also established during those years. A number of research institutes also came into being. The University nearly doubled in size during those few years, and a fourth undergraduate college was established.  At the University, my role was to create space for people to grow. What I had done essentially was create situations where people would like to take on responsibilities. This had enabled the University to grow as a whole: everybody would be contributing what they should because they feel it is their responsibility and the environment allows them to do so. I created the space at the right time for talent within the University to perform as it should, thus taking it to a new level of development.  The rapid expansion of the tertiary education sector and the increased government funding that came with it have allowed us to do a lot of things to become a top university. The most satisfying change was a scholarly atmosphere on campus – people are pursuing important things because they believe such things are important.  It was perhaps natural that my advice was sought and I became involved in technology issues in the community, especially in relation to information technology. I was able to play a part in setting up an Internet exchange in Hong Kong (then a bit of a novelty and even now providing useful service), and in promoting the establishment of the Hong Kong Science Park.  My father died in 1996, just prior to my retirement from the University – a year before the return of colonial Hong Kong back to China. My father’s ashes were brought back to England to be laid to rest with my mother – in a cemetery in Harlow. After a year of lecture tours around S. E. Asia, I stayed in Hong Kong, setting up my own company for consultancy. I was appointed to a number of companies as non-executive director.[7](https://www.nobelprize.org/prizes/physics/2009/kao/biographical/#not7) In 2009, I moved to California to be closer to my two children.  **Vignettes from Childhood and Working Life Memories** I had a fight with Lo, in my classroom. With our calligraphy brushes, we attacked and daubed each other’s faces and hands with black ink. The teacher was horrified. Lo, my mother warned me, was the son of a bad man. I was to be careful not to fight with the boy again. Otherwise school was fine. We sang French songs. The calligraphy teacher was very encouraging. This stroke is beautiful and will be marked with a red dot. But this one is leaning and unbalanced, it will get two red dots!  I made a good friend in primary school who liked to play with me at home. We got our driver to buy all sorts of chemicals. Reading from scientific magazines, together we experimented and made mud balls from phosphorous to throw at stray cats and dogs. The mud balls would explode with a bang and frighten the animals out of their wits! From glass bottles and such like I would fashion the funnels and beakers needed. I had bottles of cyanide and concentrated acids. One day as I was boiling up some nitric acid, the bottle exploded and the concentrated acid splashed onto my young brother’s trousers. It burnt the cloth entirely away but fortunately did not land on any of his skin! My parents were furious and confiscated all my chemicals, including the cyanide. I wonder where they disposed of the stuff.  One day my father brought home a big round yellow disc of food that he told us was called ‘cheese’ that foreigners ate. Food was in short supply so we ate it, but it tasted very funny. When we went outside for some walks we sometimes passed a big tall building with Japanese soldiers standing at the door. My parents told us to walk past quickly as people inside were killed there, and to bow to the soldiers.  It was sad to leave Shanghai, but I was fourteen years old and ready for new adventures. The war with Japan was also exciting. As we cowered under the desks, we could hear the aerial dogfight in the skies above. Peering from under, I could even catch a glimpse of the American planes chasing and diving after the Japanese planes!  So the waterfront of Shanghai faded away and we went to Hong Kong. The schooling there was easy and the book work was not hard at all. The class went on a field trip and some of my classmates got lost in the hills until it was pitch dark and they could see “tigers”! I was not in that group, but now, whenever we have a class re-union, the boys will relate their adventures with gusto and lots of laughs. So my years in Hong Kong passed by swiftly and I was to embark on my next adventure all by myself.  On the boat to England to study electrical engineering, I shared a cabin with three others. Two worked for the Hong Kong Government and were going to England to attend some courses relevant to their work, meteorology and water treatment respectively. The third was a professor of mathematics. During the sea trip, which lasted six weeks, the professor taught me quantum mechanics. He also took some other young students and myself under his wings. He escorted us to his friend’s home when we stopped at Singapore and taught us all to eat hot curry and to wash the fiery food down with beer.  I was nineteen years old and had never before left my family to be on my own. Everything was a new experience. To put my foot down on the soil at Port Said so I could tell myself I had been to Africa, to sail pass the Straits of Gibraltar and say I had seen Europe and the Sahara, to find England a cold and grey land! Little did I think eleven long years would pass by before I would see my parents again.  The British Council staff met me and arranged lodgings for me with a landlady in a house in Plumstead Commons, near the Woolwich Poly. Her house was old and large, with rooms for several lodgers. We all ate breakfast and dinner together under the stern eyes of the landlady. Food was still scarce after World War II and the slices of meat served for dinner were so thin, they were transparent when held up to the light! After such a sparse meal, we all trooped out for ‘fresh air’, but the real reason was to buy fish and chips, which we scoffed down hungrily on the walk back up the hill. The habit remains; I still love fish and chips.  After graduation in 1957 from the Polytechnic, I joined STC and had to walk through the tunnel under the River Thames every day to work. My first project was to build an amplifier and I got out my books to study the theories. My boss came by and said to put away the books, just do it. School work is to exercise the brain so it can think intelligently. There was no need for more revamping of theories!  I met my wife-to-be at work. She was an engineer in the coil section. We were married in 1959 and our children were born, a son in 1961, and a daughter in 1963. By then we were living in Harlow and working at STL.  The research was enthralling work and in 1966 I published the now famous ground-breaking paper, “Dielectric-fibre Surface Waveguides for Optical Frequencies”. This research was to spawn a whole new industry over the next twenty years.  People asked me if the idea came as a sudden flash, eureka! I had been working since graduation on microwave transmissions. The theories and limitations were ground into my brain. I knew we needed much more bandwidth and thoughts of how it could be done were constantly in my mind.  Transmission of light through glass is an old, old idea. It had been used in past years to shine light for entertainment, for decoration, for short distances for surgery, through a glass rod, but it had not been possible to use it over the long distances required for telephony. Light passing through a rod of glass just fades out to nothing after a very short distance of a few feet. Efforts by many research laboratories to find a way to transmit light over long distances were desperate as the public, prompted by media reports of hopeful technical advances, were expecting more and more exotic services.  I played around with what was causing the failure of light to penetrate glass. With the invention of the laser in the 1950s and subsequent developments, there was an ideal source of light that could send out pulse of light in a digital stream of noughts and ones, represented by off and on states of the pulse.  Ideas do not always come in a flash, but by diligent trial-and-error experiments that take time and thought. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0041 =**  No script |
| **Interview** |  |
| Q9 | **It’s been two months since the Nobel Prize was announced. How has the two months been?** |
|  | Charles Kao: It seems that it just came.  Gwen Kao: It seems like it’s a short while ago |
| Q9 | **Has it been welcome attention? Have you enjoyed all the …?** |
|  | Gwen Kao: Yes and no right? Media, the Chinese media gets a little too much. They are overexcited. |
| Q21 | **Were you certain that it would work? Did you know when you were doing the experiments that this was going to work?** |
|  | Charles Kao: I think that maybe I am feeling that I should do it and try to make it more easier. |
| Q2 | **Did people believe you then?** |
|  | Charles Kao: Some parts yes and it’s very nice also. I don’t know what I can put big way of saying. |
| Q3 | **What made you interested in science originally? Were you a scientist when you were a little child?** |
|  | Charles Kao: I did quite a lot of things that or which were given by people really doing, making … we have to sort of try to see how we can do and make very good way of making it so good that we know they can serve the thing easier because these things, for me, I feel always that I like. |
| Q3 | **What drew you to physics? Why do you like physics and engineering?** |
|  | Charles Kao: Physics is good, but I sort of made it very much easier.  Gwen Kao: Why? Because his parents discovered these dangerous experiments with chemistry and threw all his stuff away including a bottle of cyanide, I understand. I don’t know how his parents disposed of it.  Charles Kao: That one he actually…  Gwen Kao: Anyway, his parents confiscated it all. |
| Q4 | **Did you know, when in 1970 and there abouts, when you saw the fibers coming out of Corning and you could see that you were getting varied long range of transmission of information. Did you realise then how important it would be?** |
|  | Charles Kao: Yes, I think I made very definite thing that one has to be done using the flying … and these things also, all these things coming in really makes it very nice to do that sort of work. |
| Q9 | **Not mostly pride, mostly excitement maybe. This week is going to be a busy week. Are you looking forward to so much activity over the coming four five days?** |
|  | Gwen Kao: I think he is enjoying himself. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0042** |
| **Biographical** | I was born on August 19, 1924 in Amherst, Nova Scotia and raised in the village of Wallace until I was about two years old. At this time, my family moved to Chaudiere, a small logging community in northern Quebec. My father was the local physician in the community and my mother took on the task of home schooling me. She believed in the Socratic method of teaching, asking me questions about my work for the day that required very detailed explanations as answers. She was a curious woman, and through her teaching I developed a strong curiosity as well. When I was fourteen, I began my formal education at Lower Canada College in Montreal. My peers were a stark contrast to the logging men I’d known growing up and I quickly earned the nickname Butch.  Following Lower Canada College, I continued to pursue my scientific interests at McGill University. Then, in 1943, I had some time away from my studies, when I joined the Royal Canadian Navy. From there I applied to the Fleet Air Arm and was trained to land Spitfire fighter planes on aircraft carriers. When the war was coming to an end, I was anxious to return home to school. The transition back to class was not easy, but I found it very helpful that my best friend from the Navy was also at McGill and we were able to work together to adapt back to our old lives. During this same fall I met my wife Betty, who made me believe in love at first sight. We married in 1946 and had our first son, and in 1947 we had our first daughter and I completed my Bachelor of Science. With two young children at home only twenty-one months apart, I continued my studies at McGill, completing my Masters in Science in 1948 and Doctorate in Physics in 1950.  After completing my Doctorate, I remained at McGill in Canada’s Radiation Laboratory for one year. I then spent two years teaching physics at the Royal Military College of Canada in Kingston, Ontario. Here Betty and I purchased our first small house for our growing family while welcoming our next son in 1952. About this time, I was met with several new opportunities for employment. In 1953, after careful consideration, I decided the academic community available through Bell Labs would become my new home. While work life became very busy, in 1955 we welcomed our youngest daughter into the world.  My time with Bell Labs was always challenging, and this led to several important discoveries. In 1962, I worked with Don Nelson to create the first continuously operating ruby laser. Also in 1962, I became the Director of Space Science and Exploratory Studies at Bellcomm, a division of Bell Labs in Washington, D.C. While at Bellcomm I supported the Apollo space program and was able to aid in the selection of lunar landing sites. I returned to Bell Labs in 1964. Then in 1969, during a brainstorming session with George Smith, we created the charged-coupled device. During my time at Bell Labs I worked on about 18 patents.  I retired from Bell Labs in 1979 as the Executive Director of the Communication Science division. Retired at 55, I sailed my 33-foot boat for six leisurely weeks up the inland waterway from New Jersey, the New York Harbor and up to Quebec and down the St. Lawrence to the house we had built in Wallace. I was accompanied by five or six different crews made up of former assistants, friends and family, and a cat and a dog. Following this I have continued to live in Wallace, Nova Scotia and Lac Tremblant, Quebec. I have been graced with ten wonderful grandchildren and one great-grandchild.  I have been active in several advisory capacities and have won several awards (listed below) for my work. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0042 = WB**  [Willard Boyle] And a good morning to you, sir!  [Adam Smith] Good morning to you! And, of course, congratulations.  [WB] Well, thank you very much.  [AS] It’s been …  [WB] I gather you were talking to George Smith?  [AS] Yes, yesterday.  [WB] That’s good. Oh yesterday even?  [AS] In fact, there was a slight mix-up and he didn’t receive the call from the Royal Swedish Academy of Science because he missed it, he woke up but didn’t actually get to the telephone. So, when I spoke to him, he hadn’t actually heard the news yet!  [WB] Oh my gosh! When was that?  [AS] Well, pretty close to the announcement time, just a few minutes after the public announcement in Stockholm.  [WB] Oh, so he did year about it, though, yesterday morning.  [AS] Yes, yes, exactly, just within … it was just maybe five minutes after the public announcement so …  [WB] Oh, well, then that’s no problem.  [AS] No, but …  [WB] I haven’t spoken to him, you see. I haven’t spoken to him for a long time. And so I was a little curious how’s he doing?  [AS] Well, he sounded well. I asked him whether he was still sailing around the world and he said, no, he’d stopped that now and the sailing vessel was docked out at the end of the garden. [WB] Sure.  [AS] You two used to sail together, I gather?  [WB] Yes, I’ve sailed with him over in Numea. In the South Pacific for a couple of weeks.  [AS] Goodness. And, we’re calling you now at home in Nova Scotia, which is where you were brought up, I gather?  [WB] That is correct, yes. So, yes, mostly my primary education was in Nova Scotia.  [AS] And, indeed, high school was the first school you attended. Until then you were home schooled, is that right?  [WB] That is correct, yes. My mother taught me and she did, I guess, a fairly good job.  [AS] Apparently so! I wanted to ask you how the last 24 hours have been, actually, since the news arrived?  [WB] The last 24 hours have been busy! The likes of which I’ve never experienced before.  [AS] Are you enjoying the experience?  [WB] A little bit more, yes. But there are little things that have to be tidied up. And I think I’ve done my series of interviews with the press. And you run out of a certain number of things to talk about with them.  [AS] Oh dear, well, at the risk of exploring some of the same ground, here goes! May I ask you a little bit about your time at Bell Labs because so many have said what a special atmosphere it was.  [WB] Absolutely.  [AS] And, of course, you know, it’s now finished. But something about it was right. Can you describe what that was?  [WB] Yes, I think the atmosphere set up by the management was very conducive to people being creative. In other words, it’s become fashionable here in North America, for example, to demand that one has a plan, some kind of a financial plan when they’re given a grant for doing research work here; ‘Now you must develop a plan and tell us what you’re going to do in the first year, and the second year, and the third year and so on of this grant of yours. And what your goal is, specifically in the way of profits and loses, and so on.’ And, you can’t possibly do that when you’re trying to do research of the kind of work that was going on in Bell Labs, because as you know … Have you counted up the number of Nobels from Bell Labs?  [AS] I never have but it’s a considerable number for sure. Do you know the number?  [WB] Well, it depends how you count it. There were people, you know, partially at the university, partially at Bell Labs. That was their secret of success, I think. But at least it was eight, and it could be two more than that even. Which is, you know, pretty good for an industrial research lab! So, I guess the other part that was very conducive to wanting to work there and enjoy it and so on, was that the management itself, they were not bureaucratic, they were not money bureaucrats, but rather they were scientists themselves. Now, Baker for example, maybe you’ve never heard of Baker? He was a chemist, and he was extremely bright and he was head of all research. And, he knew what was going on in every lab! And he’d pop around every now and then and maybe have lunch with you and so on and, ‘how are things going?’ So, he knew intimately all the people in research that mattered at all.  [AS] Yes, I’ve heard that approach described as ‘management by walking around’ …  [WB] Yes.  [AS] … being part of it all, yes.  [WB] And no … no yearly plans and things like that, ‘what are you going to achieve this year?’ You know, you can’t possibly describe that and it’s a waste of time.  [AS] And freedom to get involved in different things? You were seconded to the Apollo program for a while?  [WB] Oh yes, for two years.  [AS] And Bell Labs was happy to let you go off and do that?  [WB] Well, yes. At the end of two years, I quit and went back to my research lab. So, yes … and that was dreadfully trying and I had the utmost respect for the people that were working there. Very long hours and tremendous pressures to get things done and so on, and they were all very successful.  [AS] But a rather different environment, yes. And, this famous afternoon brainstorming meeting in which the CCD was conceived by you and George Smith; what led to it? Was it simply that you thought that you would sit down and have a brainstorm? Or were you under a directive to come up with something?  [WB] No directive. No directive, no. I was a laboratory director and George was the department head and so there were levels between us but we worked together regardless. And so there was … We had total freedom to do what we wanted to do. And we had done, and worked in many different areas. And we changed from one area to another, and …  [AS] I mean it was originally conceived as device that afternoon for increased memory storage but very quickly it became an imaging technology. When did the imaging application occur to you?  [WB] I guess at the same time. But, you know the first model that we made – that was another thing that was just so wonderful at Bell Labs: we could make models, seminconductor devices, and see whether they’d work or not. And we did that on various occasions. This was … when we made the CCD, it worked immediately and it was amazing! We never had that kind of luck before. And, actually, I think in the first paper it shows a photograph of an image made by a CCD. I think that you probably have the first paper.  [AS] And then, having developed it, did you stay with it for quite a while or was it something that you passed on to others and then went back to …  [WB] I guess I passed it on totally. And George was also heading up a group and I think his group continued and made more models and increased in complexity and diversity. And there was a one bit sensor and then it was increased and started to look quite interesting. So much so, as a matter of fact, that, well, we said, “Well, we’ve got something here that’s pretty good, we had better call it something.” And we tried various names for it even and finally settled on the CCD. We had other names and dismissed those, but that one seemed to stick and it did, it did stick. And we did that, I guess, very early, you know. As far as I know it was within a month or so of the first one that worked. But I don’t, you know, you don’t know these times exactly.  [AS] No, of course. It’s hard to piece it together in retrospect. So this was all forty years ago. Perhaps an obvious questions, but were you surprised to receive the call yesterday after so long?  [WB] Very much so! Very much so. Doubly surprised, really, because we had, from time to time, just because so many people around us had been winning Nobel Prizes, well, we sort of said, ‘well, our stuff is reasonable. It’s possible, you know, we’ll get something here, I don’t know!’ And, hoped for the best, but nothing had happened and so we’d pretty well dismissed it. And then, I guess, it was just before lights out last night with my wife, I said, “Well, you know, medicine was announced a couple of days ago, we’ll probably hear about physics and so, who knows, you know?” And she said, “Well, we know for sure because we’re not going to win because they always tell you at least a week ahead of time.” So, I … so we went to sleep feeling totally comfortable that we don’t have to worry about it; we’ll sleep in tomorrow morning a little bit. And, of course, the phone rang at five o’clock. And then she answered the phone. And all she did is, she came over and gave me a little whack, and she said, “Stockholm is calling!” And I said, “Oh, I suppose it’s that same old joker.” You know, somebody, at one time, actually had called us up I think, or at least we made this up, wouldn’t it be terrible if somebody called you? And so I knew it was real, however, because after a moment or two we heard this lovely voice and it’s from Stockholm. And there was this woman with a magnificent Swedish accent! And I suddenly thought, well, nobody is going to have gone to all that trouble just to fool us at five o’clock in the morning!  [AS] What a marvelous story. Perfect.  [WB] It’s absolutely true!  [AS] Lovely. It’s nice that the secret is still so well kept, that there was no leak at all.  [WB] Yes, totally. I have no idea how they just keep it so secret! I presume you get nominations from other winners of the Nobel Prize?  [AS] Yes, previous Nobel Laureates are entitled to nominate, that’s right.  [WB] Yeah, I suspect … I do know a couple of other winners, several as a matter of fact just because they were at Bell Labs, I guess. And, maybe they put in a good word for us, I don’t know. I don’t want to know.  [AS] No, it all stays secret for fifty years but one thing …  [WB] Good!  [AS] One thing one does know is one has to be nominated in the year of the award.  [WB] I see.  [AS] So, even if you’ve been nominated for twenty years previously, but you’re not nominated this year, you can’t be awarded the prize this year. So, after forty years, we know that somebody was still nominating for sure.  [WB] Isn’t that great? That’s surprising, you know, sort of, after all that time because some of the people, I’m sure that we knew so well, they’ve probably passed along.  [AS] Well, it’s been an enormous pleasure speaking to you.  [WB] Likewise.  [AS] And, I hope that you have an enjoyable rest of day.  [WB] I think we will.  [AS] Excellent, thank you very much indeed.  [WB] Nice talking to you.  [AS] OK, nice to talk to you, bye, bye.  [WB] Bye, bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0043** |
| **Biographical** | I was born in White Plains, New York in 1930. I grew up in several different states and attended a variety of primary and secondary schools. Upon graduation from high school in 1948, I joined the U.S. Navy where I spent four years as an aerographer’s mate (weatherman), part of it during the Korean War. While stationed in Miami, Florida, I managed to take enough courses at the University of Miami to qualify as a starting sophomore at the University of Pennsylvania in 1952. Since I served during the Korean War, I qualified for the GI Bill, which helped pay tuition. I majored in Physics and graduated with honors in 1955. I married right after graduation. I was accepted into the graduate program at the University of Chicago with a job as teaching assistant and graduated there in 1959 with a Ph.D. in Physics. During that time I also received grants from the National Science Foundation and Bell Telephone Laboratories. Upon graduation, I was offered a job with the Research area of Bell Labs and accepted it. I never bothered to interview at any other institution.  I was assigned to a new department, headed by Bill Boyle, where I started out continuing along the lines of my thesis topic, studying the electronic properties of semimetals. I branched out into other fields as well, including thermoelectric cooling materials and low temperature electronic devices. During the five years in that position I generated many papers and patents. Bill was then promoted to Director of the exploratory semiconductor device development laboratory and he offered me the position as head of a new department entitled Device Concepts. Several bright, imaginative people were assigned to my group and I was given the mandate to hire more of the same. Many fields were pursued including junction lasers, semiconducting ferroelectrics, electroluminescence, transition metal oxides, and the silicon diode array camera tube. In 1969, Bill and I invented the Charge Coupled Device and much of my time was then spent in that field. In addition, my department was renamed the VLSI Device Department where responsibilities covered the physics of devices made with submicron lithography and their use in high performance digital and analog circuits.  In the realm of academic recognition, I have been a member of Pi Mu Epsilon, Phi Beta Kappa, and Sigma Xi. I was made a fellow of the Institute of Electrical and Electronic Engineers, Fellow of the American Physical Society and a member of the National Academy of Engineering. I hold 31 US patents and am the author of over 40 papers. I was founding editor of the IEEE publication “Electron Device Letters”.  My major technical accomplishment, of course, was the inception of the Charge Coupled Device with Willard S. Boyle. We hold the basic patent  (US 3,858,232) and published the first paper disclosing the device concept accompanied by a paper on its experimental verification. A following invention of the Buried Channel Charge Coupled Device (US patent 3,792,322) significantly improved the performance of the original CCD. These accomplishments have been recognized in the following awards:   |  | | --- | | • 1973 *Ballantine Medal of the Franklin Institute*– “For the invention of the Charge-Coupled Device structure, a conceptually simple semiconductor technology with significant application to image sensing, serial memory and signal processing.” | | • 1974 *Morris N. Liebman Award of the Institute of Electrical and Electronic* *Engineering*– “For the invention of the Charge-Coupled Device and leadership in the field of MOS device physics.” | | • 1986 *Progress Medal of the Photographic Society of America*– “For the Charge-Coupled Device’s applications to electronic imaging devices.” | | • 1999 *IEEE Device Research Conference Breakthrough*– “For pioneering work in the field of Buried-Channel Charge Coupled Devices”. | | • 1999 *Computer and Communications Prize*– “For the Invention of the Charge Coupled Device (CCD)”. | | • 2001 *Edwin Land Medal*– “For the invention and development of the Charge-Coupled Device, a contribution that has had extraordinary impact on image creation and utilization”. | | • 2006 *Charles Stark Draper Prize*– “For the invention of the Charge- Coupled Device (CCD), a light-sensitive component at the heart of digital cameras and other widely used imaging technologies.” | |  | | And, of course, | |  | | • 2009 *Nobel Prize in Physics*– “For the invention of an imaging semiconductor circuit – the CCD sensor.” | |  | | Additional honors include: | |  | | • 2006 Honored for the invention of the CCD by US Senate Resolution 478 | | • 2006 Induction into the National Inventors Hall of Fame | | • 2006 Induction into the Imaging Hall of Fame | | • 2008 Induction into the New Jersey Inventors Hall of Fame | |  | | Also, I received the: IEEE Electron Devices Society Distinguished Service Award (1997). |   On the personal side, I had always wanted to sail and purchased my first boat right after joining Bell Labs and sailed it and subsequent boats on Barnegat Bay, about halfway down the coast of New Jersey. My wife also enjoyed sailing but she passed away in 1975. I then commenced to raise children aged 10, 11 and 14 as a single parent. No comment necessary. Two years later, I became partners with Janet Murphy who also loved sailing and we had many adventures sailing a small (22 foot) cabin boat from Northeast Harbor, Maine to Beaufort, North Carolina. We both decided to retire (she was a teacher) early and sail around the world. To do this, we had a seagoing 31-foot Southern Cross, named Apogee, semi custom built for the task in Bristol, Rhode Island in 1983. After two shakedown trips to Bermuda, we retired in 1986 and started around the world. We did not get back until 2003, although we occasionally flew home for a short visit. We now live in our home on a lagoon leading to Barnegat Bay. Apogee is moored to our dock in the back yard and we still enjoy sailing on the bay. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0043 = GS**  [George Smith] Hello  [Adam Smith] Good morning. May I speak to George Smith please?  [GS] Speaking.  [AS] Oh hello. My name is Adam Smith. I’m calling from the official web site of the Nobel Foundation, in Stockholm, Sweden.  [GS] Oh my goodness.  [AS] Have you heard the news that you have… It has just been announced in Stockholm that you have been awarded this year’s Nobel Prize in Physics.  [GS] No! My goodness.  [AS] Well, I’m very pleased to be the person to tell you the news.  [GS] Ah, thank you. I’m amazed. Let’s see, myself and Bill Boyle, I guess?  [AS] Exactly. Yourself and Bill Boyle, and then also Charles Kao for his work on optical fibers.  [GS] Oh, very good.  [AS] Well, my congratulations. Of course the work you did was done in the late 60s, in 1969 in particular. I imagine it’s something of a surprise hearing the news at this point.  [GS] Yes, that’s correct. And let’s see, what’s your name again, sir?  [AS] My name is Adam Smith, and I’m the Editor-in-Chief of the Nobel Foundation official website, based here in Stockholm. We have a tradition of recording very brief interviews with the new Laureates as soon as they are announced and, hence, my call. And, indeed, there is currently a press conference going on at the Royal Swedish Academy of Sciences, here in Stockholm, where the announcement has just been made and Bill Boyle, I think, is actually on the telephone and will take part in the press conference there.  [GS] Ho ho. OK. Is there anything I should do?  [AS] No, I’m sure that the Royal Swedish Academy will be phoning you very, very shortly. I gather that they have been trying to reach you already so perhaps …  [GS] Yes, I got up, but by the time I got to the telephone, they had hung up. That’s why I’m awake now.  [AS] Ok, well, lucky for me. May I just ask you a very few questions and then I’ll get off the phone and allow them to call you?  [GS] Sure.  [AS] Thank you. The first thing is that you, I gather, have a record for having submitted the shortest thesis on record at the University of Chicago – just three pages.  [GS] Something like that, yes.  [AS] So, you were obviously a star student. And you moved straight from being a graduate student to Bell Labs?  [GS] Yes, that’s correct.  [AS] What was it about the atmosphere are Bell Labs that was so special? Many have talked about it, but could you tell us?  [GS] Oh my goodness. It was just very exciting. And, I started off in the research area, where there was essentially not much direction in that you were allowed to do yourself and stand or fall on your own. And that was nice. Most of all, there were just a lot of exciting, intelligent people around that you could interact with.  [AS] And, it gave you, presumably, the freedom to think and invent unlike anywhere else?  [GS] Oh, yes, that’s a true statement.  [AS] And, the idea for formulating the CCD, where did that come from?  [GS] Our heads, actually. Will Boyle and I got together one afternoon. It was actually a one afternoon shot and, well, we had a habit of batting things back and forth. We have a couple of other patents together too, around about, I don’t know, thirty or forty something overall. And, you know, things just happened to get together. I could give you a longer explanation. I actually give a talk on the invention and what I think was the elements that lead into it. But that would take a little time now.  [AS] Well, hopefully you’ll come to Stockholm in December to receive the award and at that point we get a longer time to interview you, so we can speak at length then. Just one last thing. I know that you’ve been sailing around the world for many years. Are we lucky to catch you at home now or are you now done with sailing?  [GS] No, I’m at home. I’m done with it. The boat is still sitting at our dock out in our backyard. But, we don’t plan on doing any ocean cruising any longer. I’m 79 years old now and, I think, a little long in the tooth for ocean sailing.  [AS] Ok, well, I’m delighted to have caught you. And, thank you very much indeed for speaking to us. And, I’m sure that people will be bombarding you now, but in particular I know that the Royal Swedish Academy of Sciences would like to speak to you so I’ll get off the telephone.  [GS] OK, and I definitely will not go back to sleep.  [AS] I don’t think that’s going to be possible on a day such as this. My congratulations once again.  [GS] Well, thanks again.  [AS] Thank you.  [GS] Thanks for calling.  [AS] Thank you, a pleasure, bye, bye.  [GS] Bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0044** |
| **Biographical** | I was born in 1921 in Tokyo and grew up in the provincial city of Fukui. I studied physics at the Imperial University of Tokyo from 1940 to 1942, graduating at the level of M.S. Then I was drafted into an army radar laboratory. After the end of the war, in 1946, I returned to the University of Tokyo as a kind of research associate. I received a doctorate in 1952. In 1950 I became professor at a newly created Osaka City University, a position I held until 1956. But from 1952 to 1954 I stayed at the Institute for Advanced Study in Princeton, USA, as a member, and from 1954 to 1956 at the University of Chicago as a research associate. I was made associate professor at Chicago in 1956, professor in 1958, and Distinguished Service Professor in 1971. From 1973 to 1976 I served as chairman of the department of physics. In 1976 I became Henry Judson Distinguished Professor, from which position I retired in 1991 and became Emeritus.  I married Chieko Hida in 1945 and have a son, Jun-ichi. I have been a citizen of the United States of America since 1970. I hold honorary degrees from Osaka City University (1980), Northwestern University (1985), and Osaka University (1997). I have been a member of the United States National Academy of Sciences and the American Academy of Arts and Sciences since 1971, and an honorary member of the Japan Academy since 1984.  A partial list of the prizes I received in the past is:  • Dannie Heineman Prize, American Physical Society (1970) • Order of Culture, Government of Japan (1978) • United States National Medal of Science (1982) • Max Planck Medal, German Physical Society (1985) • Dirac Medal, International Centre for Theoretical Physics, Trieste (1986) • Sakurai Prize, American Physical Society (1994) • Wolf Prize, Government of Israel (1995) • Gian Carlo Wick Medal, World Federation of Scientists, Lausanne (1996) • Bogoliubov Prize, Joint Institute for Nuclear Research, Dubna (2003) • Benjamin Franklin Medal, Franklin Inst., Philadelphia (2005) • Pomeranchuk Prize, Inst. Theoret. and Exper. Physics, Moscow (2007)  My interests in physics have been mainly on the theoretical side. The University of Tokyo was good in condensed matter physics, but I was more attracted to nuclear and particle physics where names like Nishina, [Tomonaga](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/tomonaga-facts.html), and Yukawa in other institutions were making great contributions. As a student I was exposed to cosmic ray and particle physics by attending seminars held by Nishina and Tomonaga at their laboratory nearby. I started my research career at the time when Tomonaga was developing his theory of renormalization, for which he would receive the Nobel Prize. I was able to approach his group and start working on his theory and other topics in particle physics. At his recommendation I obtained the position at Osaka City University and later was invited to the Institute for Advanced Study. I owe my move to Chicago to M. L. Goldberger.  My work on spontaneous symmetry breaking (SSB), for which I am receiving the Nobel Prize, started around 1959. It is a result of my experience in both condensed matter and particle physics. The BCS theory of superconductivity in 1957 led me to the idea of SSB as a general phenomenon in physics. My work on its specific application to particle physics as a mechanism for generation of the nucleon mass and the pion was first published in 1960. Since then I have pursued the subject in various areas. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0044 = YN**  No script |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0045** |
| **Biographical** | I was born in Nagoya, Japan on April 7, 1944. As it was in the middle of the Second World War, I was evacuated to Kawagoe Village in Mie Prefecture the following year to escape the aerial bombardment over Nagoya. Soon after the war ended, my father passed away. As I was only two years old at the time, I have no memory of him. My father, Hisashi, was a physician. At the end of his career, he served as the director of the central public health centre in Nagoya.  After my father’s death, we moved back to Nagoya to find our house rendered to ashes by the bombing. So we stayed at my mother’s family house. My mother, Ai, came from a family surnamed Kaifu. At that time, my mother’s parents and their elder son’s family were living in the house. The latter’s elder son, Toshiki Kaifu, who is my cousin, later became Japan’s prime minister. I don’t have any siblings. I have another cousin named Norio Kaifu, who is an astronomer. He served as the director of the National Astronomical Observatory of Japan.  In 1975, I married Sachiko Enomoto. My son, Junichiro, was born in 1977. Sachiko died of cancer at age 39. Junichiro majored and obtained a master’s degree in urban planning. He currently works at a private consultant firm. In 1990, I remarried. My wife Emiko Nakayama’s father, Tadasi, was a mathematician known for his research on Frobenius algebras. As he had passed away when Emiko was a child, I never had a chance to meet him. Emiko gave birth to our daughter Yuka.  I went to elementary and middle school in regular public schools. Nothing particularly unique happened during those school years. In high school, I played tennis every day. Though I never really got that good at it, I continued to enjoy playing tennis in my adult years. At around that time, I read *The Evolution of Physics* by [Albert Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/) and Leopold Infeld. I don’t remember to what extent I actually understood it, but the book sparked my interest in physics.  I entered the Physics Department of Nagoya University. Besides it being my local university, what also attracted me to Nagoya University was the presence of Shoichi Sakata on its faculty. The model of elementary particles bearing his name was famous, so even high school students knew about it.  As a graduate student, I began my research in particle physics as a member of Prof. Sakata’s lab. A free atmosphere abounded in the lab; treated impartially, the graduate students were allowed to participate in discussions among the researchers. I believe that I learned a lot from that experience.  Regrettably, Prof. Sakata passed away while I was still a graduate student. During my graduate student days, I engaged in many discussions with Prof. Yoshio Ohnuki. I also met Toshihide Maskawa at Nagoya University. As I recall, our first contact was when I was an undergraduate student: As a graduate student he was helping us with our studies. I began conducting joint research with Maskawa after entering graduate school. Our research theme at that time was on chiral symmetry. We were trying to approach the subject from a quark-model perspective.  In March 1972, I received my doctoral degree in physics from Nagoya University. At that time, it was not easy for post-doctoral researchers to find positions. Fortunately, however, I was hired as a research associate in the Physics Department of Kyoto University. I moved to Kyoto in April. I resumed joint research with Maskawa, who had transferred to Kyoto University a little before me. We worked on CP violation, the research for which we later received the Nobel Prize. Engaging each other in discussion, we advanced our research quickly, completing it in a relatively short period of time. By the end of August, we had finished writing our paper.  With the discovery of the J/ψ-particle in 1974, there was, as in other countries, quite a commotion in Japan. Many theories were ventured as to the character of the J/ψ-particle; ultimately, however, it was determined to be a charmonium, which is a bound state of the c quark and its anti-particle. Before that, the hint of a fourth quark was obtained by Kiyoshi Niu in his experiment exposing emulsion chambers to cosmic rays. In this connection, a few Japanese groups, including mine, were investigating a four-quark model but were not able to predict the long lifetime of the charmonium state.  In 1975, the tau lepton was discovered. Since this indicated the existence of third-generation quarks, the six-quark model we proposed began to attract attention.  Although I did not contribute directly to the development of the six-quark model, I did write a paper with Katsuhiko Sato that was somewhat related. We tried to elucidate the limitations on the mass and lifetime of neutrinos in using cosmological arguments when similar flavour mixing exists within a leptonic sector.  During that period, KEK (the National Laboratory for High Energy Physics, now the High Energy Accelerator Research Organisation) had started operating its proton synchrotron accelerator, and discussions were underway on planning the following TRISTAN project. My first relationship with KEK was participation in these discussions. I was then hired as an associate professor in KEK’s Theory Division and moved to Tsukuba in 1979. At that time, the Theory Division was headed by Hirotaka Sugawara. Motohiko Yoshimura came to the Division about the same time I did. That year, I received the Nishina Memorial Prize.  Upon arriving at KEK, I became engaged in drafting the proposal for the TRISTAN project. It was originally intended to be an electron-positron-proton collider. However, the one approved for construction in 1981 was an electron-positron collider. Operation started in 1987. With our dream to discover the top quark unfulfilled, TRISTAN was shut down in 1995.  During that period, I spent three months at CERN (the European Organisation for Nuclear Research) from November 1982. While I was there, the W particle was discovered. It was a very exciting experience for me. However, just before my arrival at CERN, J.J. Sakurai died while working there as a visiting researcher. In 1985, I was awarded the J.J. Sakurai Prize, instituted in that year by the American Physical Society. The same year I received the Japan Academy Prize.  In 1989, I was appointed head of KEK’s Physics Division II, where I assumed responsibility for an experimental research group. We set about in earnest to prepare plans for a post-TRISTAN accelerator: It would be the B-factory accelerator constructed inside the TRISTAN tunnel and operated with an aim to proving CP violation in a B-meson system. Construction was approved and started in 1994. Experiments using the B-factory started in 1999, and the initial results were obtained in 2000.  In 2003, I was appointed director of KEK’s Institute of Particle and Nuclear Studies, in which position I was directly responsible for the Institute’s experimental research activities including those conducted using the B-factory accelerator. At the time, Yoji Totsuka was appointed as KEK’s director general. Later, I came to work together with Totsuka again at the Japan Society for the Promotion of Science (JSPS), but he regrettably passed away in July 2008.  During my tenure as director of the Institute, KEK was converted from a government organisation to an independent research corporation. I became very busy in carrying out this reorganisation. Fortunately, we are able to steadily improve the performance of the B-factory accelerator during that period. Its experimental results showed our 6-quark theory to be virtually accurate.  I retired from KEK when my tenure as the Institute’s director ended in 2006. For a while, I was able to enjoy a life of relative freedom. In the meantime, I was invited to become an IIAS (International Institute for Advanced Studies) Fellow. In that capacity, I travel on occasion to the Kansai district to hold discussions and write papers with Taichiro Kugo, with whom I have conducted joint research in the past.  In October 2007, I became an executive director of JSPS, where I enjoy many opportunities to meet researchers from a wide spectrum of fields.  In recent years, I received the Person of Cultural Merit award in 2001 and the Order of Culture award in 2008, both from the Japanese government, and the High Energy Particle Physics Prize in 2007 from the European Physical Society. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0045 = MK**  [Makoto Kobayashi] Hello.  [Adam Smith] Hello.  [MK] Hello. This is Kobayashi speaking.  [AS] Ah, Professor Kobayashi…  [MK] Yes.  [AS] Thank you very much. This is Adam Smith from the Nobel Foundation’s web site in Stockholm. Congratulations of course on the award of the Nobel Prize.  [MK] Thank you very much.  [AS] I imagine it came as a surprise?  [MK] Yes, well, of course.  [AS] And, where did you hear the news? Were you at home or at work?  [MK] At my offices at the JSPS.  [AS] Right. You were awarded the prize for your work on broken symmetry.  [MK] Yes.  [AS] And in particular, for your model which predicted the existence of three new families of quarks.  [MK] Yes.  [AS] You were very young when you made this prediction.  [MK] Yes. It was 28.  [AS] And, it took almost 30 years for the prediction to be found to be correct by the discovery of these families of quarks in accelerators.  [MK] Yes, that’s right.  [AS] Were you confident throughout that your theory would be shown to be correct?  [MK] Ah. Actually, our work consisted of two parts. One is the … four quarks is not enough to explain the CP violation asymmetry. And that is a quite logical consequence of the argument. But the second point is then that … what is the … what kind of the new particles can explain the actual CP violation. And there are quite many possibilities logically. But just one proposal, six quarks, came as one possibility. So, in that sense we are confident about the first part because it’s quite logical. But the second point was quite uncertain at that time. And so the second experiments show that there is actually that many quarks existing. So, at first we were not confident about this six quarks scheme, but gradually we came to believe that this actually is the case.  [AS] There seems to be an interesting push-and-pull relationship between theoretical physics and experimental physics. Sometimes theoretical physics leads experiment, and other times it is almost catching up with experiment.  [MK] Yes, yes, yes.  [AS] Which period do you think we are in now?  [MK] This is actually quite a new phase. We had … the issue of the standard model is almost over. So then, now we are waiting for some kind of new physics. In the sense theoreticians predict, propose many theories, and we just wait experimental proof of those models.  [AS] I suppose in that light, it’s interesting that this announcement of your Nobel Prize comes shortly after the world has been focused on the development of the new LHC at CERN.  [MK] Yes.  [AS] And everybody is waiting to see what will come out of that.  [MK] Right, right.  [AS] What do you think will come out?  [MK] I personally expect that the LHC will reveal some kind of new physics, most likely the so-called supersymmetric theory. It’s … I just wait for the result.  [AS] Will the new physics replace the standard model or add to the standard model of particle physics?  [MK] Ah, not actually. Standard model is kept true, but we need to add something on the top of this standard model. That is what we expect at LHC experiment.  [AS] Right, right. And, do you feel that you were very fortunate to be a theoretical physicist working at the time you were working? Was it a good time to be a physicist?  [MK] Yes, I think so. Particularly the 1970s – the time of the liberation in the particle physics. At that time we had many chance to do many things.  [AS] Do you think there is any significance in the fact that all three Laureates in Physics this year come from a Japanese educational background? Is there something about Japanese education that makes one a good theoretical physicist?  [MK] Ah, I’m not sure, but I hope so.  [AS] And one hopes that continues to be the case. Yes, yes. Have you any idea for how you will celebrate the award of the Nobel Prize?  [MK] It’s almost 10 o’clock, midnight. So I would like to go sleep.  [AS] Then I should let you get off the telephone and go to sleep, but thank you. And when you come to Stockholm in December to receive your award, we will interview you at greater length I hope, and then we will speak more.  [MK] Okay, okay. Thank you very much.  [AS] Thank you very much for taking the time. Congratulations  [MK] Thank you, thank you.  [AS] Bye bye.  [MK] Bye bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0046** |
| **Biographical** | I was born in 1940 as the second child in a family living in Nagoya, a city with a population of around a million inhabitants. My older sister died of tuberculosis before entering elementary school and so I was an only child until my second sister, who is seven years younger than me, was born after the War. I had a weak constitution and was a thin boy having poor digestion. My parents were worried about my health and repeatedly took me to be examined whenever they heard about doctors with a good reputation. In my infancy, therefore, I did not play with other children of the same age and was raised only among adults, so that I had a very precocious way of speaking. Thanks to this, later at elementary school I could get perfect scores in Japanese examinations designed to test pupils’ ability to use words in practice, for example by constructing short sentences containing specified words. On the other hand I would get almost zero in reading and writing Chinese characters used in Japanese since I had not studied them.  After a new municipal library opened near my elementary school, I went there often and began to read books at random so that I gradually acquired the ability to read between the lines. Why did the author choose to write one way but not another even if they have almost the same meaning? In those days I got into the habit of thinking about the psychology of the book’s author. This habit proved helpful to me later when I became a researcher. When discussing with my friends, I often find myself able to obtain more information from the same papers than they do. Sometimes however, I make an error by reading what is not written in the paper.  When I was in elementary and junior high schools I did not concentrate in class and could not be called a good pupil by any standard. For instance, at the end of my third year at junior high school there was a Japanese class in which the teacher handed out manuscript papers to the pupils to write an essay which was to be inserted in a graduation memorial collection of compositions. My classmates all wrote about their future ambitions; one wanted to become a carpenter following in his father’s footsteps, another hoped to go to university to become a mechanical engineer and so on. But I wrote about the evolution of stars, about which I had been reading in a boys’ magazine at that time. I had not heard the teacher’s explanation that the essay was to be included in the graduation memorial volume.  At that time I had never thought about my future or had any definite goals. I went on to high school with no strong motivation, but simply because my friends did so. The ratio of students who went on to high school then was about 50%. There was a high-achieving girl in my class who had decided not to continue to high school. Although I was not particularly friendly with her, since our seats in the class were well separated, I remember feeling that it was one of life’s absurdities when I heard her tell the teacher that she would not go on to high school.  There is an event engraved in my memory, which I experienced in the period before entering high school but after graduating from junior high school. I purchased the textbooks required for the high school and brought them back home. While I was browsing through the mathematics textbook, I noticed a strange character; the summation symbol Σ. As soon as I read the explanation I understood how to use it. I found that, by using the linearity of the Σ symbol, I could compute the sum of the *n*-th power of integers, 1*n*+ 2*n*+ … + *kn*, for any value of *n*, in principle. I was so excited that I calculated the sum up to rather high powers *n*, although this took me a long time. Not surprisingly for a boy who had not yet entered high school, I did not have sufficient knowledge to devise a generating function for the sum.  After the war, my parents were engaged in a small business which required them to work together from early in the morning until late in the evening, so that they did not have the time to pay much attention to their children’s study. Taking advantage of this, I played and played without studying. There was also another benefit from the fact that my parents were running their own business from home. They were mainly dealing with sugar as an ingredient for cakes. The ordinary sugar was packed in 30 kg bags of kraft paper like cement bags, and Cuban sugar in 100 kg bags of hemp. They also retailed sugar, subdividing it into 10 kg, 20 kg, and so on, so that the bags were stacked to be discarded. Our parents gave them to us children instead of an allowance. Since they could be sold if they were brought to a suitable place, I had some extra spending money − more than my friends did. I spent almost all my money on books. I still cannot get out of this habit. I buy books in bulk, not selecting them carefully, and read them later when I find them on my bookshelf.  When I was in high school, it had already been ten years since the war had ended and the world was becoming peaceful again. There still remained shortages of cultural materials, however. For example, the supply of new books was still insufficient, so that I had to walk around the area of secondhand bookstores every weekend afternoon with pocket money obtained by selling kraft and hemp bags for sugar. Initially the main genres of the books purchased with this money were detective and mystery stories and novels by Ryunosuke Akutagawa. Later I gradually began buying more mathematics books.  The first book I bought in mathematics was the “Theory of Functions” published as a volume in *New Mathematics Series*by Baifu-kan, Tokyo. I was very excited to see how a mathematics book was written, since I had seen no books other than school textbooks. I only knew the term “function”, which had already appeared in the high school mathematics textbook. The book gave me a glimpse of the rich world of differentiable functions of complex variable and I felt dizzy as if wandering off into a foreign place, quite different from the world I had previously known.  During my last year of high school, the Soviet Union successfully launched the first artificial satellite, Sputnik I. After this event I began to calculate the orbits of satellites and rockets by using a slide rule and an abacus. From the relative position of the Moon and Earth, I predicted for my friends the day on which the Soviet Union would next launch a rocket. To confirm the validity of the prediction, I always listened to the short-wave broadcasts of Radio Moscow from 11:00 at night. Then I realised that there is an issue with the precision of the time shown by clocks. From my measurements I discovered that my watch had a systematic error of − 8 sec/day and a random error ± 2 sec/day. The problem was the seasonal variation of the systematic error. If the temperature rises, the balance wheel becomes larger and its moment of inertia also gets larger, so that the clock should become slower. The temperature dependence of this deviation was, however, the opposite of estimates. This annoyed me for a few years. I looked for and examined books about clocks whenever I visited big bookshops, but I could not find the answer. About five years later I found the answer at last. The thermal expansion of the balance wheel and change of the moment of inertia were actually compensated by an ingenious mechanical device. I learned a lesson from this experience. I knew that wall clocks have such a temperature correction device since it can be seen. But I didn’t associate it with wristwatches. This taught me to think matters through carefully, taking as many relevant elements into account as possible.  I entered Nagoya University after one year of hard study, motivated by my desire to avoid succeeding my father and becoming a sugar merchant. The first class at the university was mathematical analysis by associate professor H. The first thing he said was: Suppose that there are two arbitrary positive numbers ε and *a*, then there always exists an integer *N*such that *N*ε*> a*is realised. This is called Archimedes’ axiom. Having declared “I will prove this”, he began the lecture by explaining Dedekind’s cut. What is this! Why isn’t it all right that we calculate *a*/ε and take *N*to be the integer part of it plus 1? It was a Culture Shock.  Next was a biology class by professor T. When I took a seat in the front, a sheet of paper was sent to me from behind. It contained some challenges, saying, “Solve the following problems!” There were six mathematics problems written on the paper. I remember that they were problems, which required solving differential equations such as determining the catenary, the form of a suspended chain. In this way the circle of my friends increased and I was also able to meet some good teachers.  At that time at Nagoya University, the campus for freshmen and sophomores was separated from that for juniors and seniors. The campus for the general education course for the students in the first two years of study had previously been a high school in the old system of education and there were several teachers from that system remaining in the Physics Department. When, during discussions with my friends, we encountered problems which we didn’t understand, we used to go to the teachers’ room to ask for help. When these visits became frequent, many teachers began to avoid us since they found us to be troublesome. There was however an exceptional teacher N who coolly answered our questions sitting back in an armchair. He was a young professor just past thirty. When we asked questions, he used to reply in a dignified manner, “I cannot immediately answer questions which are raised so suddenly. Study them yourselves. I will lend you this book if necessary.”  This was the first time that I had met somebody referred to as a researcher. The teachers I had met until my high school days were just teachers, who taught what they knew. But I realised that teachers at the university are also required to do research, discovering novel things. The students who gathered around professor N began to form a relatively fixed group, doing many other things together. The members of this group came from the whole faculty of science, although most were students in the Physics Department. They called themselves the DEPHIO group taking the initials D([Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/)), E([Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/)), P([Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/)), H(Hilbert), I(Ingold), O(Oparin), who were great scientists representing each department. The activities of DEPHIO included, for instance, lodging together in a borrowed country house for ten days during the summer vacation and holding a seminar class at a 2,000 meter high hill of the North Japanese Alps, having carried Dirac’s thick textbook up there. Although these activities were not directly relevant from the viewpoint of doing research, I think they were meaningful in encouraging solidarity among the members of the group and in helping them become researchers together. DEPHIO is no longer active, but the members still keep contact with each another.  At that time, I was mainly discussing mathematics with the group. I also continued to visit second-hand bookshops and found, for instance, books from a series of Mathematics lectures published by the publisher Iwanami before the War. Later I happened once to tell a mathematician that I had studied non-Archimedean valuation using one of those books. He was surprised and said, “I have heard that you are a fan of mathematics, Maskawa-san. But you are even studying such things!”  In our college days, the members of the DEPHIO group discussed together whenever they met. During the year 1960, Japan was politically in chaos since public opinion was split in two as to whether the Japan–U.S. Security Treaty should be concluded or not. The treaty could not be approved in the House of Councillors, but was soon automatically approved because of the dominance rule of the House of Representatives. The prime minister at the time then resigned.  Almost all classes were cancelled during this period because of a student strike. I attended all possible demonstrations, motivated by a young man’s sense of justice. It was inadmissible to me that the clock in the House of Councillors had been stopped to make time to enable the bill to be enacted and to prevent it from being scrapped.  There were no other periods later in my life in which I had as much free time as during those days. Although there were tasks to be performed, like going to demonstrations, distributing bills, collecting signatures and so on, there was also much free time in between. We used some of this time to hold outdoor seminars. Since everybody gathered in the university courtyard, it was easy to organise such seminars with little notice.  When I moved to the Higashiyama Campus as a junior student, I was still hesitating whether to study Mathematics or Physics. Mathematics in Japan at that time still remained under the strong influence of axiomatic Bourbakism. I did not know this definitely, but intuitively felt that this was the case. On the other hand, I felt that Physics was moving forward vigorously and so I submitted an application form for the graduate course in which I wrote that I wish to study theoretical physics. In fact, I had actually not yet decided my intended field of study quite so definitely even at that stage, as illustrated by the following event. When I was walking in the campus of the Faculty of Science at that time, a Professor in the Mathematics Department said to me, “You, Maskawa, will of course take an entrance examination for a Mathematics graduate course, won’t you?” I answered, “No, I have just submitted the application form for a Physics graduate course.” The professor looked upset hearing this totally unexpected answer. Probably, I had been saying until recently that I was planning to go on to the Mathematics graduate course.  In the graduate course of the Physics Department, there were about ten beginning students who intended to major in theory, of which about six wanted to study particle physics. But this over-concentration on particle physics was merely due to their ignorance of the various interesting fields in science which they might tackle. After one year of study in the graduate course they spontaneously scattered, finding the scientific fields which fit their personalities best, like astrophysics and nuclear physics.  The students majoring in theory were not assigned to any specific laboratory for their first year, but worked in turn for three month periods in a number of laboratories: particle physics, condensed matter physics, and so on. Thanks to this system, I had the chance to study condensed matter physics. During this period, in addition to my studies, I began a voluntary circle to study a *perceptron*with a few friends, since I thought that research of the brain and human consciousness was important but not yet fully understood theoretically. We read papers in that field but, unfortunately, none of us had any knowledge about the anatomy of the cerebrum. None of the members was sufficiently enthusiastic to transfer to the medical department in order to study it, and so the circle died out in course of time. I noticed then that the number of research papers about the perceptron was decreasing significantly. I understood the reason later. Nuclear submarines had become increasingly important strategically because of the Cold War. The perceptron became a subject of military research because of the possibility that it could be used to identify nuclear submarines by sound spectrograms.  In the Physics Department of Nagoya University, the first year students majoring in theoretical physics in the masters course had to attend the seminar on field theory as a compulsory subject for a year. In those days, all the particle physicists throughout the world believed for a variety of reasons that field theory would be replaced sooner or later by a future new theory. This was also the case in Sakata’s laboratory. The curriculum of the field theory course was designed in the 1950s. It began with Dirac’s quantization of fields using the variables of amplitude and phase. The course then continued to [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/)–[Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/)‘s field theory and Fock space, and through [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/)‘s ingenious theory describing electromagnetic interaction as an action at a distance eliminating the photon picture, ending with Dyson’s renormalisation theory supplemented with [Salam](https://www.nobelprize.org/nobel_prizes/physics/laureates/1979/)‘s paper on b-divergence. This course made me pay attention to field theory, which no one took any notice of in the 1960s. Thanks to this, I got interested in theoretical problems related to weak interactions. When the importance of field theory was later appreciated, the situation had turned full circle and the curriculum of theoretical physics at the graduate school of Nagoya University was again at the forefront of world physics.  I entered the laboratory of Professor Sakata in 1964 and began my research in particle physics. However, my capriciousness did not change after this and I continued to collaborate with physicists in other fields like nuclear physics, condensed matter physics and so on.  Finally I would like to thank Professors Taichiro Kugo and Christopher Sachrajda who kindly translated my Japanese draft into this English form. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0046 = TM**  [Adam Smith] In the light of the fact that all three Nobel Laureates in Physics this year had their education in Japan, do you think there is anything particular about the Japanese educational system that helped you develop as a theoretical physicist?  [Toshihide Maskawa]  The period in which I was raised as a researcher was at a time after a brutal and reckless war and before the calmer period of the 1960’s was first coming into view. What was often said was that as there are no natural resources in Japan, we must survive on the strength of science and technology.  It was within an atmosphere such as this that I think I gained a yearning and an affinity for Science.  [AS] Is there anyone you would single out as having provided especially useful direction on your path to becoming a physicist, and why?  [TM] That would have to be my professor at university, Prof. Shoichi Sakata, a pioneer in Quark Theory and one who advocated the Composite Particle Model. He presided over our research laboratory which collectively discussed and gathered research based on research methods that vouched for the Dialectics of Nature. It was there that I studied how to think and in what form things should be accomplished.  [AS] When you submitted your paper together with Makoto Kobayashi predicting the existence of six quarks, how confident were you that your prediction would be proved correct?  [TM] In any research thesis there is always an element of supposition. The year 1972, when that thesis was written, was in period in which many researchers still didn’t consider the effective function of the Renormalization Theory. The assertion of our thesis was that if weak interaction was as described in the Renormalization Theory, then our predictions would be proved correct. Experiments have since shown the correctness of this assertion.  [AS] How long did you think it might take for the missing quarks to be discovered? And were you surprised by how long it actually took?  [TM] To be honest, I didn’t imagine that the top quark would ever be as heavy as it is.  [AS] The Nobel Prize in Physics for 2008 was awarded for studies of broken symmetry. Do you have a way of describing broken symmetry to non-physicists?  [TM] To borrow an often used example, there is a saying that if you put the grass that a cow likes in a circle around the cow, then whatever grass the cow eats, as far as the cow is concerned, will be the same. However if the cow sooner or later chooses a certain direction in which to eat the grass, then we have a state of broken symmetry. It must be remembered that this is only an example, and that this is a phenomenon that first arose based on the rule that the microscopic world holds an unlimited degree of freedom. Within examples, there are of course always elements of untruth.  [AS] In particle physics, it seems that sometimes theory leads experiment, and at other times experimental results drive the theories. If you agree, which phase do you think we are in now?  [TM] In this current age I believe that theory is taking the lead. However, for a theorist, the most exciting period is after waking early one morning to discover a truth that you could not have imagined.  [AS] What do you think we can expect from the Large Hadron Collider? And in what timescale?  [TM] Surely this will be evidence of Super Symmetry and the discovery of [Higgs](https://www.nobelprize.org/nobel_prizes/physics/laureates/2013/) Particles, as well as discoveries of such matters which we would never have imagined.  [AS] What advice would you offer young people thinking of entering theoretical physics today?  [TM] My advice to young people is to be ambitious and to have sincerity toward our natural world. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0047** |
| **Biographical** | I was born in March 1938 in the small town of Carcassonne in the south of France. Later, and until I was two years old, I lived in Toulouse, another city in Southern France, where my parents were high school teachers, my father in physics and my mother in economics. But war loomed on the horizon. My father was mobilized into the army in June 1939, two months before the birth of my brother André. In 1940, he was captured and made a prisoner of war by the Germans; he returned home only in 1945. For the duration of the war, my brother and I were sent to live at our grandparents’ farm in Montclar, a very small village in the foothills of the Pyrénées. My mother, who continued to teach in Toulouse, came to see us every weekend and so, until the age of seven, I lived the life of a country boy, milking the goats, harvesting grapes in my grand father’s vineyards, setting snares for rabbits or hares. I lived very close to the world of plants and animals and very far removed from the world of physics.  In June 1945, my father returned from prisoner of war camp and our family was reunited in Toulouse. I became a city boy with a longing for life in the countryside. My father, while continuing to teach high school, prepared his doctoral thesis. Eventually, he was promoted to the rank of Professor at the University of Toulouse where he made important contributions to the development of electron microscopy. My brother and I applied ourselves studiously to our primary and secondary school work. Our father followed our progress in the sciences. Unquestionably, his penchant for rigorous thinking had a major influence on my approach to mathematics and physics. I earned good grades in the sciences. During my adolescent years, I also developed a great interest in literature, the arts and sports. At the age of fourteen, I began playing rugby and I was very proud of being selected to play in our high school team. At the age of seventeen, having completed my studies for the baccalauréat, I found myself drawn to Paris and above all to the Ecole Normale Supérieure (ENS), not only because it was a prestigious institution but also because it had the added attraction of being in the center of Paris, close to the Latin Quarter and St. Germain des Près, and at the heart of the city’s intellectual life. I worked hard to prepare for the competitive entrance examinations at the ENS, in mathematics and physics. I was accepted. In September 1957, I lightheartedly left Toulouse for Paris and the Ecole Normale Supérieure.  My six years at the Ecole Normale Supérieure between the ages of 19 and 24 were a very intense period of my youth. The richness of my life on the small campus of the rue d’Ulm came from daily contacts with students working in a broad spectrum of disciplines, the sciences, philosophy, literature, history etc. In addition Paris was everywhere around us, with its museums, exhibitions, cinemas, concert halls and jazz clubs. I became a passionate fan of jazz, photography, film. I actually wrote a script and made a film.  It is possible that I might have been diverted from the pursuit of a career in science had I not been studying with excellent physics instructors. In particular, Jacques Friedel, who had established a physics program at the masters degree level, exposed me to the most up to date developments in condensed matter physics and emphasized as well an in depth teaching of quantum mechanics and statistical mechanics. It was this program which drew me into the study of condensed matter physics. When I returned from my military service in 1965, I began work on my doctoral thesis under the direction of Ian Campbell, in the Laboratory for Solid States Physics at Orsay which Jacques Friedel directed. I had the good luck of having the thesis topic: “Testing the suggestion of [Neville Mott](https://www.nobelprize.org/nobel_prizes/physics/laureates/1977/) (future Nobel Prize laureate) that the mobility of electrons in a ferromagnetic metal depends on the orientation of their spin in relation to their magnetic orientation”. In the course of the research for my thesis, I became hooked on physics. I discovered that the work of a young inexperienced researcher can open many doors – provided that his experiments are well executed and rigorously interpreted. My findings in my thesis, as well as my in-depth knowledge of the physics of electron transport, made it possible for me to return to this subject fifteen years later and to discover the giant magnetoresistance. I am still surprised when I see that my findings during that earlier period comprise a large part of the basis for spintronics today.  However, when I defended my thesis in 1970, the available technologies did not allow for further advancement in the direction of giant magnetoresistance and spintronics. Although a concept similar to that of GMR already had emerged from my thesis experiments on ternary alloys, it proved impossible to extend it to the case of multilayers because it was impossible at that time to produce sufficiently thin layers. Thus, the exploitation of the influence of the spin on the mobility of electrons in ferromagnetic metals had to be postponed until the mid 1980s. Following a post-doctoral appointment at the University of Leeds, I returned to Orsay to take up a position as Assistant Professor at the University of Paris-Sud and to supervise a small group of doctoral students and post-docs at the Laboratory of Solid State Physics. I was promoted to the rank of professor in 1976. Marie-Josée and I lived in Paris; Ariane and Bruno, born in 1968 and 1971, were growing up and I divided my time between family life and a very sustained effort in research and teaching. During our summer vacations, we returned to the Mediterranean and the Pyrénées, to the home of my parents in Banyuls sur Mer, close to the Spanish frontier. My research activities during the 1970s and at the beginning of the 1980s touched on a variety of subjets and yielded fruitful results. The thesis of my first doctoral student, Alain Friederich, shed new light on problems relating to the Hall effect and to the anisotropy of magnetoresistance linked to spin-orbit coupling. Today the Spin Hall effect has become a hot topic in physics. I think that some of our results from that period should have an impact on spintronics today. In 1975, in order to interpret our results on the Hall effect and the anisotropy of interactions between electrons and magnetic impurities, I began a collaboration with Peter M. Levy, a theorist at New York University. Over the course of several years, I spent many summer months at New York University and learned to love the city, Greenwich Village and its jazz clubs, SoHo and its art galleries. My collaboration with Peter Levy on theoretical problems encompassed numerous subjects and, among other results, led to the discovery of the existence of Dzyaloshinsky-Moriya interactions in spin glasses. One consequence of these interactions is the triad anisotropy in spin glasses, which was confirmed experimentally in the thesis of my Ph.D. student Dimitri Arvanitis.  In the mid 1980s, it seemed likely that techniques developed in the field of microelectronics would make it possible to grow magnetic multilayers built up from layers of nanometer thickness. It was precisely at this time that my former student, Alain Friederich, was developing Molecular Beam Epitaxy (MBE) in his research group of the Thomson-CSF company. While attending a meeting in San Diego in 1985, Alain Friederich and I were discussing our work at a bar next to the swimming pool, under the palm trees and the stars and we decided to collaborate on the growth and study of magnetic multi-layers. With help from the expert on MBE at Thomson-CSF, Patrick Etienne, as well from my doctoral students, Frédéric Nguyen Van Dau, Frédéric Petroff and Agnès Barthélémy, and two post-docs, Mario Baibich and Jean-Marc Broto, our collaboration resulted in the discovery of giant magnetoresistance at the beginning of 1988.  On the day we discovered giant magnetoresistance, we were measuring several multilayers of Fe/Cr, one after the other. We were not altogether certain about the quality of the multilayers that contained the thinnest layers of chromium. Exercising caution, we began by measuring multilayers in which the layers of chromium were the thickest. We determined that there was indeed a magnetoresistive effect. We then moved on to multilayers in which the chromium layers were progressively less thick. And wonder of wonders! The greater the decrease in the thickness of the chromium, the larger the magnetoresistance. The final measurement, which we took on 0.9 nm of chromium, was stupefying. The resistance increase between the parallel and anti-parallel configurations of the layers of iron went up to 80%! We had just participated in the birth of the phenomenon which we called giant magnetoresistance (GMR).  The International Conference on Magnetism which took place in Paris at the beginning of July 1988 offered us our first occasion to present the results of our work on giant magnetoresistance in Fe/Cr multilayers. However, my Ph.D. student Frédéric Nguyen Van Dau had only a few minutes in which to make his presentation and therefore his paper did not provoke much of a response. Peter Grünberg did not attend the conference. It was only the following week, at the International Conference on Magnetic Films and Surfaces (ICMFS) at Le Creusot in Burgundy, that Peter Grünberg and I were able to present the details of our respective results and to take the measure of the considerable interest which our findings provoked among members of the magnetism community. At this conference, I also was able to explain my earlier results which showed the influence of spin on the mobility of electrons in ferromagnetic metals and to present the interpretation of “GMR” on this basis. Interest within the magnetism community in this new type of phenomenon which exploited the spin of electrons in magnetic nanostructures grew exponentially following the publication of our article by Physical Review Letters in December 1988. I have some heartwarming memories of this period. As I neared the end of my first paper on this subject presented at a conference in the United States, I was anxious to know whether I would be able to complete my presentation within the designated time frame. I asked Bret Heinrich, the Chair, what time was left. He told me that, given the level of interest in the work I was presenting, I could continue for as long as I wanted. Every researcher dreams of such a response from the chair. Thanks Bret!  Beginning in l989, the physics of multilayers and GMR became very hot topics and subjects of research in an increasing number of laboratories. In addition, my own research activity was expanding on many fronts. Apart from experimental work on the Fe/Cr systems in the theses of Frédéric Nguyen Van Dau, Agnès Barthélémy and Frédéric Petroff, I developed, with Agnès Barthélémy, a semi-classical theory of GMR which complemented that of Camley and Barnas. I also collaborated with Peter Levy and Shufeng Zhang of New York University on the development of the first quantum model of GMR. In 1990, we began to study multilayers prepared by sputtering in a collaboration with the team of Peter Shroeder, Jack Bass and Bill Pratt at Michigan State University (MSU). This collaboration led in 1991 to the first observation of GMR and of oscillations in the interlayer coupling for the Co/Cu system by my doctoral student Dante Mosca. Stuart Parkin obtained similar results at approximately the same time in Almaden. The system Co/Cu later became the archetypical GMR system. The first observations of inverse GMR in the thesis of Jean-Marie George constitute another important result of my team in the early 1990s. Beginning in 1991, I became interested as well in results of the team at MSU on GMR in the geometry where the current is perpendicular to the plane of the layers (CPP-GMR). In collaboration with a young researcher at Thomson-CSF, Thierry Valet, I developed a theory of the CPP-GMR based on the concept of spin accumulation. Today, this theory has become essential for many current developments in spintronics. The 1993 Valet-Fert paper in Physical Review is my second most frequently cited paper, coming just after the paper in which we presented the discovery of GMR. Beginning in 1994, I collaborated with Luc Piraux at the University of Louvain, on studies of CPP-GMR using multilayered nanowires. These studies made it possible to extend the results of MSU to much greater thicknesses and to confirm the length scale linked to the effects of spin accumulation in the Valet-Fert model.  Thus, the beginning of the 1990s was for me and my research team a period of intense and fruitful activity during which we made important contributions to the development of the physics of GMR and to the establishment of essential concepts in spintronics. Peter Grünberg and I, in collaboration with firms like Thomson-CSF, Philips and Siemens, have participated as well in several projects financed by the European Community in order to further the development of new applications. The year 1994 brought us international recognition, with the ‘International Union of Pure and Applied Physics’ presentation of its Magnetism Award to Peter Grünberg and myself, and the American Physical Society’s award of the International Prize for New Materials to Peter Grünberg, Stuart Parkin and me. It also strucks me at this time that GMR was only a first step in the exploitation of spin in magnetic nanostructures and that this first step had opened the door to a much larger field of research – the field we now call spintronics. Alain Friederich, the director of the physics group at the laboratories of Thomson-CSF, proposed to me that we create a new laboratory which would bring together the CNRS and Thomson-CSF for a joint exploration of the perspectives opened up by GMR. We proposed a research program which moved us into a large number of new areas: magnetic tunnel junctions, half-metallic ferromagnets, spin injection phenomena, spin transport in semi-conductors … The project was accepted. The new laboratory, called the Unité Mixte de Physique (UMP) CNRS-Thomson-CSF, (which later became the UMP CNRS-Thales) was established in the spring of 1995, at Corbeville, a few kilometers from the University of Paris-Sud. An agreement joined the laboratory to the University, in which I continued my teaching. From the beginning, the laboratory brought together my team at Orsay, the team of Thomson-CSF with which we were collaborating and several researchers and engineers at CNRS: Jean-Marie George, Annie Vaurès, Jean-Luc Maurice (shortly thereafter, a team for the study of superconductivity joined the laboratory). Since then, it is within this Unité Mixte CNRS-Thales that my research activity has progressed in many directions in the field of spintronics. The team has been progressively enlarged with the arrival of new researchers from CNRS and assistant professors from the University of Paris-Sud – Abdelmadjid Anane, Manuel Bibes, Vincent Cros, Cyrile Deranlot, Julie Grollier, Henri Jaffrès, Richard Mattana and Pierre Seneor.  It is difficult for me to describe my research activities at the CNRS/Thales laboratory between 1995 and 2007 precisely because this activity has moved in so many directions and has explored very diverse dimensions of spintronics. I can only refer the reader to my Nobel Lecture for details about the results of this activity. It is also the case that the relatively young members of my team are achieved their independence and have progressively developed their own research programs. Numerous and diversified studies of magnetic tunnel junctions have been undertaken by Frederic Petroff, Agnès Barthélémy and Vincent Cros. Agnès Barthélémy, assisted by Manuel Bibes, is now responsible for all research on the applications of magnetic oxides and multiferroics in spintronics. Our very important activity on the phenomena of spin transfer is being developed, for the most part, by Vincent Cros and Julie Grollier, with the help of Abdelmadjid Anane and Henri Jaffres. Spintronics involving semiconductors is being studied by Jean-Marie George, Henri Jaffres, Abdelmadjid Anane and Richard Mattana. Single electron spintronics has become the domain of Pierre Seneor and Frédéric Petroff, while Frédéric Nguyen Van Dau directs the development of various applications and Jean-Luc Maurice is the expert in structural studies. An activity in molecular spintronics is in the process of being launched by Pierre Seneor, Frédéric Petroff, Jean-Marie George and Richard Mattana. All of them are currently participating in the development of nanotechnologies, with the valuable assistance from Cyrile Deranlot and from Karim Bouzehouane, Stéphane Fusil and Eric Jacquet of the superconductivity team. As for me, I am making every effort to participate in most of these current activities. I also have been able to devote myself a little more to theory: the modeling of single-electron spintronics with Jozef Barnas at the University of Poznan; the theory of the injection of spin into semiconductors with Henri Jaffrès; the theory of the phenomenons of spin transfer with Peter Levy, Henri Jaffrès, Jozef Barnas.  The year 2007 marked a high point in the activity of the UMP CNRS/Thales. It was rich in new results and publications and, for me, abundant in scientific prizes: the Japan Prize in April, in Tokyo; the Wolf Prize in Physics in May, in Jerusalem and the Nobel Prize in December, in Stockholm. The Nobel Prize, of course, has changed my life. I have received innumerable requests; and new responsibilities are on the horizon. In addition, I am eager to return to my research projects and to concretize my recent ideas. A difficult challenge! I also hope that the Nobel Prize will facilitate the entire team’s energetic communication of its ideas and messages.  In conclusion, I want to thank all those who have helped me to become who I am today: first of all my parents and Marie-Josée; also Jacques Friedel and Ian Campbell, my first guides in my trajectory as a physicist; Alain Friederich, with whom I was able to launch a collaboration which was decisive for the discovery of GMR; the team at the Unité Mixte CNRS/Thales, and, finally, all those who have worked with me during my forty year-long career as a physicist. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0047 = AF**  [Albert Fert] – Hallo?  [Adam Smith] – Hello, this is Adam Smith calling from the Nobel Foundation’s website.  [AF] – Oh yes.  [AS] – Well, it’s so nice to talk to you, and congratulations on yesterday’s prize.  [AF] – Thank you.  [AS] – How have the last 24 hours been?  [AF] – Oh, the last? OK, many journalists, many TV, and trying to explain, simply, the science, the physics.  [AS] – It’s daunting physics for people because it’s quantum mechanics.  [AF] – But it’s possible to explain simple things.  [AS] – The analogy that I’ve been using to try and explain what it is that is happening …  [AF] – Yes?  [AS] – … is the idea of crossed polaroids.  [AF] – Yes, exactly, yes.  [AS] – That you can consider electron spin to be the polarizers and when they’re crossed they prevent the passage of current and when they’re aligned they allow the passage of current.  [AF] – Yes, this is the good picture. Yeah, and the basic physics is what can be the polarizer for the spin of the electron.  [AS] – Yes, and the polarizer is the magnetic field?  [AF] – No, it’s not the magnetic field, it’s the magnetic material. And so the basic physics is; the influence of the spin on the mobility of the electron in magnetic materials. And so the concept of the GMR is to put on the way of the electrons a thin layer of magnetic materials, and this thin layer will be the polarizer, the two polarizers, or the mutli-polarizers. And so, because the magnetization can be controlled by the field, so this is also a way to detect the field, OK. But the intermediate between the field and the electrons, there is the magnetization of layers.  [AS] – Now this is an extension of the discovery made by William Thomson 150 years ago, and you discovered the new effect of giant magnetoresistance in 1988.  [AF] – Yes, in fact, between the observation you refer to, there was this physics. So the physics is the influence of the spin on the mobility of the electron in the magnetic material. This has been suggested by Mott, Sir Neville Mott, Nobel Prize, before the World War, and is something that I have confirmed, quantitatively, in my PhD. The defence was in ’70. And so, the physics basis for my idea in this field came from all the results I got in my PhD and just after on the strong dependence of the conduction in magnetic material, dependence on the spin.  [AS] – Right.  [AF] – And so, I found that this dependence can be very strong if one dopes some materials with impurity, for example, and so I propose also what is called now the two current model of the conduction in magnetic materials. But at this time, say ’70, ’74, to proceed to the GMR was not possible because the GMR, in fact we had some concept at this time really close to the concept of the GMR, but to really find the GMR of the multilayer it would have been necessary to be able to fabricate multilayers with layers as thin as 1 or 2 nanometers, a few atomic layers, and this was not possible in the ‘70s.  [AS] – So it took the development of the technologies …  [AF] – Yes, and so some ideas were put into the fridge during some time and then, with the progress in the technology of the deposition of layers, due to the microelectronics mainly, it became possible, in the mid ‘80s, to prepare such multilayers with very, very thin layers. And Peter Grünberg at this time, and myself, were more or less pioneers in the field of the fabrication of such nanostructures. You see the basic physics was … and there is another important basic physics: before the GMR, first, so this spin-dependence of the conduction (my PhD and the work just after) and the work of Peter Grünberg who demonstrated in ’86 that in multilayers of iron and chromium there was a coupling between two adjacent iron layers separated by chromium and this coupling make that the magnetization are in opposite directions, in the two iron layers. So it was possible with this system to get a system in which it was possible to change from parallel to anti-parallel polarizers. And so, by combining the idea of the conduction, the result of Grünberg, it was possible to find the GMR.  [AS] – To envisage that it was there and then to find it once the technology was available.  [AF] – This is a nice example of physics, science, advances frequently by the encounter between two domains, two different domains. The meeting between fundamental physics, on the conduction, and technological advances.  [AS] – So you have to be aware of what’s happening in both fields?  [AF] – Yeah, yeah, yeah. And in fact, the nanotechnologists are a wonderful tool for the physicists, and for the biologists and for the chemists. Because this is a tool, this is not, nanotechnology is not really a science, this is a tool for us. We used this tool to discover the GMR. Now this tool of nanotechnology is used in many other aspects of spintronics because in my opinion, more important than the GMR and its application to the hard disk and so on, in my view more important is that this has opened the field of a new type of science with this spintronics, with many other effects related to the influence of the spin on the conduction.  [AS] – And this will lead to quantum computing, perhaps?  [AF] – Quantum computing is one of the axes, one of the roads, but now in spintronics, you have for example what is called the spin transfer phenomenon. So, in spin transfer you can manipulate the magnetization of a ferromagnetic body without applying a magnetic field but only by a sort of transfusion of spin angular momentum from an electrical current and this can be used either to switch the magnetization or to generate oscillations in the microwave frequency range. This is more or less exactly the contrary of the GMR. In the GMR you detect a magnetization with a current. In spin transfer, you create a magnetization with a current, a spin-polarized current, an electrical current with different numbers of spin-up and spin-down.  [AS] – And although this is still basic research, what do you see as the practical application of spintronics?  [AF] – Practical application? The next generation of MRAM, magnetic random access memory, will use the switching of the memories by spin transfer, this is already, very good results have been announced by Sony, for example, in Japan. Hitachi too. So, one application is the switching of magnetic devices and another application I am working on intensely now is the application to the emission of microwaves. To have very small emitters because by using oscillation of the magnetization by spin transfer, this oscillatory motion, one induces also an IC voltage in the gigahertz range and this is a way of producing oscillations, emitting microwaves. So there are many applications that can be expected in telecommunications.  [AS] – Basically these are microscopic transmitters that are being created?  [AF] – Yes, microscopic emitter, microscopic oscillator. In fact it’s better to say oscillator, because it produces an electrical voltage in the gigahertz range.  [AS] – Where would you see these being used?  [AF] – For example, because these oscillators are very agile, flexible, you can tune the frequency by changing the current, the spin-polarized current, and so this can be a generation of very flexible oscillators, very flexible emitters. For example, in mobile phones you start from the oscillation in the megahertz range and after some doublings of frequency you can get the microwave range. But these emitters are directly oscillating in the gigahertz range, and also you can change easily the frequency, very easily the frequency, and so they should present a very flexible new generation of oscillators.  [AS] – That’s very exciting stuff.  [AF] – Yeah, this is one of the axes in spintronics. You know, for the researcher, for the physicist, the past is the past but the more exciting is what is emerging now, OK. So I describe where is my excitation now.  [AS] – Yes, yes.  [AF] – So this road of spin transfer, also spintronics with semiconductors, towards a fusion between classical electronics and spintronics. Spintronics with molecules also is very promising,  [AS] – This is thrilling stuff, it’s lovely to hear. Thank you for describing it. I should let you go soon, but I wanted just to return to the fact that you are the new French Laureate, and this will make you a celebrity. How do you feel about the prospect of being more of a celebrity now you have this Prize?  [AF] – The prospect for me?  [AS] – Yes.  [AF] – For me. OK, this is fantastic for me, but maybe for my team? So this certainly, all my young co-workers are very, very happy to be recognized, and that is recognition of the work of me and of the team. And so this certainly will help them in developing their research with me, of course. So this is a good opportunity, this is good for us.  [AS] – Yes, and good for the field, of course. Well, thank you very much indeed for this conversation. When you come to Stockholm in December to receive your Nobel Prize we interview Laureates again, so I hope we’ll have a chance to talk again then.  [AF] – OK, so thank you.  [AS] – Well, thank you, very much indeed.  [AF] – And see you in December.  [AS] – See you in December, bye, bye.  [AF] – Bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0048** |
| **Biographical** | Atypical question I am often asked as a Nobel Laureate in physics is “what brought you to physics?” I am not sure but I do know that in school in geography when looking at the presentations of the planets orbiting around the sun I asked myself: “What is the reason for this strange behavior?” It was a real revelation when my physics teacher Mr. Röderer explained to me that this is caused by the balance of the attraction between masses and centrifugal forces. This roused my enthusiasm and whetted my appetite. Still, during my last few years in high school, I spent more time with sports, boy scouting, alpinism, music etc. so my performance in school was reasonable but not more.  Perhaps in this context, it might be of interest to know that my father held a diploma in mechanical engineering from the Technical University of Bruenn (Cechia), and worked for the Skoda factory in Pilsen designing locomotives. He died during the last days of the war, so after our expatriation to the western part of Germany, my mother was left alone to take care of her two children, namely my two-year older sister and myself.  At the age of 19, I started to study physics at the University of Frankfurt and changed to the Technical University of Darmstadt three years later. I finished my diploma thesis there at the age of 26 and my PhD thesis at the age of 29. In both theses, I applied optical spectroscopy to determine crystal field split energy levels of rare earth ions in garnets. The director of the institute was Professor K. H. Hellwege. As I did a lot of computational work for the experiments, he used to say to me when he met me in the corridors: “Above all mathematics, don’t neglect the physical background” which he called “Physikalischer Hosenboden” (lit. “physical bottom”). This must have made a lasting impression on me because I have a strong desire to explain new phenomena which I come across in simple physical pictures and do not feel comfortable with mathematical formalism alone. Today, I would like to pass this attitude on. In computer simulations in particular we would get lost in an abundance of results if we did not maintain a critical view for the source of the effects. This is especially true for phenomena caused by a constructive or destructive interaction of various mechanisms. My direct supervisor for my thesis work was Prof. Stefan Hüfner.  The topics of garnets and crystal fields also brought me to the laboratory of Prof. A. Koningstein in Ottawa, Canada, where I worked as a postdoc for a little less than three years. As was the case until then my goal remained the determination of crystal fields, but here I used the electronic Raman effect to determine the energy levels experimentally. Since Raman scattering is mainly caused by the optical phonons, these were also included in my investigations. A photo of me at the age of 33 is shown in Figure 1.  In 1972, I was offered a position as a research scientist at the newly founded Institute for Magnetism at the research centre in Jülich. The director was Prof. Werner Zinn. His picture (marked with WZ) is included in the group photo of Figure 2. He was mainly interested in investigating the model magnetic semiconductors EuO and EuS. I also started to work on these compounds, applying optical absorption spectroscopy as well as Raman scattering (RS) from phonons. The RS experiments were performed at the Max Planck Institute for Solid State Physics in Stuttgart in the group headed by M. Cardona. My partner in Stuttgart was Gernot Güntherodt (marked GG in Figure 2) with whom I explored the magnon phonon interaction in EuO and EuS.  RS often causes a problem if we want to observe scattering with small frequency shifts below about 30 GHz (corresponding to 1 cm-1). This is the domain of Brillouin lightscattering spectroscopy (BLS), where instead of a grating monochromator as in RS, a Fabry Perot interferometer is used as the dispersive element. In the early 1970s in BLS instrumentation, an interesting development occurred due to the pioneering work of John Sandercock in Zürich. He invented the multipass operation, and later on, showed how two multipass interferometers could be used in tandem. Following this example we assembled our own setup, namely a triple-pass spectrometer without tandem operation. A schematic is shown in Figure 1 of [1]. John not only pioneered the experimental technique, he also demonstrated the improvement with the first BLS measurements of various phenomena in solid state physics. This work had a great impact on ours. At the same time, he established his own company. Researcher, inventor, entrepreneur, salesman – all at the same time: following in the footsteps of Alfred Nobel! I have the greatest respect for such people. He has since been awarded various prices, of which the David Richardson Medal of the Optical Society of America (2005) is the latest.  One of his experiments grabbed our attention in particular. It was the first measurement of acoustic spinwaves in Yttrium Iron Garnet (YIG) and there fore the first time that acoustic spinwaves were measured in ferro- or ferrimagnets by means of LS. This result encouraged us to try the same experiment with EuO. However, as this compound only orders ferromagnetically below 60 K, cooling was necessary. Fortunately, a cryostat with a superconducting magnet was available from the optical experiments, so it was brought to the BLS setup. Before long, we were able to detect BLS from the bulk spinwaves. Additional scattering with a very strong Stokes/anti-Stokes (S/aS) anomaly could finally be identified. This was due to a spinwave propagating along the surface in one direction only and not the opposite. The successful observation of spinwaves in the bulk and at the surface of EuO is described in [1]. It has the character of an anecdote that the clue for the interpretation of a certain peak, caused by a surface spinwave, was uncovered when the experimental setup broke down and had to be repaired.  Further important results in this context were obtained by John Sandercock together with Wolfram Wettling. They measured the bulk and surface spinwaves from ferromagnetic metals in the shape of platelets with thicknesses of the order of mm. Standing spinwaves in thin films with thick nesses of the order of 100 nm or less were seen by Marcus Grimsditch and Alex Malozemoff (AM in Figure 2) for the first time. References to all of this work can be found in reference 2 of [1]. From this period, I also would like to mention fruitful contacts with Burkhard Hillebrands (BHi in Figure 2), who later extended BLS to thin film structures with lateral confinement.  Often in experiments with light, strong metallic optical absorption is considered a disadvantage because it significantly reduces the interaction volume. In LS experiments on standing spinwaves, however, it turns out to be of advantage because the wavevector of the incident light is smeared out due to strong optical absorption. As a result, a whole band of wavevectors is available to fulfill total momentum conservation, which is one of the required experimental conditions in BLS. A BLS spectrum from spinwaves showing the surface – and a standing mode is displayed in Figure 2 of [1]. To emphasise the last point: the standing modes are only observed because the wavevector of the incident light is sufficiently smeared out due to strong metallic optical absorption. In contrast, unpinned standing modes with antinodes at the surfaces are not seen by microwave absorption because the total precessing moment cancels out. This aspect was later discussed by John Cochran and Bret Heinrich. Both are seen in Figure 2 on the upper right-hand side, marked JC and BH, respectively. Next, we extended the LS investigations of spinwaves to multilayered structures. We concentrated on “magnetic double layers” i.e. two ferromagnetic films separated by a non ferromagnetic interlayer. One main interest was the coupling of DE modes to the collective dipolar modes of the structure. The most important results of these investigations are summarized in Figure 3 of [1].  At this point, I wish to mention that my meanwhile permanent staff position in a government-funded research laboratory allowed me to make long-term planning and to build up equipment for sophisticated sample preparation. At that time, such components were only partly available on the market. In this endeavour, I was assisted by my technician Reinert Schreiber who designed, assembled and operated the machine to prepare the samples. A particular feature which he installed was the “wedge technique”. It provides the possibility of making thin-film samples with increasing thickness from one end to the other. Later, this turned out to be very helpful for the study of thickness dependencies. A picture of Reinert performing some leak testing is shown in Figure 3.  I did the first calculations on the dipolar-coupled DE modes, as displayed in Figure 3 of [1] myself. The extension to multilayers was done together with my colleague K. Mika from the mathematical department of our institute. Knowledge of the effect of dipolar coupling on the spinwave frequencies and the experimental result on standing modes was sufficient to be able to qualitatively predict their behaviour under the effect of ferro- or antiferromagnetic exchange coupling. This is explained in [1] in the context of Figure 3 and Figure 4.  Let us return to the “trilayer” or “magnetic double layer” structure as displayed in Figure 3 of [1]. It explains how F and AF coupling can be determined qualitatively from the frequency shift of the optic mode. What we observed at that time were only frequency upshifts. According to Figure 4 of [1], the coupling then is ferromagnetic.  In the early 1980s, it was believed that there were mainly two mechanisms that explained coupling. These were pinhole coupling via magnetic bridges across holes in the interlayers and “orange peel” or [Néel](https://www.nobelprize.org/nobel_prizes/physics/laureates/1970/)-type coupling due to meandering interlayers. Remagnetization curves which can reveal antiferromagnetic-type coupling yield no information when the coupling is ferromagnetic. For these reasons, the situation concerning coupling was still rather uncertain in the early ’80s. Under these conditions, we started a systematic search using the spinwave method. We also concentrated on the more unique case of antiferromagnetic (AF) type coupling, trying to find an example.  While on a year’s sabbatical leave at Argonne National Laboratory in the U.S. as a guest of Mervin Brodsky, this search was finally successful. We detected AF coupling in Fe/Cr/Fe structures on cleaved substrates of rocksalt. Details are found in [1]. Two of the colleagues I met at Argonne, with whom I worked on other topics namely Ivan Schuller (IS) and Sam Bader (SB), can also be seen in Figure 2. When I came back from the U.S., I continued working on the reproducibility of the effect, now using also epipolished GaAs substrates. At that time, Jozef Barnas, a theoretician from Poznan in Poland, joined my group and we started to work on a quantitative theory of the effect of coupling on spinwaves. A photo of Jozef is shown in Figure 4, and some results of our joint efforts have been published in ref. 2 of [1].  Furthermore, at that time, anisotropic magnetoresistance (AMR) was widely discussed by the community for applications in sensors for hard disk drives. So “something was already in the air” regarding MR effects and we decided to complement the available experimental techniques with magnetoresistance. Jean Pierre Renard (JR in Figure 2) had found an effect which later on turned out to be related to GMR. In our laboratory, Gaby Binasch installed the equipment during her diploma work and did the first measurements on permalloy films. Since Reinert Schreiber was now also able to make AF coupled Fe/Cr/Fe structures in a form suitable for measuring of electrical resistivity, GMR was seen before long as discussed in [1] and published in ref. 8 thereof. Gaby finished her thesis and received her diploma in physics (equivalent to masters). After this, she left Jülich for an interesting job in industry. She can be seen in Figure 5 at the celebration for her diploma.  Before I come back to GMR, let me add a few more details on coupling. As mentioned in [1], at the same time that we were working on Fe/Cr/Fe, other groups found similar phenomena for rare earth layers (Gd, Y) separated by Y layers. An RKKY-type mechanism was proposed by Yako Yafet (marked YY in Figure 2) as an explanation. Even oscillatory behaviour as a function of the interlayer (Y) thickness had already been reported. However, it seems that only after the discovery of GMR, coupling per se also received general attention. And sure there were many results and surprises. The RKKY-type mechanism was applied to structures with magnetic 3d-transition metals by George Mathon (GM in Figure 2), David Edwards and Patrick Bruno (PB in Figure 2). With the permission of the colleagues involved, I may at this point tell the following anecdote. In the course of his search for AF couplings Stuart Parkin from IBM San Jose (SP in Figure 2) had found it also for sputtered Co films with [111] texture interspaced by Cu. The Dutch group from Philips Eindhoven established oscillatory coupling for this system also for epitaxial growth with [100] orientation. Bill Egelhoff from NIST in Gaithersburg, well-known for very fine work in epitaxy, however, reproached vigorously. I still remember a MRS meeting in San Francisco where he defended his point. Then Professor Gradmann (UG in Figure 2) from Clausthal University in Germany stood up and said: “Bill I think I know what happened. Your samples are too good. You have very fine surfaces which nucleate antiphase domains that give large angle grain boundaries between the islands. Then you can get diffusion of Co along the boundaries and the formation of magnetic bridges. The resulting F-coupling swamps any possible AF coupling.” Bill Egelhoff responded simply: “I like that”. Indeed, somewhat later, Jürgen Kirschner (JK in Figure 2) and his group from Freie Universität in Berlin added further evidence to this case by showing that grain boundary formation comes from the usual competition between ABABA.. and ABCABC… type stacking for hcp type structures along [111].  Coming back to Bill Egelhoff, it has also to be mentioned that he used oxygen as a surfactant to set the record values for GMR of 25% in the simple trilayer structures of Co and Cu and also dual spinvalves with two Cu interlayers. In multilayered structures, of course, the effect is much stronger.  This example demonstrates again that coupling can change drastically with growth, which makes comparison with theory ambiguous. Then how do we know that current theories on this phenomenon as proposed for the first time by Yako Yafet are in essence correct? An important contribution to this comes from Bob Celotta’s group at NIST. They grew trilayer-type samples, where one magnetic layer was replaced by a single crystal Fe whisker, which at the same time is used as a substrate. For the interlayer, various materials were chosen with an emphasis on good matching to Fe. Coupling is seen via magnetic domains, which are made visible using SEMPA. These indeed superb and celebrated experiments left no doubt that theories based on the RKKY interaction predict oscillatory coupling correctly. In real cases, however, the coupling could still be good for many surprises, as is clear from the anecdote above.  Another phenomenon which also finally turned out to be related to growth is that of 90° type coupling. This was proposed by A. Hubert and his group as the reason for special magnetic domain structures occurring in samples with wedge-shaped interlayers. Alex Hubert (AH) and his coworker Rudi Schäfer (RS) can also be seen in Figure 2.  For the size of the GMR effect, there is a large difference between trilayers and multilayers, as shown in Figure 9 of [1]. This has been known since GMR was announced publically for the first time at the ICMFS in Le Creusot in France, organized by Irena Puchalska (IR) and Horst Hoffmann (HH). I also met Albert Fert there for the first time (as he attended the conference but was not in the original group photo, I added his photo (AF) on the right-hand side of Figure 2). After we had compared our results and came to the conclusion that we had seen the same effect and thus confirmed it to each other, we were ready for a glass of red wine from Burgundy. We were not the only ones who enjoyed that conference. A group of talented musicians among the participants entertained us with piano concertos (Alex Malozemoff-AM and Jaques Miltat-JM), Urich Gradmann (UG) and Mrs. Yafet on the violin and finally Klaus Rohrmann (KR) with a solo on a water hose. The arrival of GMR was adequately celebrated indeed!  When I returned home from the conference in Le Creusot, I was in a very fortunate situation that I had two excellent theoreticians as visiting scientists in Jülich. A picture of Jozef Barnas has already been shown in Figure 4, a passport photo of Bob Camley is shown in Figure 6. I now had both previous collaborators on spinwaves together again in Jülich, but now the new exciting topic was the GMR effect. Before long, they had worked out a theory based on Boltzmann’s diffusion equation for the evaluation of the GMR experiments which became known as the Camley-Barnas model.  The title of my Nobel lecture includes the term “beyond”. The team with whom I obtained most of the results “beyond” can be seen in Figure 7. We conducted much work on Si interlayers, where we found very strong coupling but disappointingly weak tunnel magnetoresistance. We also started on “current-induced magnetic switching” (CIMS) as invented by John Slonczewski and Luc Berger. John and his wife Ester stayed with me various times in Jülich. My successor as group leader, Daniel Bürgler, can be seen in the middle of Figure 7 (3rd from the left).  In 1998, I was invited by Hiroyasu Fujimori (HF in Figure 2 and 3rd from left in Figure 8) to come to Sendai, Japan, for 6 months. During this stay, I also spent two months in Tsukuba as a guest of Yoshishige Suzuki. A group photo with my collaborators in Sendai is shown in Figure 8. Of these, Koki Takanashi in 1995 had spent a year in Jülich as a postdoc. Terunobu Miyazaki is one of the inventors of tunnel magnetoresistance (TMR) at room temperature and with Akira Yoshihara I worked on BLS from spinwaves.  I officially retired in 2004, but kept a desk and some office space in the research centre within the Institute for Electronic Properties headed by Claus Schneider (CS in Figure 2). In this way, I was able to maintain my contacts with the scientific community, of which I wish to mention only as example a visit to Vladimir Ustinov (VU in Figure 2) in Ekaterinenburg. Now – as a result of being awarded this great Prize, I have also received a new contract from my employer in the form of a “Helmholtz professorship”. As a result, I call myself jokingly a “re-entrant magnetician”. I should explain to the non-expert that re-entrant magnetism is where magnetism disappears as a function of some parameter (like pressure or temperature) at a certain value but comes back at another value. Obviously, the relevant parameter in my case is age!  I would like to add a remark on my religious believes. Brought up rather conservative catholique I see religions now more or less in the spirit of Lessing’s (German dramatist 1729-1781) ring parabola which I would top by saying that – not only does nobody know which is the right ring (standing for religion) – but there indeed is no such thing as a right or false ring. Per se they are all equivalent. What really counts is how religions are practised, for example, with tolerance. And yet I believe that there is more than what we see, hear etc., or can detect with instruments. But it is a feeling borne out of many details of my personal experience and therefore impossible to share or communicate. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0048 = KS**  [Kosta Schinarakis] – Kosta Schinarakis, hallo.  [Adam Smith] – Hello, may I speak to Peter Grünberg please?  [KS] – It’s very complicated at the moment, he’s drinking champagne with his colleagues.  [AS] – I can imagine.  [KS] – Who are you?  [AS] – My name is Adam Smith and I’m calling from the Nobel Foundation’s website, Nobelprize.org.  [KS] – So, one moment. Just, I see Mister Grünberg, he’s … one moment.  [AS] – Yes.  [Peter Grünberg] – Hello, here’s Peter Grünberg.  [AS] – Oh, hello, Professor Grünberg, this is Adam Smith from the Nobel Foundation’s website, Nobelprize.org. We have a tradition of recording very brief telephone interviews with new Laureates for the archives …  [PG] – Yes, OK.  [AS] – Thank you very much. Well first of all, of course, many, many congratulations on the Prize.  [PG] – Thank you.  [AS] – Where were you when you heard the news?  [PG] – I was, in fact, in my office already. Of course, and then there were many people coming and congratulate me.  [AS] – I gather I just interrupted you drinking champagne, so I apologize.  [PG] – Yes, right, I still have the glass of champagne, not quite emptied, before me.  [AS] – Oh, splendid, OK. So, the award is for your discovery, together with Albert Fert, of giant magnetoresistance …  [PG] – Right.  [AS] – … how did you become aware that Albert Fert had made the same discovery that you had made?  [PG] – Oh that was on a conference in France, in 1988, we had the ICM, which is the main magnetism conference, in France, in Paris, and after that we had the ICMFS, in Le Creusot, and then both of us gave a talk and after the talk we stated “Yes, we obviously found the same kind of an effect.” We found it in double-layer structure, with two magnetic films, and Albert had a multi-layer structure, and therefore it was stronger in his case, but we realized that it is the same kind of physics which leads to this effect in the two different systems. And so one can really locate it to the conference in Le Creusot in 1988, when we heard from each other for the first time.  [AS] – And it must have been amazing to find that the two of you had the same results. What was your reaction to this?  [PG] – Well in a way, I think, the history before was in a way similar. Because two years before that time I had discovered the antiferromagnetic interlayer exchange coupling and I reported on that in various conferences and Albert Fert heard one of these talks and decided that, for the kind of physics he is doing, namely magnetoresistance effects in diluted alloys, that would be an interesting configuration to investigate this effect also in the layered structures with the antiferromagnetic exchange coupling. So via this kind of coupling we came into the same kind of area in physics.  [AS] – Right. So your research paths were linked before the actual discovery.  [PG] – Yes, so it was in fact linked before really, only that I didn’t know that he was looking also now into this coupling phenomena.  [AS] – And were you looking for the effect that you discovered, or was it a surprise discovery, when you were looking for something else?  [PG] – Well we did the experiment because we expected something. I expected that electrons which penetrate into a layer with the opposite spin direction as to the magnetization would be differently scattered at the interface than an electron which is parallel to the magnetization. And based on this expectation then we expected that there should be a difference for the resistance between the parallel and the anti-parallel magnetization alignment in such double-layered structures.  [AS] – Right.  [PG] – So that’s why we started to work on this and made proper samples and so on.  [AS] – Yes, for those who find the quantum mechanical properties we’re describing very complicated could one make the analogy to say that this is a little bit like polaroid filters for light? That if you cross the polaroids you don’t get light passing through.  [PG] – Yes, yes, very much so, yes. This describes it very well.  [AS] – And now the applications of giant magnetoresistance have been many and various. Which ones excite you the greatest?  [PG] – At the moment, well, in fact we were wondering and discussing the application in hard disk drives so much and I think economically this really was a breakthrough for the hard disk drives. But since we are discussing that now for many years, I’m not so excited any more about this. But I found other applications where the giant magnetoresistance comes nicely very in and that is for the detection of genetic material, which you can separate by magnetic separation. You can attach antigenes to antibodies, you can attach the antibodies to so-called magnetic beads and via magnetoresistive sensors you can then detect genetic material. And this is a topic which is very broad and if it works it has many, many applications. It works together with magnetic nanoparticles which are superparamagnetic. And so that’s an application which is also discussed now for some time and famous labs also zero-in on this topic and treat that.  [AS] – That’s fascinating, and so I imagine you had no idea when you were working on this in the late eighties that your discoveries in physics would extend to biology?  [PG] – No, no. In fact it’s a funny situation in this because we are using magnetic nanoparticles in that, magnetic nanoparticles which are superparamagnetic. And we are used always from the hard disk drive so that superparamagnetism is, so to say, the enemy of application, and one tries to avoid superparamagnetism. And here, in this area, in these small magnetic particles it’s welcome, we want superparamagnetism. So this is another aspect which leads over into other areas in magnetism and the magnetism of very fine particles, nanoparticles and so on, and superparamagnetism.  [AS] – Right, very exciting. And your plans for the rest of the day then? How do you intend to continue your celebrations of the news?  [PG] – Well certainly, what I know, is that at 2 o’clock we will have a press conference here, and we will talk much more about all these effects and why it is important. I think it is important in the context really of computers. Computers have really changed our lives in the last few decades. I really enjoy very much to browse in the internet and get so many informations. Daily one can really experience the advantages of having computers and for the computer it was also a very important contribution, for the storage capacity of computers. So in this context I found this also a very important contribution.  [AS] – Yes.  [PG] – But not only this, but, as I just said about these genetic investigations, I think there it also plays an important role.  [AS] – Yes, the Prize very clearly links the discovery to the greatest benefit of mankind. It’s a very nice, and understandable, example.  [PG] – Yes.  [AS] – So, thank you very much for sparing the time to talk to us. When you come to Stockholm in December to receive the award, we will speak further I hope.  [PG] – Good, I’m looking forward.  [AS] – Thank you very much indeed.  [PG] – OK, thank you too.  [AS] – And congratulations, bye, bye.  [PG] – Thank you, bye, bye. |
| **Interview** |  |
| Q5 | ***Who, or what, inspired you to enter your field of achievement?*** |
|  | Myself, and my desire to do something significant. To some extent it was also against the plan of my supervisor, but he was tolerant enough to finally accept my activities. |
| Q9 | ***In one word, can you describe your reaction when you knew you had been awarded the Nobel Prize?*** |
|  | Well I knew that I had been traded as possible candidate years before. So the reaction was more like: “Ah, finally”. |
| Q2 | ***Has there ever been a time in your life and/or work when you have doubted what you were doing to the point that you seriously considered abandoning said work?*** |
|  | My research topics gradually changed all the time but I tried always to build upon knowledge that I had gained before. So for me it was very important to have continuity. I wanted to do good work which could be published in well-reputed journals. When I started my research I didn’t expect that finally there would be the Giant Magnetoresistance effect. It was only when we had found antiferromagnetic type coupling two years before the discovery of GMR that we thought this could be possible and installed the necessary equipment. |
| Q9 | ***First of all, congratulations! What will you do with the prize money? You have done something extraordinary to win the Nobel Prize – perhaps you deserve to spend it all on yourself!*** |
|  | Having been honoured so high on an international podium I see this Prize money as an obligation to be internationally available. The requests and demands are manifold. Believe me, it is not a comfortable life and often I have expenses which I pay for from my own pocket. So I see being a Laureate as a job which is paid adequately from the prize money. |
| Q20 | ***At any given time you obviously have several questions in your mind that you want to find answers for in your research. How do you choose which ones to pursue first and spend most of your efforts on?*** |
|  | Yes, indeed I have had other ideas that could have turned out to be of importance or even a breakthrough. Whenever I have such an idea I make a corresponding note on the last pages of my notebook. But my nature is to concentrate only on one thing at a time.  Of course, when you start to have doubts that your project will be successful you play with other ideas also, and then I consult my notebook. Before giving up one should carefully test out all possibilities, but also not fall into stubbornness. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0049** |
| **Biographical** | I was born on Aug. 7, 1946 in Roanoke, Virginia, a small city near Blacksburg where my father was a young faculty member at the school now called Virginia Tech. For some generations, my family on both sides has been populated with scientists and school teachers. My father, Robert E. Mather, was starting his research career in animal husbandry and statistics, specializing in dairy cattle breeding and feeding, having just received his PhD from the University of Wisconsin. My mother, Martha Cromwell Mather, was teaching high school French. When I was about a year and a half old, my parents moved to the Rutgers Agricultural Experiment Station, also known as the Dairy Research Station, in Sussex County, New Jersey. This is a very peaceful farming area, in the rolling foothills of the Appalachians, and our house was just a mile from Appalachian Trail. James Turner, a stockbroker from Montclair, New Jersey, and originally Scottish from Glasgow, founded the experiment station in 1931 I believe. So my childhood from age 1 to 15 was spent in that house, at the top of a long hill, looking over a valley filled with fields and farms and forests. My earliest memory is of a medical procedure in the hospital in Sussex – I had my tonsils out when I was about 2.5 years old, and they gave me ice cream. Our home looked out at a barn where 20 bulls were kept, and they were the sires of half the cows of northern New Jersey. Within a few yards of the house was an office building where the analysis of the experiments was done. Down the hill past the barn was a laboratory with calorimeters, chemical analysis equipment, liquid nitrogen tanks for keeping semen frozen until it was needed, some radioisotope equipment for studying metabolism, and so forth. As I understand it, my dad was responsible for developing a test that measured the protein content of milk, and thus indirectly for the re-optimization of the dairy industry to produce more protein and less butter. Later on, he became one of the early users of computers, and had the milk production records of 10,000 cows on punched IBM cards.  I attended Wantage Consolidated Elementary School near Sussex, N.J., which had about 600 students, and was established in the 1930’s so that the one-room schoolhouses in the area could be closed. So I rode a big yellow school bus to school, along with many other students, most of them children of independent farmers. Their farm labors made them very strong, and our school athletes excelled at baseball and wrestling, but I was not among the athletes. I was much more interested in reading everything that came my way, hiding a book behind the desk while the other students learned from class. My interest in science started quite early. My earliest school recollection, from age 6, is actually of mathematics, realizing that one could fill an entire page with digits and never come to the largest possible number, so I saw what was meant by infinity. I recall that my parents took my sister and me a few times to the American Museum of Natural History in New York, after a journey of at least two hours, which to me seemed forever. The first time, Mars was very close to the Earth, and there was great excitement about whether the canals could be seen. My father bought a small telescope from Sears Roebuck, but of course it could not show the canals, and Mars was extremely small even with the telescope. He also bought a book *Astronomy Made Simple*, which got me started. My parents also enjoyed reading aloud from various books, including biographies of Darwin and Galileo. I was fascinated with the museum displays of the sequence of different kinds of skeletons of fishes, showing their changes through time, so evolution was quite the obvious conclusion even to a child. I liked thinking about geology and hunting for fossils in the roadside streams, as I built little dams of mud and pebbles. I didn’t really appreciate what science was about yet, but it sounded very exciting, and a little dangerous in case one discovered things that were not consistent with previous knowledge, especially religious thought. In later years, I occasionally dreamed of being in court, defending the right to teach evolution in the schools. My mother’s father, Hobart Cromwell, was a bacteriologist with Abbott Laboratories in suburban Chicago. I never got to know him well, as he died very young, but he was always a heroic figure in our family, wise and gentle and intelligent by reputation, with the courage to fight against the McCarthyites.  By the time I was in fourth grade (age = grade + 5 years) I was already pretty sure I liked scientific and engineering things, including electronics. For Christmas I got a one-tube radio kit, and then I saved my allowance for a 5-tube shortwave Heathkit radio that I put together so I could listen to exotic languages and broadcasts from far-away places. Around that time, the IGY (International Geophysical Year) was starting up, and at our spot in New Jersey it was marked by a fabulous bright aurora, the only one I’ve every seen. I think 4th grade also marked my first entry into the school Science Fair, and I submitted 4 little projects. Hardly anyone else at the school was much interested in science at the time, but I had one friend who walked several miles to come visit us at our house and talk about these things. We did have a Bookmobile, a traveling library from the County that visited the farms every couple of weeks, and I borrowed as much as I could. I started reading about optics, and I saved my allowance and ordered some lenses from Edmund Scientific and assembled small refractor telescopes. One summer my parents sent me off to a summer camp in the Poconos, a place that stood out because it had a science program. Another summer, they sent me to a day camp with the high school science teacher in Newton, New Jersey, Ben Cummings. With him, we climbed a hillside near High Point State Park and came back with bags of trilobites. And one year, I wanted to do a science fair project with a “robot” that I designed with vacuum tubes and remote controls. It never worked but I got a lot of experience, and now looking back on it I recognize that my parents contributed a substantial research grant when I was only about 11. So I had a lot of opportunity to learn science, even in our very rural setting.  When I finished 8th grade, it was time to go to high school, and my parents decided to send me to Newton High School, where they thought we would get the best available education in our area. That turned out well for me, and I had some excellent teachers in science, math, and English that I really liked. I took biology in 9th grade, chemistry in 10th grade, and physics in 11th grade. I was very fortunate to have the opportunity to go away to summer schools. After 10th grade I went to Assumption College for about 10 weeks to learn about the foundations of mathematics, and after 11th grade I went to Cornell University for a summer physics program. That was truly extraordinary for me, with an introduction to quantum mechanics, special relativity, optics, nuclear physics, and cosmology. Coming back from these programs, having done fairly well, I was convinced that I could have a future in science, and I was very glad to have a head start relative to others of my age. The National Science Foundation sponsored these summer programs, and they certainly did a wonderful thing for us. I competed in the nationwide math contest and placed 7th in New Jersey, I think, and in a statewide physics contest I placed first. With all this success I was feeling pretty good, but my parents reminded me frequently that I would still have to work hard in college, since I had been a big fish in a little pond, and I didn’t know what was yet to come in the big world.  I never got very excited about dairy cattle, but my father did help me learn two important things, statistics and calculus. For one of my science fair projects, I had 8 baby rats that I kept in cages under the table in the kitchen, and I fed them various diets to see what they needed. My mother must have had immense patience with me to allow me to have my experiment there. My dad showed me how to design a Greco-Latin square for the experiment, and how to do the analysis of variance. The answer: dog food and vitamins are good, and corn flakes alone are inadequate. One summer, he returned to college to learn calculus himself, and when he got home I borrowed his textbook and studied it instead of taking an advanced science course in high school. This was another way of getting a little ahead of my cohort, and when I got to college it was a good thing.  I chose Swarthmore for college, largely because the atmosphere felt good and the faculty promised a complete education in physics. I tried without much success to learn a little of the humanities and the arts, but even passing the courses in art history and music history was a challenge. In those courses I understood what other folks felt when they saw me doing so well in physics; I knew it was hopeless to compete on that territory, but I persevered and even took up piano lessons again, with enjoyment but not skill or talent. I jumped a little ahead again, skipping the second half of freshman physics and diving right into sophomore physics. I got a lot of special attention from the faculty there and really appreciated it. I was in the honors program, with four seminars in math and four in physics and two in astronomy. When it was time to graduate, David Wilkinson, a young professor at Princeton, was one of my honors examiners. He asked me a question about everyday effects of relativity, and I said that magnetism was a relativistic effect of electron motion, or something like that. Years later, David was a founding member of the COBE team.  For graduate school I chose Princeton, and was making plans to go there, when a friend Ted Chang, who was my friend at Assumption College for a high school summer, sent me a photo of himself sitting on the fountain in Berkeley in January, wearing short sleeves. He sent me an application form for a summer job, and I went. As it happened, my job was at the Lawrence Berkeley Laboratory, working with Henry Frisch on control electronics for a spark chamber. Henry’s father was a physicist too, and taught my co-Nobelist George Smoot. I liked Berkeley, and changed my mind about Princeton. Being a little churlish, I wrote to Princeton and told them that I was withdrawing because they had no women students. I was fortunate that my fellowship from the National Science Foundation was portable, so switching was easy.  At Berkeley, I found a big old brown shingle house to share, and my rent was very low. The household was organized by John Hauptman, another physics student, and held about 8 other people. Roy Torbert, now a space physicist at UNH , was a member of our little group. One household member, Richard Rotblatt, was a former architecture student and nuclear reactor piping engineer, and he was also an excellent chef. Now he’s an accomplished wine maker as well. For a while my best friend from high school lived with us after returning from Vietnam. This old house could tell many stories of the times, with people of all sorts moving in and out.  At first, I thought I wanted to be an elementary particle physicist like my hero [Richard Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html). I took my courses for two years, during which my faith in my future was being challenged by the Vietnam War and by the Peoples’ Park demonstrations that rocked the city. Governor Ronald Reagan’s helicopters tear-gassed the campus, and people were seriously injured, but I tried to stay out of this trouble and had little sympathy with either side. Because I was very nearsighted, I was not drafted to be a soldier, so I didn’t have to really deal with the great issue of principle that involved so many of my classmates. For a while I considered studying the law, in order to defend the country from the Government of the day, but when I read the course catalog I couldn’t imagine studying those subjects. Now as a long-time Government employee my perspective has changed a bit. I also talked with my plasma physics professor about developing nuclear fusion power for the good of humanity, but he seemed to think this would be an extremely long and difficult project, as it has turned out to be.  So in 1970 I was looking for a thesis project, and interviewed with various faculty members. I found that Paul Richards was working with [Charles Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html) and a young postdoc Michael Werner to start up projects on the newly discovered Cosmic Microwave Background Radiation. I liked all three of them immediately, as well as the proposed experiment, and I started right in. It was a new world for me, much more tangible than years of books and classes and late nights in the library. The first project was to build a small far infrared spectrometer to take to the Barcroft station on White Mountain in eastern California, where the University was studying physiology at high altitudes. This project worked out well, but was of course limited in accuracy by the interference of the Earth’s atmosphere. We were able to set some interesting limits on the CMBR intensity, and we got a few publications from them. Then, Paul went off to England on a sabbatical and came back with a concept for a new experiment, a balloon-borne far infrared interferometer to measure the CMBR spectrum. He explained it to the graduate students, and David Woody and I started trying to work out and build the design. This was the beginning of a baptism by fire, in the art of building instruments that would work in remote and hostile locations. It was a time to learn something of almost every area of engineering, from mechanical to optical to cryogenics to electronics. I’m afraid that my skill was stronger in understanding than it was in implementation, and it’s a true story that the antenna on the balloon payload fell off while it was on the launch pad. It was my solder joint that failed. Fortunately this fault was noticed, and the payload was launched successfully.  However, it also was true that we had gotten tired of testing, and our instrument did not work, for three different reasons. It was an awful feeling, one that stayed with me for the rest of my life, and it was one of those ways of learning what one does not want to learn. Murphy’s law had been proven one more time. Also, I wanted to finish my thesis, and had already lined up a job in New York as a postdoc with Pat Thaddeus. So Paul agreed, and my thesis described the ground-based work and the design for the balloon instrument. David Woody designed a test chamber for the payload, a cubical box of Styrofoam and plywood, and filled it with dry ice. He found out why the instrument had failed, fixed it, and made it work right for the second flight, the summer after I left Berkeley for New York. He analyzed and published the data and got his thesis out of the project too. Now he’s a radio astronomer at CalTech, designing antennas for the new ALMA observatory in Chile.  With Pat Thaddeus at the Goddard Institute for Space Studies, a part of NASA housed in a building adjacent to Columbia University, I was hoping to go into a new field of study. I thought that my work on the background radiation was awfully difficult, and it was going to be hard to do much better with balloons. I suppose I was reacting too much to the failure of the instrument. At any rate, I arrived in New York at the end of January 1974, only days before the last possible allowed date, and I started theoretical and observational work on naturally occurring SiO masers. I learned how to build a microwave receiver with brilliant machinists and technicians, and I took it off to McDonald Observatory in Texas and to the Navy’s Maryland Point observatory on the Potomac. We did observe the SiO emission at 43 GHz, which had never been seen before in space, and I made a little progress writing a giant Fortran program on the IBM 360 computer, but it never came to anything, and years later I threw many boxes of IBM cards into the trash, finally admitting defeat.  However, in the summer after I arrived, my trajectory took another abrupt turn when NASA issued Announcements of Opportunity 6 and 7, for Scout and Delta-launched satellite missions. My optimism was returning, and when Pat asked for ideas, I cheerfully asserted that my thesis experiment would have worked a lot better in space. He suggested that I call up Rainer Weiss, David Wilkinson, and Michael Hauser, and with their colleagues Dirk Muehlner and Bob Silverberg together we conceived of the new mission. It would have four instruments, a far IR interferometer to measure the CMBR spectrum, two instruments to measure its anisotropy (difference in brightness in different directions), and an instrument to hunt for the diffuse IR background from the first galaxies. Then, the balloon payload flew successfully after David Woody fixed it, and things were looking up. We sent in our proposal, typed by hand on real typewriters, and I at any rate thought that our odds of success were very low. None of us had any prior experience with space missions, and none of us knew that there would be about 150 other proposals, or that two of those (from JPL and Berkeley) would be direct competitors for ours. However, NASA was interested after all. There was already a negotiation with the Netherlands and the UK to build the Infrared Astronomical Satellite (IRAS). Ball Aerospace built the US part of the IRAS. So, the first expression of interest in our idea was to see whether the spectrometer could be miniaturized and given a ride to space as some part of the IRAS. I got a little money to study it, and I presented my concept to the IRAS science team at a meeting near Amsterdam. It went over with a resounding thud, for good reasons. I am very glad I never had to build this version of the instrument I had conceived, but I did learn a lot about what could be done, and I learned about the IRAS mission, which was to have a large helium cryostat much like the one we needed for the COBE.  So in the fall of 1976, NASA decided formally to study our concept, but not just with our team. Nancy Boggess, the Program Scientist at NASA Headquarters for infrared astronomy, appointed four members of our original team (Hauser, Weiss, Wilkinson, and me), along with George Smoot of UC Berkeley and Sam Gulkis of JPL, to form a Mission Definition Science Team. Anticipating this event, Mike Hauser had offered me a job at Goddard Space Flight Center in Greenbelt, Maryland, so I was already in place as a proper civil servant. We were assigned a manager, Martin Donohoe, and we were to compete with about 11 other missions that were also viewed as promising. Our little team elected a Chairman (Rainer Weiss), and three Principal Investigators (me, Mike Hauser, and George Smoot), and NASA assigned me the job of Study Scientist, to work with the engineering team to make this project happen. So this was the beginning of the COBE satellite project. Mike Hauser, who had hired me, was my main mentor, and I have learned to seek his advice whenever times are tough. Among all my colleagues, Mike is my greatest hero and example to follow.  We submitted our report, and the results were favorable, so NASA assigned us a larger team of seasoned engineers, namely the team that had almost finished the IUE (International Ultraviolet Explorer), led by Jerry Longangecker. This was a time when the Space Shuttle was being considered by Congress, and NASA made a deal that would set its future for a long time: all new launches would be made with the Shuttle, and all the expendable rockets like the Deltas would be canceled. We argued but we had no success, and we had to redesign the COBE to go on the Space Shuttle. This wasn’t so easy, since the COBE needed a polar orbit, achieved by a launch from California, at around 900 km altitude. Most of the Shuttles would be launched from Cape Canaveral (then called Cape Kennedy), so our requirement was a challenge in any case. By around 1979, NASA decided to build the COBE satellite in-house at Goddard, meaning that engineers and scientists at Goddard would work together very closely. This is an exception to the usual way that NASA obtains satellites, which is by writing contracts to major aerospace organizations and university laboratories. In our case, two of the three Principal Investigators were already at Goddard, and the third (George Smoot at Berkeley) was willing to have Goddard build that instrument too, so this new arrangement was very good for us. We had daily interactions with our engineering colleagues, we could walk into any laboratory to talk about any problem, and we made significant forward progress, and I really enjoyed that part. On the other hand, part of the deal was that our project was a training project for new engineers, and a reservoir for talent. When other projects got into trouble, our team was raided for top talent to go solve emergencies elsewhere, and of course there were many of those. I was very frustrated about this, but I had to admit, the Hubble Space Telescope really did have priority.  In 1980, I made a major decision, to marry Jane Hauser (no relation to Mike). I had met Jane in New York back in 1974 while I was taking a workshop in re-evaluation counseling, one of many personal growth experiences that I sought as a part of my emotional education. (My sister Janet became a teacher of this subject for many years, and so did one of my many bosses at Goddard.) Jane is a ballet teacher, but she was taking computer programming and math courses as she completed her undergraduate education, and I was very impressed. So on Nov. 22, 1980, a hundred scientists, engineers, and dancers threw us a potluck banquet after our wedding, and I have never seen so much good feeling and good food at one time and place, at least until I arrived in Stockholm. Jane has been my life partner, my best friend, my best editor, and my best advisor ever since. With her I have traveled to many amazing places, and become quite fascinated with understanding how ancient civilizations managed to accomplish their engineering feats. We’ve seen Tycho Brahe’s observatory in Denmark, we’ve seen Ulugh Beg’s observatory in Samarkand, and I think most amazing of all, we’ve seen Pompeii, with plumbing, faucets, running water, and so many signs of modern life that one can hardly imagine how that knowledge was lost. Sometimes I think it would be a lot of fun to write books about how great cities were built, but I seem to have something else to do right now.  From 1980 on through the rest of the COBE project, my professional life was almost entirely consumed with the COBE. For a while I was a Branch Head at Goddard, in charge of the group that Mike Hauser had created. I went off for training courses in all kinds of personnel matters. It was an interesting education, and reinforced the idea that the manager is really working on behalf of the employees. It also emphasized what has become a recurring theme for me: life is a team sport, and it matters who’s on the team, and which team(s) one chooses to be on. For the year after the COBE was launched, Werner Neupert acted on my behalf. Later on, Chuck (Charles L.) Bennett became the branch head as well as continuing as Deputy Principal Investigator for the Differential Microwave Radiometers on the COBE; he’s also one of my other favorite advisors and great heroes.  I can’t imagine telling anything like a complete biographical story about my work on the COBE. I made an attempt in the book *The Very First Light*, written with John Boslough, a professional science writer. Some people have told me that they were exhausted after reading this book, the story was so full of terrifying moments. Needless to say, the COBE team was exhausted too at various times. For more details, please see the [Nobel Lecture](https://www.nobelprize.org/nobel_prizes/physics/laureates/2006/mather-lecture.html) accompanying this note, and the numerous technical publications from our team. But the main point I need to make is that the COBE mission was a team effort. Our team gave their complete concentration and support for a very long time, they dealt with having to redesign the mission after the Challenger explosion, they tested the observatory extensively, and they fixed the problems that they found. The analysis team found ways to compensate for “systematic errors” that were built into the designs, and in the end got measurements far beyond the formal requirements for sensitivity and accuracy.  After the COBE work was completed, I was wondering what to do next. For years I had successfully repelled all challenges to my concentration on one overwhelming responsibility. Now, it was done, and I switched my attention to developing new mission concepts. My colleague Harvey Moseley was working on the IRAC (Infrared Array Camera) for the SIRTF mission (Space Infrared Telescope Facility, later named the Spitzer Space Telescope). He said the next telescope needed more angular resolution, because the IRAC was so sensitive that its long exposures would be confusion-limited, i.e. that the fuzzy images of distant galaxies would be so numerous that they would overlap. I started thinking about this question and thought we needed to build a small (2 meter) telescope that would be deployed after launch, so it could be squeezed into an inexpensive launch vehicle. I presented this idea at a colloquium one day and my colleagues laughed and said NASA would never fly such a radical departure from tradition, and anything with a mechanism was dead before starting. I also learned about the Edison mission concept, being developed by Tim Hawarden and Harley Thronson and an extensive international team. This mission was proposed to NASA but was summarily rejected, based on grossly inaccurate thermal calculations made by some reviewer. Curiously enough, the marriage of these two rejected ideas has become the concept for the James Webb Space Telescope (JWST), which is now my major passion.  My involvement with the JWST began in the fall of 1995, when I received a phone message from Ed Weiler at NASA Headquarters, asking that I submit a proposal the next day for a study of the Next Generation Space Telescope. I was completely astonished – I had no awareness of this topic, or of the fact that an entire conference had been held at the Space Telescope Science Institute to argue for such an observatory. However, I wasted no time and said yes immediately, and then called around to find out the background information. John Campbell, Project Manager for the Hubble Space Telescope, already had an idea, and there was a committee, chaired by Alan Dressler, preparing a report on “HST and Beyond”. That report called for an infrared-optimized telescope to study topics from the early universe to the formation of stars and planets near home. It also called for an interferometer called the Terrestrial Planet Finder, to examine nearby stars for planets like our own Earth. So with this background, my creative juices were flowing, and so were those our colleagues. This initial phase of trying out wild ideas and hunting for ways to go far beyond anything ever done before is one of my biggest thrills. When Alan briefed our NASA Administrator, Dan Goldin, there was a real resonance, and Goldin told the January meeting of the American Astronomical Society that Alan’s vision was much too small, and NASA would build a bigger telescope. Goldin got a standing ovation from the meeting. In the next few months, we started up two serious industrial/university partnerships to develop concepts that competed with the NASA concept, we had public briefings of the results, and we were well on our way. At the time, all the studies concluded that an 8 meter observatory could be built for the target price of $500 M in FY96 dollars, not counting civil service salaries, technology development, or the operations phase, but the designs were at the level of “viewgraph engineering” in the days when NASA was under the spell of “faster, better, cheaper,” so it should not be surprising that later details have driven up the cost. We initiated technology developments for all the main inventions that were required for the mission, and those have all been successful. We negotiated a partnership with the European and Canadian space agencies, and when we finally chose a prime contractor (TRW, later purchased by Northrop Grumman), the observatory was named after James Webb, the second NASA administrator. Webb is a very appropriate honoree, as he is the person responsible for getting human beings to the Moon with the Apollo project, and he also insisted to President Kennedy that for the good of the Nation, there had to be a scientific research program at NASA and in universities. The National Academy’s Decadal Survey ranked the JWST project as top priority in 2000, and thanks to this endorsement is one of the few large projects still continuing forward in NASA’s science portfolio. I think the others will be revived as soon as budget can be found for them, since the need has not disappeared. At the moment, the JWST is in excellent technical shape. All the major technological developments have been completed to the required level, called TRL-6, which means they have been tested in the relevant environments. Also, the most difficult items to obtain, the mirrors, the detectors, and the microshutter arrays, are being fabricated with their final flight designs as I write this note. The JWST is now planned for launch in 2013. My role is called “senior project scientist,” and I chair the science working group and ensure that the mission will meet the scientific requirements. Now, after 11 years of this project, it is quite mature, which means that huge teams of people are doing the serious work.  The JWST is not the only wild idea that I’ve been pushing forward. From conversations with Harvey Moseley came the concept for a far infrared interferometer to map the sky with the same image quality that we get with the Hubble Space Telescope; this mission is now called the SPECS, the Submillimeter Probe of the Evolution of Cosmic Structure as David Leisawitz named it. One of these days (but not very soon) it will fly. On another day, I talked with David Bennett at Notre Dame, and we created the idea of a satellite mission to find planets around other stars using the micro-lensing phenomenon. David took it seriously and has submitted several proposals for it, and I think it will fly one day too, because it does things no other planetfinding mission can do. Probably my wildest idea was to send a miniature telescope to the outer solar system to see the cosmic infrared background light directly, without interference from interplanetary dust. This idea was half-baked but it was fun to work on it, and I got a little money to have a young technician build a miniature radiative cooler. That part worked brilliantly but it wasn’t enough for a mission. I still enjoy developing new mission concepts, and have recently been trying to persuade people to work on yet another way of hunting for planets around nearby stars.  Now, as I have passed the age of 60, and the Nobel Prize has recognized our COBE work, my life has changed again. I am giving many public lectures, to help the public understand the work we have done and hope to do in the future, and to inspire young people to be as excited about science as I am. I am also broadening my perspective one more time, trying to learn about the entire range of space science, and helping to guide NASA science towards the discoveries of the future. On April 2, 2007, I will take on the job of Chief Scientist of the Science Mission Directorate of NASA, so I will have the opportunity and responsibility to advise NASA on the proper balance of scientific programs from Earth science to cosmology. The panorama of amazing research programs is almost overwhelming, and I am looking forward to seeing it.  Any biographical statement would be empty without thanking the people who helped me through life. My parents, my sister, and my wife have all helped me immeasurably in finding my way through the challenges, and maintaining my faith in humanity despite all the disappointments that happen. My teachers in high school, college, graduate school, and my postdoctoral advisor Pat Thaddeus have led me to water and urged me to drink, and I have sometimes followed their advice. Their enthusiasm was contagious and I do my best to pass it on to my colleagues and to the public. My professional mentors at NASA have shown me how to work successfully and cheerfully with a giant organization full of talent. NASA’s review panels have saved our projects over and over, though we often hate to hear their opinions, and I especially thank the people who told us when we were doing things wrong. It is so much better to know about it before we push the launch button! And I suppose it is obvious, but the technical infrastructure developed by our modern society, partly in response to the Soviet Union and its scientific and engineering accomplishments, has made all of this possible beyond any imagining in 1946 when I arrived on Earth. |
| **Autobiography** |  |
| **Podcast** | **0049 = Mather**  Clare Brilliant: Welcome to Nobel Prize Conversations. I’m Claire. Brilliant and I’m here with our host Adam Smith. Hi, Adam.  Adam Smith: Hello, Clare.  Brilliant: We’ve been digging through our archives of previously recorded conversations, and today we’ll be hearing from John Mather. When did you speak to him, Adam?  Smith: The conversation was recorded in 2014. He’d been awarded the Nobel Prize in physics back in 2006 of his work on the Coase satellite and mapping the cosmic microwave background radiation. In 2014, he was busy with another satellite.  Brilliant: I guess he’d had a few years to get used to the prize.  Smith: Yes. he had indeed, the prize tended not to interfere too much in his work because he’s very focused and putting together these teams that have to get satellites to actually work, get launched and work in space.  Brilliant: I think that really comes across in the conversation, the immense task of bringing these huge teams of people together and how dedicated he’s to that.  Smith: Exactly. He was building the James Webb Space telescope back in 2014 which now of course has been launched and is sitting a million miles from earth mapping deep regions of space and revealing all sorts of amazing things.  Brilliant: Yes. It was really interesting to him talk about the potential of what he thinks these satellites are going to tell us.  Smith: Absolutely. I mean, the data’s coming back now and showing that water in the atmosphere of exoplanets orbiting stars in distant constellations or indeed the signatures of carbon containing molecules in those atmospheres. He feels that such signatures will be the footprint of life elsewhere. We will eventually find.  Brilliant: I really found it fascinating, his conviction that we will find extra-terrestrial life.  Smith: I also love his observation that extraterrestrial life may, if it becomes advanced, actually not emit much of a signal that he says, although we beam energy into space all the time, that may be just a stage in our lack of advancement, and once we get past this stage, we might become quieter, if you see what I mean. Fascinating.  Brilliant: Really fascinating.  Smith: Yes. He says that he can’t ever remember not wanting to be a scientist, and you can certainly tell that from listening to him. So let’s tune into the episode now.  John Mather: Morning.  Smith: You’ve just come back from the Intel Science and Engineering Fair in Los Angeles.  Mather: Yes, indeed. It was a pretty amazing group of young people there. Very inventive, very creative, very self-propelled young people. Several of them told me no, they did it themselves. Their parents didn’t even understand what they had done. If the world is to be based on what these young people are doing, we’re in fine shape.  Smith: That’s the thing. In a way one might associate doing science projects with a sort of old fashioned approach. Certainly lots of the laureates we speak to did great science projects when they were young. But nowadays it’s really good to hear that young people are still doing science projects in their spare time.  Mather: Oh, well, yes. They just want to do these things. They think of something, they’re inspired. They look things up on the internet. They take online courses. They go far beyond what their school teachers are leading them to do. They’re amazing.  Smith: It’s been going a long time, this science fair, but it’s becoming a more and more international affair. Is that right?  Mather: Yes. I think they told me there were 1600 people from 80 countries. Yes, it’s indeed huge.  Smith: Anybody you saw you wanted to recruit?  Mather: Oh, there are lots of really smart people there. One of the astronomers that I know is already going to be a summer intern at NASA Goddard where I work. We certainly recruit right people occasionally, but before that we’re ready to hire them into NASA we need them to have college degrees mostly.  Smith: Yes.  Mather: Of course. We’re just eager to see what they’re going to do next.  Smith: Did you tinker with science projects when you were young?  Mather: Yes, I participated in some. I know in ninth grade I had a project about nutrition and rats. I kept eight baby rats under the table in the kitchen for a few weeks while I found out what they did on different kinds of food. Later on, I think in 11th grade, I had a project about trying to measure the orbits of asteroids. I would say that it was a complete failure, but it was fun to try.  Smith: Were there any mishaps with your projects apart from failures, actual disasters?  Mather: No. No serious disasters. Nobody hurt.  Smith: Were you in a very supportive scientific environment when you were doing these things? Or were you out on your own?  Mather: Yes and no. My dad is a scientist, so he certainly was encouraging. My mother was encouraging, but they didn’t know personally very much about what I wanted to do. My dad did teach me statistics in ninth grade, so I learned about analysis of variance from him. That was a good thing to know about. My other project was asteroid orbit. Now I was definitely on my own.  Smith: When you were doing these things, had you already decided that you wanted to become a scientist?  Mather: I don’t know if I ever decided that. I think it’s more like I noticed that. I can’t remember ever not wanting to be a scientist.  Smith: Really. Even when you were very young?  Mather: I don’t remember anything when I was very young, but I know around third or fourth grade, I was already reading everything I could about science.  Smith: Gosh. So that’s eight or nine years old and you were already… Wow. That must’ve been pretty unusual.  Mather: I suppose so. But I have no way to tell. I would expect that many of the science fair students there also started there when they were eight or nine years old.  Smith: Can you summarise what it is about science that you found and find so fascinating?  Mather: Several things. One is that there’s a sense of discovery that you can just find out things that nobody knows before. If you find them yourself, and you’re the first one to find them, that sounds really important. Maybe it’ll help change the world for the better in some way. This idea that it’s also a little dangerous. I grew up on stories of Galileo and Darwin and the fact that they got in trouble just proved to me that it was important.  Smith: I love the idea of a nine-year-old boy reading about science and thinking of it as a kind of wonderful, dangerous profession.  Mather: Growing up here in the States where there’s so many fundamentalists around and people who would disagree with you if you wanted to tell them about evolution. I used to have dreams about that. Suppose I were teaching in school and I was teaching the students about evolution. It wasn’t so long ago we had our trial, jury trial over here about whether it was okay to teach evolution. Anyway, we keep on fighting that fight.  Smith: Exactly. Because things haven’t changed all that much. I mean, they’re not jury trials now, but there’s still a big debate.  Mather: Yes.  Smith: Do you still feel yourself to be fighting that battle on a daily basis to try and get people to attention?  Mather: Actually, no. I don’t fight it on a daily basis. I just do what I do. I don’t think it’s my job or anybody’s job to try to convince other people of the righteousness of my opinion. I think it’s each person’s job to figure out how they look at the world.  Smith: One of the things that you had to have when you eventually came to be the lead scientist, making sure that the Kobe Satellite Project actually worked was an incredible degree amount of confidence, I would’ve thought.  Mather: I actually don’t think so. Confidence, I think, is the sort of wrong feeling to convey. I think more it’s a sense of the importance of the work and the determination to make it to work. So rather than confidence, one has to have persistence and determination and certain degree of worry. If you don’t worry, then you don’t understand how hard the job is. In fact, it’s the job of our space engineering experts to make sure that something works. They have to spend a vast amount of effort to think of everything that could go wrong and make sure that doesn’t happen. It’s like the opposite of confidence.  Smith: Okay. But when somebody handed you the task of pulling all these people together and acting as the kind of center point for everybody, building Kobe, putting everyone in the same direction. They must have seen in you the sort of person who could inspire confidence in others, at least.  Mather: Yes. I guess they must have. It’s hard for me to remember those days. When I was hired into NASA to work on this project I was only 30 years old. So I thought they don’t have much basis for confidence. It’s just I’m willing to stand here and say I’m going to do this project. They were willing to bring in the top talent to make it happen. I didn’t actually organise all of that. Top engineering management did that. I think what they saw was a young team of scientists with some good ideas, and they thought it was important enough to recruit the best engineers that they could to make sure that it would happen. So it’s much more an organisational process with other people’s leadership than it is me.  Smith: Right. Is that, in a way, the secret of NASA’s success? That they’re able to organise the right teams of people to make these projects work?  Mather: Yes, I think so. It’s NASA, it’s every other large organisation that succeeds has to have a set of people and a culture and a process that leads to success. If you were to just set one bright person in the middle of the world and say now build me a telescope it would be a very long time of recruiting the top talent and organising them to find out who could do what and making sure that things would work. An organisation like NASA or its great contractors, all of them have this collection of people and process at the same time. There’s no way we could be building a great telescope today, just because a few people were smart. It takes this huge crowd of people with history who know how to do things.  Smith: When you finished the project, when Cobe finally delivered, in your case, the evidence that the CMBR had a black body form, do you remember the moment of finishing that project, of the data coming in?  Mather: Well yes and no. There are a number of special moments. One is of course the launch. And you realise that no, it did not explode.  Smith: Yes.  Mather: Then a few hours later, you realise and signals have come back and you say, well, the satellite’s alive. Then it takes a few days before we are able to open the cover on the hibi cryostat to find out if everything works. Then two days after that things are not behaving quite right. We have to debug and figure out what to do about all that. But within a couple of weeks, I think we were already getting our first interferogram and our first data saying, yes, things are functioning. I do have somewhere in my keepsakes a signed interferogram where my team members working late into the night were able to make a printout that showed the data were coming in correctly. That’s a special moment. It didn’t take too long after that before we realised we could make the famous spectrum that we presented six weeks after launch at the Astronomical Society.  Mather: I think when I presented that spectrum and we got a standing ovation for it, I came to understand that it was much more important than I had ever guessed. That was pretty special. Then two years later, as you know, we put forward our first maps of the sky. Again it was hugely important to the world because it was a map that they could print on the front end of the newspaper, and it was got even more publicity than before. I’d like to, by the way, mention that there was a process leading to that. About six months before that event Ned Wright, who was a member of our science team, had done his own personal analysis of the microwave map, and showed the science team that yes, it had spots on it. Our conclusion was that’s probably right, but we better check it. We spent the next six months verifying that it was correct before we could go public with it.  Smith: That six months was important.  Mather: Yes. There’s a history of people going off half-cocked in science. The more important it is, the less careful they get sometimes. That was in the days of poly water, which was apparently a fraud and cold fusion, which was apparently another fraud. We knew for sure we’d better not be announcing something that would have to be retracted. That’s why we were so careful.  Smith: When you come to the end of a project like that, and you had lived with a cosmic microwave background radiation for a long time, because you’d tried to make a map from balloon borne experiments prior to you ever beginning on the satellite project. When you come to the end of it, it must be very hard to conceive of starting something else. You finished a major chunk of your life’s work. How do you begin again?  Mather: That was a tricky question. At the end of the Kobe project, I did indeed think, what am I going to do now? I started poking around at different ideas. I thought for a while, well, maybe you know, they were planning the Spitzer Telescope at the time. My friend said, well, you know, it’s not a big enough telescope. We need to make a bigger one. I started making sketches of how you would unfold a telescope in outer space. I had in mind something only about two meters across, which was about a little over twice what the size of Spitzer Telescope is. So I thought, well, that would be fun. I presented my ideas to a small colloquium, and my friend said, oh, we’ll never do that. That’s much too hard. A couple years after that, I got a phone call from NASA headquarters that it said, it’s time to start the new telescope. What turned into the James Webb Space Telescope would I like to participate. They needed a proposal the next day for how to proceed. So I thought, I certainly can tell what’s to do now.  Smith: Did you have your scrap of paper where you had your doodles ready?  Mather: Yes.  Smith: When will the James Webb Space telescope launch?  Mather: It’s planned for October, 2018. So just over four years from now. We will have the observatory at the launch site. That’s the plan.  Smith: And the launch site will be where?  Mather: It’s in French Biana. It’s on the equator in South America because the European Space Agency is buying the rocket for this mission, and that’s where they launch.  Smith: How assured is that rocket launch? Is there a huge competition for those, or can you really book in advance like that and say, this is going to be a model?  Mather: No, we can book those in advance. It’s a commercial product. They launch them many times a year. I think they have a track record of about 50 in a row, good runs. We’re pretty pleased with that. It’s about as good as it gets in the space business.  Smith: Do you have a kind of backup plan for if you happen to be the rocket that does explode?  Mather: No. We don’t even think about that.  Smith: Okay, let’s not talk about that.  Mather: Better not to think about that. Just make sure the one that we have does the right thing.  Smith: Okay. When it gets into space and becomes functional, what will it see that hasn’t been seen before?  Mather: It’s designed to do infrared astronomy, which we accomplished by making the telescope cold so it doesn’t emit infrared light itself. It’s also very large. It’ll be able to look much farther out into space and farther back in time to look for the first objects that formed after the Big Bang, the first stars, the first galaxies, the first black holes, the first supernova, the first everything. Then try to understand how that led to our existence today. How did the stars explode and the material fall back into make new generations of stars and planets? How are stars being formed today? We know where they’re doing it nearby, and so let’s please have a look inside those dust clouds, see them do it. Ideally to learn a lot more about planets planetary systems. So for instance tracking down all the planets that have been discovered by the Kepler. We’re even developing a NASA mission called Tess, which is a transiting exoplanet survey satellite, I think. Anyway, it will basically extend the Kepler technique to all the nearest and brightest stars so that with luck, we’ll have a pretty nearby candidate object that might be like Earth. If we are extremely lucky, then we will find one that has enough water vapor to have an ocean, and that will be remarkable.  Smith: From the James Webb, you are going to be able to see planets as they transit across their stars and tell whether they have enough water vapor to potentially have oceans.  Mather: Yes. Isn’t that amazing?  Smith: It’s indeed.  Mather: When we first conceived the telescope, we didn’t know that was possible. We’ve only made the tiniest design changes to make it possible for the telescope to see those things.  Smith: Is it the case that you have a kind of raft of ideas coming in all the time for how you can improve it, and what else you can add to it to the mission? Do you have to have a shutdown time at which you say, after this point, no more ideas, let’s just do what we’re doing.  Mather: Pretty much the scientific requirements were frozen in about 2002. So hardly any changes have been made since.  Smith: That’s amazing. That’s a very long time. Why does it have to be set so far ahead of the 2018 launch?  Mather: Well, of course, in 2002, we didn’t think the launch would be in 2018. We thought we had much shorter time. But it was still the correct plan because you can’t build stuff while you keep changing your mind. So you have to decide what you’re going to do. So we did, however, have to continue to demonstrate our technologies. We had 10 different things that had to be invented and perfected before we could use them on this observatory. It took until 2007 before they were even ready to trust. That included things like the mirrors, the two kinds of infrared detectors that we need, a very low temperature amplifier and a computer to run the detectors. The ability to unfold the telescope in space and focus it after launch, all of those things had to be understood before you could even finish the design. So it’s very intimidating. There’s a plenty of good reason why you don’t keep changing your mind.  Smith: When people talk about the Apollo missions in the sixties, they look back and they say that it was an unbelievable feat. It happened, but the advancement in technology in the lead up to the moon missions was so great that it was just unlike any kind of invention that had been seen before or pace of invention. Is it like that every time you do one of these, that you conceive things that have to be invented to make this project work? It seems incredible when you conceive them that it can be done within the time, but somehow people find the resources to make it happen?  Mather: I don’t know about every time because every mission is different. We have different requirements, for instance for studying the earth than we do for doing astronomy. For earth science we need to have continuity where we know that the new equipment agrees with the old equipment as measuring trends of the earth is very important. You want to know if it’s getting warmer or colder, wetter or drier, dusty or less dusty. All those things require continuity. For that territory sometimes less change is good. Many more of the same kinds of equipment is good for astronomy. We push the frontiers by building something that’s more powerful than before. They’re infrequent enough that technology changes a lot in between. For instance the next even bigger telescope after the James Webb Telescope would probably be built specifically optimised to study those planets around other stars, the exoplanets. That probably means it needs to be two or three times as large as the James Webb telescope. When we get to do that, it’ll take yet another set of inventions.  Smith: Do you have to start designing the next one before you’ve got the current one up and working so that you have to predict what you’re going to see from this experiment and use that to design the next experiment without actually knowing whether this one is going to give you what you need?  Mather: I think actually our challenge is a little different. The most uncertain thing about these great telescopes is can we do them? People are quite concerned about what happens if it doesn’t work. I think that’s the number one thing to establish that it’s possible for an organisation to produce a working product, even if it is complicated. I think setting out the next kind of science to do isn’t actually so hard to figure out because we already have good evidence for what it should be.  Smith: How do you decide which experiments to do? How do you prioritise and given the vast number of possibilities, and whether NASA or invest in space missions or whether they invest in satellite technology? How do you begin to decide?  Mather: For a particular mission like the James Webb telescope we set out committees of scientists to say, well, what’s most important to you? What do you think we’ll be wanting to do in 10 or 20 or 30 years from now? How do you know that somebody else isn’t going to do it? We looked for the things that could never be done in any other way. That sort of basic idea, don’t do something somebody else can do. It’s too hard and takes too long to cut up a space mission. We could tell that nobody was going to do this particular science because nobody could see through the earth atmosphere at the wavelength that we were working on. Whatever progress that was going to be done, had to be done with a space mission. That told us our idea was unique. That’s if you want to say, how do you choose what general idea to pursue what kind of observatory? We have giant committees in the US. They meet every 10 years. They produce a survey of all kinds of astronomy and what should be done next. Europe has its own process for doing those things and so do individual countries like the UK. Committees of scientists get together and argue. That’s a good thing.  Smith: Is there a big debate about whether it’s better to do missions that have great popular appeal? I suppose searching for exoplanets is a good one as far as the public are concerned. Certainly putting people onto Mars would have popular appeal or projects that are more for the scientist, if you like, where the popular appeal is not quite so obvious.  Mather: I don’t know. Clearly the public pays for these things with their taxes for large NASA and European missions. If we’re sending people to Mars, it may happen differently because it takes a different way to go than what we’ve been thinking about. For instance there’s a company called SpaceX where they’ve been making good progress on lowering the price of space launches. The company owner, Mr. Elon Musk, says that he’s motivated by the desire to go personally to Mars. That may actually change our whole interplanetary travel process if he’s successful.  Smith: Do you think he will be?  Mather: He is doing awfully well now, so I encouraged the thought that he can do well. Travel to Mars is dangerous no matter how you think of it. But we can do better. So maybe we will.  Smith: Did you ever entertain the thought yourself of trying to get up into space?  Mather: Not very much. I’m not very much of an athlete. I think I would be a little concerned about surviving the launch, but if I could go safely and comfortably, of course, I would want to go.  Smith: I wanted to ask you just one film related question. Having watched Gravity on an airplane journey myself the other day, is that picture that’s portrayed in the film, gravity of a rather crowded near Earth orbit with a potential of things to bump into each other, satellites bump into each other, becoming true? Are we getting a bit crowded in the near Earth space?  Mather: It is crowded, and we’ve already had a one definite accident where two satellites collided, and they weren’t even trying. They were just a random accident. Of course, that showers the area with debris. We’ve had a couple of events where satellites were intentionally shot at by the people that owned them, one US to prove the ability to do it and one Chinese. In both cases debris occurs and this has a significant hazard for astronauts. There’s more and more of this debris up there. It’s not imminent, but certainly it’s predictable that there will be a time when there’s so much debris that you can’t go there anymore.  Smith: That will happen? There will be such a time?  Mather: It depends on what people do. We could continue to work on ways of reducing that debris to go out and catch it or destroy it, or send it into the atmosphere, or various things we could do that would help.  Smith: Hard to conceive, because…  Mather: Now you probably already know that there’s an international treaty that requires all satellites in those areas to be disposed of safely when they’re done. But bad things still happen.  Smith: I can’t begin to see how you might, you might begin to get rid of the debris. You’d need some sort of vast vacuum cleaner sucking it all up.  Mather: Yes, you do. There are a lot of ideas but none of them turned out to be very… none have been chosen yet, I think.  Smith: I mean, presumably it’s a very important problem to solve, because otherwise you also can’t put satellites up because they’re gonna bump into the debris every so often.  Mather: Yeah. It’s harder than it sounds. Let’s put it that way.  Smith: Yes, quite so. One of the other things you spoke about with reference to James Webb, was that it’s going to look further back in time to the formation of some of the earliest large structures in the universe. That’s an extraordinary thing to sort of begin to conceive. I wanted to ask, do you stop every so often and just marvel at the wonder of what you are looking at and what you’re trying to look at? The enormity of it?  Mather: I do every day. I’m thinking about the marvels of what I’m looking at. To me, the even more mysterious part is the biological world. I’ve studied physics long enough to have some idea of how these things function and how we’re now able to simulate in the computers how galaxies form and things like that. How stars form how planets form. When I listen to my biologist friends talk about what they’re working on, I’m thinking how completely astonishing it all is and how unutterably complex it all is. How almost miraculous it seems, even when you understand about evolution. There’s just no end to the complexity of what we have inside us. To think about the fact that we’re probably all descended from the same original single cell living thing 3.8 billion years ago. Here we are continuing 3.8 billion years of life. So you and I, and the bacteria and the viruses are all direct relatives.  Smith: It’s very nicely said, very beautifully said. But one of the strange things perhaps, is that people in general, I would say are much more grabbed by the wonders of the universe than they are by the wonders of life on Earth. If you look at sort of newspaper headlines, as soon as there’s a new beautiful picture of the universe, or some fact that brings out the enormity of space, it hits headlines and people lap it up. Somehow the marvel of life on Earth is perhaps more mundane to people. They see it, they sort of encounter it every day, and somehow they’re used to it, and they don’t tend to be as amazed by it I would say.  Mather: I know, that’s true. We make beautiful pictures in our astronomy, and people are inspired by those pictures. How life works is just much more complex than you could possibly imagine. So it’s hard to make a pretty picture of it. But if you know a little bit about it, you say, oh, how completely amazing. Because life is more, is far more than I ever knew. It’s done digitally. We have digital code in our RNA and our DNA. The little tiny computer hardware inside each cell, they read the code and do things with it. Things are switched on and off digitally. Nobody gets excited about computer code, except maybe the people who write it. We just are, we just use it without knowing anything about the marvels inside it. People aren’t amazed at the wonderful engineering inside their cars either. But 200 years ago, we didn’t have any. That’s an equally mysterious and wonderful story.  Smith: Is there one undiscovered question in space research in astronomy that you very much hope to see answered in your career?  Mather: I’d say every day when somebody asks me that question, I have a different answer, because there’s so many wonderful mysteries out there. I’ll just say a few that are really intriguing and there’s a chance that we can make some good progress. What are the dark matter and the dark energy? There’s a fair chance that we can have a reaction of some kind of dark matter in a laboratory setting. There are several experiments worldwide hunting for that. Maybe we’ll know more about the dark matter and maybe we’ll finally get a satisfactory unifying theory of physics that says what it is and what it’s supposed to be, and makes some decent predictions that would help the closer to home. Are we the only ones here in the universe, or is this planet the only one that’s alive? Or are there many planets that are alive? I’ll tell you my prediction is that wherever there’s liquid water there’s life. That’s what I think. And how would we know? We have to go a few places where you can examine this. On Mars, there at least was, and probably still is, liquid water in places. I think when we dig carefully and well, we will discover signs of life on Mars. We know we can get there with the equipment to do that.  Smith: Let me just explore that question a little bit more. Where will the liquid water be on Mars, do you think?  Mather: It’s not on the surface because it’s too cold and too dry, but underground, there could be liquid water in the rock.  Smith: When do you think it is likely that we might be able to get to that place?  Mather: Oh, that’s pretty hard. It depends on how far down into the rock you think you might have to drill, or whether you think some signs of it will be on the surface. I think we have to send a continuing series of probes to go looking around. We have chemistry labs on the surface of Mars now. In fact one of them was built right here at Goddard Space Flight Center. My colleagues are every day rehearsing in their lab in Greenbelt, Maryland, how to do the analysis on Mars. There will be a continuing series of miniaturized instruments to go to Mars to do these analyses. Another great hope that that team has there is to bring back some rocks so we can study them here at home, with even more powerful equipment. Bringing rocks home, that’s clearly a major job. It’s difficult and expensive also, but it’s still not nearly as difficult and expensive as getting people to Mars and back. I think it’s clearly a next step in our in our travel plan to Mars is to learn how to go there with robots and bring back rocks for study.  Smith: Sorry, now that we’re on this topic of extra terrestrial life, and given that you surely expect to find liquid water on innumerable planets in time, and thus there are innumerable points in the universe where there is life, do you think it’s odd that there has been no sign of that life seen in emissions that we’ve been looking for in space?  Mather: No, I don’t think it’s odd. I think it is what I would expect. There’s no particular reason for a civilization to be transmitting large amounts of radio power out into space. We do it here on earth sort of by accident. We need radars, we have televisions. It wouldn’t surprise me a bit if in another century that we abandoned all that stuff because there’s some other better way to do it. I think it’s quite possible that if there are civilisations out there carrying on a high technology civilisation that there’s no reason for them to be sending us a signal.  Smith: Yes. It’s very hard to think of things in any other way than the way you live, isn’t it? So I suppose, all future predictions sort of see us going on just as we are, but we won’t be.  Mather: Anyway, I think space is also very large. I think that there’s a suggestion here that even if life is common, that intelligent life is not common. The evidence that we have here on Earth is that very soon after the asteroid bombardment ended about 3.8 billion years ago we got signs of life in the fossils. Probably it was very quick here after life could occur with liquid water that we had. So that says the formation of life is quick, but then it took the rest of history for us to get here. Modern civilisation has only been here, maybe you call it a hundred years, where we could transmit radio power which out of the age of the universe is just like nothing. We’re so new and so brief here, it’s hard to tell where we can go or what we will do.  Smith: Yes. It is a good point that, what is it? Is it three and a half billion years since life first began on Earth, perhaps? How would that compare to the normal time that it would take an intelligent civilisation to occur on any planet? How long do planets normally have before something happens?  Mather: Right. Good question. No one doesn’t know. We have our only one case that we’ve noticed, which is ourselves. There are many serious writers who think that our case is rare. There’s a book called ‘Rare Earth’ that makes the case for that. That’s not the only one. I think they’re probably right that the history of events here on the surface of the earth is unusual. We have a particularly unusual situation with a large moon, which stabilises the spin axis of the earth. We have volcanism and continental drift and just the right amount of water to have both land and ocean. Those might all be necessary for the formation of intelligent life. We don’t know, but what if all those are necessary, then it would be rare.  Smith: Do you think you might see extraterrestrial life on Mars in your lifetime?  Mather: I think quite so. I think we could but I don’t know. Because Mars is large, if you were to sit down a probe in the desert here on earth, you might have a hard time discovering that there was something underground that was alive here too.  Smith: Yes.  Mather: Each probe can only examine a few square meters of territory at that, out of millions of square kilometers. If you don’t find it the first time, it doesn’t mean there’s none. It just means you didn’t find it.  Adam Smith: I suppose every scientist likes to point out how each time you ask a question, the answer just leads to more questions. It couldn’t be more true in the case of astronomy and space exploration and cosmology.  Mather: Certainly true. Absolutely. But that’s one of the great marvels of science too, that when you see a little farther, you see that everything is more complex. I think it’s a fair guide to science to figure everything is more complex than you can possibly imagine. If you can just peel off another layer you’ll have more work to do.  Smith: Yes. That’s a great advertisement for a career in science, isn’t it?  Mather: Yes. Our job is not going to be done anytime soon.  Smith: Good. What a fascinating conversation. At least for me. I’ve enjoyed it tremendously.  Mather: Thank you, Adam. I love talking with you.  Smith: Thank you so much.  Brilliant: This podcast was presented by Nobel Prize Conversations. If you’d like to know more about John Mather, you can go to nobelprize.org. Where you’ll find a wealth of information about the prizes and the people behind the discoveries.  Nobel Prize Conversations is a podcast series with Adam Smith, a co-production of FILT and Nobel Prize Outreach. The producer for Nobel Prize Talks was Magnus Gylje. The editorial team for this encore production includes Andrew Hart, Olivia Lundqvist and me, Clare Brilliant. Music by Epidemic Sound. You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| **Telephone**  **interview** | **0049 = JM**  [John Mather] – Good morning.  [Adam Smith] – Good morning, may I speak to Professor Mather please?  [JM] – This is John Mather, yes.  [AS] – Hello, my name is Adam Smith and I’m calling from the official website of the Nobel Foundation.  [JM] – Oh, yes.  [AS] – I know you’ve just been on the phone to the Royal Academy of Sciences but we have a tradition of recording very brief telephone interviews with Nobel Laureates immediately after they have been informed, so would you mind if I asked you a few, quick questions.  [JM] – No, please do, that’s fine.  [AS] – Thank you. It’s pretty early there, what were you doing when you actually heard the news?  [JM] – Well, I was asleep. I’m just barely waking up. So …  [AS] – I can imagine …  [JM] – I did receive a phone call from the Academy this morning.  [AS] – Must have been quite a surprise.  [JM] – Yes.  [AS] – You and George Smoot have been awarded the prize for your discovery, or rather for the satellite measurements of faint signatures of the early universe left behind in the form of background radiation. Why is it so important to observe this background radiation from space?  [JM] – Well, it really is very difficult to observe it well from the ground. The atmosphere of the earth absorbs the radiation somewhat, and even at wavelengths where the radiation does come through, the atmosphere emits its own radiation, which confuses matters quite a lot. So it really was important to get up into space where it’s cold and quiet.  [AS] – And I gather it took many, many years of work to get up into space with the COBE satellite?  [JM] – Yes, 15 years from proposal to launch and then we operated the satellite for 4 more years, and kept on analyzing data for another several years after that.  [AS] – So you need some considerable patience before you reach your Eureka moment?  [JM] – Yes. Well one suspects, in the beginning, but one doesn’t know, and so extreme care is required, especially for these kinds of things because there’s basically no other way to tell if the equipment got the right answer.  [AS] – And once the data did start flooding in the first key finding was that the cosmic background radiation did indeed display a perfect blackbody radiation spectrum. What does that tell us?  [JM] – Well, it says that the radiation really did come from the big bang. There really is not a good alternative explanation for having such a perfect blackbody spectrum. Many people looked, but no good explanation was found, and so the big bang theory is confirmed by that spectrum.  [AS] – Right. Now what are we actually seeing in the CMB? Is it a snapshot of a particular moment, or rather the accumulated trace of hundreds of thousands of years?  [JM] – Well, I think of it as the accumulated trace of everything. The history is roughly this; the early universe, in the first submicroseconds, was extremely [word inaudible] and all of the cosmic particles, protons, electrons, unstable nuclear particles, neutrinos and photons and background radiation were all hot and were all together. Then, as the universe expanded, progressively each kind either disappeared, because it was unstable, or annihilated some other kind of particle, or did not. But in any case they all cooled down and so the cosmic microwave background radiation is actually a remnant that traces back to those very earliest moments. But we see features of it that were finally set later. For instance, the spectrum that we observed to test the big bang theory could have been modified as late as, say a year after the big bang. And even in most recent times of course things in our own galaxy, and other galaxies, can emit small amounts of radiation that would confuse the measurements.  [AS] – Quite. So presumably all hot bodies are leaving their own, small background signatures?  [JM] – Absolutely. And similarly the spatial distribution, the map that we obtained, that shows the hot and cold spots, that shows the universe as it was approximately 389,000 years after the big bang.  [AS] – That’s very precise. And is there further information hidden in the CMB?  [JM] – Yes, we certainly think so. One of the continuing investigations is to get the polarization of this radiation. The polarization (is expected and some has been measured) tells us already that the first luminous objects after the big bang were quite early, when the universe was less than a 20th of its present size. So that’s already been measured with the WMAP satellite, and much more is thought to be lurking there in the radiation if we could measure even better. Traces from the gravitational waves of the earliest universe, for instance.  [AS] – Right. So increasing precision will yield more data.  [JM] – Yeah.  [AS] – What’s the main challenge to getting that increased precision?  [JM] – Well, it’s extremely carefully done because the signature is extremely faint. The radiation itself is called ‘faint’, but it’s not so faint; it’s about a microwatt per square meter coming to us, you can actually say that. But the spectrum measurement was made to a part in a hundred thousand accuracy and the hot and cold spots are about a part in a hundred thousand. Now this polarization is maybe a hundredth of that, so we’re getting down to signals that are measured in nanoKelvins.  [AS] – And COBE was a NASA project. Is it becoming more of an international effort as time goes on?  [JM] – Well, the European Space Agency is about to launch the Planck mission and maybe they will even make some progress with this polarization question. They certainly will have sensitivity to finer scale features on this guy.  [AS] – I suppose the last question that I wanted to ask was how you intend to celebrate the award of the prize with your team, which I know is very large?  [JM] – Good question. I think I will need to talk to them.  [AS] – That’s fair enough. OK, well many, many congratulations on the award and thank you very much for sparing the time to speak to us.  [JM] – Thank you. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0050** |
| **Biographical** | Winning a Nobel Prize is a life-changing event. It is a turning point. In addition to many people asking about how you arrived at this point, the winner is asked to produce his biography. All this and the events related to the Nobel Prize have resulted in each one of us going back and reviewing his life and influences that led to the present.  There were many key people, events and circumstances that had a substantial effect upon my development. However, it is clear that family has by far had the largest effect.  Both of my grandfathers were judges, along with my uncle and my cousin. One grandfather, Tal Crawford, served as Chief Justice during the Nuremberg War Crimes trial. Although I knew him only as a child, he had an influence upon me directly from his great kindness and careful discussions, and also through my mother Talicia. My father even started college with the intent to major in law, but World War II and interest in engineering and science diverted him.  Another key person was my Ph.D. thesis advisor at MIT, Professor David Henry Frisch. I was recently at a meeting with his son, University of Chicago Physics Professor Henry Frisch, and recalled how Professor Frisch related from time to time how he had descended from thirteen generations of rabbis, and that now there were two generations of physicists. (Henry is sure that the next generation will change the trend more quickly.) My family had a tradition in law, specifically as judges. We thought that all three of these professions had a number of traits in common: in particular, being able to reason, remember, and listen to others (perhaps less obvious for physicists but still true in collaborations).  The first pivotal event I remember in my development as an inquiring scientist began with a visit to my cousins, as we took a night drive across the state of Alabama. At that time, this was a very long distance to me. Excited from the visit with my cousins, I stayed awake, looking out the window rather than taking a nap. I noticed that the moon was following us mile after mile tagging along like my dog but with greater speed and persistence. I asked my parents, “How does the moon know to follow us?” They told me that the moon followed all the cars, not just ours. I was intrigued and wanted to know how the moon did this. Was it immensely superior to a dog? My parents patiently explained that the moon was very big and very far away and thus the angle did not change noticeably as we drove for miles. They gave examples of near and far objects that we could see along the way, so I could realize this for myself. I was impressed by how big and far away the moon must be but even more impressed that one could understand what they saw in the world and that it was so beautifully simple and clear when visualized in the proper way. It was a startling revelation that the world could be understood by simple rational evaluation.  Not long after that, I got another glimpse at the importance of education, study, and reason. My father (George Smoot II) and mother both had resumed their college educations after the war was over, and life settled down following the birth of myself and my sister Sharon. They would drive from our hometown to their new college, Auburn University (closer than their original Tulane and Sophie Newcomb alma maters). At that time, I attended Auburn day care. That semester, my mother had a biology/zoology class that ran late so I would sometimes attend the class with my mother, sitting on her lap, reading the text, and listening to the lectures. It was clear how my parents, the professors and students valued education, and that they were discovering and understanding things about the outdoors that I played in everyday. I liked the respect that the other students and professors gave my mother for being a good student, and that they tolerated me being in the class and sometimes explained things to me. The seriousness of learning was tempered with fun, so it was a wonderful place to be and opened up a whole new way of understanding the world as a child. I still spent a lot of time outside exploring nature but now tried to tie that to reason and a network of knowledge.  This thoughtful, educational life was very different from the other difficulties that my family was facing. With two kids, the budget was tight. Part of my father’s education was covered by the G.I. bill, but my mother’s had to be raised separately along with paying for other expenses. My father had started a lumber business after the war to provide additional income. The business was successful, providing just enough for us to eke by. Then, a problem came up. In one of the stands of trees that my father purchased, a large, productive still was producing bootleg alcohol for this dry portion of the state. As soon as the tree cutting began, the group operating the still approached my father. They offered him cash not to cut the trees and expose their substantial operation. My father, at some personal risk, informed the police, and the trees were promptly impounded (no longer allowed to be cut for the saw mill) by the judge handling the case. He was apparently on the payroll of the illegal distillery operation as some police must have been. This caused an immediate financial crisis for us, as much of the saw mills/lumber company funds were tied up in trees, which were to be cut down and then milled to lumber and sold. Suddenly there was a shortage of trees to mill. Needless to say, our lumber company was soon in bankruptcy. We quickly moved out of our house and in with my grandmother, while my parents struggled to finish their college courses and graduate.  Meanwhile, my father had migrated from law into engineering. I later wondered if it was due to the contrast between the spirit and the letter of the law as practiced in this lumber case or whether his natural talent and interest in engineering and mathematics emerged. Shortly after graduation, my father landed a job with the U.S. Geological Survey in hydrology (water resources). We were on the road to getting out of debt with no asset but our family education.  Soon, an opportunity opened up in the form of a USGS job in Alaska, a location where my father had served as a fighter pilot during World War II. He jumped at the chance to work in the field and its pioneering efforts. Our family moved to Alaska, piling all our belongings into a homemade trailer and making the trip up the Alaska-Canada highway. We felt like the early settlers going west in covered wagons to a new life and land. There were mishaps – failures of the trailer – and other adventures on the long trip north to the Matanuska Valley (in sight of Mt. McKinley). We were still very poor and did not have one of the new-fangled TV sets that could get reception from the few stations in Anchorage. Instead, I spent my time exploring the outdoors, viewing the night sky, and studying. By now (1954), I needed glasses due to the long hours reading and studying I did during the winters. My mother also worked, starting her career as an elementary school teacher. I was in the Palmer, Alaska unified school district from grades 2–6 where I received a good education. Many of my classmates were Eskimo and Indian orphans as the state orphanage was near by presumably a big step up from being left on an ice floe. The experience seemed like a great opportunity to meet people from all over the Valley and learn things. We only occasionally had to do the duck and cover drills in case of nuclear or air attack. In this isolated area, we felt fairly safe except for the occasional moose wandering onto the playground. Because the ground froze hard during the winter we could also ice skate at the low end. There I got my first classic lessons in conservation of angular momentum for spinning ice skaters (arms in and out) as well as learning not to stick my tongue on the cold metal figures of the playground equipment.  During this period my father showed me how to be an intrepid explorer as he regularly procured data for hydrology and hunting. We did not buy the meat we ate during our four years in Alaska but acquired our own from hunting and fishing. My father and his friend Frank Trainer would go into the wild and bring back whatever was in season: moose, mountain sheep and goats, caribou, and bears. We also had a large vegetable garden. Though the growing season was short, the light days were long. The Matanuska Valley is famous for its large vegetables. For example, one year that we lived there, a 40-lb (18 kg) cabbage won at the state fair. During the winter, moose would come down into our now frozen vegetable garden and dig out some of the remaining edibles.  My father would also reconnoiter on field trips to measure the properties and flow of rivers, often having to chop through many feet of ice to get to the water in question. He invented a number of devices and techniques, which we were able to see. Eventually, word spread of his efficiency, and he was called to the research station in Columbus Ohio, and then later to headquarters in Washington, D.C. As a leader in the field, my father traveled worldwide to measure rivers and other water sources. Our family still has clippings of the time that my father went to Brazil to work with the local agency to make the first measurement of the water flow rate of the Amazon River. Shortly after that, he went to measure the Mekong River and its tributaries something new and fairly dangerous. He traveled to many places, and later worked for the World Bank on issues related to irrigation projects and water resources. The role model he provided and message he sent was: go anywhere and do anything necessary to get the data or the meat for meals.  My mother also was a tremendous role model. Though soft spoken, she continued to develop her career as a teacher. When we moved to Columbus Ohio, she entered the Masters Degree program at Ohio State and received credentials to teach science. I remember very well how she performed her teaching job, took care of my sister and I, and also went to night courses studying at Ohio State; this while my father was traveling. Some nights my sister and I accompanied my mother and studied in the library. Again, the importance that my parents attached to education, both by word and deed was apparent. While traveling, my father found a bargain used bookstore and over a number of visits, bought for me the complete Modern Library of what were considered the great books. At first I was encouraged to read these but soon eagerly awaited the arrival of the next batch.  While I was in high school, Sputnik appeared in the October sky (1957), and the nation and world became interested in science and space. My mother tutored me in additional science and history, while my father drilled me in the basement, force-feeding trigonometry and introductory calculus courses that were not given at my high school. I also put together a crystal radio set and later, a transistor radio kit.  During this time, I worked to save money for college – as a paperboy for the Columbus Dispatch, mowing lawns, and working as a caddy at the Scioto Country Club (all while studying and reading). After three years of this work, I had barely enough money to pay for little more than my first semester at MIT. During my undergraduate years at MIT, I worked various jobs to help pay for my education, in addition to support from my parents and student loans. The hardest part was taking the rigorous courses and a heavy course load, but I wanted to take advantage of a good education. It was clear that MIT was definitely pushing us with as much information as we could soak up, and then some.  At the end of the four years, I received dual degrees in Mathematics and Physics along with the clarity that I really was going to be a physicist. I had come into college with a thirst for knowledge and an interest in many areas. I considered premed and other things but eventually gravitated to math and physics. At the time, MIT made every entering freshman take courses in math, chemistry, physics, and the humanities plus a choice of elective. Soon, it was clear to me that I enjoyed and did my best in mathematics and physics, and so focused in those areas. By the time four years had gone by, my interest in physics pulled ahead of mathematics. While I had a number of good and influential teachers in elementary and high school, when I got to MIT, I found professors who were also excited by the research at the forefront of knowledge and understanding. They had an attitude of anticipation and excitement about what the next day would bring and what challenges they could overcome.  My fellow students were much the same way and we studied hard. However, we had to let off steam in an MIT traditional way of hacks that is generally high-tech pranks and feats of technical derring-do. Some were simple, like welding rails in a 3-D cross in someone’s dorm room or filling a refrigerator completely full of Jell-O (you put it on its back and then right it carefully once the Jell-O gels). Some were more elaborate, like a steam catapult for hard-boiled eggs or water-balloon crossbow. One received peer kudos for technical difficulty.  We carried forward the occasional prank into graduate school. My fellow graduate student and research colleague, Orrin Fackler and I were working shifts together, putting together an experiment at Brookhaven National Laboratory’s AGS accelerator. We had determined that the most effective target for turning a positive kaon beam into a neutral kaon beam would be made of osmium. Now that was rare, more so than gold or platinum, and thus we did not think we could get it so were thinking of alternatives. Our advisor, Professor David Frisch said, “Your calculations look correct. I will go and personally sign out the osmium from the strategic metal and you can use it for your theses experiments.” This was an inspiring show of confidence in us and our willingness to do what was necessary for the experiment. Frisch was personally on the hook for a small cylinder of metal worth twice what his new car was.  No good deed goes unrewarded. While working a half-day shift 7 PM to 7 AM, we were preparing the target box to hold the new target that Frisch had brought down to Brookhaven after his Friday class. It turns out that it did not fit in the carefully designed target box because it had warped a bit during the welding process. Orrin and I discussed this and realized that we would have to spend the entire night hack sawing and filing down the inside portion of the target box. By early morning, we had a significant pile of silver colored aluminum chips and dust similar color to osmium. We knew that Professor Frisch would soon (typically 6:30 AM) come bounding in to see how much we had accomplished during the night (not much). I convinced Orrin that we should play a trick on Professor Frisch. When he came in enthusiastically greeting us, I wiped my brow as I finished screwing back together the target box and complained how hard osmium was to work with. I told him how it did not fit and how we had to saw and file on the osmium all night. Dave looked stricken with concern, but he knew I played pranks so he looked over at the always serious and more mature Orrin who added, “Yes, there was a lot to do but we worked hard to shave it down to have it ready for you today.” Dave now looked back with his hand over his heart. I responded, “What is the matter? Is it toxic?” Dave gasped, “No, but it is precious.” Orrin and I then confessed the prank, and after he recovered, Dave had a good laugh about it.  Though I was interested in many fields in physics, particle physics offered funds and opportunities for me to do a senior thesis. I was able to build equipment, take data, and learn things that no one had previously known. Soon I was hooked, and while accepted to a number of graduate programs, I chose to continue at MIT doing particle physics experiments. Working with researchers, including Professors Frisch and Osborne; other people in their groups; Kendall and Friedman; and later Professor [Sam Ting](https://www.nobelprize.org/nobel_prizes/physics/laureates/1976/index.html), was an important experience in the rigor and attitudes of seeking the best science.  I was working on my senior thesis in the 1965–1966 academic year when I heard of the [Penzias](https://www.nobelprize.org/nobel_prizes/physics/laureates/1978/index.html) and [Wilson](https://www.nobelprize.org/nobel_prizes/physics/laureates/1978/index.html) discovery of the 3K Big Bang relic radiation. I was already interested in astrophysics, and paid some attention to the discovery, but I was busy with my schoolwork and research so I filed it away as something I had learned.  A few years later in 1967, Joe Silk was writing his paper on primordial fluctuations in the CMB (which was very influential for me) and Steven Weinberg began teaching his course at MIT that eventually became the book *Gravitation and Cosmology*. This informed me that there was something exciting and fundamental happening in this area. However, I was newlywed, finishing my thesis, and my wife Maxine was applying for graduate school in English Literature. I would soon need a postdoctoral scholar position, so I could not act right away. In 1970, I finished my Ph.D. at MIT and then went out seeking a new position. I was twenty-five years old. I interviewed at a number of places including a few positions at Berkeley, where I was persuaded by 1968 Physics Nobel Prize winner, Professor [Luis W. Alvarez](https://www.nobelprize.org/nobel_prizes/physics/laureates/1968/index.html) to choose Berkeley, and his group in particular, because they were willing to venture into completely new areas. This was also a position that would make good use of my newly acquired skills and knowledge.  A major reason that I came to Berkeley (University of California and the Lawrence Berkeley National Laboratory) was the I-can-do attitude along with the emphasis on first-rate science. There were many excellent researchers in Berkeley including a number of Nobel Laureates. Luis Alvarez was among the notables and it was his recruitment that attracted me so directly. However, there were many others whom I knew and interacted with besides Luis Alvarez including [Emilio Segrè](https://www.nobelprize.org/nobel_prizes/physics/laureates/1959/index.html), [Owen Chamberlain](https://www.nobelprize.org/nobel_prizes/physics/laureates/1959/index.html), [Edwin McMillan](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1951/index.html), [Glenn Seaborg](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1951/index.html), [Don Glaser](https://www.nobelprize.org/nobel_prizes/physics/laureates/1960/index.html), [Melvin Calvin](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1961/index.html), [Yuan Lee](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1986/index.html), and more recently [Steven Chu](https://www.nobelprize.org/nobel_prizes/physics/laureates/1997/index.html). There were many more outstanding scientists, engineers, and technicians that made Berkeley an excellent environment conducive to outstanding research. Interacting with such individuals provided many role models, along with the knowledge that these were real people who organized and applied themselves to do exceptional work.  I left MIT in 1970 and came to the University of California at Berkeley to work with Luis Alvarez on the NASA-funded High-Altitude Particle Physics Experiment (HAPPE, pronounced happy) at Lawrence Berkeley National Laboratory. The goal of HAPPE was to probe for interactions at higher energies than accelerators could produce at the time. The unfortunate Pacific Ocean crash of the HAPPE instrument, followed by the approval to construct a new high-energy accelerator (at what is now FermiLab) resulted in the reevaluation and new direction of the HAPPE program. Our goal was to design an experiment to find evidence of the Big Bang, which had become scientists favored explanation for the formation of the universe. The Belgian priest and astronomer Georges Lemaître first proposed the Big Bang theory in the late 1920s, after Edwin Hubble discovered that distant galaxies were rapidly receding from us; hence the universe was expanding. Its many critics, particularly Fred Hoyle, scornfully called the theory the “Big Bang” but the concept gained wide acceptance after 1964 when Arno Penzias and Robert Wilson of Bell Telephone Laboratories fortuitously discovered the cosmic background radiation – microwave remnants from the creation of universe. The existence of the cosmic background radiation was initially predicted by George Gamow in 1948, and eventually supported by other Big Bang theorists.  According to the basic Big Bang theory, 14 billion years ago our observable universe began with a rapid expansion of an extremely small but extraordinarily dense region in which energy and matter were one, and it has been expanding ever since. The initial subatomic particles and antiparticles arose from the primal energy, and eventually their daughters formed protons, neutrons, and electrons. After only 3 minutes, light nuclei formed. For the first 380,000 years, the temperature was too hot for the electrons to attach to the protons and nuclei to form atoms. The radiation that filled the universe could not travel freely without being scattered by the free electrons; therefore no light could escape and the universe was opaque. After 380,000 years of darkness, the universe cooled sufficiently for electrons to affix to nuclei, freeing radiation to escape (a process known as decoupling) and allowing light to become visible. The radiation that filled the universe during the period of decoupling continues (filling the universe) in the form of cosmic background radiation, but it has cooled from about 3,000 Kelvin at decoupling to less than 3 Kelvin. Kelvin is a scale of temperature in which 0 K, or -273.15° Celsius, is the temperature that molecules have no random thermal kinetic energy left. Following decoupling, the atoms joined to form molecules, and these aggregated under the force of gravity to create the universe known today.  Our first balloon-borne detectors were designed to see if a portion of the cosmic rays were antimatter. We knew from particle experiments that whenever matter is created from energy, an equal amount of antimatter also forms. Simple extrapolation implies that the early Universe had essentially equal amounts of matter and antimatter present. So the primary question was: Where is the antimatter? We designed a balloon-borne superconducting magnetic spectrometer to search for antimatter in the incoming cosmic rays, the only sample of material we had from beyond the solar system. Beginning in 1971, I served as team field leader in such remote areas as Palestine, Texas and Aberdeen, South Dakota on several balloon launches designed to study the sky from a high altitude, away from misleading signals on the ground and produced in the intervening atmosphere. Our search for antimatter found no evidence of it, changing the question to: Why was there this slight part-per-billion excess of matter over antimatter in the early universe? Our limits for anti-carbon and anti-oxygen reached the one part per 10,000 level, and the observation was recognized in 1973 by the American Institute of Physics as one of the world’s twelve outstanding physics experiments of the year.  In 1973, I turned my attention back to the cosmic background radiation, which was thought to be relic from the Big Bang and it could be used to indicate whether the universe was rotating, as some had theorized, or simply expanding without rotation. It could test if the expansion was uniform in all directions and also should have residual traces from the primordial perturbations that eventually made clusters, galaxies, and eventually stars and planets. I chose to work on measuring cosmic background radiation partly because I knew this: whatever we learned would be fundamental. Regardless of what we found, our observations would tell us about the early universe. [James (P.J.E.) Peebles](https://www.nobelprize.org/prizes/physics/2019/peebles/facts/)‘s 1971 book *Physical Cosmology* helped convince my colleagues that this was a worthwhile area of research.  With the backing of Luis Alvarez and the help of NASA, we developed and used a differential microwave radiometer (DMR) mounted in a high-flying U-2 spy plane to study the cosmic background radiation in 1976. The DMR measured differences in temperature as small as one-thousandth of a degree in the microwave radiation between two points. It consisted of two rotating measuring horns at sixty-degree angles pointed through the upper hatch of the plane. The observations showed that the universe is not rotating and that it is apparently expanding with uniform speed in all directions. Those findings were important, and the only signal we found was a dipole anisotropy. The Doppler effect makes the radiation appear warmer in the direction towards which we are moving, varying smoothly to cooler in the opposite direction – forming a dipole pattern. This observation determines the direction and speed of movement. The most startling discovery was that it was in the opposite direction of our galaxy’s rotation. That dipole indicated that the Milky Way is moving at over a million miles per hour relative to the rest of the universe. This means that an object of enormous size (such as a supercluster of galaxies) must be exerting gravitational pull on our galaxy. Billions of years of acceleration brought it to this rapid speed.  Announced in 1977, astronomers regarded the discovery suspiciously, because most of them believed that objects were spread evenly throughout the universe. We won over skeptics by repeating the experiments in the Southern Hemisphere, eliminating the possibility that we had been misled by a location affect.  In a homogeneous universe, objects large enough to produce that much gravity could not have formed. The data implied that some regions of the universe are virtually devoid of galaxies while others are filled with billions of galaxies – forming superclusters. That revelation demanded a reassessment of the origin of the universe. In the era of decoupling, it was assumed that matter was evenly distributed throughout the universe, hence the cosmic background radiation (CMB). The CMB is an image of that time, and should show a uniform temperature (as tests had indicated). With the new knowledge of the uneven distribution of matter, scientists came to believe that for the universe to develop as it has there must have been some areas denser than others at decoupling, with the consequence that there should be variations in the temperature of the cosmic background radiation visible today.  Since 1974, I had worked on a proposal to NASA for a satellite to measure and map cosmic background radiation, in addition to the balloon-borne and U-2 experiments. Out of the 120 proposals submitted to NASA, my group and two others interested in cosmic background radiation were selected. In 1976, NASA told us to join forces. The collaboration was named the Cosmic Background Explorer, or COBE (pronounced COH-bee), which carried three instruments to examine the beginnings of the universe: (1) three DMRs similar to the U-2 (but more sensitive) to map the cosmic background radiation; (2) a far infrared absolute spectrophotometer (FIRAS) to measure the spectral curve of cosmic background radiation, which would tell if the CMB was truly from the Big Bang; and (3) the diffuse infrared background experiment (DIRBE) to look for cosmic infrared background – the glow from the earliest galaxies. It took six years to convince NASA that we knew what we wanted to do and knew how to do it. During that time, as head of the DMR team, I began commuting between Berkeley (where I was working on various ground-based, balloon-borne and the U-2 experiments) and NASA’s Goddard Space Flight Center, in Greenbelt, Maryland, where COBE was based.  NASA finally accepted the proposed mission and scheduled it for a late 1988 launch on the space shuttle instead of its original rocket concept, but after the January 28, 1986 Challenger shuttle tragedy, the COBE team was told that they would not have a place on future shuttles. Two other rocket mishaps in 1986 resulted in an indefinite postponement of all launches, leading the team to consider using a French rocket. Confronted by the possible embarrassment of having a major American science project launched on a French rocket, NASA gave the go ahead to launch COBE in 1989 on the last small Delta rocket, which required the size and weight of COBE to be greatly reduced. Having less than three years to redesign the satellite forced a hectic schedule. Scientists from the three instrument teams had to work closely with engineers to bring COBE to fruition. According to the Goddard Engineering Newsletter, “the transformation of COBE from [the space shuttle] to a Delta launch is probably one of the greatest engineering challenges ever undertaken by the [Goddard Space Flight Center].” On November 18, 1989, the $160 million COBE satellite was launched from Vandenberg Air Force Base in California. Controlled from Goddard, COBE traveled about 16,800 miles per hour in a polar orbit about 560 miles above Earth and completed a survey of the sky every six months.  The first important discovery showing that the cosmic background radiation resulted from the Big Bang came from the FIRAS instrument, led by Dr. John Mather. The radiation measured by FIRAS perfectly matched the blackbody curve, meaning that the CMB had a thermal origin in the early universe.  In early 1991, anisotropies at many angular scales became evident in the data from the DMRs. By fall of that year, evidence of smaller ripples, or wrinkles of temperature fluctuations, had been detected by the DMR team. In light of the many invalidated theories and discoveries, I was obsessed with detecting possible causes of error. I demanded secrecy until we were sure that we were seeing cosmic fluctuations instead of signals from the sun, moon, galaxy, the instruments themselves, or anything else. To inspire diligence in seeking errors, I offered anyone on the team two free plane tickets to anywhere in the world if they could prove a mistake had been made.  The scientific community grew restless waiting for the results, and there were papers stating that the Big Bang was in trouble since the temperature variations had not been found. There were heated arguments among the COBE team about whether the findings were ready for publication. In late 1991, we had become convinced that the analysis was correct, and had a program of tests and studies to ensure this. I wanted to check for one more possible source of error: radio interference from our galaxy that was greater and different from the meager amount we anticipated. There were already maps of our galaxy’s microwave emissions, but if they were wrong, then the DMRs’ data could be as well. Therefore, in November I led a team to Antarctica for a month to make our own celestial map and to check the brightness of the sky in a new area. Antarctica was chosen because in the cold, dry air, it has less interference from earthly signals. Even though it was austral summer and the sun never set, the working conditions were harsh. Oxygen was thin and the temperature could easily reach seventy-five degrees Fahrenheit below zero. Withstanding the elements, we confirmed previous celestial maps. After my return, I donned a tuxedo to signify and lead the formal portion of the review that certified the COBE DMR results were a success in terms of data quality, collection, and analysis. The review included NASA officials and COBE and DMR team members on the testing, checking, and progress of COBE DMR results. However, we held back on announcing the finding until the fluctuations could be extensively examined and characterized, and publications were ready with interpretation and back up.  This great care was necessary because of the technical difficulties of finding such a small signal in a large background. In *Wrinkles in Time*, I compared looking for the cosmic background radiation to “listening for a whisper during a noisy beach party while radios blare, waves crash, people yell, dogs bark, and dune buggies roar.” The key was removing unwanted signals from the picture to identify temperature fluctuations of a mere 1/100,000 of the CMB intensity. We examined the hundreds of millions of measurements in a myriad ways. The team confirmed the previously discovered dipole with relative ease, but identifying the smaller wrinkles proved difficult. The observed quadrupole was lower than anticipated compared to the higher order fluctuations (having smaller angular scale). Finally, removing the quadrupole effect from the data, I found that the observed fluctuations fit predictions for a scale invariant spectrum. To confirm this finding, I suggested to graduate student Charles Lineweaver to look at the data with the quadrupole removed, without telling him what he would find. He used a different version of the analysis program that he had written rather than the one I wrote. When Lineweaver left the office in the early morning hours, he slipped his results under my office door with the message: “Here are the plots you asked for. Eureka?” They agreed precisely with my own calculations. Everything had fallen into place.  After three more months of intensive work, and fine-tuning the details and text, the team submitted manuscripts for publication in the *Astrophysical Journal* and announced the results at a meeting of the American Physical Society in Washington, D.C., on April 23, 1992. I had the lead talk at the meeting and was followed by six other COBE members. The announcement that COBE had not only detected a quadrupole effect, the first evidence of structure in the early universe, but also smaller ripples in the temperature of the cosmic background radiation that are consistent with Big Bang theory, stunned the audience of scientists and touched off a media frenzy rarely seen at scientific conferences. Much of the attention and excitement outside of the scientific world focused on my comment at the press conference that “If you’re religious, it’s like seeing God.” Michael Turner, an astrophysicist at the University of Chicago, declared, “They have found the Holy Grail of cosmology,” and Stephen Maran, the editor of the *Astronomy and Astrophysics Encyclopedia*, said “It’s like Genesis.” The commotion was capped off by a *Newsweek* headline reading “The Handwriting of God.” These creation analogies brought widespread notice and discussion of our findings.  The detected ripples in space-time, which measure about a part in 100,000 or 30 millionths of a degree differences in temperature, started as small lumps (quantum fluctuations) but over time expanded to the immense size that COBE observed. The denser areas of the early universe, which appear slightly cooler in the cosmic background radiation, would have had sufficient gravity to attract more matter. This resulted in a snowball effect, in which more matter causes more gravity that attracts more matter, and the lumpy areas and voids become more and more defined.  The major importance of the COBE team’s findings is that it gives quantitative measures for what had been merely speculation, providing firmer ground on which to base further study. The discovery of wrinkles in spacetime neither confirms nor refutes many of the abundant variations of the Big Bang theory. Indeed, while weeding out some theories, the findings have invigorated many theories and new concepts, with scientists arguing over whether the wrinkles offer proof or refutation. This richness has resulted in a blossoming of the field and attracted many new young people to the discipline of cosmology. Successive experiments, including the satellite missions WMAP and Planck, have exploited these fluctuations as a tool to understand the properties and evolution of the universe with unprecedented clarity and precision.  In the years following this discovery and especially since receiving the Nobel Prize in 2006, I have seen that it is now my turn to be a mentor and role model as well as a spokesperson supporting basic science research in general. I want to encourage the next generation and hand over a science enterprise to my successors, which is as good or better than the excellent one I came into as a young scientist. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0050 = GS**  [George Smoot] – Hello.  [Adam Smith] – Good morning, I’m sorry to call so early, may I speak to Professor Smoot please?  [GS] – Speaking.  [AS] – Oh, hello, my name’s Adam Smith. I’m calling from the official website of the Nobel Foundation. We have …  [GS] – The which?  [AS] – The official website of the Nobel Foundation.  [GS] – Oh, I see, OK.  [AS] – We have a tradition of recording brief telephone interviews with Laureates shortly after they’ve won. Well, first of all, many, many congratulations. Presumably you were asleep when they called you, since you’re in California?  [GS] – Yes, because it was still before three o’clock in the morning.  [AS] – So, did you manage to get back to bed or have you been up since then?  [GS] – Unfortunately I’ve been up since then because people have been calling ever since.  [AS] – I’m sure, you must be exhausted. So just a few questions. You’ve been awarded the prize particularly for your painstaking work revealing that the cosmic background radiation …  [GS] – Right, co-awarded it with John Mather.  [AS] – Exactly, and indeed we spoke to Professor Mather a little earlier. But your part was particularly revealing that the background radiation contains these minute variations which are the whispers of earlier galaxies – of the earliest galaxies.  [GS] – Right, and actually galaxies and also clusters of galaxies and even larger scale structures.  [AS] – And what time are we looking back to when we observe these?  [GS] – We’re looking back to a time which is between 300,000 and 400,000 years after the Big Bang, which seems like a long time, but we’re, you know, 15 billion years, 14 billion years after the Big Bang now. So, in human terms the analogy I usually give is that it’s like looking at an embryo that’s a few hours old. That’s how far back we’re looking, in terms of – you know, putting the universe in human terms.  [AS] – It’s a very vivid analogy, yes. They were predicted to exist, these variations or anisotropies?  [GS] – Actually they were predicted to exist. They were predicted to exist at a level – in the percent level, and then at the tenth of a percent level, and then at the hundredth of a percent level, and they had to exist by the part in ten thousand that we found them. Because otherwise we’d have to have a whole new model of how the universe was put together, which was always possible, but did not turn out to be the case.  [AS] – Right. And you just needed to get up into space to actually be able to observe them?  [GS] – Right. We not only had to wait to get into space, we actually had to improve – we had a delay because of the shuttle disaster and during that time I was able to convince NASA headquarters to give us additional time and funds to improve the receiver quality so that we could actually detect it.  [AS] – So the delay worked in one’s favour a little bit. And I once heard that you offered a plane ticket to anywhere in the world for anyone who could find a mistake in your data. Is that true, and was it just a clever strategy to get rid of annoying people?  [GS] – No, that was a strategy – the problem was that once we discovered it, you know, your job as a scientist and my job as the leader of the team was to make sure that there wasn’t some mistake or something wrong. And so it was for the members of my team to try and really probe; instead of taking that we’ve just made the discovery, look at it really carefully and make sure that we haven’t made a mistake because a part in ten thousand is very tiny, and even a small mistake could cause that effect.  [AS] – And as a result of these observations, the Big Bang theory is now pretty much accepted as proven. Is that correct?  [GS] – Well, the Big Bang theory is the accepted theory of cosmology. You never prove anything completely, but it’s the accepted theory of cosmology. And we continue on, in my group, we continue on with balloon observations, and then there’s the WMAP and now we’re getting the Max Planck surveyor satellite ready with the European Space Agency, who is sponsoring that. So there’s a whole sequence. What it was, was that was the opening shot and saying OK, there’s some gold to be discovered in the hills, go looking for it.  [AS] – So the cosmic microwave background radiation contains yet more, unrevealed information which you’re now looking for?  [GS] – We don’t know if there’s more unrevealed information. The better we measure the more precisely we’re going to know the general parameters of the universe.  [AS] – Presumably it also contains the signatures of more recent cataclysmic events? Is it just that the strength of the signal from the early universe is so strong that it masks what happened later?  [GS] – It’s the strength from the early universe that we’re really interested in, so we choose the places where we look and the frequencies that we look at in order to emphasize the early universe. Clearly you can see the imprints from clusters of galaxies and from other things in that, but they’re generally on different energy scales and in particular places in the sky, and so you try and either average over them, or avoid those regions, including our own galactic plane.  [AS] – Right. I’ve kept you for long enough but I just wanted to ask whether you’ve had any time to think about how you’re planning to celebrate today, you and your team?  [GS] – My problem is I have to finish making my breakfast (which I’m doing right now) and then I have to rush in because we’ve scheduled a press conference for 10. And then I’ve got to get my – I have a mid-term exam to give tomorrow and I’ve got to get that finished, and ready for my students.  [AS] – Life goes on. Well, we were very lucky to catch you for these few minutes. Thank you very much and we speak at greater length when you come to Stockholm in December so I hope we can continue ….  [GS] – OK. Well, I look forward to that. I’ve got to figure out how to schedule my final exam so I can come.  [AS] – I’m sure your students will be as helpful as they can be.  [GS] – Be understanding? I don’t think so!  [AS] – It’s not the nature of students.  [GS] – I’ve got 170 students. I bet only a few of them will understand.  [AS] – I’m sure they will be very proud though. Anyway, once again, congratulations, and thank you for talking to us.  [GS] – Thank you very much.  [AS] – Bye, bye.  [GS] – Bye. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0051** |
| **Biographical** | What is it that makes a dedicated scientist out of a kid with an everyday background? Is it the ungovernable forces that seem to shape all our lives, or is it the development of our own curiosity and tastes that tips the balance of randomness? I’ve always been puzzled by those questions and can’t claim to have found serious answers. Perhaps these recollections will reveal one, even if it escapes me as I write.  To be a traveling salesman in the 1920’s gave one possession of a company-owned car, acquaintance with a potentially vast area of the country, and a slightly better income than one would earn within the tight confines of New York City. My father, having enjoyed some experience with that life before getting married in 1924, couldn’t wait to get back on the road, a possibility he had to postpone for about three years until his wife and new-born son, at last aged two, were ready to travel. The itinerant life was a restless one and quite disconnected. After long hours spent driving through endless farmlands we would stay overnight at the houses of farmers who had hung the sign “Tourists-Vacancy” near the road – never two successive nights in the same house. That was long before the days of roadside motels. Even hotels were scarce in some of the small towns we visited. Rural electrification was not yet a reality and I became quite accustomed to the smells of illumination by kerosene and acetylene lamps, as well as all the odors of barnyards and outdoor plumbing.  The periods in which my father was visiting customers, in whatever small town we were passing through posed a problem for my mother. Trained as an elementary school teacher, but pregnant before she had begun teaching, she was determined to make these passages as instructive for me as she could. The most interesting place in each town, as well as I could make out, was the fire department. We received guided tours of their living quarters and fire engines all over the Midwest. Where fire departments were lacking, visits to assorted courtrooms, police departments and even local lockups would do for my introduction to civics.  In one Cleveland hotel room when I was four, we actually had a radio. It occupied a wooden cabinet about the size of a steamer trunk. I remember insisting there must be a man inside it. He had given his name as Maurice Chevalier. Discovering that the cabinet top was hinged, I opened it and can still feel my bafflement at discovering within it only a few glowing radio tubes.  The 1929 market crash had an immediate impact for me. The company my father was working for failed and the car we had been using was repossessed. The result was my first ride on a train, an exciting experience that there was no occasion to repeat, once my father had another job and a most imposing new car, a Marmon, a kind of Cadillac of its day, and one of many brands destined for early extinction.  The arrival of a baby sister in 1931 and my need to begin school meant that we had somehow to settle down. My folks decided we would return to New York, but the only way to do it, under the circumstances, was for our family to move into a crowded apartment in upper Manhattan with my father’s mother and aunt. It was quite a shock, moving there from the wide-open expanses of the middle west, to sit in the crowded classrooms of an ancient school building. I had had very little experience playing with other kids in the small towns we had visited, and had no idea how to deal with the crowds of kids who managed somehow to play on the concrete sidewalks and in the adjoining gutters.  My mother was talented at crafts of various sorts. She sewed and embroidered well and, though untrained, she sketched and painted quite skillfully. She encouraged me to draw as soon as I could hold pencils or crayons steadily. That was the beginning of my career as a creative artist, specializing in speeding trains, airplanes and the occasional dirigible. It was a necessary release from the need to get fresh air by playing on the sidewalks.  After one school year in Manhattan, we found an apartment in Sunnyside, an attractive area of Queens, and moved there in 1932. It was a kind of deliverance. The neighborhood was spacious and not yet fully built up. It consisted largely of modest single family houses that all had at least a small area devoted to yards or gardens. All the blocks of individual houses and even our apartment building had central areas of lawn with space for children to play – not withstanding all the “Please keep off the grass” signs. There were even vacant lots with tiny hills and semi-permanent puddles that lent excitement to the four daily treks to and from school.  Sunnyside’s residents consisted mostly of young families, quite a few contending with the unemployment so widespread in those depression years. The building for Public School 150 however was clean, well lit, and quite new. Its teachers were mostly young and optimistic, a vigorous contrast to the atmosphere that prevailed more generally in the country. The school’s annual Christmas play, written by the fifth and sixth graders in 1932 was entitled, “Santa’s Depression.” It depicted Santa Claus as being broke, and unable to afford to make his usual rounds, until everyone pitched in to help him out. In the same period there were demonstrations against foreclosures on home mortgages going on in the neighborhood streets. Those depression years cast a long shadow over the lives of children no less than their parents.  My earlier years had left me with no experience in sports of any kind so it wasn’t easy discovering how to engage in outdoor exercise. Unlike Manhattan, however, Sunnyside had many residential streets with little traffic. The best solution to my exercise problem was roller skating – with steel-wheeled skates that clamped to one’s everyday shoes. Those steel wheels were quite noisy rolling or scraping on concrete, notwithstanding their good ball bearings, and I wore down to those very bearings many a set of skate wheels, cruising the neighborhood streets.  Electricity mystified me throughout childhood and I vividly remember once at age seven trying to see what it was all about. Plugging lamp cords into wall sockets must lead to the flow of something through those wires, but whatever it was, one never got to see it before it was swallowed up by the lamp. One morning I awoke early, determined to catch sight of it. I screwed the wires of a short length of lamp cord into a male plug and inserted it into a wall socket, leaving free the frayed wires at the other end. There was a bright blue flash at that end, accompanied by a muffled bang. That was followed by silence, till my parents awoke and began wondering why none of the light switches seemed to be working. The fuse was easily replaced, but I never overcame my surprise that what passed so silently through slender wires could behave so aggressively.  My most interesting projects were the ones I could pursue indoors like building models of contemporary airplanes of all shapes and of ships and locomotives. I had no cash allowance to spend on such projects and so was wholly dependent on gifts of construction kits from uncles and aunts. When those were scarce, as they sometimes were, I ventured into other areas, attempting to use crate wood to construct the projects suggested in various instruction books written for young boys. The most interesting of these projects usually failed, and I began to conclude the authors could never themselves have really built the exciting things they were describing. Their version of a guitar, for example, fashioned from a cigar box and some cheese box wood never had the rigidity to permit stretching a guitar string tightly enough. My guitar looked a bit like one but couldn’t sound a single note.  There were other failures, many of them, but each brought new experience in the use of hand tools. An uncle, to encourage this construction bent, presented me with a three-year subscription to Popular Mechanics magazine. That magazine, besides celebrating all of the mechanical wonders of the age, included brief plans for all sorts of home projects: door chimes, folding tables, towel racks, bookends and knife sharpeners. The subscription did a great deal to keep my interest in mechanical things alive, but I can’t say I ever succeeded in building any of those worthy projects. And I doubt that anyone who didn’t have a machine shop at his disposal ever did either.  All the sawing, drilling and sanding I was doing at home left little time for drawing and painting, but there was ample opportunity to pursue those interests in school. Tempera paints were available there, a certain amount of free time, and a good deal of encouragement from the teachers, who felt a need to keep the backs of their classrooms decorated with mural paintings executed by the kids. They were painted over large areas of brown wrapping paper that covered the rear blackboards. I enjoyed designing those huge works and loved the freedom painting them gave me from sitting at my classroom desk.  The school produced a magazine every term and when my design for the cover of the Christmas 1935 issue was accepted, I felt like the Michelangelo of the fifth grade. In fact I did have some involvement with sculpture as well. Small carvings in soap, greatly encouraged by the Procter and Gamble Corporation, were a medium of the day, and I made many of them, mostly of musicians playing instruments. But a more conventional medium was Plasticine clay, which remains permanently soft. Those sculptures tended not to last long, but we managed, with a teacher, to take a few to a real sculptor’s studio, and I was fascinated there to learn to make plaster molds and permanent castings.  If I was fully determined in the fifth and sixth grades to become an artist of one sort or another, it was not without a certain note of caution. My uncle, Sam Adler, was a gifted artist who had not yet succeeded in selling any of his work, nor did those years seem to promise that he ever would. His advice to me was that becoming an artist was an excellent idea, provided my motivation was so strong as to leave no alternative. I began then to feel that my artwork was not spontaneous enough, that if I were a true artist I shouldn’t have to think so hard before even starting drawings; they should just pour out more instinctively. My involvement with art receded to a hobby.  The years in which the depression lingered must have been difficult ones for the owners of apartment buildings. Faced with many vacancies, they offered rent-free months and other incentives to new tenants, so there was always a certain degree of restlessness among the city’s apartment dwellers. In 1936, when I was ten, my parents decided that the higher ground of the Bronx – and the top floor of a six story apartment house – would be a better place to live than the flat sea-level expanse of Long Island. A precipitous increase of the local population density went with that move, and it became once again impossible for me or my sister to spend much time outdoors, in the streets. My first salvation was reading. I visited the local public library regularly and began reading the great adventure stories of Jules Verne, Alexander Dumas and Walter Scott. The junior high school I went to seemed mired in a curriculum too timid to do anything serious, and altogether flat-footed at what it did undertake. Mathematics, I remember, consisted of memorizing the decimal equivalents of the familiar “business fractions” and doing compound interest calculations out longhand. I was so put off by those lessons I occasionally got failing grades. Our premature introduction to French required our memorizing a list of proverbs which didn’t literally translate into their English counterparts.  That junior high school experience was typified by what was called “music appreciation” in the auditorium assemblies. The principal, a Mr. Snyder, had himself written words to accompany several dozen themes of the great works of music. Singing his rhyming words he evidently felt, set to the themes of the great composers, should imprint those masterpieces on our young memories. Indeed they did, but it was at the expense of burdening those themes permanently with his infernal doggerel.  But that school did offer my first exposure to science and it was exciting. We were shown how to coil wire around nails and make them into electromagnets with the current from dry cells. Those 6 volt dry cells, widely used to power doorbells, cost 25 cents in the local 5 and 10. From that time on I was never without them.  My ambition to be an artist was further dampened by an art appreciation course largely devoted to biographies of the less scandalous painters, and punctuated by black and white lantern slides of their masterpieces. That course also had a creative element devised to avoid, at all costs, creating any sort of untidiness in the classroom. I was encouraged to draw with pastel crayons, again on a large sheet of wrapping paper hung at the back of the classroom, while the other kids who felt less inclined toward art, were set to copying mounted cartoon panels on drawing paper. Neither the pastels for me, which were intended evidently to make the room look like an art class, nor the cartoon exercises for the other kids, seemed to have any instructive value.  My lingering interests in art presently became centered on puppetry and marionettes. The instructions I saw for making them in some magazine articles and a handbook seemed to offer an interesting combination of sculpture and construction. After fashioning several puppet heads of papier maché and painting them, I set about constructing their marionette bodies and string controls. When it came the turn of our class to present a play in the school auditorium I volunteered to produce a small troupe of marionettes and a stage appropriate for the class presentation. Our decision to stage the fairy tale “Rumplestiltskin” turned out to set a more imposing task than I had imagined. Fortunately my mother came to my aid, offering not only to costume the marionettes but to help in constructing several. The task kept both of us busy for a solid month. The eventual presentation by the class, speaking for and operating the marionettes, must have been some sort of success since we had to repeat it several times. But I was ultimately embarrassed by the fact that so much of the work had been visibly my mother’s, and resolved that any further projects of mine would be wholly independent.  When did my interest in science become more serious? It really wasn’t too serious, I’d have to admit, until still another change of location. My parents, realizing in 1937 that the move to the Bronx had not been a success, decided to move to an apartment at the north end of Manhattan. We lived in a more spacious neighborhood there and across a peaceful street from Inwood Park – the only uncultivated area left in Manhattan. The school I attended there for the ninth grade, which was nominally the first year of high school, was materially less boring than the prior year’s. Algebra was an altogether new beginning and even a redemption for mathematics. It was finally freed of all that dismal arithmetic. That was a joy more than sufficient to overcome the uselessness of so many of the procedures that the curriculum did include. Who, even in those days, could imagine seriously needing to carry out long division of lengthy polynomials, or see any need to teach that procedure to children? No one with experience beyond teacher training could have been responsible for that curriculum.  But general science was another of the subjects we studied, and the energy and enthusiasm of its young teachers more than made up for its attenuated subject matter. I had read an elementary book on astronomy by that time and had been taken by my Aunt Sarah on an exciting visit to the Hayden Planetarium. I found that I could easily visualize the diurnal motions of the stars, the monthly motion of the moon, and somewhat more sketchily the motions of the readily visible planets, Jupiter, Saturn and Venus. The images associated with astronomy quickly captured my imagination, and I began to read everything about it I could find.  The encyclopedia had some simplistic diagrams of how a telescope works, and they seemed to assure me that I could build one from some ordinary magnifying glasses I had accumulated. I did that and was amazed by the rainbow colored edges I saw on the image of the moon and presently dismayed by its overall fuzzy quality. It took a bit of reading to discover what the trouble was – chromatic aberration, endemic to all such primitive refractors. The cure – the only one accessible to me – would be to build a reflecting telescope. But that would be a long-term project, fortunately one that had already been pioneered by quite a few adult amateur astronomers. There was a book, in two volumes, in fact, that drew together the experiences of several amateurs and gave a good deal of guidance, if not detailed instruction, for grinding, polishing and figuring the mirror, and for constructing the remaining optical elements and the mounting. Going through the entire procedure required nine months of work. Coarse and fine grinding of the mirror took only a couple of weeks, but polishing and figuring it to its final shape consumed months. Building a stable yet flexible mounting for the telescope, one that would permit me to follow objects in the sky, compensating for the earth’s rotation was another matter entirely. I had only a few hand tools appropriate to woodworking, and still no access to machine tools of any sort. Constructing the wooden cell to house the mirror involved strenuous use of a coping saw and a wood file for several days on end.  The steel polar axis for the equatorial mounting was originally the steering shaft of a Ford car. The proprietor of the junkyard I found it in was happy to give it to me free. But I had somehow to put a 41° bend into that shaft, to equal the latitude of New York. I drew an outline of the bend I needed on a sheet of asbestos and took it off together with the three foot shaft to a garage that I knew refashioned truck housings. The owner was tickled by the project, heated the shaft in his forge till it glowed brightly and pounded it into the precise shape I needed. It must have taken him a good three-quarters of an hour altogether, and I felt I owed him payment for his time. He thought the matter over, and I recall his smile as he said that would come to 25 cents. In fact I got a good deal of aid over those months from people who were pleased to help an ambitious kid with virtually no money to spend on his projects. My accumulated savings of $10 were no more than half spent during those nine months.  Stability of the telescope mounting demanded that it be fairly massive but not too heavy to be carried by hand. The only way I could use it, after all, was to carry it upstairs to the roof of the apartment building. A weight of 40 or 50 pounds seemed appropriate for the base of the mounting, and I would have to make it of cast concrete. A schoolmate kindly brought me a sack of cement and a bag of sand contributed by his father, a local contractor. I fashioned a mold of the appropriate shape from recycled box wood and filled it with the concrete mixture called for by the instructions on the cement sack. The only place available to me for the casting operation was the wooden floor of my bedroom, between my bed and work desk. I had taken some precautions against the leakage of a little water by covering the floor first with waxed paper and a layer of newspaper. My understanding of the setting of concrete was that some miracle of chemistry would incorporate all of the water into the finally hardened product, with none left to leak out onto the floor. That is how plaster of Paris had hardened. But the result was a memorable lesson. I couldn’t say what fraction of the hardening was eventually due to drainage, but it must have been appreciable. I had to spend an entire day mopping up pools of water around the hardening mass. I suppose when concrete sidewalks harden their leakage just seeps into the ground below. In my case it would have been the apartment downstairs.  Observing with the telescope wasn’t too easy either. In winter the apartment house roof was cold and often windy. Because of the city lights the sky was rarely dark enough to permit seeing the fainter objects, usually diffuse nebulae or distant galaxies. Still there were the thrilling topography of the moon, frequent views of the major planets and countless planetary nebulae, double stars, and clusters of all sorts. Lacking the means to find the fainter objects mechanically, I had to go about tracking them down by locating their positions on star maps relative to the brighter stars or objects easier to find. By putting in at least a little time on most clear nights I managed over the next year or two to find most of the hundred or so extended objects catalogued by the Italian astronomer Messier. I even managed to fashion a film holder and cardboard shutter for the telescope so that I took through it a sequence of moon pictures during the lunar eclipse of November 8, 1938.  The possibility of performing optical tests as exquisitely sensitive as the Foucault test of the telescope mirror’s figure with even the most primitive sorts of equipment convinced me that optics was full of miracles. Some other miracles I had seen involved the mysteries of light polarization. The Polaroid Corporation was sponsoring an exhibit I had visited at the Museum of Science and Industry at Rockefeller Center that showed, among other things, the remarkable colors that appeared in transparent materials like cellophane when seen between crossed sheets of Polaroid film. How could I procure any of the magical Polaroid film? That seemed hopeless for a 12-year old, but I had heard of the possibility of light polarization by reflection. The best reflectors for the purpose would be smooth and black – to avoid the complications of transmission. My father, who at that time in 1937 was selling jewelry displays made of just such black glass, found me several rectangular pieces of the right size. I was able then to mount all the optical elements, including a 25 watt light bulb within a cigar box and use the device to reveal the same sorts of polarization phenomena I had seen at the museum. Seeing the unseen in that way turned out to be as much of a thrill as any I had with the telescope.  In the late 30’s an organization with the imposing name The American Institute of the City of New York began organizing activities for young people interested in science. They held science congresses during the Christmas vacations and science fairs during the spring school break, both at the Museum of Natural History. The science congresses were patterned after professional scientific meetings, and split just as incoherently into many sessions, according to fields and specialties. Each session had several ten minute talks presented by the kids as contributed papers. One of those presentations in 1937 was my own description of the plans for the forthcoming 200-inch telescope at Mt. Palomar. It was a visionary image that kept my spirits up while I was having troubles of my own building my 6-inch diameter telescope. The sponsoring Institute saw to it that our talks were attended by at least a sprinkling of mature scientists whom they could somehow persuade to volunteer. I was flattered that my own talk was attended, if only briefly, by Dr. Clyde Fisher, the curator of the Hayden Planetarium. One of his assistant lecturers, Dorothy Bennett, stayed for the whole ten minutes and dropped a suggestion to me that added immensely to my experience over the next four years.  Dorothy Bennett was something of a wonder. Seeking a career in New York, she had arrived there as a fresh graduate of the University of Minnesota just in time for the economic debacle of 1929. With boundless energy and no prior acquaintance with astronomy she found a position working on the plans for the City’s new planetarium. One of her many inspirations was to begin in 1930 a citywide astronomy club for kids of high school age. It met on Saturday evenings biweekly, in an imposing auditorium on the top floor of the Roosevelt Memorial building, adjacent to the Planetarium. There the kids, who came in by subway from the far reaches of the city, heard invited lectures by real astronomers. It was that club that Miss Bennett suggested I try attending. I was indeed excited by it and caught up in it from the first meeting I went to. It then formed a large part of my life till I went off to college.  Watched over by Dorothy in a kind of godmotherly role, the Junior Astronomy Club actually had a permanent office in a former watchman’s apartment in the basement of the Roosevelt Memorial. There it held committee meetings, originated large mailings to the membership and ground out its monthly mimeographed publication, the Junior Astronomy News. I rushed to take part in all of those programs, ceaselessly amazed that the club could manage all of its activities on dues that only came to 25 cents per year. The secret of that miracle was that Dorothy had assigned to the club the royalties of a book she had inspired,*The Handbook of the Heavens*, and the proceeds from the sales of a rotating star map, a planisphere she designed. Enough copies of those publications had been sold to keep the club afloat for over ten years. Dorothy left the planetarium for a position in the publishing industry in 1939, entrusting supervision of our club to a group of its older alumni, who carried on the tradition for quite a few years more.  I often wondered what happened to Dorothy in the years after that. She didn’t just vanish into the publishing world, I found. Within a couple of years she had become the originator and editor of the Little Golden Books of Simon and Schuster. Those small paperbacks, devoted at first to assorted topics in natural science or history, became one of the wonders of the publishing industry. They were colorfully illustrated and were sold in vast numbers at newsstands and stores everywhere. Countless kids must have owed their knowledge of fossils, seashells, or trees to those books and to Dorothy. When eventually the publishers decided to extend their franchise into more commercial and less educational material, Dorothy left them and took up a succession of new careers in archaeology and ethnography. Her adventures extended to many other novel areas of public education.  In September 1938 a new high school was opened by the City, with the declared intention of providing a more extensive background in science. That school, the Bronx High School of Science was to have an entrance examination and a freshly chosen staff of young teachers. It was established however in an old building still used as an annex for a traditional local school, and three years had to pass before its growing student body had displaced the more disaffected population originally present. It was interesting being a pioneer in this way, but not without problems. Although the two populations didn’t overlap in classes they did – and experienced friction – everywhere else.  My choice of this high school required long trolley car rides between upper Manhattan and the Bronx, but it proved fortunate in several respects. The kids were better informed about most things than average high school kids, and were often interesting to talk to. Not many of them entertained ambitions of becoming scientists however. They were there, mainly, it seemed, in search of somewhat higher educational standards. The lawyers, doctors and businessmen who emerged from my cohort, in fact, greatly outnumbered the handful of eventual scientists. Although all high schools offered some elective courses, it would have been difficult in most of them, to take both a science and a math course in each year. If we were able to do that, it was at the expense of studying Latin or taking a second modern language course. I was more than pleased at the time by those omissions, but have come to regret them since.  Whatever may have been the weaknesses of the school’s physical plant or its curriculum, the faculty members seemed to make up for them. They were mostly young, energetic and unjaded. We seemed to have the depression years to thank for that. Most of the teachers had graduated from the tuition-free city colleges during the early 30’s and, seeing no future in continuing their studies, had taken refuge in positions with the school system. The subjects they taught, like European history and economics, seemed to have real substance, for a change, and mathematics stood out among them. It was the real thing, not just an introduction one would have to repeat and improve upon in college. When algebra became more serious in the second year of high school it became more interesting. My teacher in intermediate algebra, Samuel Altwerger, appreciating my involvement with astronomy and my growing enthusiasm for mathematics, suggested that it might be a good idea for me to learn calculus. He assured me I could learn it just by reading a textbook. He gave me one small book for that purpose and borrowed a larger one for me from the library. I found, to my surprise, that he was right. I had no trouble with these and absorbed an understanding of elementary calculus quickly. In fact that was well before I really needed calculus, but the experience marked a kind of turning point for me. I had never felt inclined toward mathematics before, but what I had learned by the time I reached college permitted me to skip several elementary courses there.  However much I came to like mathematics, my passion was still building optical instruments. I had been reading about the pivotal role played by spectroscopy in developing an understanding of atoms, and I resolved to build a spectroscope myself. Most of its parts would have to be made of metal, and that meant even more numbing use of hand files, this time not on wood, but brass. It wasn’t difficult putting the spectroscope together. Neither its structure nor its optics presented other problems. But there was one central element missing. I had neither a prism nor a diffraction grating to use as the dispersive device that generates the spectrum. Fortunately the principal of the new high school, Dr. Morris Meister, had been given a replica diffraction grating as a graduation present, and he was happy to loan it to me. That spectroscope, entered in the 1939 science fair, won two prizes. I had very little chance to use it after that, since the American Institute exhibited it over many months in a display case at the New York World’s Fair of 1939 and its repetition in 1940.  The Junior Astronomy Club also had an involvement in those World’s Fairs. Part of the extensive Westinghouse exhibit was devoted to the scientific hobbies of kids of high school age. I was happy to organize demonstrations of the grinding of telescope mirrors for the exhibit and enlist a succession of our club members, each to spend a week or two on public display at the task. When my own turns came I became good friends with the young chemist who worked next to me, notwithstanding the shower of ashes his synthetic volcano blew over my optical surfaces. Young Frank Pierson never did become a chemist. He became a well-known screen writer and for several years president of the Academy of Motion Picture Arts and Sciences.  In the Science Congress of 1939, I gave a talk that presented some of the photographs I had managed to take through my telescope, my spectroscope, and through a borrowed microscope. It won one of the prizes, a visit to the Westinghouse Corporation in Pittsburgh, Pennsylvania, where I had a chance to visit their “atom smasher”, a vertically mounted Van de Graaf generator, and to talk briefly with a couple of real scientists, including a well known theorist, E. U. Condon. Then, as a climax to the trip, I was ushered into the office of the president of the corporation. He promptly drew from his top desk drawer a tattered old pocket notebook. It was his official record, he explained, of the hours he had worked for the company at the turn of the century for a wage of only a few cents an hour.  The junior year of high school meant starting to think about going to college. The teacher assigned as my guidance counselor, thinking perhaps of the experience of his colleagues, assured me that there were too few positions available anywhere for astronomers or physicists, and that I would be best off going to an engineering school. He felt I should apply to a range of them, but he saw Rensselaer Polytech as the ideal compromise. The father of my best friend, a Harvard graduate himself, gave me rather different advice. Disappointed at the unlikelihood of his own son’s admission to Harvard, he guessed that I might make it. More to the point, he suggested that scholarship support could be available. Neither my parents nor I would otherwise have been so presumptuous as to imagine that large a leap in social status. I did fill out the lengthy Harvard applications, however, and take the several required examinations. The application for the scholarship awarded by the New York Harvard Club involved a searching interview conducted in a large, oak-paneled room by a dean and half a dozen club member contributors. I was eventually admitted to a number of colleges, including Rensselaer , but without scholarship aid. Harvard, on the other hand, granted me a Harvard Club scholarship, while making it clear that there were many more exams to take before I would be declared admitted.  Beginning at Harvard in the fall of 1941 meant suddenly being treated like a member of the gentry. We had waiter service at our dining tables and daily printed menus listing alternative dishes. Of course, some fraction of the waiters were fellow classmates, working for board. Our society was stratified in many other ways as well. The rents for the dormitory rooms were graded according to their location, with the result that the scholarship students were clustered in the less desirable areas. They never got to meet the occupants of the higher priced real estate. I scarcely minded any of that. I had come from a different world than those normal Harvard students. College was for them primarily a social experience, overlaid by a burden of course work. For me, on the other hand, having skipped a couple of grades along the way, and some two years younger than most of my classmates, it was the other way around. I enjoyed a few social contacts, but worked hard at my studies, finding them demanding at times, but on the whole well planned and satisfying.  That freshman year was punctuated on December 7 by the Japanese attack on Pearl Harbor and by the entry the next day of the U.S. into the war in Europe as well as the Pacific. The next few months saw significant changes in our lives as students. The rather searching physics course I was taking had been planned as the first half of a two year cycle. Because faculty members were departing for war work the remaining year-long course would not be offered as planned. It would instead be packed into the second semester of the first-year course. That proved to be quite a tall order but a fast way of learning.  The entire school then began operating during the summer and accelerating its course programs with the thought of providing as much education as possible before the young men left for the armed forces. In the meantime Harvard’s dining halls lost their graciousness and were transformed into the cafeteria-style mess halls they have been ever since. The draft age, then 21, was presently lowered to 18 and the university began losing students in large numbers. With its faculty depleted the Physics Department announced that its graduate courses were shortly to be given for the last time “for the duration.” That announcement made it a good idea to jump directly into the graduate courses, skipping the intermediate ones which had looked neither demanding nor very interesting anyway. It was with the war thus cracking the whip that I managed to assimilate most of the courses of a graduate school education by the time I turned 18 in September 1943. At that point I felt ready for war work myself and filled out a questionnaire sent out by an agency called the National Roster of Scientific Personnel. Its purpose was to ascertain scientific training and try to place people accordingly.  The armed forces by that time had become vastly larger than the country’s immediate needs. The Army developed what it called a Specialized Training Program, in effect for storage of its legions in the universities for a year or so, until they would be needed in the invasion of Europe. The program exposed a large population of draftees to college courses for the first time and was a productive experiment in education. I was given a position teaching elementary physics in the program and had my hands full doing that along with taking a full program of courses of my own.  Then one day in October 1943, a stranger in a dark suit appeared in the Physics Department office evidently asking for me. He introduced himself as a Mr. Trytten from Washington, D.C. and asked to speak privately to me. We withdrew to a faculty meeting room in which the blinds were never raised. Closing the door, he asked if I would be interested in joining a new project that was engaged in interesting work. That it was “out west” was the most he would tell me about either the location of the place or what it was doing. It sounded fascinating nonetheless, and I found the security questionnaires he put before me easy to fill out. Having so little prior history helped. His seeking me out seemed to relate to my having filled out the National Roster blanks. It was then a matter of several weeks before my security clearance had been completed and I was instructed to send whatever belongings I needed to the now famous Post Office Box 1663, Santa Fe, New Mexico. In my case it was a trunk sent not by mail but by Railway Express. There were many occasions, then and later to imagine what a capacious P.O. box that one must be.  I could find many tiny hints at what was going on out there, all of them questionable and several, as it later became clear, completely wrong. The most solid hint was in fact a negative one. For about two years after the discovery of uranium fission in 1939 there had been occasional notes in the New York Times speculating on the possibility of starting a chain reaction. They had stopped appearing, it was hard to say just when, but at least two years earlier. So I had no idea whether it had become a dead issue, or my offer of a position implied some real progress toward a chain reaction.  The train ride from New York to Lamy, New Mexico, the stop for Santa Fe, consumed two and a half days. A driver from Los Alamos had come to the station principally to meet a short man in a black overcoat, but took me along, stopping first at an unassuming project office in Santa Fe, where I learned that the man in the overcoat was John von Neumann, a legendary mathematician.  The ride from Santa Fe up to “the Hill” was an experience I shall never forget. First there was the breathtaking scenery of the canyons of the Pajarito Plateau. Then there was my fellow passenger, John Von Neumann, who engaged in a lively conversation for most of the trip with the driver, whom I learned only later was a mathematician who had worked with “Johnny” earlier. With a thought perhaps of maintaining security, they discussed some calculations underway using the most outlandish mathematical terminology, and describing mathematical errors in physical terms that I knew represented physical impossibilities. The ride was an incredible mixture of visual thrills and intellectual enigmas.  I was astonished, shortly after arrival at the project, to be told that the chain reaction had long since been achieved in Chicago and the present intention was to construct a reaction fast enough to be a bomb. It was disturbing news and I recoiled from it at first, but the challenges and uncertainties involved helped reconcile me to it. More importantly, I felt, as everyone else on the project did, that whatever these uncertainties might be, the Germans, possessing the same understanding we had, were likely to be working on the bomb as well. And if they reached that goal before we did they would not be sentimental about using it to stave off eventual defeat. That fear applied only to the known expertise of the Germans. The conflict with Japan didn’t appear to motivate anyone’s involvement in the project.  The project was only a few months old when I joined it but most of its eventual leaders were already there. Not many were yet well-known. They were remarkably youthful. Oppenheimer in his late thirties was one of the oldest. He had a universal understanding of the work and an eloquence in describing it that kept us spellbound. [Hans Bethe](https://www.nobelprize.org/nobel_prizes/physics/laureates/1967/index.html), the leader of the theoretical division, had a penetrating understanding that seemed capable of formulating absolutely anything quantitatively and evaluating it effortlessly, an aura he maintained even many years later. [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) was there as leader of a small theoretical group. He was often cantankerously teasing the security people. His lectures were always offbeat performances demonstrating novel approaches to problems in ways devoted as much to entertainment as to the technical message. There were others, too many to mention, and among them as an occasional visitor, [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html), whom we called Nicholas Baker for obvious reasons, together with his son [Aage](https://www.nobelprize.org/nobel_prizes/physics/laureates/1975/index.html).  Overwhelmed by these giants, my own position in the Theory Division at age 18 was a modest one. There were many problems in neutron diffusion such as finding the critical mass that required more careful formulations than had been carried out in the earliest projections. I worked on those for the better part of the two years I spent at Los Alamos and wrote three lengthy secret papers on those subjects.  There were many delays before the Trinity Test of the bomb in July 1945 and with them the uncertainty of how well it would work grew steadily. Unable to secure a position among the experimenters at Alomogordo, I had to be content with watching for the flash from the top of Sandia Peak near Albuquerque. I saw the flash indeed and some of the glow that followed from a distance of over a hundred miles. The test was followed by some tense days, leading up to August 6, when the use of the bomb at Hiroshima was announced. One thing the portentous announcement meant was a certain release from secrecy. We could now resume contact with the outside world. We could say, if only in general terms what we had been working on. But there were no celebrations of any sort until the war was over a few days later.  Resuming the life of an undergraduate at Harvard early in 1946 proved surprisingly difficult, even though I needed only a few credits to graduate. Having had a team of assistants to do calculations for me at Los Alamos didn’t make it any easier, I found, to do my own homework back at school, particularly when I felt I had moved beyond all that. Fortunately that time was brief, and then I became a graduate student. But I had already taken most of the graduate courses on offer, and so was largely left on my own, being allowed to register, in effect, for independent reading and research. The principal reason for my remaining at Harvard was the addition of [Julian Schwinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html) to the faculty. I had met him during a brief appearance he made at Los Alamos, late in 1945, and was immediately so impressed with his knowledge and his incredibly informative lecturing style that I felt he was unique among teachers and would be the ideal thesis advisor as well. I became friendly with Julian over the next three years and was never less than amazed by his ability to construct elegant mathematical structures that would permit him to see further than any of his contemporaries. There were times in those postwar years when it seemed he was responsible for most of the progress in theoretical physics, and very likely would be for years to come. His lectures were brilliantly delivered and notes on them were highly prized and reproduced wherever they could be found. Many students crowded in to work with him, however, and he limited the time he spent with them, so they didn’t always produce great theses. Though nominally registered to work with Julian, I actually worked by myself and produced in 1949 a quantum field theoretical thesis that was useful to my later development but scarcely much better than the others of the day.  Robert Oppenheimer, who seemed to know more of me than I had imagined, invited me to spend my first postdoctoral year in Princeton at the Institute for Advanced Study. The group of 20 or so postdocs who gathered there included quite a few eventual leaders of the postwar generation of theorists. None had a stable position anywhere else and so the atmosphere was quite competitive. In the first term of 1950, [Wolfgang Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html) was scheduled to visit the Institute. Following the advice of friends who had worked in Zürich, I arranged with Pauli to return with him to Zürich and work with him until the fall of that year when I would return to Princeton and the Institute. Having a few months to live in Zürich and to travel over Europe was the principal experience of that encounter. Pauli at age 50 had relaxed into the role of a critic and was no longer inspiring much research. He did retain a mordant sense of humor, however, and was forever doing his best to tease me. Teasing others as well, if not insulting them outright, he was always interesting to be around.  After another year I spent at the Institute, Oppy found me a teaching position. It was only a temporary one, replacing Feynman at Caltech. Feynman was to spend the year in Brazil, where by his own account, he worked hard on the bongo drums, and Caltech needed someone to teach quantum mechanics. The chemistry department out there, under [Linus Pauling](https://www.nobelprize.org/nobel_prizes/chemistry/laureates/1954/index.html), seemed to be an exceptionally active one. My research for the year was devoted to resolving a puzzle they had encountered in studying electron diffraction by molecules. Solving the problem didn’t interest me in molecules very much, but it did involve me deeply in scattering problems in which the incident particles were of wavelength much smaller than the ranges of interactions. Those problems, I understood, would become steadily more important in nuclear physics as accelerator energies were increased. I continued studying those problems then when I was invited back to Harvard in the fall of 1952 and for some years after that. The result was a species of nuclear diffraction theory analogous in some ways to optical diffraction theory, but generalized to include inelastic collisions between incident particles and complex nuclear systems. The theory is even used these days to treat the high-energy collisions of pairs of heavy nuclei.  Once I was back at Harvard I began to climb the academic ladder of professorial positions and was able to direct the thesis work of a number of gifted students. Theoretical physicists weren’t nearly as specialized in those days as they are now. All of theory was considered one’s province and so those theses ranged over half a dozen fields, as did my own work.  The late 50’s proved to be an exciting time for many reasons. A radically new light source, the laser, was being developed and there were questions in the air regarding the quantum structure of its output. That was particularly so in view of the surprising discovery of quantum correlations in ordinary light by Hanbury Brown and Twiss. A second source of excitement, all my own, was that I had met the young woman I was to marry, Cynthia Rich, and had been going out with her since 1957. We married in July 1960, bought a contemporary house a year later, and settled into quite a happy life together. That was the period in which I began to work on quantum optics with a surmise that the Hanbury Brown-Twiss correlation would be found absent from a stable laser beam, and then followed it with a sequence of more general papers on photon statistics and the meaning of coherence.  Our first child, a son Jeffrey, arrived in 1963. I remember feeling his arrival was a kind of redemption, a species of renewal for which I was more than grateful at age 38. I was doing a good deal of traveling in those days, particularly during vacations, and it always amazed me how transportable the baby was. We had no trouble taking him on short domestic trips anywhere, but thinking back on my own experience perhaps, waited till he was nearly 4 before taking him on a longer trip to Geneva for a sabbatical at CERN. My work in this period gravitated back to high-energy collision theory, since experiments had begun to reveal many of the results my diffractive multiple scattering theory had predicted.  Our second child, a daughter Valerie, didn’t arrive until 1970, and by that time our placid and comfortable academic life had been roiled up in many ways. Years of demonstrations against the Vietnam War, the anguish of the black liberation movement, and finally the bitter recriminations of militant feminism had left the world of our university seriously fragmented. My wife, joining with the militants, decided that the days of traditional marriage were over, and that her own should be one of the first to go. The law, she found, would permit her to end it, of her own choice, while retaining custody of the children. Devastated by her decision, I simply couldn’t believe she would hew to it, and it took some time to try to reach a settlement. By that time, indeed, she no longer sought active custody of the children, and having taken care of them earlier, I proceeded thereafter to raise them as a single father. It was a time-consuming occupation, but an immensely rewarding one, and I managed fortunately to remain involved and reasonably productive in my work. I’m sure there is some number of papers I never got to write as a result, but raising those children and seeing them succeed was not an experience I would trade for the missing papers or any sort of recognition. Both Jeff and Val have families of their own now and are busy raising my grandchildren. I envy them that privilege, and wish I had the opportunity to be raising them all myself. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0051 = RG**  – May I speak to Joanna Rose, please?  – Yes. Here I am. Is this Dr Roy Glauber?  – Yes, it is.  – My congratulations on the Nobel Prize.  – Well, thank you. [laughter] I haven’t … Things are going to get even more confused before they get better. I’m really going through something now.  – I just wanted to ask you a few questions.  – Surely.  – This is your first day as a Nobel Prize-winner. How is it?  – Well, it’s like being swept up into the vortex of a bit of a tornado. It’s not quite that chaotic, but it’s every bit as vigorous.  – How did you come to learn that you had won the Prize?  – A telephone call came at 5.36, in the pitch blackness, this morning here.  – And how did you react to this phone call?  – Well, I could scarcely believe it. I certainly had not any anticipation, even though I knew this was that time of year.  – And did you expect a call … somehow?  – No, I certainly did not expect a call of any sort, certainly not at that hour of the morning!  – People call you the Father of Quantum Optics.  – I’m sorry, I didn’t catch that.  – People used to call you “Father of Quantum Optics”.  – Oh, yes. Well, they seem to use that term, which … I hope it’s not just a reference to my considerable age. The subject, in a sense, did not exist before the early 1960s; and yet, in another sense, it had already existed for sixty-odd years. It was very well understood that light has a granular structure, even though nearly everything one observed was explained by continuous waves. But there were various things about this granular structure which were not taken fully seriously, because it didn’t appear that they were necessary. In the context of the older optics which dealt only with the intensity of light, the average intensity, and not with the statistical properties of light, you could get away with using the older form of the theory; and so people were rather lazy about it. I had the impression in the early ’60s that a couple of developments that had taken place were beginning to call for a much more vigorous version of the quantum theory, the full quantum theory that goes by the name, the frightening name: Quantum Electro-Dynamics.  – Hopefully that we can develop that in an interview in December. But you mentioned your age and I have another question: at your age, actually most do retire, but you still teach physics.  – Well, I have very little taste for retirement, I have to tell you. I have just taught a class and worn out my voice, doing it, as you can perhaps hear.  – So what would you like to tell young people about how to become excellent in science?  – Well, that isn’t what I was telling them. But I would try … I’d be happy to tell more of them that. Nobody asked me to tell that to anyone, but I’d be happy to try sometime.  – I see. Do you think that the Prize is just a reward, or does it mean new responsibilities for you, from now on?  – Well, certainly a reward. Whether it will lead to new responsibilities, I really don’t know, because I am at an age at which I was beginning to have my responsibilities lightened, quite considerably. And I wasn’t, I would say, always happy about that.  – So you have just taught, had a class with students. How will you continue this day?  – Well I will continue to teach. This was a small class, a seminar – only nine students who do a certain amount of reading, and reading that I comment on; I must say I … One isn’t really supposed to lecture in a seminar, but I sure did that … [laughter] in the last hour. And, in the spring, I teach a rather large elementary course which is full of very vigorous demonstrations.  – Yes. With laser physics?  – That’s right.  – Yes, I understand. What’s going to happen today, later today?  – Well, we’re going to hold a reception of some sort at 4 p.m. And we’ll have some sort of a party, I think, next week, because I heard there was a big meeting in California for [Charles Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html), which begins on Thursday, and I’ll have to fly out there tomorrow.  – I think your colleague, Nobel Prize-winner, Dr Hänsch …  – Is Ted Hänsch going?  – Yes. He is already there, I think.  – Oh, he’s already there. Good for him! Okay. I was wondering whether I would get to see him. He’s a good friend.  – I understand. Did you have time to think a little … Did you have time to think about what you would do with the Prize money?  – No. To tell you the truth, I heard not a word about it. I’ve still heard nothing official about it at all, and I’ve not thought for a moment.  – I see. I’m looking forward to meeting you in December. Thank you so much for taking the time …  – Well, I have your name and I shall certainly look for you, in December.  – Thank you very much.  – You’re quite welcome. Bye! |
| **Interview** |  |
| Q3 | **I thought that perhaps we could start from the beginning, and I’d like to ask you how did you start in science? Dr Hänsch.** |
|  | Theodor W. Hänsch: I grew up in the city of Heidelberg in a street called Bunsenstrasse, named after the chemist, Robert Bunsen, and we lived in the house that at one time had belonged to Robert Bunsen so as a child, being very impressionable, as I felt that being a chemist must be something important, because you get streets named after you. I asked my father what Bunsen had done, and he, the next day, brought home a Bunsen burner, or Bunsen burner, one of these gas burners that we hooked up to the gas stove in the kitchen. He would put table salt into it and the flame would turn yellow, and my father, who had worked at a pharmacy during the First World War, he knew other powders that one could buy in the pharmacy that would make the flame look red or green. He explained that the atoms have a characteristic colour that one sees there. So that’s something that stirred my interest and my fascination in light and atoms, at the age of six or seven. |
| Q3 | **Did you ever think about becoming a scientist and winning the Nobel Prize?** |
|  | John L. Hall: No, I have not considered that until the middle of the night a month or two ago. |
| Q3 | **Did you think about a Nobel Prize? That you can make science so great? Did you ever dream about a Nobel Prize? Did you think that as a scientist …?** |
|  | Theodor W. Hänsch: I was at Stanford University when in the early 1980s, -81, [Arthur Schawlow](https://www.nobelprize.org/prizes/physics/1981/schawlow/facts/), my mentor and friend and colleague for many years, when he was awarded a prize for work that we had done together, so at least I felt that the kinds of research that we’d been doing is not so far away from what they give Nobel Prizes for, and maybe I started to think about things like that. But getting prizes is not the reason why we do science. The joy comes from inventing things that allow you to do what could not be done before or from understanding something for the first time that nobody else has understood, and that’s the real reason why we enjoy science so much. |
| Q2 | **How do you find the questions or the problems to make your research about?** |
|  | Theodor W. Hänsch: I think it’s different for everybody. I like to play, I like to do experiments that require nice toys. I’m also very curious, so constantly new questions come to mind, and I don’t have a very long attention span. I follow my inclinations and every once in a while one finds something good. |
| Q17 | **Are you also the kind of scientist that are playing around? With toys?** |
|  | Roy J. Glauber: There’s an awful lot to do with play. I try and explain that in the elementary class I teach, that it looks as though we are just a bunch of kids doing demonstrations and playing with toys, but anybody who follows the psychology knows that playing with toys is the way kids learn about the world. It’s certainly the way we learn about the world. As far as the Nobel Prize is concerned, I’d like to add to what Ted has just said. The notion that one engages in science as a competition to win the prize is absolutely ridiculous, an even nasty, because what it does is to make failures of thousands of people who are doing the most constructive things they can. It’s, to some degree at least, accidental who wins the prize. It’s correlated, perhaps, with talent, but it’s correlated with contribution and everybody is trying to make contributions. |
| Q20 | **Yes. But not everybody who’s playing is a good scientist, so I wonder, you have lots of students and young PhD researchers, can you recognise the talent, the exceptional talent? Can you see that?** |
|  | Roy J. Glauber: We’re pretty good at. If you want to know where there’s competition, it’s to sign up the talent. It’s to gather in those talented people. |
| Q7 | **How do you see that people are talented in science? How do you recognise that?** |
|  | Theodor W. Hänsch: Sometimes in a ten-minute conversation, it’s just what makes them excited, what are the questions that they ask. It gives one a feeling. It’s not a science, it’s an art, but after dealing with so many young people I feel that I can decide rather quickly whether this is somebody I like to work with. |
| Q4 | **So it’s very important to have the right personality somehow.  Can you tell us, Dr Hänsch, how you for the first time got the idea of the frequency comb?** |
|  | Theodor W. Hänsch: It’s a long story. Way back in the 1970’s at Stanford University we had a mode-locked picosecond dye laser and it already made a frequency comb that could be used to measure the distances between two spectral lines, but it was not a frequency comb that could be used to compare and upgrade the frequency to a microwave frequency and to make an absolute frequency measurement. That was just the technical state of the art, but in that context, together with my student, Jim Eckstein, we already worried about the phase slips from pulse to pulse, the shift that you could not really tell from the repetition frequency of the laser where the comb lines are, what one would need to do to find that out, but then we left it at that. Then in the early -90s, suddenly making ultra short pulses became very much easier with the invention of Kerr-lens mode-locking and titanium sapphire lasers. Suddenly, what required several PhDs, you could buy as a box and turn it on. We actually bought a femtosecond laser in 1994 for our laboratory, with the intention of trying to measure optical frequencies, but we didn’t go after it very seriously.  What I think was the point that triggered my intense interest was one afternoon in Florence, Italy, I was working with a young researcher, Marco Bellini, who had an amplified femtosecond laser and he would focus the light into a crystal plate and white light would emerge, which is common and used in many ultrafast laboratories as a broadband broad beam, and so I asked the question, and together we answered it. What would happen if we take our laser beam and split it in two and focus at two different spots? These two white light sources, will they form interference stripes? Is this rendered white light or is the face of this white light linked to the laser? We did that experiment, we saw beautiful interference stripes and that made me realise that it should be possible to make a train of pulses of white light pulses that have a comb spanning more than an octave, and then it’s easy to figure out where the comb lines are. At that time I wrote down a five or six page detailed proposal for what I had called a universal optical frequency comb synthesiser, and then we went to work with Thomas Udem and a little bit later Ronald Holzwarth to try and turn it into reality. |
| Q21 | **This was this crazy idea, as I understand you Dr Hall, you didn’t believe that it was possible to realise?** |
|  | John L. Hall: Another way to describe Hänsch idea or of Chebotayev’s explanation was something like, have something which produces white light and then do it 100 million times per second. Then no matter what’s the character of this, if it works the same each time then this light will have some comb character in it, and then, as Ted’s already mentioned, some issue of what the phases are. Combs have been used for a long time, I did my thesis with a comb. It was a commercial system produced for World War II, because there would be people on two teams and they would like to listen each to their home base and which frequency do I adjust this variable capacitor to? There would be a crystal and its harmonics could be found then by tuning the receiver. |
| Q4 | **How was your proposal received, then?** |
|  | Theodor W. Hänsch: I did not publish it. I had it witnessed by a few friends, and then showed it to some experts, including John and I think at that time John felt that was a goofy idea, he didn’t want to study it carefully.  John L. Hall: I agree with that. |
| Q11 | **Almost 20 years ago. Can you compare the style of doing science in the States and in Europe? Is there similarities or differences?** |
|  | Theodor W. Hänsch: In some ways it’s a different style. Of course, science is so international nowadays that we all meet at conferences, we visit each other and it’s no longer isolated, the US and Europe. Nevertheless, of course their styles are a little bit different. At Stanford, of course that was a hotbed of the creation of Silicon Valley, a lot of interesting people and a lot of excitement about innovation. In Germany, at least, I think innovation, people are at first sceptical, they feel life is good the way it is, why do you want to innovate? The spirit is, at least of the general public, is maybe not so enticing for doing research, but of course we live in our own world, in our circle of students and colleagues, who share our excitement. One thing that actually might be better in Germany, at least in the Max Planck Society, is that for basic research we don’t have to declare what we are going to do next year or the year after that, but we have a stable level of support, quite comfortable, that makes it possible to pursue also risky long-term research projects. |
| Q21 | **Do you recognise that? You have short-term financial?** |
|  | John L. Hall: There is maybe less long-term investment made in the United States now, but it did exist. The Science Foundation was particularly visionary, and as well the Office of Naval Research, in accepting people and I think they are graded not by what they propose, but after some experience the men with responsibility for the money and the researcher on the other side have a respect and the money is transferred on the basis of thinking this guy hasn’t done anything fantastic for two years, but I guess he’ll do another thing like he did four years ago, and let’s give him money. |
| Q4 | **No? You are a theoretician from the beginning, and I have a question. What is the relation between the theory, the quantum theory of light, and your experimental work? Where do you meet?** |
|  | Theodor W. Hänsch: If you would like me to say something from my perspective. Roy is the one who explained how light can behave like a classical wave. Of course, we know since [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) and [Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) that light is made up of quanta of photons, so how can a laser wave behave like a classical wave? Roy was the first to explain that in mathematical terms, and to show that there are very many intriguing aspects of light that are lost if you think of it as a classical wave. Many correlation effects.  John L. Hall: And as well, if you think of it as only photons, you need them both.  Roy J. Glauber: Well, go ahead. You’re saving me a lot of trouble. |
| Q4 | **It’s strange anyhow. Can it be that light is neither particles nor waves, so to say, it’s something that we just don’t have any idea …?** |
|  | Roy J. Glauber: I think that’s true.  John L. Hall: I think you’d need an operational discussion. What is the box you’ve put on the table that’s your detector, and if it registers clicks then you’re going to detect a photon character, and if it measures frequency then you’d see some other parameter.  Roy J. Glauber: It does depend on the experiment you perform, but that’s the fascinating thing about this entity. It has different faces, if you like, that it shows to different kinds of experiments.  Theodor W. Hänsch: The wave aspect and the particle aspects are things that we know from our everyday world. We know what are waves, and sound waves, and we know things like objects, but you’re certainly quite right, that the quantum world does not really conform with these classical concepts that we depend on, from which angle we look. It mimics particles or waves, but it’s something different altogether. |
| Q14 | **If we go back to science, do you have any favourite problems that you would like to have solved, before you quit science? What is the dream ahead?** |
|  | Theodor W. Hänsch: Something where we might actually have a chance to find an answer with the frequency combs, is whether fundamental constants are truly constant, or maybe it’s slowly changing with the evolution of the universe. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0052** |
| **Biographical** | John Lewis Hall (known by many as Jan) was born on August 21, 1934 in Denver, Colorado. His father (John Ernest Hall) was trained as an electrical engineer and worked for the U.S. Bureau of Reclamation on many hydroelectric projects here and overseas. His mother, Rae (Long) Hall was an elementary school teacher and singer. Jan’s interests in electricity and radio were supported by parental interest, but his research on black-powder rockets was gradually replaced by social activities such as scouts and a church youth group. After completing public schooling in Denver, he received a Westinghouse Scholarship to Carnegie Institute of Technology (now Carnegie Mellon University ) and earned a B.S. (1956), M.S. (1958) and Ph.D. (1961) in Physics from the Carnegie Institute of Technology. He did his thesis work with Professor Robert T. Schumacher using a self-made electron microwave spin resonance spectrometer to study the hyperfine spectrum of interstitial hydrogen atoms in CaF2 crystals. While considering various postgraduate opportunities, he chanced on a call for applications to be a National Research Council Fellow at the (then) National Bureau of Standards in Washington, D.C. Toward the end of that postdoc year, he was invited to join others from NBS to start a new research group at the University of Colorado Boulder campus. Since 1962, he has been responsible for a number of major innovations and developments in laser technology at JILA (formerly called the Joint Institute for Laboratory Astrophysics), operated by the University of Colorado and the National Institute of Standards and Technology (NIST).  He has authored more than 230 articles in refereed journals, and holds ten U.S. patents. He is a member of the National Academy of Sciences, and is a Fellow of the Optical Society of America and the American Physical Society. He is also a Senior Fellow Emeritus of the National Institute of Standards and Technology, and a Fellow Adjoint of JILA. He has trained numerous graduate students and post docs in the Physics Department at the University of Colorado , Boulder. He retired from NIST in November, 2004, but is still active in JILA and his consulting company (Hall Stable Lasers, LLC). His electronics hobbies and electro-optical systems design provide satisfying outlets for him, often leading to specialized instruments and other technical innovations, and occasional patents.  His JILA work has been recognized through a number of awards from NIST, the Department of Commerce, and the U.S. Office of Personnel Management. He has been awarded many professional, peer-generated honors by the Optical Society and the American Physical Society. In 2004, he received the IEEE Rabi Award and became a member of the French Légion d’Honneur. In 2005, he shared the Nobel Prize in Physics with T.W. Hänsch and Roy Glauber for “contributions to the development of laser-based precision spectroscopy, including the optical frequency comb technique.”  Also a graduate of Carnegie Mellon, his wife, Lindy, (the former Marilyn Robinson) holds a Bachelor’s degree from the Margaret Morrison Carnegie College (1957), and a Master’s degree from Carnegie Library School (1959). She taught 7th grade English in a Boulder junior high for 17 years, followed by 5 years as media specialist there. She remains active as a consultant in curriculum development. They have three children, two in Colorado: Carey, who teaches in a middle school of the Jefferson County School District, and Jon, who specializes in automotive computer and engine troubleshooting. Thomas is a field engineer for a computer company near Boston. With his wife Margaret, and their two daughters (aged six and eight) and son (now two and a half), they often accept visiting grandparents at their home in Quincy, MA. A favorite gathering place for all is their mountain cabin at Marble, Colorado, in the mountains west of Aspen, which was built by the family during the 1980’s. Jan is very proud of having done the electrical wiring and plumbing entirely by himself, and receiving no red flags or violation notices from the State inspectors.  “I am truly fortunate to have somehow always been in the right place at the right time, to capitalize on technical advances that made increasingly precise laser measurements and applications possible. I have worked with amazing colleagues and students, whose enthusiasm and vision made for excellent physics, great fun and the building of important programs. It was a great 45 years and I have really loved it. But nowadays Lindy and I often discuss my in-process transition on to personal plans and activities, leaving the next dazzling science to my younger friends.” |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0052 = JH**  – This is John Hall.  – Hello. My name is Joanna Rose. I’m calling from Nobelprize.org which is the official website of the Nobel Foundation. My congratulations on the Prize.  – Thank you!  – How does it feel?  – It’s completely amazing. I hadn’t … of course, expected it or even contemplated it. So it’s a big surprise.  – How come you did not expect to receive the Prize?  – As I’m … rewarded hugely by the chance to be employed and keep building nice tools, that somehow fit together with the ones that I made last week or a month ago; and one is getting the possibility to see … better and better ways to make super-precise measurements. That’s the driver for me; and to be paid to do that as a salary is a great benefit; and to win a prize for it is … I mean, beyond astonishing.  – You have never thought about it? Do you think about Nobel Prizes?  – I have seen a few cases, where people have packed it by that and I’ve thought that there was some slightly different way of our earth collectively bringing rewards to people and organise anything which is worth noticing is the result of many people, and that builds on the tradition that some other people have made it. One of the key things is to be active, have a long working life – and one can feel that! It’s just that … Anyway the Nobel Prize is completely marvellous. It’s … not seriously contemplated by me before.  – I see. How will you celebrate?  – Oh, I will take my best wife out for lunch today, I think.  – It’s very early in the morning, isn’t it?  – Yes, it surely is. It began at three a.m.  – And you received the call in the middle of the night.  – Right. So I have of course read some other people’s experience – to receive a call in the middle of the night. I certainly didn’t personalise that myself. But it did happen!  – What will you say to other people about that?  – I hope that, by the time I get fully wide awake, to have some nice quotable remarks. But, just at the moment, I’ll be still just coming back to life for the next day.  – Yes. Thank you very much for this short conversation and I’m looking forward to meeting you here in Stockholm in December …  – I have appreciated the elegant and useful website which your organisation makes. There’s a need for communicating with young people about what is happening in science; and many of the articles in that, that are accessed from that, are just perfect for high-school people that sometimes come by and they’re not quite /—/ – they don’t see the whole future.  – Thank you very much. That’s great to hear.  – Yes, it really has been helpful … We captured – you and me together – we captured back at least one person that was clearly not on a good track. So I feel quite happy about that. Clearly it’s that the principle role of an adult is to put back into the society the love and investment that they made in him, or her.  – Yes. Thank you very much; and see you in Stockholm soon.  – Okay. Thank you.  – Bye-bye. |
| **Interview** |  |
| Q3 | **I thought that perhaps we could start from the beginning, and I’d like to ask you how did you start in science? Dr Hänsch.** |
|  | Theodor W. Hänsch: I grew up in the city of Heidelberg in a street called Bunsenstrasse, named after the chemist, Robert Bunsen, and we lived in the house that at one time had belonged to Robert Bunsen so as a child, being very impressionable, as I felt that being a chemist must be something important, because you get streets named after you. I asked my father what Bunsen had done, and he, the next day, brought home a Bunsen burner, or Bunsen burner, one of these gas burners that we hooked up to the gas stove in the kitchen. He would put table salt into it and the flame would turn yellow, and my father, who had worked at a pharmacy during the First World War, he knew other powders that one could buy in the pharmacy that would make the flame look red or green. He explained that the atoms have a characteristic colour that one sees there. So that’s something that stirred my interest and my fascination in light and atoms, at the age of six or seven. |
| Q3 | **Did you ever think about becoming a scientist and winning the Nobel Prize?** |
|  | John L. Hall: No, I have not considered that until the middle of the night a month or two ago. |
| Q9 | **Did you think about a Nobel Prize? That you can make science so great? Did you ever dream about a Nobel Prize? Did you think that as a scientist …?** |
|  | Theodor W. Hänsch: I was at Stanford University when in the early 1980s, -81, [Arthur Schawlow](https://www.nobelprize.org/prizes/physics/1981/schawlow/facts/), my mentor and friend and colleague for many years, when he was awarded a prize for work that we had done together, so at least I felt that the kinds of research that we’d been doing is not so far away from what they give Nobel Prizes for, and maybe I started to think about things like that. But getting prizes is not the reason why we do science. The joy comes from inventing things that allow you to do what could not be done before or from understanding something for the first time that nobody else has understood, and that’s the real reason why we enjoy science so much. |
| Q4 | **How do you find the questions or the problems to make your research about?** |
|  | Theodor W. Hänsch: I think it’s different for everybody. I like to play, I like to do experiments that require nice toys. I’m also very curious, so constantly new questions come to mind, and I don’t have a very long attention span. I follow my inclinations and every once in a while one finds something good. |
| Q17 | **Are you also the kind of scientist that are playing around? With toys?** |
|  | Roy J. Glauber: There’s an awful lot to do with play. I try and explain that in the elementary class I teach, that it looks as though we are just a bunch of kids doing demonstrations and playing with toys, but anybody who follows the psychology knows that playing with toys is the way kids learn about the world. It’s certainly the way we learn about the world. As far as the Nobel Prize is concerned, I’d like to add to what Ted has just said. The notion that one engages in science as a competition to win the prize is absolutely ridiculous, an even nasty, because what it does is to make failures of thousands of people who are doing the most constructive things they can. It’s, to some degree at least, accidental who wins the prize. It’s correlated, perhaps, with talent, but it’s correlated with contribution and everybody is trying to make contributions. |
| Q20 | **Yes. But not everybody who’s playing is a good scientist, so I wonder, you have lots of students and young PhD researchers, can you recognise the talent, the exceptional talent? Can you see that?** |
|  | Roy J. Glauber: We’re pretty good at. If you want to know where there’s competition, it’s to sign up the talent. It’s to gather in those talented people. |
| Q7 | **How do you see that people are talented in science? How do you recognise that?** |
|  | Theodor W. Hänsch: Sometimes in a ten-minute conversation, it’s just what makes them excited, what are the questions that they ask. It gives one a feeling. It’s not a science, it’s an art, but after dealing with so many young people I feel that I can decide rather quickly whether this is somebody I like to work with. |
| Q4 | **So it’s very important to have the right personality somehow.  Can you tell us, Dr Hänsch, how you for the first time got the idea of the frequency comb?** |
|  | Theodor W. Hänsch: It’s a long story. Way back in the 1970’s at Stanford University we had a mode-locked picosecond dye laser and it already made a frequency comb that could be used to measure the distances between two spectral lines, but it was not a frequency comb that could be used to compare and upgrade the frequency to a microwave frequency and to make an absolute frequency measurement. That was just the technical state of the art, but in that context, together with my student, Jim Eckstein, we already worried about the phase slips from pulse to pulse, the shift that you could not really tell from the repetition frequency of the laser where the comb lines are, what one would need to do to find that out, but then we left it at that. Then in the early -90s, suddenly making ultra short pulses became very much easier with the invention of Kerr-lens mode-locking and titanium sapphire lasers. Suddenly, what required several PhDs, you could buy as a box and turn it on. We actually bought a femtosecond laser in 1994 for our laboratory, with the intention of trying to measure optical frequencies, but we didn’t go after it very seriously.  What I think was the point that triggered my intense interest was one afternoon in Florence, Italy, I was working with a young researcher, Marco Bellini, who had an amplified femtosecond laser and he would focus the light into a crystal plate and white light would emerge, which is common and used in many ultrafast laboratories as a broadband broad beam, and so I asked the question, and together we answered it. What would happen if we take our laser beam and split it in two and focus at two different spots? These two white light sources, will they form interference stripes? Is this rendered white light or is the face of this white light linked to the laser? We did that experiment, we saw beautiful interference stripes and that made me realise that it should be possible to make a train of pulses of white light pulses that have a comb spanning more than an octave, and then it’s easy to figure out where the comb lines are. At that time I wrote down a five or six page detailed proposal for what I had called a universal optical frequency comb synthesiser, and then we went to work with Thomas Udem and a little bit later Ronald Holzwarth to try and turn it into reality. |
| Q21 | **This was this crazy idea, as I understand you Dr Hall, you didn’t believe that it was possible to realise?** |
|  | John L. Hall: Another way to describe Hänsch idea or of Chebotayev’s explanation was something like, have something which produces white light and then do it 100 million times per second. Then no matter what’s the character of this, if it works the same each time then this light will have some comb character in it, and then, as Ted’s already mentioned, some issue of what the phases are. Combs have been used for a long time, I did my thesis with a comb. It was a commercial system produced for World War II, because there would be people on two teams and they would like to listen each to their home base and which frequency do I adjust this variable capacitor to? There would be a crystal and its harmonics could be found then by tuning the receiver. |
| Q4 | **How was your proposal received, then?** |
|  | Theodor W. Hänsch: I did not publish it. I had it witnessed by a few friends, and then showed it to some experts, including John and I think at that time John felt that was a goofy idea, he didn’t want to study it carefully.  John L. Hall: I agree with that. |
| Q11 | **Almost 20 years ago. Can you compare the style of doing science in the States and in Europe? Is there similarities or differences?** |
|  | Theodor W. Hänsch: In some ways it’s a different style. Of course, science is so international nowadays that we all meet at conferences, we visit each other and it’s no longer isolated, the US and Europe. Nevertheless, of course their styles are a little bit different. At Stanford, of course that was a hotbed of the creation of Silicon Valley, a lot of interesting people and a lot of excitement about innovation. In Germany, at least, I think innovation, people are at first sceptical, they feel life is good the way it is, why do you want to innovate? The spirit is, at least of the general public, is maybe not so enticing for doing research, but of course we live in our own world, in our circle of students and colleagues, who share our excitement. One thing that actually might be better in Germany, at least in the Max Planck Society, is that for basic research we don’t have to declare what we are going to do next year or the year after that, but we have a stable level of support, quite comfortable, that makes it possible to pursue also risky long-term research projects. |
| Q12 | **Do you recognise that? You have short-term financial?** |
|  | John L. Hall: There is maybe less long-term investment made in the United States now, but it did exist. The Science Foundation was particularly visionary, and as well the Office of Naval Research, in accepting people and I think they are graded not by what they propose, but after some experience the men with responsibility for the money and the researcher on the other side have a respect and the money is transferred on the basis of thinking this guy hasn’t done anything fantastic for two years, but I guess he’ll do another thing like he did four years ago, and let’s give him money. |
| Q13 | **No? You are a theoretician from the beginning, and I have a question. What is the relation between the theory, the quantum theory of light, and your experimental work? Where do you meet?** |
|  | Theodor W. Hänsch: If you would like me to say something from my perspective. Roy is the one who explained how light can behave like a classical wave. Of course, we know since [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) and [Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) that light is made up of quanta of photons, so how can a laser wave behave like a classical wave? Roy was the first to explain that in mathematical terms, and to show that there are very many intriguing aspects of light that are lost if you think of it as a classical wave. Many correlation effects.  John L. Hall: And as well, if you think of it as only photons, you need them both.  Roy J. Glauber: Well, go ahead. You’re saving me a lot of trouble. |
| Q4 | **It’s strange anyhow. Can it be that light is neither particles nor waves, so to say, it’s something that we just don’t have any idea …?** |
|  | Roy J. Glauber: I think that’s true.  John L. Hall: I think you’d need an operational discussion. What is the box you’ve put on the table that’s your detector, and if it registers clicks then you’re going to detect a photon character, and if it measures frequency then you’d see some other parameter.  Roy J. Glauber: It does depend on the experiment you perform, but that’s the fascinating thing about this entity. It has different faces, if you like, that it shows to different kinds of experiments.  Theodor W. Hänsch: The wave aspect and the particle aspects are things that we know from our everyday world. We know what are waves, and sound waves, and we know things like objects, but you’re certainly quite right, that the quantum world does not really conform with these classical concepts that we depend on, from which angle we look. It mimics particles or waves, but it’s something different altogether. |
| Q14 | **If we go back to science, do you have any favourite problems that you would like to have solved, before you quit science? What is the dream ahead?** |
|  | Theodor W. Hänsch: Something where we might actually have a chance to find an answer with the frequency combs, is whether fundamental constants are truly constant, or maybe it’s slowly changing with the evolution of the universe. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0053** |
| **Biographical** | I was born in Heidelberg, Germany, on October 30, 1941. My parents had moved there from their native Breslau a few years earlier. As far as I can tell, I am the only academic in our family. My father Karl Hänsch was a businessman engaged in the export of farming machinery, while my mother Marta raised her three children as a house wife. My younger brother Julius entered the book printing business, and my sister Lucia married a fellow physics graduate student and now helps run a small electronics engineering company.  Growing up during and after the second world war left some vivid memories. I can still see our family huddled together in the basement bomb shelter of our home in Heidelberg listening to the piercing sound of air raid sirens. After the war, our family had lost its estate in Breslau, and we had to share our small ground floor apartment in Heidelberg with some war refugees as subtenants. Without childhood diversions such as television, my brother and I gained a strong sense of independence and adventure by playing in the bombed ruins at the nearby railway station or exploring the many hiking trails on the slopes of the Gaisberg and Königstuhl. My father had long been disillusioned with the Nazi political leadership and raised us as rebels in spirit, distrusting any official authority.  My father also kindled my early interest in science. During the first world war, volunteering at a pharmacy, he became interested in medicine and chemistry. In Heidelberg we lived at Bunsenstrasse 10, in the house that had once belonged to the chemist Robert Bunsen. When I was about six years old, I asked my father what Bunsen had done to have a street named after him. On the next day he brought home a Bunsen burner which we connected to the gas stove in the kitchen. With a sprinkle of table salt, the blue flame turned to a bright yellow. My father explained that this is the characteristic color emitted by sodium atoms that are excited in the flame. It was obvious to me that I had to find out more about light and atoms. A little later, my father took me to visit the metallurgical laboratory of the Heinrich Lanz AG in Mannheim, where I was impressed by researchers in white lab coats who allowed me to look into their fancy microscopes. At a time when other boys dreamt about steering steam locomotives, I started to see myself as a future scientist.  In 1952, I entered the Helmholtz Gymnasium in Heidelberg, then located at the Kettengasse in the old town below the castle. Although the school emphasized modern languages and science, my father enrolled me in a rather small class with Latin as the first language to maintain my option of studying medicine. During the later years we enjoyed some remarkable teachers. Dr. Mampel, our physics and chemistry teacher, gave me free reign of the school’s collection of demonstration apparatus, and Dr. Biser, a Kaplan at the nearby Jesuitenkirche, who later became an eminent religious philosopher, turned the obligatory religious studies into a fascinating course on Western philosophy.  Early on, my interest in science dominated my activities outside school. I eagerly read popular science and science fiction books from the public library until I learned how to check out textbooks from the University library. I also liked doing experiments with my own hands. Intrigued by the world of chemistry, I started to spend my weekly allowance in pharmacies willing to sell substances like fuming nitric acid or white phosphorous to a young boy who stored his growing collection of chemicals in the bedroom of his parents. After an intimidating accident with bomb-making materials, my interests moved from chemistry to physics and electronics. Around 1957, I acquired an old cold cathode X-ray tube which I operated at home after winding a large Ruhmkorff-style induction coil. I also built a transistorized Geiger counter to perform experiments with a radioactive sample of 0.1 millicurie of Mesothorium which I had bought at a factory for radioactive luminous paint. To calibrate the Geiger counter, I went to the nuclear physics laboratory of Professor Otto Haxel at the University of Heidelberg, where an assistant was very kind and willing to introduce me to the real world of physics research. At that time, I set my sights on becoming a nuclear physicist and university professor.  **Study at the University of Heidelberg** After the Abitur in 1961, I enrolled at the University of Heidelberg as a physics student. During the first two years most of my energy went to the study of mathematics. The lectures on physics and chemistry seemed like entertaining diversions by comparison. I was awed by the power and elegance of pure mathematical reasoning. But after a while I realized how much the complexity of an abstract formalism can sometimes distract from true physical insights. Since then I have acquired a compulsion to always try and construct the simplest possible intuitive model to “understand” a physics phenomenon. Such models have often helped me to perform quick order of magnitude estimates and to rapidly weed out half-baked ideas. Playing around with intuitive concepts I frequently arrive at interesting ideas only to find out that the results have been worked out long ago and are well known. But every once in a while I have experienced the immense joy that comes with some entirely new insight or invention.  After the Vordiplom in 1963, I enrolled in the Betatron laboratory of Professor Hans Kopfermann for the Grosspraktikum, an initial laboratory project of about six months. Unfortunately, Professor Kopfermann had died just before I could begin work on my assignment, the construction of a transistorized fast linear gate for a semiconductor detector of alpha particles which I quickly completed. In the spring of 1964, I attended my first meeting of the German Physical Society. Listening to different talks in a nuclear and particle physics session describing the work of large teams working at big machines, I lost some of my enthusiasm for this kind of research.  Instead, I became intrigued by the growing excitement about lasers which had been invented a few years earlier. In the neighboring Institute of Applied Physics at Albert-Überle-Strasse, Professor Christoph Schmelzer had started to design a linear accelerator for heavy ions that was later realized at the GSI in Darmstadt. Since he felt that lasers might help to synchronize the phases of the individual resonators, he had hired Dr. Peter Toschek, a former student of Professor Wolfgang Paul in Bonn, as an assistant to set up a laser group in Heidelberg. Visiting this laboratory I was awed by the sight of a helium neon laser with its glowing discharge tube emitting an intense collimated beam of red laser light that produced an otherworldly speckle pattern. I sensed a large unexplored new world, and I instantly decided to switch fields. Fortunately Peter Toschek accepted me into his group so that I could pursue my two years of diploma research on gas lasers. Since commercial lasers were not yet available, we had to build everything ourselves, including the glass discharge tubes with their electrodes and Brewster windows, the gas filling stations, the high voltage power supplies, and even the dielectric mirrors and their adjustable mounts. In hindsight, this was excellent training for a budding experimentalist. My adviser was a scholar of high intellectual standards who made sure that we kept track of every single publication in the emerging field of lasers and quantum electronics. In my diploma research, I studied saturation effects in the gas laser medium by observing the light emitted spontaneously to the side. In the end I was able to determine a number of previously unknown radiative transition rates in the neon atom.  After receiving my physics diploma degree in 1966, I continued to study laser saturation phenomena in my thesis research. I had become intrigued by the sharp central Lamb dip, a drop in laser power, that Ali Javan had first observed when scanning the frequency of a single mode gas laser across the Doppler-broadened gain profile. The Lamb dip allowed a new kind of nonlinear Doppler-free high resolution spectroscopy, albeit limited to the study of laser transitions or to the “inverted Lamb dips” produced by molecular absorption lines in accidental coincidence.  In my own experiments, I studied the cross saturation of two coupled laser transitions in neon that share the same lower level. Soon, I observed strange line asymmetries that could not be understood within a hole burning model. I tentatively ascribed the observed phenomena to Raman-like two photon transitions and the dynamic Stark effect. After laboring for considerable time as a theorist I was able to explain the observations quantitatively with a semiclassical model that relied on the density matrix formalism to account for quantum interference effects in coupled three-level systems. This work, published in 1970 with Peter Toschek, is still cited frequently today, because it laid the groundwork for the understanding of phenomena such as lasers without inversion, electromagnetically induced transparency, and slow light. In January of 1969, I received my doctor degree from the University of Heidelberg (Dr. rer. nat., “summa cum laude”), and I continued to work in Heidelberg for another year as an assistant of Professor Schmelzer. Aspects of coherence and quantum interference have remained a recurring theme in my later research, with intuitive insights from classical wave optics often guiding my thoughts and ideas.  **At Stanford University** In March 1970, I left Germany to join the laboratory of Professor [Arthur L. Schawlow](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html) at Stanford University as a NATO postdoctoral fellow. I had first met Art Schawlow, co-inventor of the laser, at a summer school at Carberry Tower in Scotland in 1969, and I was immediately captivated by his warmth, his keen mind, and his contagious sense of humor. Fortunately, Art agreed to take me on as a postdoc.  On my way to California, I stopped at the east coast to visit Ali Javan at MIT and Bill Bennett at Yale University. I also arranged a visit to the famous Bell laboratories at Holmdel. There, Charles Shank and Herwig Kogelnik showed me a small pulsed dye laser, pumped by a nitrogen laser made by AVCO. At a repetition frequency of 100 Hz, the beam looked almost continuous to the eye, and the color could be changed by simply tilting the angle of a diffraction grating. This was a far cry from the few pulses per minute that I had obtained playing with a simple home-built flashlamp-pumped dye laser at Heidelberg, following the discovery of lasing in organic dye solutions by Peter Sorokin and Fritz Schäfer in 1966.  When I arrived at Stanford, I told Art Schawlow about the interesting experiments at Bell Laboratories, and I proposed that I would like to try and make a nitrogen laser pumped dye laser so highly monochromatic that it could be used for Doppler-free saturation spectroscopy of gaseous absorption lines. When Art asked me how I would go about it, I explained that I would try holographic diffraction gratings, Lyot filters, etalons, or whatever else was necessary to restrict laser action to a single longitudinal mode. Art and I had already discovered that we shared a strong passion for clever gadgets. Art was sufficiently intrigued by my proposal to let me purchase an AVCO nitrogen laser, using funds from a post-Sputnik era Army contract. The nitrogen laser arrived in July of 1970 and immediately proved to be an irresistible toy. During the next six months we enjoyed some very entertaining experiments, ranging from edible lasers to dye laser image amplifiers.  Soon, I found the intellectual atmosphere at Stanford quite exhilarating. I was surrounded by legendary scientists such as [Felix Bloch](https://www.nobelprize.org/nobel_prizes/physics/laureates/1952/index.html) or [Robert Hofstadter](https://www.nobelprize.org/nobel_prizes/physics/laureates/1961/index.html), and I could discuss laser science with some heroes of my graduate student years, including Tony Siegman, Steve Harris, and Robert Byer. At the heart of budding Silicon Valley , one could sense a “can do” atmosphere that seemed immensely liberating. Art kept pointing out that one did not have to know everything about a field in order to discover something new. If our German approach to research had resembled well-planned agriculture, the work at Stanford could be compared to game hunting. With my instinctive aversion against organized planning, I enjoyed this atmosphere tremendously. At least, we did not have to be afraid of research results that made all planning obsolete. We soon found ourselves at the heart of a revolution in laser spectroscopy that brought plenty of such results.  Towards the end of 1970, I began to focus my efforts increasingly on the goal of making a widely tunable dye laser highly monochromatic. Like other experimenters before me, I was working with a rather small beam diameter inside the dye laser cavity. Suddenly, I realized that the spectral resolving power must be limited if only a small number of grating lines is illuminated. I happened to carry a small Zeiss monocular telescope in my pocket, which I often used to read the small print of slides or transparencies from the back of a lecture room. Quickly I mounted this telescope as a beam expander inside the cavity to fill the grating area more efficiently, and instantly I observed a dramatic improvement in the laser line width. With a larger beam expanding telescope and an additional etalon inside the cavity, the spectral width of the pulsed dye laser could be reduced to an unprecedented 0.0004 nm, and an additional external filter etalon soon permitted the first experiments on Doppler-free saturation spectroscopy of atomic resonance lines. To this end, I devised a scheme for saturation spectroscopy outside the laser cavity that was highly immune to the intensity fluctuations of our still primitive dye lasers. The technique became later known as Hänsch-Bordé method, since Christian Bordé in Paris had independently pursued similar ideas.  When Art Schawlow saw the first Doppler-free spectra of the sodium D lines which I had left on his desk after an exhilarating night, he suggested that we should do the same with the red Balmer-alpha line of atomic hydrogen. This line had been at the center of attention of atomic spectroscopists in the 1930s, because of suspected discrepancies from the predictions of the relativistic Dirac theory. With Issah S. Shahin, a graduate student from Jordan, we quickly set up an old-fashioned Wood-style hydrogen gas discharge tube, and soon we were able to resolve single fine structure components of the red Balmer line for the first time so that we could observe the 2S Lamb shift directly in the optical spectrum. A few years later, the first laser measurement of the Rydberg constant improved the accuracy of this important fundamental constant by an order of magnitude. This was the beginning of a long quest for ever higher resolution and measurement precision in optical spectroscopy of the simple hydrogen atom which permits unique confrontations between experiment and fundamental theory. This pursuit has culminated in the invention of the femtosecond laser frequency comb, a tool that is revolutionizing precision measurements of time and frequency, as recounted in my Nobel Lecture.  When word about the new tunable laser and its powers spread, an unending series of visitors began to file through our unpretentious little laboratory, and an article describing the dye laser soon became a “citation classic.” This experience taught me that a simple and imperfect proof-of-principle experiment can sometimes find a much wider resonance than a complex experiment of intimidating perfection. In 1973, [Art](https://www.nobelprize.org/nobel_prizes/physics/laureates/1981/index.html)Schawlow and I were named “California Scientists of the Year” by the California Museum of Science and Industry in Los Angeles for this work. At the same ceremony, William Hewlett and David Packard were honored as “California Industrialists of the Year”. With such recognition, it became easy for me to clinch tenure as Associate Professor at Stanford. Soon, I received offers of full professorships from the University of Heidelberg, Yale University, and Harvard University. In the end, I decided to remain at Stanford, accepting a promotion to full professor in 1975, and I continued to work close to Art Schawlow for another 11 years before returning to my native Germany in 1986. Our early work with hydrogen was prominently cited when Art Schawlow received the Nobel Prize for laser spectroscopy in 1981.  Many of my other highly cited papers from the Stanford years describe relatively simple experiments such as ultrasensitive fluorescence spectroscopy with the power to detect the light from single atoms, sensitive intracavity absorption spectroscopy with a multimode dye laser, the first demonstration of continuous wave Doppler-fee two-photon spectroscopy, and the experiments with my student [Carl Wieman](https://www.nobelprize.org/nobel_prizes/physics/laureates/2001/index.html) on Doppler-free polarization spectroscopy. Even the roots of the laser frequency comb can be traced to the exhilarating seventies at Stanford. With my student Jim Eckstein and visiting Lindeman Fellow Allister Fergusson we used the comb of regularly spaced longitudinal modes of a mode-locked sub-picosecond dye laser to measure some fine structure intervals of atomic sodium.  Discussing possible ways to increase the interaction time of hydrogen atoms with a laser beam, Art Schawlow and I came up with the idea of laser cooling of atomic gases in early 1974. Vladilen Letokhov in Troisk was one of the first to start experiments with one-dimensional radiation pressure cooling of a sodium atomic beam. It took ten more years until [Steve Chu](https://www.nobelprize.org/nobel_prizes/physics/laureates/1997/index.html) and his team at Bell Laboratories realized “optical molasses”, demonstrating 3-dimen-sional Doppler cooling as envisioned in our original proposal. Considering the dramatic subsequent developments, Art and I have often regretted that we did not immediately follow up our proposal with our own experiments. However, we did not know how to laser cool hydrogen atoms, and there were many other interesting things to do that seemed much easier. Art still remembered some unhappy experiences with atomic beam machines during his thesis research at the University of Toronto and advised against any experiments involving serious vacuum. In 1978, my former thesis adviser Peter Toschek and his group were the first to demonstrate the related method of laser cooling of trapped ions that had been proposed in 1975 by [Hans Dehmelt](https://www.nobelprize.org/nobel_prizes/physics/laureates/1989/index.html) and [David Wineland](https://www.nobelprize.org/nobel_prizes/physics/laureates/2012/).  At Stanford in the mid-seventies, one could smell the beginning microcomputer revolution, ignited by the early microprocessors introduced by the nearby Intel Corporation. After soldering together my own IMSAI computer from a kit and advancing from binary programming by flipping switches to assembly language, I became an avid visitor of the weekly meetings of the Stanford Hombrew Computer Club, where Bill Gates sold rolls of punched paper tape with 4k and 8k versions of ALTAIR BASIC. Steve Jobs, the later founder of Apple Computer came to my undergraduate classes on electricity and magnetism. In 1976, Art Schawlow and I bought one of the first Apple I Computers, a bare printed circuit board, at the Mountain View Byte Shop. Both Art and I succumbed to the microcomputer fever, spending a forbidding amount of personal money on a growing collection of computers and peripherals, so that our offices started to look like space mission control centers. Art sometimes joked about our role as early adopters: “The pioneers are the ones with the arrows in their backs.” Around 1980, Art and I even started a small mail-order software business, selling a little graphics program Autoplot written in BASIC for the Radio Shack TRS80 computer. Sales were brisk until the hardware became obsolete.  My long exposure to the challenging intellectual climate at the Stanford Physics Department played an important role in shaping my own academic values. I witnessed an Assistant Professor who ruined his chance for tenure by writing a book reviewing science rather than pursuing original research. I could easily live with such priorities because doing research is what I love most. For the same reason, I have always resisted trading academic freedom for the power that comes with administrative and management responsibilities, even though I am grateful to colleagues who shoulder such burdens.  **At Munich and Garching** In 1978, Professor Herbert Walther at the University of Munich invited me to spend a sabbatical in Germany with an Alexander von Humboldt Senior US Scientist Award. Herbert had played a key role in promoting laser science in Germany. Since 1976, he was directing a Project Group for Laser Science in Garching that became the Max-Planck Institute of Quantum Optics in 1981. A few years later he helped to organize a very tempting lure to make me return to my native Germany. In March 1986, after agonizing over the decision for almost two years, I accepted an offer to join the Ludwig-Maximilians University of Munich as a Professor of Experimental Physics and to build a new Division of Laser Spectroscopy at the Max-Planck-Institute that was just about to move into an attractive new building at the southern end of the Garching research campus. The University laboratories which I inherited from my predecessor, Professor Josef Brandmüller, where located downtown, at Schellingstrasse 4, in the Max-Vorstadt, surrounded by bookstores and small restaurants. Since I felt that the downtown location would make it easier to attract graduate students, I decided to set up laboratories at both locations.  With considerably more space, positions for assistants and graduate students, and ample start-up funds at my disposal, it was obvious that I could pursue many more projects than before, but I could no longer maintain my Stanford style of closely working with a small group of graduate students, getting intimately involved in every detail of one or two hot experiments. At first, this adjustment felt rather painful. I had to rely much more on the initiative and judgement of my students and postdocs, resigning to the expectation that they would make expensive mistakes. Fortunately, I have been able to attract some exceptionally gifted young coworkers, sometimes with an energy, patience, and discipline that far exceeds my own. The best young people blossom if they feel free to follow their own ideas. Therefore, I try to guide my students in rather subtle ways, letting them arrive at their own insights, goals, and research plans during our discussions, while I show enthusiasm and excitement when they move in a promising direction. Once the fire is lit it tends to become self-sustaining, and I am rather proud that more than thirty of my former students and postdocs are now Professors at universities around the world, running their own laboratories. Since I did not want to give up my own hands-on experimental work, I established my own small “toy” laboratory at the University, equipped to set up improvised laser and optics experiments quickly, to explore crazy new ideas and to stay abreast of ever advancing technologies.  Shortly before moving to Munich in April 1986, I had met [Gerd Binnig](https://www.nobelprize.org/nobel_prizes/physics/laureates/1986/index.html), the co-inventor of the scanning tunneling microscope, at the laboratory of Cal Quate at Stanford. Gerd told me that he was planning to move to Munich to set up an IBM physics group as an advance guard for an envisioned new IBM research laboratory. Since I was intrigued by the possibility of combining tunneling microscopy and laser spectroscopy, I offered Gerd to set up shop for a few years at our downtown university laboratory. In October of 1986, we could help celebrate the Physics Nobel Prize to Gerd Binnig and [Heinrich Rohrer](https://www.nobelprize.org/nobel_prizes/physics/laureates/1986/index.html). Even though IBM soon abandoned the plans for a Munich laboratory, Gerd and his group stayed with us for the next ten years. During this time we pursued some serious research on scanning microscopy ourselves, including studies of biomolecules on graphite surfaces with Wolfgang Heckl, and a proof-of-principle demonstration of an aperturte-less scanning optical near field microscope with Johannes Pedarnig. Unfortunately, the combination of tunneling microscopy and laser spectroscopy did not advance as easily as hoped. When Gerd Binnig returned to the IBM Rüschlikon laboratory in 1996, scanning microscopy had evolved into a large worldwide enterprise and we would have had to concentrate considerable resources to compete effectively. Instead, we decided to abandon our microscopy research.  At that time the quantum physics of ultracold atoms had become an important focus of our research. With Andreas Hemmerich we were the first to demonstrate and explore two- and three-dimensional optical lattices bound by light in the early 1990s. With Tilman Esslinger, we explored new tricks for laser cooling below the recoil limit. After Immanuel Bloch joined us as a graduate student in 1997, we realized Bose-Einstein condensation (BEC) of rubidium atoms in a novel magnetic QUIC trap in February of 1998, as only the second group outside the USA. We later exploited the high magnetic stability of our small trap in the first continuous wave atom laser that dominated German science news in 1999.  In the same year, Markus Greiner joined our group as a diploma student. He constructed a new BEC apparatus transporting cold atoms magnetically from a magneto-optical trap into a high vacuum glass cell where BEC can be achieved without the need for any further laser cooling. Continuing his work as a Ph.D. student, Markus Greiner used the unobscured optical access to load the Bose-Einstein condensate into a three-dimensional optical dipole force lattice potential formed at the intersection of three orthogonal far-detuned standing wave laser fields. In 2001, he and Immanuel Bloch were the first to demonstrate a reversible quantum phase transition from a wave-like superfluid atomic state to a particle-like Mott insulator crystal, by simply adjusting the height of the lattice potential wells, as theoretically predicted in 1998 by Dieter Jaksch and Peter Zoller. This realization of a strongly correlated quantum gas has triggered an avalanche of work at the interface between atomic physics and condensed matter physics, with the prospect of quantum simulators for elusive phenomena ranging from antiferromagnetism to high Tc superconductivity.  Starting around 1994, we have also been exploring microscopic magnetic traps for the manipulation of cold paramagnetic atoms. Such traps can produce large field gradients and field curvatures without the need for strong currents. From the beginning I was intrigued by the prospect of tayloring complex magnetic potentials with lithographically fabricated circuit patterns, combining traps, wave guides, and other atom optical elements to create a quantum laboratory on a chip. Discussing such ideas with some of my coworkers in a Schwabing restaurant, I sketched a proposal for a conveyor belt for cold atoms onto a napkin. Graduate student Wolfgang Hänsel soon modeled the magnetic fields on his computer and concluded that such a device might work. In 1996, Dr. Jakob Reichel had joined our laboratory after receiving his doctorate with Christoph Salomon and [Claude Cohen-Tannoudji](https://www.nobelprize.org/nobel_prizes/physics/laureates/1997/index.html) at the ENS in Paris. Together with Wolfgang Hänsel, and later joined by Peter Hommelhoff, he set out to realize such an atom motor chip. With the mirror-MOT we found an effective method for loading cold atoms into microscopic traps, and soon we could demonstrate a working conveyer belt for atoms. In June 2001, we were the first to achieve Bose-Einstein condensation entirely on a microfabricated atom chip. More recently, Philip Treutlein has demonstrated long atomic coherence times close to the chip surface, raising the prospects for accurate atomic clocks and for quantum information processing on a chip.  One fascinating line of research at the MPQ until today has been pursuit of precision laser spectroscopy of the simple hydrogen atom. Numerous graduate students have advanced the state of the art during their thesis research in Garching, starting in the late 1980s with Reinald Kallenbach, Claus Zimmermann, Ferdinand Schmidt-Kaler, and Martin Weitz. With a beam of cold hydrogen atoms and a highly stabilized continuous laser source of 243 nm, we could greatly improve the resolution of the sharp 1S-2S two-photon resonance. Thanks to the efforts of Thomas Udem we later learned to build optical frequency interval dividers which made it possible to measure the optical frequency of the ultraviolet 1S-2S resonance against the infrared frequency of a transportable methane-stabilized He-Ne laser. This intermediate reference was shuttled to the PTB in Braunschweig many times for calibration with an elaborate harmonic laser frequency chain. These experiments led to a new Rydberg constant, tests of QED, such as a precise measurement of the Lamb shift of the 1S ground state, and accurate determinations of the rms charge radius of the proton and the structure radius of the deuteron. Krzysztof Pachucky, Savely Karshenboim and Ulrich Jentschura have provided essential theoretical support.  Starting in 1997, our efforts to measure the frequency of laser light led to the vastly simplified approach of the femtosecond laser frequency comb, as recounted in my Nobel Lecture. In a proof-of-principle experiment in the fall of 1998, we used a commercial mode-locked femtosecond laser with a comb spanning 70 THz to compare the frequency of a blue dye laser directly with the microwave frequency of a commercial cesium atomic clock in our own laboratory. In June of 1999, we reached a precision of 14 decimal digits in a comparison of the 1S-2S frequency with the transportable cesium fountain clock built at BNM SYRTE in Paris. A second such measurement in February 2003, took advantage of an even simpler octave spanning laser frequency comb synthesizer as first demonstrated in late 1999 in Boulder, CO and at Garching. Frequency comparisons of this kind allow sensitive searches for possible slow variations of fundamental constants.  Our laboratory has long been a partner of the ATRAP collaboration, one of two international teams working at CERN with the goal of applying precision laser spectroscopy to anti-hydrogen, searching for conceivable differences between matter and antimatter.  Over the years, I have enjoyed the hospitality of many universities around the world hosting me as a visiting professor or lecturer. I am particularly grateful to Professor Salvatore Califano and Massimo Inguscio at the University of Florence, Italy, who gave me the opportunity to participate in the creation of the European Laboratory for Nonlinear Spectroscopy (LENS) and to teach a course on laser spectroscopy to Ph.D. students at the University of Florence in the enchanting hills of Arcetri. A serendipitous experiment with Dr. Marco Bellini at LENS in February of 1997, on the coherence of white light continuum pulses produced with an amplified femtosecond laser was actually the crucial step that convinced me that an octave-spanning optical frequency comb synthesizer could be realized.  During my Stanford years, I had enjoyed the liberating climate of entrepreneurship that was omnipresent in the heart of Silicon Valley. Fortunately the old aversion between academia and industry is now waning in Germany, and in 2001, my former students Ronald Holzwarth and Michael Mei have taken the risk of starting a spin-off company, Menlo Systems GmbH, to develop commercial frequency comb synthesizers. The name has been inspired by Menlo Park in New Jersey where Thomas Alva Edison invented the light bulb.  Sometimes people comment on my growing collection of prizes and awards, wondering if I am pursuing research in order to win still more prizes. The honest answer is that I like prizes to reassure our sponsors that their money is being spent well. Prizes are also important as recognition and source of pride and motivation for our team. However, the most important reward for me has always been the joy that comes from new insights, discoveries, and inventions. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0053 = TH**  – Yes?  – Hello. My name’s Joanna Rose. I am making a recording for Nobelprize.org which is the official Website of the Nobel Foundation.  – Ah, that sounds important. However, I’m just about to leave for the airport; my driver is waiting downstairs. So this is not the way just now… I’m going to Berkeley, California.  – But this is just like three minutes …  – Okay, okay.  – I just want to congratulate you.  – Thank you very much.  – Did you expect it?  – Well, of course, one cannot expect something like that. I had a little spark of hope, I have to admit, though.  – Every year? Or this year?  – For a number of years.  – How many years?  – Oh …! But no, of course, when the news comes, it’s totally unexpected. And I’m still up in the clouds; I haven’t settled down yet.  – You’ll be in the plane …  – Right, right!  – What does it mean to you, to get the Prize?  – Well, I mean, it’s the ultimate recognition that scientists can hope to receive. It’s recognition not just for my person, but, I think, for our entire team, for the organisations that have supported our work. And I think for Germany it is certainly a sign that, hopefully, will attract more young people into science, because for a while it looked like we were out of luck with modern Nobel Prizes. Of course, in the early days, Germany did pretty well.  – Yes. Do you think that the Prize is just a reward, or can you …?  – Well, it depends on what one does with it. Certainly there will be more opportunities to give opinions outside my area of expertise. I will try to avoid that! And I will try to use it as a means to be able to continue our research, hopefully beyond my official retirement age in Germany.  – Is there any special field that you would like to …?  – Well, for me, the field of light, of lasers, of atoms, molecules, has been an unending series of surprises, and I can’t think of a better field. But I think we will, hopefully, find new things, in the future.  – How will you celebrate the Prize?  – Well, we’ve already celebrated in a rushed ceremony, because I have to leave for the airport and, as I said, the driver is waiting downstairs. So I am, just now, going on a trip to California. And maybe over there we will continue to celebrate in a circle of friends who are all gathering to honour [Charlie Townes](https://www.nobelprize.org/nobel_prizes/physics/laureates/1964/index.html) on his ninetieth birthday.  – Ah, I see. Thank you very much for this conversation.  – Okay. You’re welcome.  – Hope to see you in Stockholm. Bye-bye.  – Bye-bye. |
| **Interview** |  |
| Q1 | **Theodor Hänsch, welcome. We’re here in Lindau, on Lake Constance, for the Lindau Nobel Meeting, which gives young researchers from around the world a chance to mix with Nobel Laureates, listen to them and speak to them. What do you hope that the young people here will take away from this meeting? What do you think they can learn from this?** |
|  | Theodor Hänsch: It’s a thrill, of course, to be at the meeting where so many Nobel Laureates whom one only knows as legends and from the literature, where they are there to touch, to ask questions, to get autographs. So it’s a special spirit, but I think it’s enjoyable also to the Laureates, because otherwise they wouldn’t come so many times. But it’s also an opportunity to try to transfer some of the enthusiasm and excitement that we feel for our science, to the young generation, because, after all, science depends on young people entering. |
| Q11 | **Right, that’s very interesting. And do you know, I mean every, not every country in the world, but a very large selection of countries is represented here, do you notice big differences in the sorts of questions you’re getting asked by people from different regions, or …?** |
|  | Theodor Hänsch: But still of course it’s also nice to see people from so many different countries. Some ladies dressed up even with a veil, only with a slit open for the eyes. |
| Q3 | **Auspicious beginning. But you played with chemistry as a child, and then at some point the budding chemist became a budding physicist. How did that transition happen?** |
|  | Theodor Hänsch: Partly it had to do with an event that left me almost without hearing, and certainly without eyebrows, some mixture, I think, of red phosphorous and potassium perchlorate that blew up in my face. |
| Q16 | **Perhaps your parents decided this too. Right, so you made a pragmatic decision to go for a safer subject. And you went to read physics at university in Heidelberg, and was it … I mean you had liked being an experimentalist with your hands, and you continued to be an experimentalist, was there ever a point at which you wanted to make the decision of whether you should be a theoretical physicist or an experimental physicist?** |
|  | Theodor Hänsch: Well starting out as a student at the University of Heidelberg, the initial hard work was mathematics, and I started to admire the power of the mathematical formulas, and so clearly the thought crossed my mind, should I maybe do that? But I enjoyed so much experimenting, working with my own hands, being in touch with nature, whose laws we want to uncover, that I decided, no that’s maybe not the right choice for me. |
| Q5 | **That’s a very nice way of referring to experimental science, being in touch with nature. And you felt that even then? You felt that experiments were leading … were uncovering truths for you?** |
|  | Theodor Hänsch: Well I had done experiments as a hobby for many years, even before I decided that I wanted to be a physicist, and it’s been one of the sources of enjoyment, so I didn’t want to give that up. But also I felt that the formulas of mathematics, powerful as it may be, it can obscure the genuine nature of physical laws, and our intuitive understanding of course plays a powerful role. If I want not only to solve problems according to the book, but if I want to invent new tools, new directions, I need to develop some intuitive feeling, and as a theorist I think it’s harder to do than as an experimentalist. |
| Q4 | **That’s interesting. So it’s by practising and playing that you develop ideas of how things might work, and you can just try them out? And theory is your back-up, in that case, I suppose, or maybe …?** |
|  | Theodor Hänsch: So I might have an intuition that this is a promising direction, and once we start working of course then I need to do mathematical modelling and all that. But mostly it’s some more or less intuitive thought that starts it, the gut feeling that this might be interesting. |
| Q6 | **I mean the laser is common or garden now, and we all see laser pointers, and everything, but then lasers were fairly new and do you remember your first encounter with the laser?** |
|  | Theodor Hänsch: So lasers, I think the first visible laser was invented in … the first visible continuous wave laser in 1961, and in 1964 I saw my first laser in Heidelberg, at the University of Heidelberg, at the Institute of Applied Physics, there was Christoph Schmelzer who had plans to build a heavy ion accelerator using individual radio frequency resonators, and he wondered how to phase synchronise these cavities, and he had the idea that lasers, these new fangled devices, maybe would provide a way to send a synchronising signal along the accelerator. And so he had hired Peter Toschek from Bonn, a student of Wolfgang Paul, as an assistant, and when I saw my first laser he already had, I think, two students. The group was very small, but they actually had built a working helium neon laser, and to me this red beam was the strange speckled pattern that I had seen for the first time, it just seemed like a new world that I wanted to explore. So I told the people in nuclear physics that they shouldn’t count on me, I will start over at the Institute of Applied Physics. |
| Q10 | **It must’ve been enormously empowering. And obviously it was, cause you stayed, the environment suited you. Do you think that it’s still the same? Is the environment in places like Stanford as it was then? Is it as easy?** |
|  | Theodor Hänsch: I think it’s got more difficult, for instance to get research money, if you do fundamental research. In our days you could still get a lot of money easily, say, from the US Navy, and they didn’t require you to do any military work, one could just follow one’s ideas, and we didn’t have to write big reports, we just gave them a list of publications and they’d see what we’ve done last year. If you give us more money, we will do more of the same. OK, here, you have the money. Now I think many of my colleagues are complaining because they spend so much of their time writing proposals and reports. A typical NSF grant might be $100,000 a year, which is just barely enough to have a single graduate student. So I think it’s not quite the same in … but nonetheless I mean in a place like Stanford, still it’s a place where many excellent minds are working and it’s still a very stimulating environment. |
| Q10 | **And presumably that was in part then dictated by the fact that the Max-Planck-Institut would allow you to have that same kind of freedom of research that you’d known at Stanford?** |
|  | Theodor Hänsch: Right. I think that, this combination really is what enticed me. Even the colleagues in the US envious for the amount of freedom we enjoy at the Max-Planck Society. It’s a society that is dedicated to basic research, so we don’t have to promise applications, we can follow crazy ideas, we can start risky projects, and we know that we have pretty much ensured funding for foreseeable years, even if after two years we find, oh, that was the wrong decision, we should re-group. So to have that luxury is something that’s fairly unique of the Max-Planck Society. |
| Q10 | **And Stanford was presumably the right place to be?** |
|  | Theodor Hänsch: Right. There was the Homebrew Computer Club meeting every Wednesday evening at SLAC, the Stanford Linear Accelerator Centre, and people like Bill Gates or Steve Jobs were there. Of course he had no idea what they would be able to move. It was essentially a hobby organisation. |
| Q17 | **I love the name Homebrew, that’s nice, yes. It think it really captures it. But on the tool and the research, so you like playing and the tools enable you to answer the next question you want to answer?** |
|  | Theodor Hänsch: Also I mean in the early days, unless you were extremely diligent, it was hard to find out what already had been done in a field, you would have to spend weeks in the library, there was no Google scholar. And I always felt that I make my life much easier if I start a new field where other people haven’t worked before, then I don’t have to go to the library and look up what has been done. And so by having tools that simply were not available before, one could take this easy way. |
| Q4 | **So you eliminated the Doppler, and brought a more accurate picture?** |
|  | Theodor Hänsch: Right, right. So in atomic hydrogen many of the old time physicists, they remembered that in the ’30s there was a big debate whether the Dirac theory would correctly describe the line profile of the red Balmer alpha line of hydrogen. And some people suspected that it doesn’t, and these suspicions led to the discovery of the Lamb shift, but with our laser we could see plainly resolved the Lamb shift for the first time in the optical spectrum. So that was one little thing. But there were many other things we could do. We could excite fluorescence in atomic gases at very low density. For instance, in sodium vapour we could work at temperatures, I think, of -50°C where there is one hundreds of an atom in the volume of sight, but we could see this, it was fluorescing. And a lot of other things followed. And so it really revolutionised in a way optical spectroscopy, and the trick to do it was exceedingly simple, so it was like once one knows it, one can build such a laser in an afternoon, and many laboratories did so. There were hundreds of laboratories that followed in the footsteps and built these so-called Hänsch design dilator. |
| Q14 | **So I wanted to ask you which ones you’re most excited about, which ones are most likely to be realised soon?** |
|  | Theodor Hänsch: It depends on whom you ask. In a way, I’m also an entrepreneur, I’m part of a start-up company that is actually selling frequency combs, and there I would have to say those are of interest to the largest number of people. But if you ask me as a scientist, well I’m interested in using combs to ask questions like, do we already understand quantum physics well, or are there some small level shifts that are not included in present day theories, or second stage Lamb shift? Our fundamental constants … |
| Q4 | **So by reinvestigating spectra with greater precision, you’ll be able to reveal that, yes?** |
|  | Theodor Hänsch: Our fundamental constants, or can we do laboratory experiments that would uncover slow drifts of fundamental constants? |
| Q4 | **Fundamental constants have never had any drift associated with them, they are just … as far as everyone knows?** |
|  | Theodor Hänsch: When they were named fundamental constants, people assumed that they must be constant, and on the other hand astronomers have speculated that spectral lines in the light of distant quasars, that they might carry some indication that the electromagnetic fine structure constant was a little bit smaller in the early universe than it is now. But of course nobody can go there and check what are the circumstances. But the rates of drift that they have speculated about should be of a magnitude that one can observe them in a laboratory experiment if you compare two different clock now, and again in a year or two years from now. And such comparisons have been made, and so far there is no evidence for any changing constants. |
| Q4 | **So are they now widely accepted as the standard clocks, or is it still experimental?** |
|  | Theodor Hänsch: Now there is a race who gets to redefine the second? And of course I believe that NIST would like to do that. On the other hand, I think for the clocks, it’s not just important that they are accurate, they should also be reliable, easy to work, easy to replicate, so that lots of people can have one. And I hope that the committees will hold back, that they won’t redefine the second too quickly. |
| Q22 | **That’s interesting, yes, cos then everybody is just left depending on one source, I see, yes. What about your commercial applications, what is your company selling these things for?** |
|  | Theodor Hänsch: I think most of the labs that are working on optical clocks, by now have bought a frequency comb from our company, there are 45 systems worldwide been sold. And of course we are looking for applications beyond precision clocks, so one project right now involves the European Southern Observatory, ESO. We are trying, also jointly with our research group, to make frequency combs fit to calibrate large astronomical spectographs. For that we have to thin out the comb spectrum.  Right now our comb lines are typically spaced maybe 100 MHz to 1 GHz, we’d like to have them further apart, like 30 GHz. And one way to do that is to filter them out with an external cavity, and we have already done some first steps in this direction. We brought one of our fibre based frequency combs to the vacuum tower telescope on the island of Tenerife earlier this year, and superimposed a comb to a solar spectrum, in the infrared range around 1.5 micron, and so you see all the nice Fraunhofer lines irregularly spaced, and then you see the comb spectrum very regular, like a ruler. And without any big analysis, immediately we could improve on the calibration accuracy that they are used to with this telescope. But the dream is that we can do much better in the future, and that you might be able to, for instance, watch directly the expansion of space with the evolution of the universe by taking some objects with certain red shift of the spectral lines, and see if this red shift itself drifts with time. |
| Q4 | **Over what period would you have to look to actually witness the expansion?** |
|  | Theodor Hänsch: It depends how far away the objects are. But of course your hope is that this can be done with observations of months or years, or human scale times. But on the other hand I mean we could measure now, record it, and astronomers a century from now might go back to this data and try again. |
| Q4 | **Would this be the first direct visualisation of the expansion of the universe?** |
|  | Theodor Hänsch: Another thing that has already been done, is you could search for planets or distant stars, by looking at variable Doppler shifts of Fraunhofer lines of these stars. Because the star and planet, they really orbit around Earth, come with a centre of gravity, and so this has been successful in the search for Jupiter-like big planets. But if you can improve the accuracy with which one can calibrate these spectral lines, you might have a chance to discover Earth-like planets in distant solar systems. |
| Q15 | **So with all this to do arising from that innovation, it’s perhaps surprising that you don’t stop there, but you go on. I mean your talk here, for instance, is entitled *Quantum Laboratory On A Chip*, and you’re moving into new areas, you’re constantly innovating. Is it just again the need to keep playing and keep experimenting?** |
|  | Theodor Hänsch: Right, I think my approach has been, I want to be able to make mistakes quickly. So I don’t like to have large scale complex experiments. Some of my students, though, do, and some of my postdocs, so we are doing some of the serious research also, but in my own work I like to be able to improvise quickly, and if I see, oh, that’s a bad idea, move onto something else. |
| Q4 | **What’s quickly? What defines quickly?** |
|  | Theodor Hänsch: Say a week timescale, to get the first feeling whether an idea is feasible or not. |
| Q6 | **And yourself, do you still go into the lab regularly and play?** |
|  | Theodor Hänsch: I actually have my own lab. I have downtime at the University of Munich, the so-called toy store or toy lab, which has a collection of toys, I have to admit. But for me it’s the only lab where I know where everything is in the lab. So if a student works, if I need a screwdriver I spend 15 minutes to open all the drawers. In my own lab I know where the screwdriver is and which lenses I have, and where to find them. So I can try out things quickly, and I take advantage of that. |
| Q7 | **And turning to the students, when you pick students to come and work with you, what do you look for in the people that you want to bring on?** |
|  | Theodor Hänsch: Well of course I look at grades and letters of reference. Typically I talk to them for a while, I show them experiments. Other people in the group show them, and I try to judge from the kinds of questions they ask, and whether they show interest or not, whether they have the potential to be successful. And I think one indication is that indeed they can get excited about something, and that they have in the past done things like hobby holograms, or who knows? Just something that shows that science is indeed something that they enjoy. |
| Q11 | **That makes sense. If you are looking around the world for people, though, how do you tap the resources that are now coming out of China and India, and things like that? Are you able to reach out to students worldwide and bring them to work with you in Munich?** |
|  | Theodor Hänsch: We have students from different countries. We have a very good student from Japan. We have French students. Not so many from China, I think there are very good Chinese students, but in the past they would go to the US universities. But maybe that’s changing, in particular with the visa problems in the US. |
| Q1 | **I suppose that must be working to Europe’s benefit, yes, to a certain extent, yes. And a last thought, the students here at Lindau, would you have any particular advice to them that they should take away about managing their scientific careers?** |
|  | Theodor Hänsch: I think it’s very important to find something that one is deeply interested in and that one enjoys. If they find something like that, they don’t mind working day and night because they’re obsessed. Then I thin they’re on the right track. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0054** |
| **Biographical** | I was born in Washington, D.C., on February 19, 1941, the eldest of four sons. My father, Bertram Meyer, born in Philadelphia, son of immigrant Jewish parents from Czechoslovakia-Hungary, had attended the University of Pennsylvania as an English major. From 1941 a staff member of U.S. Sen. James E. Murray of Montana, he helped after the war to write the Employment Act of 1946. This law established a commitment to maintain high levels of employment and a charter for the President’s Council of Economic Advisers (CEA). He served as the first assistant to the chairman after the CEA was established in 1948. My mother, Nora (Faine), was born in the Ukraine. In 1914 her father immigrated to the United States and was joined by his wife and two children only after the end of World War I. Remarkably, both children made their way to college; my uncle completed Harvard Law School, and my mother graduated from Barnard. She majored in chemistry, but never pursued a career in science, an unimaginable goal for women in the midst of the Depression.  My childhood in Arlington, Va., a middle class suburb of Washington, was uneventful. Ours was a very intellectual family, and we were encouraged to read at a very early age. My father would always try to interest his children (and everyone around him) in his intellectual passions. At the age of 11, I was recruited to proofread his book, *The Legislative Struggle: A Study in Social Combat*, which he later used to launch an academic career. I was paid 10 cents per page, and by the time I finished, I hated the word “committee”. Dinner conversation – always intense – dealt with adult issues, mostly politics and issues of social justice. My father and mother treated us children as intellectual equals, thus greatly bolstering our self-confidence and our interest in ideas of all kinds.  In 1953 my father joined a group of Democratic exiles from the Eisenhower administration who were sent to Israel as an advisory team together with the first US aid package. Israel, then struggling to absorb half a million refugees from Europe and construct a new state, accepted the money and the advice (but was wise enough to disregard most of the advice). After the team returned in 1955, my father remained and joined the Hebrew University, where he set up the School of Business Administration. Jerusalem in the 1950’s, a small, divided city, the seat of the government and the University, contained a remarkable mixture of religious fanatics and refugees from all over the world. It was a far cry from Arlington, Va.  I was thrust into school with no knowledge of Hebrew, and for the first time in my life had to struggle to compete with my peers. One of the great advantages of growing up in Jerusalem at that time was the absence of television and the many other distractions present then and now in the United States. Thrown back onto my own resources, I became an avid reader. My brother Larry and I rapidly went through the library run by the United States Information Agency at the American Consulate in Jerusalem. Then, when my father became a professor at the Hebrew University, we had access to the University Library on Mount Scopos. From the age of 13, I was attracted to physics and mathematics. My interest in these subjects derived mostly from popular science books that I read avidly. Early on I was fascinated by theoretical physics and determined to become a theoretical physicist. I had no real idea what that meant, but it seemed incredibly exciting to spend one’s life attempting to find the secrets of the universe by using one’s mind. I studied mathematics on my own and soon, exceeding the knowledge of my teachers, was excused from mathematics classes. My high school education was excellent, with many highly qualified teachers, who today would be employed in higher education or in industry.  Determined to become a theoretical physicist, upon graduating from High School, I entered the Hebrew University and majored in physics and mathematics. The physics education was reasonably good, and the mathematics education was excellent. After receiving a B.Sc., I applied to graduate schools in the United States. At that time (but soon to change) students from Israel were unheard of, and I was only accepted at the University of California at Berkeley. In retrospect this was fortunate. Berkeley was, for many reasons, at that time a very exciting place. It was at the center of the remarkable social and political developments of the 1960’s – the fight for social justice and the opposition to the war in Vietnam. But more importantly for me, Berkeley was the center of elementary particle physics, with new discoveries appearing monthly at, what was then called, the “Rad Lab” (short for “Berkeley Radiation Laboratory”), before “radiation” became a dirty word, and the name was changed to the “Lawrence Berkeley Laboratory.” In 1964, I started to do research under the supervision of Geoffrey Chew, the charismatic leader of the S-Matrix “bootstrap” approach to the strong interactions. I found this revolutionary new theory very exciting at first, but gradually became disillusioned. I rapidly finished a thesis and spent most of my last year at Berkeley in thoughts of new directions.  Concluding my graduate studies in 1966, I was nominated to the Harvard Society of Fellows, a wonderful institution that appointed eight fellows each year in many different fields. The only responsibilities were to dine together with the senior fellows once each week. Harvard was a bit of a shock after Berkeley. Schwinger had long dominated the theoretical scene, but now a new generation of theorists was just appearing. In 1966, Shelley Glashow and Sidney Coleman had returned to Harvard and Steve Weinberg was visiting. After Berkeley I found Harvard rather formal and unfriendly, but the Society of Fellows was a wonderful experience. I was fortunate to have as junior colleagues Curtis Callan and Roman Jackiw, with whom I had very productive collaborations. This is where I started on the path that led to the discovery of asymptotic freedom and QCD (the story told in my Nobel lecture.)  In 1969, I had many offers for faculty positions. I decided to go to Princeton, as an assistant professor, where I was to stay for 27 years. Murph Goldberger and Sam Treiman were my senior theoretical colleagues at Princeton. They created a marvelous atmosphere to do research and were enormously supportive of me. They promoted me to tenure in 1971, before I had even begun to think of the possibility. Thus I was able to pursue my research free of the anxiety that often plagues young scientists today.  Between the University and the Institute for Advanced Study, there was a remarkable group of young theorists. I collaborated with John Schwarz and Andre Neveu on string theory for a while, but mostly with Curtis Callan and with Roger Dashen on gauge theory. At Princeton I was lucky to have had a remarkable group of graduate students, especially Frank Wilczek and Edward Witten. This was fortunate since I have always enjoyed collaborating with other physicists and students rather than working in solitude. I find that my best ideas emerge in heated exchanges with others. My way of dealing with students, then and now, was to involve them closely with my current work and very often to work with them directly. I always found it difficult to come up with trivial or easy problems that I could assign to a student and preferred to involve students in the much more ambitious projects that I myself was working on. Such collaborations were no doubt good training for my students, but were also very beneficial for me.  After the discovery of asymptotic freedom and the emergence of QCD, I spent many years on the dynamics of gauge theories in the attempt to solve QCD. Much progress was made, but the goal of a controllable analytic solution of the theory was not realized. By the beginning of the 1980’s, I along with many others had shifted my attention to more speculative physics – the challenge of unifying all the forces of nature and confronting quantum gravity. In the early 1980’s, I began to work on string theory again and in 1984, together with a group of younger collaborators, discovered the heterotic string, which at the time seemed to offer the possibility of explaining the Standard Model from string theory. Since then I have been working largely on string theory. Although remarkable advances have been made, the potential of this theory to unify all the forces of nature and to make contact with experiment has not yet been realized.  In 1996, I was offered the directorship of the Institute for Theoretical Physics in Santa Barbara (for the second time). I was ready for new challenges and attracted by the opportunity to come to the renowned ITP. Having spent thirty years teaching and doing research, I was ready to take on other responsibilities. The directorship of the ITP was a wonderful opportunity since most of the work consisted of scientific leadership and not administration, and I could continue a vigorous research activity as well. I have not regretted this decision and have greatly enjoyed directing the Institute and participating in the remarkable span of science that takes place at its programs.  I have two children with my first wife, Shulamith (Toaff): Ariela Gross, who is an historian and professor of law at the University of Southern California and the mother of my grandchildren, Raphaela and Sophia; and Elisheva Gross, who is completing her doctorate in psychology at the University of California at Los Angeles. I now live with my second wife, Jacquelyn Savani, and my stepdaughter, Miranda Savani, in Santa Barbara, California. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0054 = DG**  – Hello?  – Is this David Gross?  – Speaking.  – Hello. This is Joanna Rose from Stockholm. My congratulations to the prize.  – Thank you.  – I’m calling from Nobelprize.org, which is the official website of The Nobel Foundation. So, we thought that we can put this interview on the web, if it’s O.K. for you?  – You want to do an interview right now?  – Yes. We are doing it now.  – O.K.  – How are you feeling?  – I’m still in shock. And I haven’t been able to get off the phone with people calling for interviews.  – So, did you expect this prize this year?  – I don’t know, I thought it was a one in three chance.  – So, how did you sleep this night?  – Not very well. And certainly not for the last hour or two.  – But the discovery that you got the prize for, it was made more than thirty years age.  – That’s right.  – So, did you wait for the prize every year?  – No. Just the last twenty-nine years.  – I understand.  – I’m just kidding.  – I talked to Frank Wilczek. He said that he was sure that the discoveries were worth a Nobel Prize. But, not you.  – You mean he was sure at the beginning? Day one? He was sure that it was worth a Nobel Prize?  – Yes.  – Is that what he said?  – Yes. That’s what he said.  – Well, he was much younger and innocent. Well, you know, I think it took me three years before the experimental evidence was strong enough that I said, ”this is definitely true.”  – You wanted to be convinced?  – Yeah.  – So, this is what you think, that the theory must be experimentally …  – Oh sure. Theorists can be wrong, only Nature is always right.  – Are you fostering now new Nobel Prize winners?  – I certainly hope so. Well again, I’m the director of an institute of theoretical physics where I see lots of potential Nobel Prize winners. But, as theorists, one has to wait for Nature’s verdict as well. Some of the ideas are wonderful. And, maybe they’re even true.  – I understand. Like, what ideas?  – Well, I’m most excited about ideas in string theory. But they’re not yet at the stage where it’s clear that it’s true.  – They say that string theory is very very far from being experimentally …  – Right. But there are even other ideas that have been around. Theorists have wonderful ideas which take years and years to be verified. Super symmetry is one that we’re waiting and waiting for verification.  – Could you see in the beginning of the 70’s that those two young students, Frank Wilczek and David Politzer, had this potential? That they were extraordinary in some sense?  – Well, Frank was essentially my first student.  – Ever?  – Yeah. I was pretty young too. I was about 31 when he started working with me. So, he was my first graduate student and I thought that, well, all graduate students are as good as Frank.  – Did you have to revise this?  – Well, I’ve had some other good students since … Ed Witten. But yes I think I learned that not all students are as good as Frank.  – So, do you have any good advice to young students today? How they can behave and study to get the Nobel Prize once?  – Well, the advice I tell students is to think about the big problems. I mean, work on anything you can work on where you can make progress. But always keep in mind the big problems. The ones that are truly important. And, watch carefully what Nature is trying to tell us.  – How early can you do it? Did you think about becoming a scientist when you were a child?  – Yeah. Actually, I was more or less determined to be a theoretical physicist at the age of thirteen.  – O.K. So please, once more my congratulations to the prize, and I hope to meet you here in Stockholm in December.  – I look forward to it. Thank you. |
| **Interview** |  |
| Q9 | **Dr David Gross and Dr Frank Wilczek, my congratulations to the Nobel Prize. You have been waiting for a long time for this prize, how was it?** |
|  | David Gross: 63 years! |
| Q4 | **When did you realise that this was a realistic possibility?** |
|  | Frank Wilczek: I think when the experiments really started to crystallise in the late 1970’s and early -80’s, once that happened I thought it was possible. But they’re very conservative; they want to see very solid experimental evidence. They were telling in fact over lunch today they really wanted to see the curve with the arrow bars, so it took a long time for the experiments to catch up with our theories. |
| Q4 | **What was a crucial experiment?** |
|  | David Gross: There are many, it’s really been accumulation. It depends on who you talk to. Many of the original experiments were in 1974, enough to convince people, but then there were the discovery of jets which were really indications you could see quarks and then finally the discovery of jets where you could see gluons. Then, what Frank is really alluding to is the last, especially the last ten years with LAPP and then HERA where you have high precision tests, something I really never thought I would see, tests of detailed predictions and dozens of them to less than 1% accuracy, I still love to see them.  Frank Wilczek: David goes back a little bit further but even when I was a graduate student the concept that the strong interaction, this mysterious thing where the ideas were so vague and where you had the background of nuclear physics which has never become anywhere near as precise even now, that you would be talking about a few percent accuracy and these precise calculations just seemed completely off the radar screen, completely inconceivable. Just ridiculous.  David Gross: You’ll remember one of our mentors …  Frank Wilczek: Yes.  David Gross: … Sam Truman who told me about a month after we made this discovery and started to explore QCD [Quantum chromodynamics] he said, David this is a great theory and maybe you’re right but one thing I’m sure of it’ll never be proven. |
| Q4 | **But you are a theoretician so there is not such a problem, if you can prove the idea then the idea is right?** |
|  | Frank Wilczek: If it’s an idea about the physical world you can have logical coherence and you can have aesthetic congruence, but it reaches an entirely different level when it describes the actual world, real phenomena, at least to me.  David Gross: Yes, and you could be wrong.  Frank Wilczek: Could be wrong.  David Gross: Believe me! I have been wrong, very rarely.  Frank Wilczek: I’ve been wrong maybe more often, but some of my best ideas have proved, what I think my best ideas have proved not to be right or at least not right in the original form they were proposed.  David Gross: There were other issues at the very beginning. Our advance was made on trying to understand what kind of theory could explain the behaviour of quarks at very short distances. The discovery of asymptotic freedom made it immediately clear that you could do that and calculate and test those ideas, but then the converse that the force became strong at large distances and led to the confinement of quarks that was still … When we very tentatively said that the fact that the force grows strong at large distances could explain confinement was *very* tentative and we had at the time no analogues, no other examples of that and it was such a crazy … I used to have arguments with a very famous physicist at Princeton, [Eugene Wigner](https://www.nobelprize.org/prizes/physics/1963/wigner/facts/), for years and years and each time he would say, Quarks can’t exist because you could never produce them. The idea that you could base a theory on objects that you could never see directly seemed to most people – and even to us – dangerous. It took a long time, well, a long time, a few years at least, until one could see how this happened in toy models and then actually discover that there were analogues of that, analogues that people knew very well, like the Meissner effect in superconductivity that said ok well, now it’s not such a strange phenomena, there are examples in ordinary materials. Once those theoretical concepts were clear, I felt a lot more comfortable with the theory.  Frank Wilczek: It was very nervous making to be proposing a theory all of whose ingredients were unobserved particles and none of whose ingredients were observed particles. |
| Q2 | **Do you still remember this feeling, how it is to get this crazy idea?** |
|  | David Gross: It was exhilarating and scary because as Frank says it was clear from the beginning that the stakes were big, this was a big thing. Many of our smart colleagues realised this immediately and many were immediately convinced. But because the stakes were so big and the chances of it being wrong – as often happens – where none zero it was a little giddy and scary. As I remarked in my lecture, we didn’t have time much to think about it because there was so much to do, it just opened up, we got start calculating and physicist theorists really like to calculate. That’s the fun part of our business when you can … |
| Q2 | **But the idea itself it was so crazy, was it like a revelation or?** |
|  | Frank Wilczek: It was putting together several, it wasn’t just one idea, it was really putting together several ideas that were formulated in different areas and then also ideas. After the central calculation there was a lot of work in working out its consequences. I don’t know if you’ll agree but I think in many ways the most important idea was not to worry about certain problems, just to go ahead and do the things we could do and not to worry about this problem of confinement.  David Gross: One couldn’t help to worry.  Frank Wilczek: Yes, but we probably didn’t do anything about it!  David Gross: I started to think about it. No, I started to think about it but, and of course being optimists. I originally thought that well, it’ll take us maybe three to four or five years to solve the theory completely. We still haven’t of course, it’s still on-going, a very alive subject to have analytic total control over the theory at large distances where it’s hard. It’s an enormously exciting field that remains. it’s hardly dead. The fact that it got a Nobel Prize does not mean that it’s over by any means, inuclear physics, QCD is still very much alive and exciting. |
| Q3 | **It is about the future of physics. What is your favourite there?** |
|  | David Gross: There’s so many.  David Gross: Those are all fascinating things but the things that I really want to know the answer to before I disappear, have to do with the next goal of fundamental physics which is unifying the forces and also understanding the … We can push back the history of the universe to almost its beginning where everything breaks down, even this enormously ambitious and exciting and promising approach based on strength, breaks down so far as well. Something very deep is missing but we have a lot of clues and a lot of very surprising clues and I will be very disappointed if those questions aren’t answered in my lifetime. |
| Q4 | **Do you think that string theory is the right way to get so far back in time?** |
|  | David Gross: Yes.  Frank Wilczek: It may be an important direction, I don’t know even know what string theory is but they’re clearly something important. It’s not a well formulated theory in a sense of QCD, with algorithms and clear-cut predictions, there are clearly some important ideas there.  David Gross: QCD is a revolution in the making and so we’re somewhere in the beginning or the middle or who knows where? But as Frank said it’s not yet a well formed … We don’t know what string theory is, in fact we’ve discovered recently that … Frank was somewhat conservative in his middle ages, he’s always been somewhat more sceptical about string theory than I, but we’ve now got the situation where string theory can’t be killed because certain of its aspects is almost the same as the theories that make up the standard model. It’s so continuously part of the physics of the standard model that it can’t be wrong, it might and it might very well be insufficient and at a conceptual level I’ve no doubt that in its present formulation or where we have so far got in understanding what string theory is or should be, its missing some fundamental new concepts. But it’s really not as we’ve learnt much to our amazement, any different than the gauge theories that we use to explain all the fundamental forces that so far observed. |
| Q4 | **What concepts do you have I mind?** |
|  | David Gross: What is space and what is time. |
| Q4 | **What is space and what is time?** |
|  | David Gross: What is space and time?  Frank Wilczek: I mentioned in my lecture yesterday how much physics and several Nobel Prizes in theoretical physics have come out of reconciling special relatively with quantum mechanics. String theory is one aspect perhaps or one attempt to reconcile general relatively with quantum mechanics which poses at least equal problems.  David Gross: Personally I really think that we’re in a pre-revolutionary state and that the next revolution that will deal with the deeper meaning of space and time, quantum space and time which is what general relativity is, and string theory already suggests some, will be greater than that of quantum mechanics because a deterministic view of the world was, is sort of what you naturally think of although it’s not so clear from talking to ordinary people. But our macroscopic view of space and time is truly built into not only to the way we lay people think about the world, but the way we formulate physics. Physics after all is supposedly the science of taking the present and predicting the future. If time itself is just an approximate concept which works for large time how do we formulate the laws of physics? For me it’s the most interesting question.  **And you hope to hear the answer.**  David Gross: I have very little faith that I will be able to answer that myself, but some young person might and I hope to be around to hear about the answer, yes. |
| Q4 | **So, we have to wait like another four years?** |
|  | Frank Wilczek: Yes, that may very well happen on a timescale of three or four years, so well within our productive lifetime, so that’s a really exciting prospect.  David Gross: That might allow us to indirectly write test theories like string theory.  Frank Wilczek: Sometimes the crucial clues come from domains where they’re not anticipated, that certainly happened for QCD. The crucial experiments were these …  David Gross: Absolutely.  Frank Wilczek: … SLAC [SLAC National Accelerator Laboratory] experiments which were not highly anticipated by the large body of theoretical community.  David Gross: Almost no-one wanted them.  Frank Wilczek: They were these kind of funny, no-one wanted to do them or think about them!  David Gross: My favourite example is the year of Einstein, 1905, he wrote three papers one proving the existence of atoms to even Ernst Mach’s agreement after Einstein’s work on Brownian motion Mach denied the existence of atoms, agreed that they must exist. He didn’t actually observe atoms, they didn’t have scanning, tunnelling microscopes, he explained a phenomena that was discovered by a botanist 50 years before the motion of little grains by the random movement of atoms. What more indirect evidence for atoms could you imagine but it was sufficient to explain, to convince the most die-hard, positivist of all Ernst Mach.  Frank Wilczek: It was very quantitative and non-trivial, that’s what made it convincing.  David Gross: I still have faith that if we had enough understanding of the theory which Einstein did at that time, of atomic theory, that we could indirectly test a theory like string theory but the theory has to do better. Unless, we can’t always rely on experimentalists to give us the clues. |
| Q9 | **Now you know you received it, what is the difference before and after?** |
|  | David Gross: It’s a bit of a relief.  Frank Wilczek: It’s a relief. An honest relief, we’ll sleep better in October now and feel better about ourselves.  David Gross: I used to say October is, to paraphrase [T.S. Eliot](https://www.nobelprize.org/prizes/literature/1948/eliot/facts/), October is the cruellest month! |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0055** |
| **Biographical** |  |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0055 =** |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0056** |
| **Biographical** | The most deeply formative events of my scientific career long preceded my first contact with the research community; indeed, some of them preceded my birth.  My grandparents emigrated from Europe in the aftermath of World War I, as young teenagers; on my father’s side they came from Poland and on my mother’s side from Italy, near Naples. My grandparents arrived with nothing, and no knowledge of English. My grandfathers were a blacksmith and a mason, respectively. Both my parents were born on Long Island, in 1926, and they have lived there ever since. I was born in 1951, and grew up in a place called Glen Oaks, which is in the northeast corner of Queens, barely within the city limits of New York City.  I’ve always loved all kinds of puzzles, games, and mysteries. Some of my earliest memories are about the questions I “worked on” even before I went to school. When I was learning about money, I spent a lot of time trying out various schemes of exchanging different kinds of money (e.g., pennies, nickels, and dimes) in complicated ways back and forth, hoping to discover a way to come out ahead. Another project was to find ways of getting very big numbers in a few steps. I discovered simple forms of repeated exponentiation and recursion for myself. Generating large numbers made me feel powerful.  With these inclinations, I suspect I was destined for some kind of intellectual work. A few special circumstances led me to science, and eventually to theoretical physics.  My parents were children during the time of the Great Depression, and their families struggled to get by. This experience shaped many of their attitudes, and especially their aspirations for me. They put very great stock in education, and in the security that technical skill could bring. When I did well in school they were very pleased, and I was encouraged to think about becoming a doctor or an engineer. As I was growing up my father, who worked in electronics, was taking night classes. Our little apartment was full of old radios and early-model televisions, and with the books he was studying. It was the time of the Cold War. Space exploration was a new and exciting prospect, nuclear war a frightening one; both were ever-present in newspapers, TV, and movies. At school, we had regular air raid drills. All this made a big impression on me. I got the idea that there was secret knowledge that, when mastered, would allow Mind to control Matter in seemingly magical ways.  Another thing that shaped my thinking was religious training. I was brought up as a Roman Catholic. I loved the idea that there was a great drama and a grand plan behind existence. Later, under the influence of [Bertrand Russell’s](https://www.nobelprize.org/nobel_prizes/literature/laureates/1950/index.html) writings and my increasing awareness of scientific knowledge, I lost faith in conventional religion. A big part of my later quest has been trying to regain some of the sense of purpose and meaning that was lost. I’m still trying.  I went to public schools in Queens, and was fortunate to have excellent teachers. Because the schools were big, they could support specialized and advanced classes. At Martin van Buren High School there was a group of thirty or so of us who went to many such classes together, and both supported and competed with one another. More than half of us went on to successful scientific or medical careers.  I arrived at the University of Chicago with large but amorphous ambitions. I flirted with brain science, but soon decided that the central questions were not ready for mathematical treatment, and that I lacked the patience for laboratory work. I read voraciously in many subjects, but I wound up majoring in mathematics, largely because doing that gave me the most freedom. During my last term at Chicago, I took a course about the use of symmetry and group theory in physics from Peter Freund. He was an extremely enthusiastic and inspiring teacher, and I felt an instinctive resonance with the material. I went to Princeton University as a graduate student in the math department, but kept a close eye on what was going on in physics. I became aware that deep ideas involving mathematical symmetry were turning up at the frontiers of physics; specifically, the gauge theory of electroweak interactions, and the scaling symmetry in Wilson’s theory of phase transitions. I started to talk with a young professor named David Gross, and my proper career as a physicist began.  The great event of my early career was to help discover the basic theory of the strong force, QCD. That is the subject of the following lecture. The equations of QCD are based on gauge symmetry principles, and we make progress with them using (approximate) scaling symmetry. It was very gratifying to find that the ideas I admired as a student could be used to get a powerful and accurate theory for an important part of fundamental physics. I continue to apply these ideas in new ways, and I am certain that they have a great future.  An aspect of my later work that is not much reflected in the lecture, has been to use insights and methods from “fundamental” physics to address “applied” questions, and vice versa. I’m not sure that fractional quantum numbers, transmuted quantum statistics, exotic superfluidities, or the gauge theory of swimming at low Reynolds number have really arrived as applied physics (yet?), but I’ve derived a lot of joy from my discoveries in these areas.  To me, the unity of knowledge is a living ideal and goal. I continue, as in my student days, to read voraciously in many subjects, and to think about them. I hope to further expand the horizons of my writing and work in the future. |
| **Autobiography** |  |
| **Podcast** | **No script** |
| **Telephone**  **interview** | **0056 = FW**  – Hello?  – Hello, is this Frank Wilczek?  – Yes. Hello.  – Hello. My name is Joanna Rose. I call from Stockholm.  – Oh, hello.  – My congratulations to the Nobel Prize.  – Thank you very much.  – I’m calling from Nobelprize.org, which is the official website of the Nobel Foundation, and I would like to make a short interview with you, which we will put on the website. Have you ever visited it?  – I have. Not recently, I guess I felt maybe it would be a jinx, but maybe a year or so ago I looked at it.  – Now you will be there as a Nobel Prize winner.  – Yeah, that will be nice. Probably I’ll visit more frequently now.  – How does it feel now?  – Well, I haven’t really had a chance to absorb it. I’m very happy of course, and a real high point was I very quickly called my parents. It was really really gratifying because they’re second generation Americans. Came as refugees from Europe – their parents were refugees from Europe, and they struggled during their Depression and really worked hard to get an education, and to get me an education. It was a real milestone for our family.  – Where do they live?  – They live on Long Island, sort of the outskirts of New York City.  – Did you expect the message?  – Well, it certainly was not a shock. I mean, I’m well aware of the value of our work. And many people have mentioned the possibility … even if I didn’t think of it myself. But of course there are many worthy contenders, and in any particular year the odds are favorable. So, it’s both not surprising and surprising.  – When did you realize that the discovery was really worth a Nobel Prize?  – I thought so, I think probably around 1975 or -6. Before very many people realized it. Maybe before anyone else. David Gross, who I worked with wasn’t so sure that it was of that significance early on. But I thought so right away.  – So, how come you believed in it?  – Well because I believed … the fact that we had a theory that was mathematically precise, that couldn’t be changed and that was correct in explaining some crucial phenomena, meant that it was right and that it would open up … since it was right, it would only be a matter of time to really proving decisively for everyone that it was right, and then beyond that it turned out to be much more fruitful than I anticipated. It not only … really cracked the problem of the strong interaction – what makes protons and neutrons, and what holds atomic nuclei together – but the nature of the solution, which was to show that things become simple at high energies and short distances, really opened up the possibility of thinking about the early universe in a controlled scientific way. And also of analyzing high-energy experiments in a powerful, insightful way that would have seemed impossible before.  – But you were still very young when you made the discovery weren’t you?  – Yes, I was 21.  – But sure of the result.  – Well, sure of the result because I calculated it. The significance that … I mean, that took longer I guess. It took a couple of years before I really was convinced that it was the right thing, and then once I was convinced of that I realized that it was very very important. And it has only become more important over the years.  – But, this was even before you received your PhD.  – Yes. This was part of the process of searching around for something to do for a PhD.  – So, what advice would you give young students today?  – Think for yourself. And think about Nature.  – So, what are your plans for today?  – My immediate plan I think is to … after I talk to you … is to hang up the phone, ignore other phone calls and take a walk, take a long walk, and try to clear my head. And then I’ll have a plan.  – That’s great. Thank you so much for speaking to me.  – Alright. Thank you very much.  – Bye bye.  – Bye. |
| **Interview** |  |
| Q9 | **Dr David Gross and Dr Frank Wilczek, my congratulations to the Nobel Prize. You have been waiting for a long time for this prize, how was it?** |
|  | David Gross: 63 years! |
| Q21 | **When did you realise that this was a realistic possibility?** |
|  | Frank Wilczek: I think when the experiments really started to crystallise in the late 1970’s and early -80’s, once that happened I thought it was possible. But they’re very conservative; they want to see very solid experimental evidence. They were telling in fact over lunch today they really wanted to see the curve with the arrow bars, so it took a long time for the experiments to catch up with our theories. |
| Q4 | **What was a crucial experiment?** |
|  | David Gross: There are many, it’s really been accumulation. It depends on who you talk to. Many of the original experiments were in 1974, enough to convince people, but then there were the discovery of jets which were really indications you could see quarks and then finally the discovery of jets where you could see gluons. Then, what Frank is really alluding to is the last, especially the last ten years with LAPP and then HERA where you have high precision tests, something I really never thought I would see, tests of detailed predictions and dozens of them to less than 1% accuracy, I still love to see them.  Frank Wilczek: David goes back a little bit further but even when I was a graduate student the concept that the strong interaction, this mysterious thing where the ideas were so vague and where you had the background of nuclear physics which has never become anywhere near as precise even now, that you would be talking about a few percent accuracy and these precise calculations just seemed completely off the radar screen, completely inconceivable. Just ridiculous.  David Gross: You’ll remember one of our mentors …  Frank Wilczek: Yes.  David Gross: … Sam Truman who told me about a month after we made this discovery and started to explore QCD [Quantum chromodynamics] he said, David this is a great theory and maybe you’re right but one thing I’m sure of it’ll never be proven. |
| Q4 | **But you are a theoretician so there is not such a problem, if you can prove the idea then the idea is right?** |
|  | Frank Wilczek: If it’s an idea about the physical world you can have logical coherence and you can have aesthetic congruence, but it reaches an entirely different level when it describes the actual world, real phenomena, at least to me.  David Gross: Yes, and you could be wrong.  Frank Wilczek: Could be wrong.  David Gross: Believe me! I have been wrong, very rarely.  Frank Wilczek: I’ve been wrong maybe more often, but some of my best ideas have proved, what I think my best ideas have proved not to be right or at least not right in the original form they were proposed.  David Gross: There were other issues at the very beginning. Our advance was made on trying to understand what kind of theory could explain the behaviour of quarks at very short distances. The discovery of asymptotic freedom made it immediately clear that you could do that and calculate and test those ideas, but then the converse that the force became strong at large distances and led to the confinement of quarks that was still … When we very tentatively said that the fact that the force grows strong at large distances could explain confinement was *very* tentative and we had at the time no analogues, no other examples of that and it was such a crazy … I used to have arguments with a very famous physicist at Princeton, [Eugene Wigner](https://www.nobelprize.org/prizes/physics/1963/wigner/facts/), for years and years and each time he would say, Quarks can’t exist because you could never produce them. The idea that you could base a theory on objects that you could never see directly seemed to most people – and even to us – dangerous. It took a long time, well, a long time, a few years at least, until one could see how this happened in toy models and then actually discover that there were analogues of that, analogues that people knew very well, like the Meissner effect in superconductivity that said ok well, now it’s not such a strange phenomena, there are examples in ordinary materials. Once those theoretical concepts were clear, I felt a lot more comfortable with the theory.  Frank Wilczek: It was very nervous making to be proposing a theory all of whose ingredients were unobserved particles and none of whose ingredients were observed particles. |
| Q2 | **Do you still remember this feeling, how it is to get this crazy idea?** |
|  | David Gross: It was exhilarating and scary because as Frank says it was clear from the beginning that the stakes were big, this was a big thing. Many of our smart colleagues realised this immediately and many were immediately convinced. But because the stakes were so big and the chances of it being wrong – as often happens – where none zero it was a little giddy and scary. As I remarked in my lecture, we didn’t have time much to think about it because there was so much to do, it just opened up, we got start calculating and physicist theorists really like to calculate. That’s the fun part of our business when you can … |
| Q6 | **But the idea itself it was so crazy, was it like a revelation or?** |
|  | Frank Wilczek: It was putting together several, it wasn’t just one idea, it was really putting together several ideas that were formulated in different areas and then also ideas. After the central calculation there was a lot of work in working out its consequences. I don’t know if you’ll agree but I think in many ways the most important idea was not to worry about certain problems, just to go ahead and do the things we could do and not to worry about this problem of confinement.  David Gross: One couldn’t help to worry.  Frank Wilczek: Yes, but we probably didn’t do anything about it!  David Gross: I started to think about it. No, I started to think about it but, and of course being optimists. I originally thought that well, it’ll take us maybe three to four or five years to solve the theory completely. We still haven’t of course, it’s still on-going, a very alive subject to have analytic total control over the theory at large distances where it’s hard. It’s an enormously exciting field that remains. it’s hardly dead. The fact that it got a Nobel Prize does not mean that it’s over by any means, inuclear physics, QCD is still very much alive and exciting. |
| Q3 | **It is about the future of physics. What is your favourite there?** |
|  | David Gross: There’s so many.  David Gross: Those are all fascinating things but the things that I really want to know the answer to before I disappear, have to do with the next goal of fundamental physics which is unifying the forces and also understanding the … We can push back the history of the universe to almost its beginning where everything breaks down, even this enormously ambitious and exciting and promising approach based on strength, breaks down so far as well. Something very deep is missing but we have a lot of clues and a lot of very surprising clues and I will be very disappointed if those questions aren’t answered in my lifetime. |
| Q4 | **Do you think that string theory is the right way to get so far back in time?** |
|  | David Gross: Yes.  Frank Wilczek: It may be an important direction, I don’t know even know what string theory is but they’re clearly something important. It’s not a well formulated theory in a sense of QCD, with algorithms and clear-cut predictions, there are clearly some important ideas there.  David Gross: QCD is a revolution in the making and so we’re somewhere in the beginning or the middle or who knows where? But as Frank said it’s not yet a well formed … We don’t know what string theory is, in fact we’ve discovered recently that … Frank was somewhat conservative in his middle ages, he’s always been somewhat more sceptical about string theory than I, but we’ve now got the situation where string theory can’t be killed because certain of its aspects is almost the same as the theories that make up the standard model. It’s so continuously part of the physics of the standard model that it can’t be wrong, it might and it might very well be insufficient and at a conceptual level I’ve no doubt that in its present formulation or where we have so far got in understanding what string theory is or should be, its missing some fundamental new concepts. But it’s really not as we’ve learnt much to our amazement, any different than the gauge theories that we use to explain all the fundamental forces that so far observed. |
| Q4 | **What concepts do you have I mind?** |
|  | David Gross: What is space and what is time. |
| Q4 | **What is space and what is time?** |
|  | David Gross: What is space and time?  Frank Wilczek: I mentioned in my lecture yesterday how much physics and several Nobel Prizes in theoretical physics have come out of reconciling special relatively with quantum mechanics. String theory is one aspect perhaps or one attempt to reconcile general relatively with quantum mechanics which poses at least equal problems.  David Gross: Personally I really think that we’re in a pre-revolutionary state and that the next revolution that will deal with the deeper meaning of space and time, quantum space and time which is what general relativity is, and string theory already suggests some, will be greater than that of quantum mechanics because a deterministic view of the world was, is sort of what you naturally think of although it’s not so clear from talking to ordinary people. But our macroscopic view of space and time is truly built into not only to the way we lay people think about the world, but the way we formulate physics. Physics after all is supposedly the science of taking the present and predicting the future. If time itself is just an approximate concept which works for large time how do we formulate the laws of physics? For me it’s the most interesting question.  **And you hope to hear the answer.**  David Gross: I have very little faith that I will be able to answer that myself, but some young person might and I hope to be around to hear about the answer, yes. |
| Q4 | **So, we have to wait like another four years?** |
|  | Frank Wilczek: Yes, that may very well happen on a timescale of three or four years, so well within our productive lifetime, so that’s a really exciting prospect.  David Gross: That might allow us to indirectly write test theories like string theory.  Frank Wilczek: Sometimes the crucial clues come from domains where they’re not anticipated, that certainly happened for QCD. The crucial experiments were these …  David Gross: Absolutely.  Frank Wilczek: … SLAC [SLAC National Accelerator Laboratory] experiments which were not highly anticipated by the large body of theoretical community.  David Gross: Almost no-one wanted them.  Frank Wilczek: They were these kind of funny, no-one wanted to do them or think about them!  David Gross: My favourite example is the year of Einstein, 1905, he wrote three papers one proving the existence of atoms to even Ernst Mach’s agreement after Einstein’s work on Brownian motion Mach denied the existence of atoms, agreed that they must exist. He didn’t actually observe atoms, they didn’t have scanning, tunnelling microscopes, he explained a phenomena that was discovered by a botanist 50 years before the motion of little grains by the random movement of atoms. What more indirect evidence for atoms could you imagine but it was sufficient to explain, to convince the most die-hard, positivist of all Ernst Mach.  Frank Wilczek: It was very quantitative and non-trivial, that’s what made it convincing.  David Gross: I still have faith that if we had enough understanding of the theory which Einstein did at that time, of atomic theory, that we could indirectly test a theory like string theory but the theory has to do better. Unless, we can’t always rely on experimentalists to give us the clues. |
| Q9 | **Now you know you received it, what is the difference before and after?** |
|  | David Gross: It’s a bit of a relief.  Frank Wilczek: It’s a relief. An honest relief, we’ll sleep better in October now and feel better about ourselves.  David Gross: I used to say October is, to paraphrase [T.S. Eliot](https://www.nobelprize.org/prizes/literature/1948/eliot/facts/), October is the cruellest month! |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0057** |
| **Biographical** | I was born in 1928, June 25, in Moscow, USSR (now Russia). My parents were physicians. I graduated from high school in 1943 and was accepted as student of the Institute for Power Engineers. In 1945 I transferred to the Physics Department of the Moscow State University, from which I graduated in 1948 with a diploma (M.Sc. degree). After that I was accepted, as a Postgraduate (Ph.D. student) to the Institute for Physical Problems (now P.L. Kapitza Institute). My scientific adviser was L.D. Landau. After I defended in 1951 a thesis on thermal diffusion in completely and incompletely ionized plasmas, I got the Candidate of Science (Ph.D.) degree and was taken to the staff of the Institute, as Junior Scientist.  In 1951–1952 I worked with the experimentalist of the same institute, N.V. Zavaritskii on the experimental verification of the predictions of the recently published Ginzburg-Landau theory of superconductivity on the critical magnetic field of thin films. This resulted in our discovery of the “superconductors of the second group” (now *Type II superconductors*). After that I started to work on the magnetic properties of bulk Type II superconductors, and came to the conclusion that the transition from superconducting to the normal state happens gradually in increasing field with two limiting critical fields. Between these two values the field gradually penetrates the superconductor forming thin threads of magnetic flux surrounded by vortex currents. The array of these quantum vortices forms a regular structure (now referred in the literature, as *Abrikosov vortex lattice*). I compared my results with the magnetization curves obtained experimentally in the 1930s for superconducting alloys, and there was a perfect fit. The experimentalist explained their data, as due to inhomogenity of their samples. My paper was published in 1957 but the experimentalists accepted the vortex lattice only 10 years later, after it was demonstrated by decoration experiments.  In the middle of 1950s I worked also on the transition from the insulating molecular phase into the atomic metallic phase in hydrogen and on the structure of hydrogen planets. Another my topic was quantum electrodynamics at high energies. The latter works became my Doctor of Science thesis (this degree is similar to Habilitation in Germany), which I defended in 1955.  In the end of 1950s – beginning of 1960s we worked with L. Gor’kov on the microscopic theory of superconductivity. We constructed the theory of superconductors in a high-frequency field (with I.M. Khalatnikov) and the theory of superconductors with magnetic impurities, where we discovered the possibility of the so-called *gapless superconductivity*. We also solved the mystery of the finite Knight shift at zero temperature, taking into account the spin-orbit scattering. Simultaneously we worked with I.M. Khalatnikov on the theory of nonsuperfluid He3: thermodynamics, kinetics, sound dispersion, light and g-ray scattering, etc. These works were based on the theory of a Fermiliquid by L. Landau. I worked also during this time on the theory of strongly compressed matter.  In 1961 we published a book with L. Gor’kov and I. Dzyaloshinskii “Quantum field theory methods in statistical physics”. Originally written in Russian, it was translated into English, German, Chinese, Japanese, and became (and still is) the main textbook on the subject.  In 1962–63 with my postgraduate L. Falkovsky we constructed the theory of semimetals of the Bi type. These substances have a very small number of charge carriers (in Bi ~ 10-5 per atom) and a very peculiar crystalline lattice differing from a simple cubic lattice by two small deformations. The resulting lattice has two atoms per unit cell, and in principle it could be an insulator. However, a simple cubic lattice has one atom per unit cell, and should be a “good” metal with the number of carriers of the order of one per atom, and small deformations cannot transform it into an insulator. This paradox can be resolved by constructing an artificial phase, which at zero deformation has an energy higher than a conventional metal but the energy decreases with deformation, so that eventually this phase becomes energetically favorable. This gives the opportunity to approach the (almost) insulating phase continuously. In this series of papers the energy spectrum was calculated, and the metalinsulator transition with the vanishing of the energy gap was predicted. The infrared properties were analyzed and the transparency thresholds in the frequency were established. All this was confirmed by experiments.  In 1962 our dear teacher, L.D. Landau, got in a car accident and suffered heavy injuries. His life was saved but his brain was damaged, and he never returned to science after that. He died in 1968 from remote consequences of the accident. After the accident the attitude towards theorists at the Institute for Physical Problems changed, and the Landau Group started to think about leaving.  In 1964 I was elected Corresponding Member of the Academy of Sciences, USSR (now Russian Academy of Sciences). In 1966 I was awarded the Lenin Prize together with V.L. Ginzburg and L.P. Gor’kov “for the theory of superconductivity in strong magnetic fields”.  In 1965 I became the head of the Condensed Matter Theory Department in the newly organized Institute for Theoretical Physics (later named L.D. Landau Institute). I was one of its organizers.  In 1965–68 I published several papers on the Kondo effect at low temperatures, where I established the appearance of a resonance in the scattering amplitude of an electron from a magnetic impurity atom (now *Abrikosov-Suhl resonance*).  In 1971 I published a book “Introduction to the Theory of Normal Metals”, which was translated into English. In 1972 I was awarded the International Fritz London Award for my works on low-temperature physics.  In 1970–75 I constructed the theory of gapless semiconductors, where I showed, that in substances of the type of HgTe, a strong interaction region close to the band matching point exists where the dependencies of various quantities on temperature and magnetic field are described by nontrivial power laws. At the same time I worked on the theory of an excitonic transition in Bi in strong magnetic fields. The predictions were in complete agreement with experimental data.  In 1975 I was awarded the title Doctor of Sciences Honoris Causa by the University of Lausanne (Switzerland).  In 1977–81 with my postgraduate, I.A. Ryzhkin we constructed the theory of one-dimensional and quasi-one-dimensional metals. The main results were a) the probability distribution function of resistivity of a one-dimensional wire, where due to mesoscopic effects there was no self-averaging, and b) the conclusion that suppression of superconductivity in (TMTSF)2PF6 by nonmagnetic defects was an evidence of triplet pairing. Later this was confirmed. During the same years I worked on the theory of spin-glasses with short-range interaction, including semiconductor-based spin-glasses.  In 1982 with a group of experimentalists I was awarded the State Prize of USSR, for the works on semimetals and gapless semiconductors. In 1987 I was elected Full Member of the Academy of Sciences. In 1988 I published the book “Fundamentals of the Theory of Metals” on which I worked three years. It was translated into English and Japanese. The same year I was elected Director of the High Pressure Physics Institute in Troitsk, Moscow District. In 1989 L. Gor’kov, I. Dzyaloshinskii and myself were awarded the L.D. Landau award of the Academy of Sciences, USSR, for our book “Field Theory Methods in Statistical Physics”.  In 1991 I accepted the offer of the Argonne National Laboratory, USA, and became Distinguished Argonne Scientist. Since then I continue to work at the same position. In the same year, together with V.L. Ginzburg and L.P. Gor’kov I was awarded the International John Bardeen Award and was elected as Foreign Honorary Member to the American Academy of Arts and Sciences. In 1992 I was elected Fellow of the American Physical Society.  Being at Argonne I became interested by the high-Tc layered cuprates. This interest resulted in a theory which was based on the Bardeen-Cooper-Schrieffer approach but took into account the specific features of the electron spectrum, namely the quasi-two-dimensionality and the existence of the “extended saddle point singularities”, or “flat regions”, which made the motion of quasiparticles in some regions of the Fermi surface quasi-one-dimensional. At the same time these regions had the maximal density of states. Another idea was the resonant tunneling mechanism of the electron transport between the CuO2 planes. On the basis of these ideas I was able to explain almost all the unusual behavior of the high-Tc layered cuprates, including the isotope effect, neutron scattering, pseudogap and the metal-insulator transition.  In 1998 in connection with the experiments performed at Argonne and the University of Chicago I introduced a new phenomenon: *“Quantum Linear Magnetoresistance”*. The analysis of experimental data showed that it was first discovered experimentally by Piotr Kapitza, as early as 1928, but was confused with a different phenomenon. During these years in connection with experiments I studied also the effects of quantum interference on the magnetoresistance of layered substances and constructed a theory of an s-type superconductivity in UGe2.  In 1999 I became a naturalized US citizen. In 2000 I was elected member of the National Academy of Sciences USA, and in 2001, as Foreign Member of the Royal Society of London, UK. In 2003 I received the title Doctor of Sciences Honoris Causa from the University of Bordeaux (France) and together with V. Ginzburg and A. Leggett was awarded the Nobel Prize in Physics “for pioneering work on the theory of superconductivity and super-fluidity”.  Apart from research, almost all my life I was teaching. First I was assistant, associated, full Professor at the Moscow State University, 1950–1969, then Professor at the Gorky (now Nizhniy Novgorod) University, 1970–72, and eventually Chair for theoretical physics at the Moscow Institute for Steel and Alloys (Technical University), 1976–1991. In USA I am Adjunct Professor at the University of Illinois at Chicago and the University of Utah. In addition I am a Leverhulm Adjunct Professor at the University of Loughborough, UK. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0057 = AA** |
| **Interview** |  |
| Q5 | **And what would you say, Dr Abrikosov?** |
|  | Alexei A. Abrikosov: My answer, for me the inspiration was always experiment. Some experimental facts which were strange, could not get an immediate explanation, and so on. These were always my source of inspiration, and I think that only that. Yes, I am very closely connected to experiment. Not mathematics, not models, nothing but experimental data. And so of course after that, what Tony said, the thinking and so on, even sleepless nights, that is of course how it comes. However … |
| Q4 | **It’s a part of scientific research?** |
|  | Alexei A. Abrikosov: Yes. However, my ideas I just take from experiments.  Anthony J. Leggett: Yes, I would certainly agree with that. I always find that the main stimulus to theory is some curious experimental result that seems totally outrageous and unnatural. And one tries to understand it. |
| Q4 | **But there is one problem in your fields of research in super conductivity, I would say, that you can’t predict the super conductors that would work in high temperatures, like room temperature. What do you think about that?** |
|  | Alexei A. Abrikosov: You see, there was some experiment which was actually performed in the United States by an American physicist and a Russian visitor, which inspired these experiments where they tried to find high temperature super conductivity. They failed eventually. However, they inspired me to some extent, and therefore I even published some model, how high temperature super conductivity could be achieved. No such, actually, it was not achieved on that path but actually nobody tried to find it on that path, and so therefore I still have hopes that that is at least one of the good paths for searching high temperature super conductors. |
| Q4 | **And Dr Leggett, what about prediction?** |
|  | Anthony J. Leggett: Well, if you … there are about 100 elements known, if you consider a compound which involves six of these elements, then crudely speaking there are, let me think, a trillion such compounds. Nature has never made most of these compounds. We will certainly not be able to make most of these compounds in any reasonable time. Somewhere out there I would take a large bet that there are substances that will be super conducting at a room temperature. We just don’t know where they are in this immense space. Once we have a generally accepted theory of cuprate super conductivity, I think we may be in a much better position to go and look for them. |
| Q4 | **Yes. So you have to find something entirely new. Do you agree with that?** |
|  | Alexei A. Abrikosov: But with some idea.  Anthony J. Leggett: Well, I think, yes, I agree with part of that, at least. I would tend to, without wanting to express myself about whether we at the moment have a satisfactory theory or not, I would tend to agree with the belief that within the cuprate family it’s not very likely we’ll get much higher transition temperatures. What I do believe, however, is that there may be ways of understanding the cuprates, which will lead us to other classes of materials which might be room temperatures in the conductors. So I’m also optimistic. |
| Q20 | **So maybe, somehow you need the genius. I’d like to ask you what is it that makes some people do the discovery and others who work as hard as the discoverers don’t do that?** |
|  | Anthony J. Leggett: A large element of luck. Somehow, I suppose the people who do make big discoveries are ones who somehow manage to free themselves from conventional ways of thinking and to see the subject from a new perspective. But how you quantify that I wouldn’t know. |
| Q21 | **It’s also good luck if something doesn’t work as you expect, as I understand. What would you say about that?** |
|  | Anthony J. Leggett: Well, yes. Again, some of the most stimulating experiments, to a theorist, are those which don’t come out as you confidently expected them to. |
| Q4 | **You mean that the cat is both alive and dead?** |
|  | Anthony J. Leggett: Well, it’s difficult to express the result in classical terms, but if you take the interpretation of quantum mechanics seriously and you apply the same interpretation at the level of the cat as you do at the level of the atom, then you do seem to reach the conclusion that it is not definitely in one state or the other until observed. And that, of course, is the famous quantum measurement paradox or [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/schrodinger-facts.html)‘s paradox. That’s a very different situation from what one normally gets in the sort of standard applications of super conductivity and super fluidity. |
| Q4 | **I understand. But what about quantum mechanics? Isn’t it bizarre that you have those super positions somehow, or whatever you call it, but somehow it doesn’t follow the logics.** |
|  | Alexei A. Abrikosov: I must say, I am in a sense much simpler. The existence of liquid helium that is actually at low temperatures and that it doesn’t solidify at ambient pressure is a quantum phenomenon. That is. It is a paradox. Such an object should not exist according to classical laws. And so, already here it starts.  Anthony J. Leggett: Well, yes, I would agree. And I think that is one of number of cases one could quote in which one sees in one sense or other the macroscopic effects of quantum mechanics. But I’m including the difference between a liquid and a solid, as a macroscopic difference. But I do think there’s a big difference between this kind of case and the genuine Schrödinger’s cat kind of situation, which is one which we have not yet been really able to probe directly in experiments, although we’re working towards it. |
| Q4 | **There was a big problem even for, I would say, the most famous scientist in the world,**[**Albert Einstein**](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html)**; he could never accept quantum mechanics, because it was too bizarre for him. Do you think it is real, somehow?** |
|  | Anthony J. Leggett: I personally think it’s entirely possible that in the year 3000 we will still believe that quantum mechanics is the whole truth about the world. If we really do still believe it in the year 3000, then I think in some sense our attitude towards the physical world at the everyday level will be radically different from what it is today, because we will really have had to face up to this weirdness, which by that time I’m confident will have been amplified to the everyday level. I think it’s at least equally probable and perhaps more so, that as we go from the level of the atom to the level of the cat, we will find that somewhere along the line quantum mechanics breaks down and some new theory of which we can have at present no conception will take over. I am personally hopeful that it’s the second thing that happens. |
| Q4 | **What about quantum computers?** |
|  | Alexei A. Abrikosov: What Tony was telling you, it was exactly about quantum computers.  Anthony J. Leggett: Well, if quantum mechanics does describe the whole universe at all levels, then it seems, as far as I can see, that there is no reason in principle why one should not build a functioning quantum computer. I think, however, one may well find that the practical difficulties of doing that are just so enormous that in the end people will conclude that it just isn’t worth it. That although the price tag on a quantum computer that can factorise, say, a 500 digit number, is very large, it’s not infinite, and at some point people may just conclude that it isn’t worth the effort. |
| Q14 | **It will be too expensive, you mean?** |
|  | Anthony J. Leggett: Yes. Well, or just take too long and involve too many people etc, etc, yes. |
| Q4 | **Do you have any other guesses?** |
|  | Alexei A. Abrikosov: No, no. I don’t. Somehow, this topic, I never loved, and so therefore I always decide, I never go to conferences on quantum computing, and so on, so that’s not for me. That’s not for me. |
| Q4 | **It’s the weird, or too far from experiment, or why?** |
|  | Alexei A. Abrikosov: Well, it’s far from experiment, and from my point of view, it’s dull. I don’t know, my taste is so, I like objects, you know, I can see and I can feel them.  Anthony J. Leggett: I would think it’s probably fair to say that at least right now the challenge of quantum computing is not throwing up any very deep new conceptual questions. It’s a matter of in some sense engineering, and so whether it’s a matter of taste, whether you regard that as interesting or not. |
| Q15 | **So what is the challenge for the future, do you think, in your field?** |
|  | Alexei A. Abrikosov: In my field, first of all, if you speak like that, that we have many challenges, actually. And every time I am working on something and that is maybe a small problem from your point of view, but usually one should not divide the problems into small and large problems, because every small problem can become a large problem, or eventually, you know, develop into something. So therefore one must just, if one has a problem, one has to solve it. And that’s all. That is the main important thing. Of course, the general challenge is high temperature super conductivity, room temperature super conductivity, in my field at least, yes, but however, I understand very well that I am alone unable to solve it. Yes, and so it requires an effort of many people and for some time, yes, and experimental efforts, actually, not just theoretical attempts, yes? A theorist can give an idea where to search. However, he cannot predict that just this and this substance will be the one. No. No way.  And so just when I was in Washington, there was such a session that was dedicated to 50 years since Eisenhower gave a talk at the United Nations about peaceful applications of atomic energy. So then I said there, I spoke about it, room temperature super conductivity, and I said that in order to reach that goal, and it is reachable, I am absolutely sure about that, then the funding system for science for this particular thing must be changed entirely, because it is a long term project and you cannot expect immediate, immediate success and you cannot even predict when that success will happen. However, if you conceive that topic is solvable and so on, so then, you must just give money for that, and people will do research, yeah, and that’s all, yeah? And then Ray Orbach, who is the head of basic energy sciences in the Department of Energy, so he said: I heard what you said, and I will think about that. And so he definitely has some positive thoughts about that. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0058** |
| **Biographical** | Apart from several monographs on physics and astrophysics, I have published two books which are collections of various articles of scientific, semi-scientific and general, social and political character[1](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not1). Some of these articles contain rather much biographical material. So I do not have any wish to come back to my autobiography. However, it is the wish of the Nobel Foundation that the Nobel Prize winners, together with the lecture, wrote their autobiography. I respect the wishes of the Foundation, so I am writing. Of course, I could confine myself to brief biographical information, while writing a detailed autobiography is rather dangerous – it can bring reproach for “exhibitionism” and immodesty. Nevertheless, I have decided to write in a rather detailed and frank way, as it corresponds to my habits and tastes. One more reason to justify this decision is that I am already 87 and will hardly ever have another occasion to write about myself and my views.  1. I was born in Moscow, on 4th October, 1916, that is, as far back as in the tsarist Russia (at that time even the calendar was different, so the date of my birth was 21st September). My father, Lazar’ Efimovich Ginzburg, almost half of his life lived in the 19th century, he was born in 1863, that is only two years after the abolition of serfdom in Russia. Having graduated from Riga Polytechnic, he was an engineer engaged in purification of water and had a number of patents. My mother, Avgusta Veniaminovna Vil’dauer-Ginzburg, was a doctor, she was born, in 1886, in Mitava (Latvia). I was the only child in the family. Mother died of typhoid in 1920, I only remember one episode at her bedside and her funeral. Mother’s younger sister, Rosa, who started to live with us, did everything she could for me. She died in 1948. Father had died still earlier (in 1942), when we were living in Kazan’, where we had been evacuated. Below I will tell about this period in a more detailed way. And here I will only note that, as is well known, life in Russia during World War I, and especially during the period of military communism, and later in the Soviet Union was hard, in many cases even very hard. But in Moscow, where we lived all the time, with the exception of about two years (1941-1943) spent in Kazan’, material conditions were better than in the majority of other regions of the country. However, one of my childhood memories is a wagon loaded with half-covered coffins with dead bodies and pulled by a horse past our house in the center of Moscow. Another memory is of buying fresh meat “for the kid”, which turned out to be the meat of a dog. Normally dogs had never been eaten in Central Russia. However, in general we did not starve, but we lived in a so-called communal apartment – into my father’s four-room apartment two more families had been placed after the revolution. Still, it is not these hardships, more or less general, but my loneliness that really sticks in my mind. It was exacerbated by my being sent to school only at the age of 11 (in 1927 I entered the 4th form). My parents (or, to be more exact, father and aunt) must have been afraid that school at the Soviet times had become quite bad, and sending children to school was not obligatory. I am not acquainted with the general state of education in the USSR in the twenties. But school No. 57 belonging to the Sokol’niki district department of education, which I entered, does not seem bad to me. It was a former French grammar school, with many good old teachers still working there. True, as far as I remember, history was practically not taught, being substituted with rendering the contents of some reports of comrade Stalin and, it seems, some other material like that. What was really bad came later. In 1931, just at the time when I finished the 7th form, yet another school reform happened, and the full high school (I do not remember whether it had 9 or 10 forms) was abolished. After the 7th form pupils were to enter vocational schools (FZU), which were supposed to train skilled factory workers. After a FZU one could, in principle, enter directly higher educational establishments (VUZes), including universities – through “rabfaks”, that is worker preparatory departments. I am putting it somewhat vaguely, because, not at all willing to enter a FZU, I did not follow this way. So for a while I, then a boy of 15, remained rather lost and unhappy. My aunt, already mentioned, was working in an organization dealing with purchases of foreign scientific literature. One of their customers was Evgeni Bakhmet’ev, a professor of a technical VUZ. He was a very picturesque figure – a former submarine sailor, a Bolshevik, who became a specialist in the field of X-ray structure analysis. His fate, like the fates of many other people of such kind, was tragic – later he died, crushed by the millstones of Stalin’s terror. But in 1931 he helped me to get a job of a laboratory assistant in the X-ray laboratory of the technical VUZ where he was teaching. I will not go into details. I will confine myself to saying that the key figure in the laboratory was Veniamin (Venya) Tsukerman, quite a young man (only three years older than me) with truly uncommon inventiveness and initiative. With him and his school friend and peer, Lev Al’tshuler, we communicated and worked together for about two years. Both Tsukerman and Al’tshuler became physicists, from 1946 they were very close to their superior, Y.B. Khariton, who headed the main center where the atomic and hydrogen bombs were created (now it is the town of Sarov, as this place was called as far back as in the tsarist time; after a nuclear center was created there it was called by some code name, which I have already forgotten, and later its name was Arzamas-16). Tsukerman and Al’tshuler have a lot of awards, and, the main thing, many works of theirs are already declassified and well-known. Both of them are people with interesting fates, in part already described in literature. Giving a more detailed description here is evidently impossible (I will only say that Tsukerman died in 1993, and on 9th November, 2003 we celebrated Al’tshuler’s 90th birth anniversary).  What I acquired at the laboratory is not so much some concrete knowledge as the taste for work, for physics, for inventiveness. I also remember reading, with interest, the book by O.D. Khvol’son “Fizika Nashikh Dnei” (The Physics of Our Days) – a popular writing about the achievements of physics, a rare book for that time. Anyway, I decided to become a physicist, especially as I did not have any talents, but in physics I was at least interested. So I decided to enter the physical department of the Moscow State University (MGU). Just at that time, from 1933, it became possible to enter the MGU by open competition (previously for several years the entrance procedure was somehow different – for instance, through rabfaks or in some other way, but certainly not by competition). But for entering the University it was necessary to have finished the complete high school course, while I had finished only 7 forms. So I had to learn in three months (partly with a tutor) what had to be learned in the senior forms. I am going into these details because all my life I have wished I had got a normal school education. And I want to warn those who think that school wisdom is not worth spending many years on it. Indeed, though having, as I am convinced, merely average abilities, I managed to do in three months the program of three school years. But at what cost? The cost is, first of all, a total lack of automatism in elementary mathematics and in the orthography even of the Russian language. Specifically, at school I would have solved, for instance, 100 problems in algebra, trigonometry, etc., while on my own I solved only about 10. Why solve more if it is not demanded? Such things lead to lack of skills in the further learning of mathematics. The same with orthography. Illiteracy “in the people” was at that time being combated, though, and, as far as I know, there were some good results in this field. But at the higher level things were much worse. Suffice to say that already at the second year at the university we had a check (a dictation) in Russian, and half of the students of the year (!), me including, got unsatisfactory marks. After that an obligatory course of Russian was introduced for those who had failed this exam. I doubt that it gave any noticeable benefit – making up for what has been missed in childhood is very difficult. Neither do I know foreign languages, although later I had to master English, but only in the limits necessary for my doing physics. I hope that the given information is not superfluous, both in terms of understanding the life in the USSR in the years described and in terms of my biography.  I did not fail any of the entrance exams at the MGU, in 1933, but on the whole I did not make out brilliantly. As a result, I was not admitted, although admitted were some people with slightly lower marks but more advantageous personal particulars (I was neither a member of the Young Communist League nor a worker, and my parents also were not proletarians, and so on). But still this result was not discrimination or a sign of anti-Semitism, which flourished after the war. Some of my comrades who were not admitted either decided to wait for a year, but I had already left work and had somehow become keen on studying. That is why I entered the external department, which proved possible. And here again I painfully felt my sore spot. In 1934 I managed to get transferred to the internal department, to the second year, that is, I caught up with my fellow students and started to study like everybody. But I learned how much richer and brighter their life was, with all sorts of optional courses and so on. Not to mention that, strangely, I never got acquainted with the courses of astronomy and chemistry, which I did not have to do at the external department and somehow was not obliged to sit when being transferred to the internal department.  I want to finish the story of the first stage of my life by mentioning just one of many episodes which show that man’s fate is merely a chain of chances and it is like a pathetic little boat in the sea waves, which may capsize at any moment. What I mean here is the following. At the second year at the MGU all students were enrolled in groups of two types, military and civil. Only men were placed in military groups, where they were trained and qualified as military officers. So, when being transferred to the internal department I was sent to a military hospital in order to decide where I should be placed. I was rather not athletic (height 180 cm, weight about 60 kilograms). A military doctor, who seemed an elderly man to me, poked his finger at my neck, uttered the diagnosis “struma” and sent me to a civil group. Struma is some swelling of the thyroid gland, which I have not noticed up to now. I am telling about that because practically all my fellow students who got into military groups were killed at the war. To finish with this unpleasant topic, I shall note that there were three more similar episodes. I cannot say that I was bursting to go to the front but I was not in the least trying to avoid it. Telling about all incidents would be too long, so I shall limit myself to only one. In 1941, after the beginning of the war (for the USSR it began on 22nd June, 1941) the Lebedev Physical Institute of the USSR Academy of Sciences (FIAN), where I was working at the time (and were I have been working up till now), was evacuated to Kazan’ together with the greater part of the Academy of Sciences. And, what was quite natural for the war time, at least in our country, the workers of the Institute were used for different works. So once we were sent to unload barges with logs or firewood on the Volga. By the way, everybody took part in that, for instance, [I.E. Tamm](https://www.nobelprize.org/nobel_prizes/physics/laureates/1958/index.html), also mentioned in my Nobel Lecture. My task was bringing logs from the barge to the bank. I was wearing the so-called “koza” (which means “a goat”) – something like a rucksack with a step at the rear. Such “kozas” were widely used in Russia at one time for carrying heavy weights, and they are effective indeed. Two men put a log on a “koza” and the one who was wearing it carried the log to the bank. I am still surprised what heavy logs can be carried in such a way. But still I must have overstrained myself and on the next day I had bleeding at the throat, though not profuse. Some small blood vessel must have burst. I was sent to hospital, where some petrified spots were found in my lungs. The bleeding stopped, and, like the struma, it did not show itself anymore. At the same time voluntary enlistment into some airborne landing forces was announced and I, then a YCL member, enlisted. But I was not admitted, although I had not mentioned any illnesses and, if I remember correctly, even did not know that I was on the medical books. By the way, I can say that I was not officially exempted from military service – at least, in the first years of the war. But there was some order not to mobilize untrained graduates of some professions or some specialists in general. I was never conscripted, but, expecting to be conscripted all the time, I was trying to write my thesis as soon as possible. In peace time I would not have hurried to do it, but then I wanted, though it looks a bit silly, to be able to finish before the draft what had been started. So I defended this thesis in the spring of 1942 (it was the D.Sc. thesis, as the Ph.D. one I had defended in 1940).  The life in Kazan’ was hard, the four of us (my father, my aunt, my wife and I, as my daughter with her grandmother had been evacuated to another place) lived in one room. We felt rather cold and rather hungry. But I worked a lot, as did all my fellow workers. I was exploring the propagation of radio waves in the atmosphere and something else which seemed useful for the defense. I also tackled some other problems, as the scientific life was going on.  2. However, let me come back to the student days. Starting from the second year, I did well in my study and, so to say, enjoyed studying. I liked theoretical physics, but I thought, and still think, that my ability for mathematics is average, at most; besides, I had difficulties solving problems and making calculations (see above) – while it was considered, and rather justly, that a theoretical physicist should be good at mathematics. In short, because of all that, when at the end of the third year or at the beginning of the fourth year (now I do not remember when it was) I had to choose my specialization, I did not dare to take up theoretical physics and chose optics. The choice was not accidental. At the MGU physical department at that time there were rather many really good specialists, but there were some retrogrades as well. They were struggling with each other, and this struggle had a political coloring. Just a beginner at that time, I was far from their controversy. I remember only one public debate between the partisans of the contemporary physics and the so-called “mechanicists”, who criticized the theory of relativity and so on. It took place in 1935 or maybe the first half of 1936. I do not remember the date and I did not try to find it out, having a sad possibility to say something in this respect, as I know the fate of B. Gessen – a participant of this dispute, a physicist and a philosopher, who was the dean of the physical department at one time. He was also an old Bolshevik (so they called those who joined the party whether before October 1917 or shortly after) and shared the lot of the majority of his comrades – arrested on 21st August, 1936 and sentenced to be shot on 20th December, 1936, at the private meeting of the Military Board of the Supreme Court. The sentence was carried out on the same day. Of course, he was posthumously rehabilitated in view of a complete “absence of *corpus delicti*” – let it be a consolation for the representatives of “progressive, leftist intelligentsia” defending today, because of their much talked about “political correctness”, dictators, terrorists and hooligans.  Returning to the subject, I can say that, despite a complete absence of any personal connections or acquaintances, I understood from the very beginning, putting it in the words from a popular Russian children’s poem, “what is good and what is bad”. In short, I decided to specialize in optics because the chair of optics was headed by G.S. Landsberg, one of the people grouping around the remarkable person and physicist, Leonid Mandel’shtam. The associate professor of this chair, Saul Levi, was, luckily, appointed my tutor. I remember him with a warm feeling. I write about him and my relationship with him in the article “Notes of an Amateur Astrophysicist”[2](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not2). There is also some material repeated above and further on. Maybe it would be more convenient for the readers if everything was repeated here, but I have decided not to do it, for this autobiography is becoming very long as it is. But still I shall tell how I left optics and experiment in general and became a theoretical physicist. When I was waiting, in September 1938, to be conscripted (as a matter of fact, I had already been conscripted and was waiting to be summoned to come with my things to the district military committee), which I again avoided, one can say, as luck would have it (it was the last time when the MGU post-graduates were granted a conscription deferment), I did not want to sit in the room without windows and with the walls painted black, where I was trying to measure the spectrum of radiation of canal rays. So I made an attempt to explain the effect we were looking for, notably some asymmetry in the radiation of the mentioned canal rays. Notably, I thought that the electro-magnetic field of moving charges could cause induced radiation. Such an assumption was erroneous, as the field of a charge is not equivalent to free (light) field. But I did not understand it at once and turned with the corresponding question to I. Tamm, who at that time was heading the chair of theoretical physics at the MGU and reading lectures to us. Fortunately, I. Tamm also did not notice at once that my idea was erroneous, he listened to me in a very friendly manner, advised to look something up and in general supported me. The latter was so important for me, suffering from the inferiority complex and not at all sure of my ability to obtain any theoretical results. I write about it in more detail in the article “A Scientific Autobiography – an Attempt” published in book I. In short, it turned out that I managed, without any complicated mathematics, to sort out some problems of quantum electrodynamics. Specifically, such an undoubtedly highly knowledgeable theoretical physicist, and mathematically minded at that, as V. Fock, when doing quantum calculations had come to the conclusion that an electron uniformly moving radiates electromagnetic waves. This conclusion surprises, as we in the classical theory are used to thinking that a uniformly moving charge (electron) does not radiate. But the thing was merely in formulating the problem in a different way: in the classical case it is usually a stationary problem, while the quantum calculation had been done with the inclusion of quantum interaction at a certain instant of time *t* = 0. But the latter is equivalent to the fact that when *t* t = 0 it acquires a velocity *v* and starts to interact with the field. Physically this is equivalent to the assumption that the electron at the moment *t* = 0 is instantly accelerated to a velocity *v*. It is clear that this process is accompanied by radiation. In general, using the so-called Hamiltonian method I managed to elucidate some questions and solve a number of electrodynamic problems both in vacuum and in the movement of charges in a medium, for example, the problem of the Vavilov-[Cherenkov](https://www.nobelprize.org/nobel_prizes/physics/laureates/1958/index.html) radiation in the passage of charges through crystals. Here I did not have to use any complicated mathematics. Of course, I found what had long been known – however close are physics and mathematics their connection can be most different, in particular, it is possible to make progress in theoretical physics using but a very modest mathematical apparatus, for instance, not exceeding the limits of what is taught at physical departments of universities. By contrast, in some cases theoretical physicists both use most complicated modern mathematics and develop it.  So I realized that I could work, achieve results, think up new possibilities. Such awareness brings a great joy, it is happiness. And I worked a great deal, I wrote a thesis (which is a candidate thesis with us and a Ph.D. according to the western standards) practically in a year and defended it in 1940. Thus I finished the post-graduate course at the MGU physical department in two such kind. After that they wanted to leave me, in some capacity, at the MGU Institute of Physics, but the atmosphere there was bad and, most fortunately, on 1st September, 1940 I was taken to the FIAN as a person working for the degree of doctor (such a person has to already have the first scientific degree, Ph.D., and is to prepare, officially in three years, the second thesis for which the D.Sc. is given). I had been doing my postgraduate studies under the guidance of G. Landsberg, as I had been supposed to take up optics. But he quite nobly had not hindered me from doing something quite different. And I. Tamm, who was considered to be my tutor in my doctor work, also did not hamper my doing what I wanted. In general such is the style characteristic of theoretical physicists of the USSR and Russia, at any rate, in many places and undoubtedly at the FIAN.  Thus I have been doing theoretical physics since 1938, and I have done many dozens of research works on different physical and astrophysical subjects. As for articles, I have already written hundreds of them, because for me writing is fairly easy and writing articles is an integral part of my work. Some colleagues condemned and may still be condemning me for writing such a great number of articles. And indeed, there are people who write articles in order to increase the number of references to their works. But I, believe me, have never been guided by such considerations, besides, I have got very many references as it is. For instance, I have recently seen in a certain reference book that after 1985 there have been 8,962 references to my articles, in spite of the fact that the article which we wrote together with [Landau](https://www.nobelprize.org/nobel_prizes/physics/laureates/1962/index.html) and which was published, in 1950, only in Russian, is almost always mentioned without placing the reference in the list of literature; the same source informs that after 1945 there have been 19,519 references to my articles. I began to publish them in 1939. By the way, Landau judged, though not very seriously, the age of physicists by the time when their first publication appeared. For instance, he said that I was 13 years younger than him, because his first article appeared in 1926; whereas he was born in 1908, therefore by calendar years I am only 8 years younger. I think that it is not worth attaching too much importance to the number of references, sometimes it is quite deceptive, especially if people refer to some sensational assertion which may prove to be wrong. The number of publications may be slightly more reliable in this respect, but it characterizes the style of working rather than its quality – because it is clear that one publication containing an important result is more substantial than a great number of articles with less important results. Here I will not write about the content of my works, as I can refer the readers to the article “A Scientific Autobiography – an Attempt” published in book I. Let others judge the quality of these works, although below I shall make one remark in this respect.  3. Now I must come back to my biography. In 1937 I married my fellow student, Olga Zamsha. In 1946 we divorced. We have one daughter, Irina Dorman, born in 1939; she graduated from the Physics Department of the Moscow State University, defended her Ph.D. thesis, and was occupied with the history of physics. Her husband Lev Dorman is a D.Sc., a specialist in the area of cosmic rays. They have two daughters, Maria Dorman and Viktoria Dorman. The former is not married and lives in Israel. Viktoria has got a Ph.D. in physics in Princeton, she is married to M. Petrov, also a physicist, they have twins, Grigory and Elizaveta, who are now already three years old. So they are my great-grand-son and great-grand-daughter. They live in Princeton, USA. In 1946 I married the second time, to Nina Ermakova, who became Nina Ginzburg. Thus we have been together already for 57 years. Unfortunately, we have no children. As this is not quite an ordinary marriage, at least for the Soviet Union, I have written rather much about it in a number of articles published in book II. The thing is that Nina’s father, a prominent engineer, was arrested as far back as before the war and died of starvation, in 1942, in a prison of the city of Saratov. It seems that he died in the same cell and almost at the same time as the renowned biologist N.I. Vavilov. And Nina was arrested in 1944 as a member of a group of young people allegedly going to kill comrade Stalin himself. Of course, it was merely an invention of the KGB, but I am writing about it here, especially as a warning to contemporary “revolutionaries” and terrorists, so they could foresee their fate in case of their victory. Under a totalitarian regime the punitive organs deal out justice arbitrarily, and innocent people suffer. Quite often a regular theatre of the absurd adds to that. In Nina’s case, she suffered, according to the KGB’s scenario, mainly because she lived on Arbat, the street where the great leader sometimes drove by. It was from the window of her flat that he was to be shot at. But the valorous fighters against counter-revolutionaries had not taken the trouble to learn that after the head of the family had been arrested only one room in their apartment had been left to Nina and her mother, with the windows overlooking the yard. The clearing up of this circumstance, which happened already after the arrest and ruled out the charge with terrorism, and also some other circumstances resulted in the sentence surprisingly lenient for that time – “only” three years in camps. And if her room had overlooked Arbat, we would probably have never met. In 1945, Nina was released by an amnesty, but without the right to live in Moscow and in a number of other cities. Her aunt lived in the city of Nizhni Novgorod (then Gorki), so she went there, in fact, into exile, especially as in Gorki itself she had no right to live as well and was officially registered in the village of Bor, on the opposite bank of the Volga[3](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not3).  In 1945 I was invited by a group of physicists headed by Alexander Andronov and working at Gorki University to become a visiting professor at the newly organized radiophysical department of this university. I was 29 and full of energy, but in Moscow I did not have any place to teach (I. Tamm and many others had been actually banished from the MGU by that time). During the war I had worked, among other things, on the propagation of radio waves in the ionosphere. So the invitation to head the chair of propagation and radiation of radio waves was quite natural, and I accepted it. I first came to Gorki at the end of 1945, it was then that I met Nina, and in the summer of 1946 we got married. Of course, I was permanently putting in applications to the KGB (to be more exact, it could be done once a year) with the request to permit my wife to return to Moscow and live where she was born and where her mother lived. But all the time I was refused, despite the fact that my applications were also signed by the directors of our institute, at first by S.I. Vavilov and then by D.V. Skobel’tsyn. I have written about it in a rather detailed way in “A Story of Two Directors” published in book I. My wife managed to move to Moscow only in1953, after Stalin’s death and the amnesty that followed. It goes without saying that later (in 1956) she was rehabilitated, like all the members of the imaginary counter-revolutionary group – “owing to the absence of *corpus delicti*“. Unfortunately, not all of them lived to see this moment, not to mention the long years of imprisonment for some of those who did.  In 1942 I joined the communist party (CPSU). It was just when the Germans reached the Volga, and our view of the future was far from optimistic. Thus I can hardly be suspected of any careerist considerations, not to mention the fact that I certainly hated the Nazis and all the faults of the communist rule were then receding. At the same time, I cannot help saying, with great regret and bitterness, that for many years I was virtually blind in my estimation of communism and Bolshevism. In general, I believed in “a radiant communist future”, not understanding that here we had, in fact, a regime of a Nazi type, headed by a criminal no less mean and bloodthirsty than Hitler. Having written the previous phrase I remembered the observation of [Churchill](https://www.nobelprize.org/nobel_prizes/literature/laureates/1953/index.html) that Stalin and Hitler differed only in the shape of their mustache. Anyway, I was sharing the lot of millions of people who did not understand the inevitable fate of the totalitarian regime sliding to lawlessness and terror. I have written a lot on this subject (see a number of articles in book II), and it is not possible for me to dwell on it here. I will come back to my own fate.  The years from 1946 to 1953 were far from easy for me. Living mainly in Moscow I went to Gorky when I could, to read lectures and train a number of postgraduates and workers in physics with an astrophysical and astronomical bias. All that would not have been too bad, but the clouds began to gather over my head. State anti-Semitism and different kinds of persecution of the so-called cosmopolitans, who supposedly worshipped the West, were blossoming out in the country. An offensive against modern science was in progress – first of all, in biology, but accusations of idealism and of other sins were raining down on us in physics as well. I proved to be a good target for all sorts of attacks. Indeed: a member of the party, married to a former prisoner, who had been accused of counter-revolutionary activities and therefore deprived of many rights. And a Jew at that. So finally they started to accuse me of idealism, cosmopolitism and so on, and so forth. Appearing as some culmination of all that was an article published in a newspaper widely distributed at that time, the *Literaturnaya Gazeta* (Literary Newspaper), on 4th October 1947, that is, on my birthday, under the heading “Against Servility”. In the article inspired by the rather well-known physicist D.D. Ivanenko[4](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not4), I was accused of all sins together with the biologist A.R. Zhebrak, an opponent of academician Lysenko. On the same day (what a coincidence!), on the initiative of the same Ivanenko, the High Assessment Commission of the ministry of higher education did not confirm promoting me to the rank of professor, for which I had been nominated by Gorki University. After that my name began to be mentioned wherever possible as a negative example. Even our institute evidently had to remove me from its academic council for “strengthening” it. I can only guess what fate awaited me in this situation at that time. I think that it would have cost me dear, but I was saved by the hydrogen bomb.  4. The history of creating atomic and hydrogen bombs and my role in it have been extensively described. So I have the more reasons to be brief. The Soviet atomic bomb was first blown up on 29th August, 1949, and the hydrogen bomb, on 12th August, 1953. The hydrogen bomb had been arousing interest even before the tests of the atomic bomb, but then it was completely unclear how to make it, nor was it topical, as far as I understand. It was merely something not to be missed. So in 1948 I. Tamm was attached to this work, though the authorities did not have any special trust in him (once he belonged to the Men’sheviks, who were opponents of the Bolsheviks, and his only brother had been arrested and shot). Only recently have I learned from some jubilee article (the jubilee took place on 12th August, 2003) that at first I had evidently not been admitted to the work in Tamm’s group, it only happened some time later. It is a miracle that it happened at all – that someone having the wife in exile was recruited for a top secret job (“top secret, special file”). Of course, I do not know the reasons. I think that both the task was not considered very important (see above) and I had a high “rating”. But recruiting my closest friend and a superb physicist, E.L. Feinberg, turned out to be impossible for Tamm, as Feinberg’s wife had once lived in the USA. By the way, I cannot help telling why the services of [A. Sakharov](https://www.nobelprize.org/nobel_prizes/peace/laureates/1975/index.html) were enlisted, though earlier I already wrote about it somewhere, and Sakhrov himself mentions it in his “Memories”[5](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not5). From 1945 Sakharov was a postgraduate in the FIAN theoretical department headed by I. Tamm, in 1947 or 1948 he defended his Ph.D. thesis and was going to carry on with peaceful science at the FIAN. He had a small daughter and did not have an apartment of his own, renting a room somewhere, and in general his life was rather hard. So the director of our institute, S.I. Vavilov, asked Tamm to include Sakharov in his special group in order to get a room for him, which they managed to do (Sakharov got a small room with the area of 14 square meters in a communal apartment). If he had had a room of his own before that, many fates, including both his fate and mine, would have probably turned out quite differently. But it happened as it happened. From the middle of 1948 we started working, at first reading the materials which we had and which, as far as I remember, did not raise any hopes that this problem would be solved. Then Sakharov put forward “the first idea” and I, “the second idea”, which together made it possible to build the first Soviet hydrogen bomb. It is ridiculous, but until the very death of Sakharov in 1989, that is 40 years later, all this material had been classified. That is why Sakharov in his memories writes about the 1st and the 2nd ideas without revealing their content. Now it has long been known that the 1st idea is “the layer structure” (I will not explain it here), and my 2nd idea is using as fuel 6Li (or, if you wish, 6LiD) for obtaining tritium 3H = t as a result of the reaction 6Li + n –> t + 4He + 4.6 MeV. These two “ideas” were recognized as opening the possibility of creating the hydrogen bomb. With the aim of carrying out this task Tamm and Sakharov were sent, in 1950, to “the site” (Arzamas-16), and I was not admitted there, obviously for the reasons mentioned above (it was a higher level of secrecy), and I remained in Moscow as the head of a small “group of support”, but with a sentinel sitting at out door. It goes without saying that I regard this turn of the fate as a greatest luck. I was sure of my safety, so I could visit my wife in Gorki, and could do science. It was not that I was neglecting my work, but our task was mainly performing different calculations, which is not my strong suit. Taking an interest in other people’s secrets was not customary with us in general, and I was especially indifferent to them. So I remember my surprise, probably, already in the seventies or the eighties, when by chance I asked Sakharov how “the layer structure” had “worked” or developed. He said that it had “not worked”, which I did not understand. Now it is known that the first two Soviet hydrogen bombs were made as “layer structures.” But such a construction proved to be able to increase the power of the bombs, I do not remember exactly, whether 20 or some other, comparatively small number of times more than that of the bomb dropped on Hiroshima. Somebody thought for some reason that it was not enough, and the 3rd idea (by Sakharov’s terminology) consisting in radiative compression was used. While “my” 2nd idea is probably still working. Or rather, not working, to our great luck, but merely figuring.  Trying to be worth my classified salt, as it has already been said, I was glad when Tamm or Sakharov (I do not remember exactly who of them), having once come to Moscow in 1950, told me of the problem of controlled thermonuclear synthesis and the “Tokamak” system offered by them. I took up this problem, managed to write several reports and, by the way, in 1962, when all that was declassified, even published some material from these reports. But I did it as a sort of some rather silly “revenge” for having been dismissed from this work in 1951, which must have been considered too secret for me (for more detail see book II, article 18).  Then especially terrible time came. Stalin went totally insane, repressions were going on, culminating in “the case of doctors” and a bestial anti-Semitism related to it. It seems that the corresponding documents have not been found yet. They may not have existed, as it was clear even to bandits that it was better not to leave trace. According to the rumors, “the doctor killers” were whether to be hanged on the Red Square or exterminated in some other way, and all Jews were to be exiled into some camps already built. Some “necessary” people, possibly I among them, would have been left in “sharashkas” later described by Solzhenitsyn and others (a “sharashka” is in fact a prison were scientific and technical research was conducted). It was a tremendous luck that the Great Leader did not have enough time to carry out what he had planned to do and died, or was killed, on 5th March, 1953. In the former USSR many people (at any rate, my wife and I) have up till now been celebrating this day as a great festival.  Everything in the country started to change very quickly, suffice to mention the rehabilitation of “the doctor killers” and the shooting up of Beriya, who was the head of the Soviet “atomic project” (by the way, he was a good organizer and probably not more of a bandit than all the rest). At the USSR Academy of Sciences, contrary to its Charter, there had been no election since 1946, evidently, because it had not been allowed by Stalin. Already in 1953 such an election took place, and I was elected corresponding member of the Academy. As they say, “from rags to riches” – although, in my case this saying looks somewhat exaggerated. But still, both in the USSR and now in Russia being a member of the Russian Academy of Sciences means a rather privileged position. In addition, I got some rather high governmental awards[6](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not6) and in general turned into a VIP, although second rate. No less important was that my wife managed to return to Moscow, after 8 years of half-exile, not to mention approximately a year and a half of prison and camp.  Since that time life has been going on more or less normally, but still it made you think of a popular joke: “Question: What is permanent under the Soviet power? Answer: temporary difficulties are.” The Sakharov story became a temporary difficulty for me. In 1969, dismissed from classified work, he returned to our theoretical department of the FIAN. As for me, in 1971, after Tamm’s death, I had to become head of this department. “Had to become” indeed, because I did not want it and in general I do not at all like work like that. But the workers of the department had asked me, and it was really necessary, because due to numerous “temporary difficulties” it was important that the department should be headed by a person of some rank. And in our department only Sakharov and I were academicians (I had been elected full member of the Academy in 1966). But Sakharov had already become a dissident, had taken up politics, and was totally unsuitable for the post. So I had to take over. And it was the same Sakharov who first of all became a problem, one can say, especially after his being exiled to Gorki in 1980. I have written about it in the article “The Sakharov Phenomenon”, which can be found both in book I and in the book of memories about Sakharov. I will not repeat myself. I will only say that I do not see anything for which I could reproach myself in this matter.  However, I must note that “the case” of Sakharov aggravated my already not temporary but permanent problem which I faced when under the Soviet regime I either would not be allowed to go abroad at all or would be allowed with great difficulty. The explanations always referred to the reasons of our famous “secrecy”, but it was an obvious pretext. For instance, Tamm, who really did know some secrets (while I actually knew none), went abroad many times after 1953. Of course, I was very glad for him. But I was refused permission to go because of the “sins” of my wife and my own sins. My greatest “achievement” in this area was made in 1959. That year a big international physical conference took place in the USSR, in Kiev, it seems that it was called the Rochester conference. So, some people, me including, were not allowed to go to this conference (!) under the pretext of secrecy. Landau was also not allowed at first, but he announced that he would go all the same and would make a scandal. Well done! They retreated and he went there. And I cannot make scandals and, even if I could, I would perhaps not have obtained anything by it. So I did not go to this conference, did not see many good physicists, did not learn many interesting things. I was very bitter about it at that time, and I am bitter now when I remember this insult. And the greatest pity for me is that I could not meet [Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/index.html) and speak with him – for he died in 1955 when I was already 39, understood something in physics and astronomy already and could well have had a professional talk with him if I had lived in a free world. But I was not fated to. I only remember asking L. Infeld, who came to the USSR, to give my regards to Einstein. I do not know if Infeld had an opportunity to do it.  Going on with the subject, I shall describe two latest experiences characterizing the conditions in which we lived before the collapse of the USSR. In 1984 the Danish Academy of Sciences, whose foreign member I have been since 1977, invited me to come to Copenhagen for a week, I do not remember on what occasion. I started the painful procedure of “registration”: for going abroad one had to fill out many papers and pass some commissions. Then all the documents went to the “instance”, and sometimes you were not informed until the very last moment whether or not your trip had been permitted. This time I was also told shortly before the departure that I could go but without my wife (!). I refused to go. Such a natural reaction was a rare thing at that time, so I even had a telephone call from the president of the USSR Academy of Sciences, who expressed his disapproval by saying that he, for example, did go without his wife – so how can I be so shrewish instead of being grateful for the great honor done to me and the trust given (to be honest, all the last words were not said but, in my opinion, this call could be interpreted only in such a way). A year later, in 1985, again in Copenhagen, [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html)‘s birth centenary was to be celebrated. And again I was invited, this time, if my memory does not fail me, being the only person from the USSR who was to make a report at a plenary meeting of the corresponding conference. And again I started to go through the “registration” with my wife. And again at the last moment I was told that I was allowed to go but without my wife. Probably, someone was afraid that if we went together, we might not return. By the way, we had never thought of anything of the kind. However, this time I went, though I was indignant. The thing was that I had already prepared my report, which meant quite a lot of work done, taking into account that I had to prepare it both in Russian and in English and then to pass it through censorship[.7](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not7) And, what was even more important, I certainly had a great respect for Bohr (I had heard his reports and was acquainted with him, because he had been to the USSR) and I wanted to take part in the jubilee conference. In my report at the conference and especially in private talks, I somehow expressed my indignation at our lack of freedom. I do not remember the details, but I do remember the bitter after-taste left by the indifference of western colleagues. But I was probably wrong. In the West they knew about the Soviet arbitrariness and were used to it, they protested and defended some people. And in general, why should they have taken my case to heart. Besides, I spoke in a reserved manner, without any attempts to arrange a rally, so it was easier to turn a deaf ear.  By a chance coincidence, this year, 1985, was a turning point, as M.S. Gorbachev came to power, and soon the “perestroika” began. I took part in it – although not very active, but I did. I was a deputy of the USSR Supreme Soviet (from 1989 to 1991, the year when it was dissolved), elected from the Academy of Sciences. I resigned from the party in 1991, since then I have never again been a member of any party and am not going to be one. Of course, I have always been a partisan of the democratic forces but I have not always identified myself with concrete actions of some of their representatives, Sakharov in particular. However, I do not remember anything important and interesting from my “political” activities, which would be worth describing here.  In 1988 I finally managed to free myself from heading the I.E. Tamm Department of Theoretical Physics, as it began to be called. The thing is that according to the rule which had just been introduced and which had long been necessary, people over 70 were not allowed to hold some positions. Having remained at the FIAN in the position of advisor of the Academy of Sciences, I have a small group of subordinates. Besides, I have been heading, since 1968, the chair of problems of physics and astrophysics, created that year at the Moscow Physical and Technical Institute (rather well-known as Phystech), but now I already do not read lectures and remain in this position without being payed (according to my own wish), at the request of the staff in order to be able to help the chair in solving some problems.  Since 1998 I have been chief editor of the Usp. Fiz. Nauk (Physics – Uspekhi) journal, and I actually carry out my responsibility in the journal as far as I can. From the middle of the fifties I was in charge of a physical seminar at the FIAN, on Wednesdays, and this seminar was rather popular. Running for two hours, it was attended by many people from Moscow and from other places as well. At the time of Khrushchev we had a popular saying: one should know the measure, be it maize or seeing Nehru with pleasure. The saying came into being in connection with Khrushchev’s keenness on cultivating maize even in the areas totally unsuitable for this purpose, as well as with his love for Javaharlal Nehru, which the latter may have deserved, though. I am writing about that by association. In 2001 I started to feel worse and decided that I “should know the measure”. So on 21st November 2001 the 1700th seminar was arranged in the form of an amusing party, like some other “jubilee” seminars (for instance, the 1500th and 1600th), and there, quite unexpectedly for everybody, I announced that the seminar was closing down. I did it after telling a story about an actress who had been “playing” until she could no longer walk. I still could walk, but the seminar had to be closed some day and I wanted to do it in time.  In the post-Soviet Russia and even a bit earlier (after 1986 or 1987) going abroad was no longer a problem, or at least no longer a problem of the Soviet type, that is with a lot of obstacles. I took advantage of the corresponding opportunities, but due to “the law of conservation” going abroad has become difficult for other reasons – because of the age. In 2001 I was unexpectedly given the Humboldt Prize (or grant) for going to Germany for half a year. I was already going to accept this invitation but at the last moment refused it because of the state of my health. But I used the visa already received for going to Spain to the 10th International conference on ferroelectricity, the first conference of such kind which I managed to attend (about it see above and book II, article 5). I thought that I would not go abroad any more, but here I am going to Stockholm to the Nobel award ceremony.  6. A great deal has already been written above, but nevertheless, I do not think that some more questions could be left without answers. Indeed, if we want our notion of a person to be full enough, we should learn many things about him. Specifically, we would like to know, first, what his outlook is, including his attitude towards religion. Secondly, we would be interested in his political views. What follows, thirdly, is the description of his professional activities. And finally, fourthly, very important are his personal qualities and tastes. All that should be described. But of course, it is easier wished than done. Besides, I think that there are many personal things about which a man sometimes cannot and should not write. Indeed, in newspapers and magazines we can often see the answers to such questions asked by correspondents: what qualities in people do you value above all else, what are your ideals, do you want to earn more money and for what purposes, and so forth. The answers, as far as I could notice, are rather monotonous: everybody values faithfulness, nobleness and decency; everybody has radiant ideals of fairness, and money is needed mainly for good deeds and not for buying mansions. It is quite understandable, as everybody knows what qualities and likings are considered positive and what tastes one should and can be ashamed of. So I will not go into details of my personal matters. I will only say a few words on the theme related to them. In my life, like in the lives of many other people, friends have played an important role. Now some of them are already no more, to my greatest regret. Two closest friends of mine are now having hardships connected with old age I would like at least to mention all my friends and to express my warm feelings to them. But after some attempts I have made sure that I am not able to do it in a satisfactory way. The same concerns my wife and relatives. Probably only a few people will be able to write about their feelings quite sincerely and without hitting any wrong notes.  One more category of people to be described in a complete enough autobiography are teachers and pupils. Treating the matter seriously, it is not at all simple to decide who you should regard as your teachers and pupils. Evidently each of us learned a lot from many people, in person or externally. Where I am concerned I want to solve this problem as I.E. Tamm and L.D. Landau solved it. The latter said, quite definitely and repeatedly, that it was only Niels Bohr whom he considered his teacher. As for I.E. Tamm, as far as I know, he regarded only L.I. Mandel’shtam as his teacher. For my part, acting in the same way, I believe that my teachers are I.E. Tamm and L.D. Landau. By the way, with the latter I did not have any official links – I did not take an exam in his “theoretical minimum”, he was not my tutor or superior, nor did his name appear as that. About Tamm and Landau I have written in a rather detailed way (see books I and II). As for the pupils, many people (especially mathematicians) call their pupils those who they supervised, for instance, in their postgraduate studies or for whom they were considered tutors helping to prepare a thesis. I cannot be satisfied with such an approach. I want to call my pupils, although maybe not always, those who consider me their teacher. Naturally, with such an approach I cannot present a list of my pupils. I will only say that I have never forgotten the role played for me by the support and benevolent attitude of I.E. Tamm. I tried to follow his example. Let other people judge to what extent I have succeeded in it. I will confine myself to the remark that, with only very few exceptions, I have good feelings about those with whom I happened to work together or whose tutor I used to be. Thanking them for cooperation I hope that they returned and return my feelings.  Where my professional activities are concerned, they are described in the Nobel lecture, in this autobiography and, the main thing, in a number of articles in books I and II; I would especially like to note the article “A Scientific Autobiography – an Attempt” in book I. It is interesting to consider the question of estimation of people’s scientific level and activities. It is a big theme and here I only want to take an opportunity and to tell about Landau’s opinion in this respect. Landau in general , especially in his youth, liked to classify everything. Later, when I got to know him, he himself looked at these classifications, for instance, of women, with irony. I shall only dwell on his classification of theoretical physicists who worked in the 20th century. It was done according to a logarithmic scale, with the base 10, so a 2nd class physicist was 10 times inferior to a 1st class physicist. It was a question of accomplishments, and not of the level of knowledge, of pedagogical ability or oratorical talent. Class 0.5 was given only to Einstein. Put into class 1 were Bohr, [Dirac](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), [Heisenberg](https://www.nobelprize.org/nobel_prizes/physics/laureates/1932/index.html), [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html), [de Broglie](https://www.nobelprize.org/nobel_prizes/physics/laureates/1929/index.html), [Feynman](https://www.nobelprize.org/nobel_prizes/physics/laureates/1965/index.html). I have undoubtedly forgotten several names, but, of course, I will not supplement this list in the way I see it. I remember [Pauli](https://www.nobelprize.org/nobel_prizes/physics/laureates/1945/index.html) being put to class 1.5. Landau himself was placed in his own classification, as far as I remember, at first to class 2.5 and then to class 2. Once E. Lifshits told me that Landau had upgraded himself to class 1.5. Why does this classification seem interesting to me? First, it refutes the legends about Landau’s immodesty and conceit. Secondly, it is important that he put an emphasis on the record of accomplishments, achievements. For instance, I remember arguing with Landau about de Broglie, who did not seem to me a particularly powerful figure. But Landau stood firm, and I think he was right. The wave proprieties of the electron and other particles is a guess of genius, although this idea occurred to de Broglie under the influence of Einstein’s notion of photons and the equations **p** = -h**k** and *k* = w/*c*. By the way, I am surprised that Landau put Feynman higher than himself and in general put him into the 1st class. There is no doubt that Feynman was a brilliant physicist and lecturer but it seems to me that his accomplishments cannot be compared with those of other “first-class” physicists. Probably, Landau especially valued the diagrammatic technique, thinking that he himself would not have been able to hit upon it. What looks interesting and important, from my point of view, is an enormous gap which may exist and often exists between accomplishments and knowledge, command of the apparatus and so on. I think, though I cannot assert it, of course, that “by knowledge and command of the apparatus” Landau stood even higher than Einstein, but “by accomplishments” he sober-mindedly estimated himself as much inferior to him. I am certainly not going to give my estimation of myself here, but it is undoubtedly much lower “by command of the apparatus” than “by accomplishments”.  It seems to me that working in science, in physics – at any rate, in a position of at least some authority – is absolutely impossible without having a stand on important issues of our existence and thinking about philosophical questions. Unfortunately, I have not had the chance to really acquaint myself with the philosophy and methodology of science. To a certain extent it can be explained by the fact that I have always worked at concrete tasks and not at, for instance, problems of interpretation of quantum mechanics and so on. That is why the atmosphere of dogmatism and censorship in philosophical issues, which prevailed in our country, has not had an especially pernicious effect on me, as well as on the majority of my colleagues in the USSR. I do not even want to name illiterate rogues who dictated to us under the sign of dialectical materialism how the laws of physics or, for instance, of genetics should be understood. But it does not in the least prove that dialectical materialism in itself is unacceptable (among its advocates there were quite decent and reasonable people, as for instance, the already mentioned B. Gessen). I have always understood the core of this philosophical trend as, first of all, materialism, that is the conviction that nature and matter exist beyond our consciousness and independently of it. Secondly, studying nature, which is what natural sciences do, requires a flexible approach based on facts and not on dogmas. It is such an approach which is dialectical. I do not mean anything else, although philosophers may consider my words naive, insufficient or maybe even worse.[8](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not8) A really important point is understanding that materialism is “an intuitive judgment” (using the term especially widely used by E.L. Feinberg) which can be neither proved nor disproved. Atheism and belief in the existence of God are also intuitive judgments. I am an atheist, that is, I think nothing exists except and beyond nature. Within the limits of my, undoubtedly insufficient knowledge of the history of philosophy, I do not see in fact any difference between atheism and the pantheism of Spinoza. That is why I think that Einstein was also an atheist, because in 1929, when asked what he believed in, he answered: “I believe in Spinoza’s God, who shows himself in the harmony of all that exists, but not in a God who takes care of the fate and actions of people.” Einstein, however, used the term “cosmic religion” and reckoning him among atheists may be not quite right. At any rate, I, like many others, do not have any “cosmic feeling”, and I do not see any place for God, that is, for something which is beyond nature and has created this nature (such is the opinion of deists). But evidently it is impossible to prove that God does not exist. This itself leads to the conclusion that the principle of the freedom of conscience is fair, that is, people should have the unimpeded right to believe in God and, if they so desire, to practice some religion (of course, I am not speaking about wildly fanatical sects and the beliefs justifying banditry and terrorism). The Bolshevik-communists were not merely atheists but, according to Lenin’s terminology, militant atheists. This term, being not quite clear for broad masses, was changed to the name “the militant godless”. These pursued believers, especially priests, destroyed temples (churches, mosques, synagogues) turning some of them into warehouses, stables and so on. Identifying atheists with “the militant godless”, which is done in Russia by unscrupulous people or simply by illiterate demagogues, is absolutely groundless. It is the same as identifying a respectable catholic with a partisan of the inquisition or considering all members of the Orthodox Church to be partisans of the brutal persecution of Old Believers or other “heretics”. Unfortunately, in the post-Soviet time in Russia a clerical offensive has been going on, while the voice of atheists is completely stifled. That is why since 1998 I have been defending atheism in the press, and after being awarded the Nobel Prize I managed to say about that on television as well. Here it is not a question of combating religion, as this would be at variance with the principle of the freedom of conscience, and, besides, religion sometimes does good, calling for good and for worthy behaviour. It is merely a question of atheistic enlightenment, for instance, of scientific elucidation of a complete falsehood of creationism. In general, an abstract belief in the existence of God (concretely, deism) should not be confused with theism (Christianity, Islam, Judaism) involving belief in miracles and the sanctity of the Bible, Koran and so on. By the way, no cultured person will deny the great artistic and historical value of the Bible. But a belief in Biblical miracles is another thing altogether, being incompatible with a scientific outlook, because a miracle, by definition, is something at variance with scientific data and results of scientific research (for some more details see a number of articles in book II).  7. It remains for me to write about politics, or rather about my opinion on certain questions in this respect. I have always remembered the line from a song by the popular poet and singer Alexander Galich (by the way, his real surname was Ginzburg but we are not relatives): “Be only afraid of the one who will say: I know what should be done”. Lenin, Hitler and Stalin knew “what should be done” and this knowledge cost the lives of millions of people. Even if I knew “what should be done”, I would not be able to influence the course of history. But the main thing is that I do not know this. I have some opinions, but there is a world of difference between being sure that you are right and acting correspondingly, in spite of everything – and having an opinion and understanding that it may prove to be wrong. So, my opinion is well-known enough: I am convinced that only a democratic form of rule is acceptable. Churchill was certainly right when he said, with the clarity of thought inherent in him, that (unfortunately, I do not remember his exact words) democracy is a very bad form of rule but we do not know any better. Indeed, the hereditary monarchy is presently something which remains merely by tradition. Even so, the monarchic regimes remaining in some countries of Europe do not hamper the democratic order, as far as I know. Where the totalitarian regime is concerned, Lenin, Stalin, Hitler, Mao Tse-tung and many leaders of a lower rank proved convincingly enough that this form of rule is inhuman and is inevitably leading to tragic circumstances. Millions of people, and I am not an exception, believed, putting it in a more or less modern language, in the possibility of “a communism with a human face”. To tell the truth, I still do not understand why it is impossible. But there is “a criterion of practice”, in exact sciences absolutely unshakeable and enabling us to have indisputable knowledge, for instance, that astrological forecasts are false and the law of conservation of energy is correct (though for the latter this is with a reservation about the whole known range of phenomena). In social life there is certainly no similar indisputability, but the whole experience of mankind shows that totalitarianism and dictatorship inevitably slide to the rails of arbitrary rule, crimes and atrocities. However, for democracy a high price is to be payed too, and arguments emerge about this price – how far can we go, where is the border? The whole history of the rule of the Bolsheviks and the Nazis is full of examples how democrats, progressive intellectuals, liberals, pacifists and all public like that often did not understand elementary things, and step by step they in actual fact encouraged bandits and consolidated their power. Much has been written on this theme, and I also could write a whole book. I will confine myself to just one example of “deep understanding” of Stalinism, which I have recently come across. Herbert Wells wrote, characterizing Stalin, that he had never met a person more sincere, decent and honest; that in Stalin there was nothing dark and sinister, and it was these qualities that should account for his tremendous power in Russia (!) According to Wells, Stalin was a Georgian totally devoid of cunning and insidiousness, and his sincere orthodoxy guaranteed the safety of his comrades-in-arms. And this was written in the thirties, already after the horrors of Stalin’s collectivization, known all over the world. Several years later Stalin’s comrades-in-arms who Wells had probably met were also shot. In all, the “Georgian devoid of cunning and insidiousness” signed by his own hand death sentences (shooting lists) for 44000 people![9](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not9)  There are really lots of examples of “understanding” reality in the way demonstrated above. But I have an impression that the “leftist progressive” public “has forgotten nothing and has learned nothing”. Under the slogans of “political correctness”, following democratic principles and so on, they follow the same road of protection and justification of hooliganism, banditry and terror. Even the tragic events of 11th September, 2001 were far from having a sobering effect on each and everyone. A spectacular example is the attitude to the events in Iraq. The Iraqi regime of Saddam Hussein used chemical weapons in the war with Iran, it occupied and plundered Kuwait, it committed innumerable atrocities inside the country. But they say that it has not been proved whether Saddam did or did not manage to build up a stock of chemical, bacteriological and maybe also of nuclear weapons. It has been proved, however, that he aspired for that. All vileness of this regime and its crimes have also been proved. However, the action of the USA and Britain taken in order to overthrow this regime is condemned by many people. In their opinion, they should have waited until Saddam became stronger and was able to drop something on their heads. I understand that the Anglo-American action was clumsy and this issue is complicated. But, by the way, I do not understand why the elimination of the regime of Talibs in Afghanistan had been considered more legitimate from formal point of view. It is just that 11th September was closer, and bin Laden and Afghanistan, farther. I have already said that I do not know “what should be done”. But I know, as it seems to me, that if civilized countries follow the principles of “political correctness” and observing laws literally, they will again incur innumerable calamities, like those from Hitler and Stalin. The wild fanatics killing absolutely innocent people, women and children, cannot be stopped and rendered harmless without soiling one’s hands.  As a Jew I cannot go without dwelling on the “Jewish question” here, although it is still not entirely clear to me why this quite small, suffering people turned out to be in the focus of the world politics. For understanding of what follows I must make several observations. The Jews have remained as a nation, they have not assimilated, as it is customary to think, due to their attachment to Judaism and to considering themselves a chosen nation. I am an atheist and internationalist, that is, I do not regard any people as “chosen”, in particular, I do not think that Jews are better than Arabs. I do not know any Jewish language (Hebrew or Yiddish); by the way, I wish I knew, but I do not have ability for languages and my native tongue is Russian. One would think that I must have assimilated. But it is absolutely wrong, I have never been able to even think of giving up my native people. What are the reasons? I do not know and understand them quite well myself. Of course, family roots are essential, there were some Jewish traditions in my family. No less essential is anti-Semitism. Although I have not suffered from it directly, I also used to be called “a Yid”. Not to mention the time when after the war anti-Semitism flourished with us. Anyway, because of all these and maybe some other reasons which I do not understand (what if it is merely a question of genes, their role is not quite clear yet), I am a bearer, so to say, of the Jewish national feeling. This is by no means nationalism, for I see nationalism as the opinion of the superiority of “one’s own” nation or, at least, striving to justify and defend “one’s own” people. I categorically deny having such feelings. On the contrary, with me the most spectacular display of the Jewish national feeling are shame and indignation when I face a Jew who is a scoundrel and on the whole a bad person. At the same time I am glad if a worthy person turns out to be a Jew. For instance, I am happy that Einstein was a Jew, as well as many other outstanding people. In these feelings I see nothing to be ashamed of. It is shameful to promote somebody “one’s own” at someone else’s expense, it is shameful to pardon “one’s own” scoundrel. But taking pride in “one’s own” good person is not shameful to me. However, I cannot clearly explain and understand these feelings of mine. But this is another question. By the way, to avoid suspicion of being insincere, let me say that my first and my second wives are Russian. The daughter’s husband is a Jew. The granddaughter’s husband is Russian. I do not see any problem here, life is like that.  I have burst out telling the above partly in order to explain why I was interested in Israel and its fate. I am very glad that there is such a state where at last the Jews are not a minority, often persecuted and humiliated. At the same time, many things there I do not like. Not in the order of importance, but I cannot help noting clericalism. In the history of the Jews synagogues played not only the role of prayer houses but also of the centers of a community. So some state support of religion in Israel can be understood. But it is important to know the measure. I cannot see any reasons justifying the absence (at least, in a number of places) of public transport on Saturdays and a number of other, sometimes more important, restrictions or consequences of religious character. In such a way atheists are discriminated. Still more important are a complete absence of unity in the country and the presence of a great number of cases of abusing the advantages of democracy which I mentioned above. That is why, I think, the very existence of the state of Israel is under threat, while the elimination of this state would be a new catastrophe of the Holocaust sort. I am writing about it, although I am aware that it might be irrelevant here, as I am indignant at the support of Arafat and his gangs by anti-Semitic and at the same-time progressive “left” forces of the West. And it is not because of my supposedly anti-Arab stand. Stalin and Hitler would have solved the Palestinian problem in 48 hours. They would eliminate either Israel or, which is much less likely, the Palestinian autonomy, deporting the disagreeable population to some distant place. Of course, in the civilized world such a decision is inadmissible. I am of the opinion that it is necessary and possible to have two completely isolated states. All those who know the lessons of history (for instance, the history of “friendship” of Catholics and Protestants in Northern Ireland) or who have seen on television dancing crowds of Palestinians of all ages while the television was showing the terrorist acts of 11th September, which took away the lives of thousands of innocent people, cannot have any illusions about the friendship and love between Palestinians and Israelis. To be more exact, only absolutely brainless bankrupts who had once obtained the celebrated agreements in Oslo can assume (or pretend to assume) that this might be possible in the foreseeable future. That is why I am convinced that a friendly coexistence of Israel and the state of Palestine is nowadays impossible. The only way out, in my view, is a complete isolation of these states from each other. It cannot be done with the majority of Israeli “settlements” remaining on the Palestinian territory. They should be removed, and it is not a question of law. One Israeli man told me that the land for these “settlements” had been bought and not annexed. So much the better, but it merely means that they can try to “exchange” these settlements for the land of the Arabs living in Israel. These Israeli Arabs are the only true obstacle to a complete division of the two states[10](https://www.nobelprize.org/prizes/physics/2003/ginzburg/biographical/#not10). But this obstacle is not decisive either. Of course, an impenetrable “wall” dividing the two states is also necessary. Using Palestinian workers on the territory of Israel is absolutely inadmissible. This is motivated, as I heard, by humanist considerations, allegedly by the care about poor Arabs. While in actual fact, I think, many Israelis do not want to do manual or menial dirty work, and their own workers are, in spite of the unemployment, either more expensive or unavailable. But the Palestinians working in Israel can’t help hating their rich masters, which is a source of additional antagonism. The history of importation of black slaves to America could be a good lesson. And taking care of Palestinians should be carried out by their brothers in rich Arab countries.  As for the question of the Golan Heights, it seems to me completely made up. I have been there myself and seen the ruins of a big ancient synagogue. So why is it Syrian land “from time immemorial”? Syria attacked Israel, suffered a crushing defeat and lost the Golan Heights. Now there is no Syrian population on this territory, its loss is Syria’s pay for aggression. Why those who are indignant at this situation do not demand that Konigsberg and its environs, renamed into Kaliningrad and the Kaliningrad region, should be returned to Germany? Such is a result of Germany’s attack on the USSR, and nobody, including the Germans, is not going to revise it. By the way, I am indignant that a part of Eastern Prussia and Konigsberg were named after Kalinin, this nonentity, who licked Stalin’s boots, while Stalin sent his completely innocent wife to a concentration camp.  Being a realist, I am sure that such “Ginzburg plan” will not be realized and will only arouse spite and mockery of pseudo-democrats and “peace-makers”. Why, that is their business, while I wanted to express my opinion, taking advantage of the freedom of speech.  In conclusion, one observation of general character. A tremendous progress of science has led to its deep internationalization. There is no such thing as American, Russian, Jewish or whatever else national physics. There is only one physics in the world, and when we are speaking, for instance, about British or Russian physics, we only mean the organization or the state of physics in Britain or in Russia. The Aryan science of the Nazis and the Marxist and Leninist science of the communists have long been forgotten. Whereas in the field of the social sciences and the sciences related to them, like sociology, psychology, economics and so on, the true depth and internationalization are still far from being achieved. But, I hope, here also the time of great success is near. Such is one of the factors which enable us, as it seems to me, to look into the future with hope. Another factor is the slowdown of the growth of the population of the Earth. Also positive, though this assertion is disputable, is the increase of the average life expectancy and thus the increase of the average age of the population. I remember the saying: if a man is not a communist at the age of 20, it means that he has no heart, but if he is a communist at 50, it means that he has no head. The bitter statement “what history teaches is merely that it does not teach anything” is not without reason. But still to completely agree with this statement would mean to totally lose the faith in mankind.  What has been said explains why I am still inclined to believe in the radiant future of mankind. Today on the road to it there are many obstacles, first of all, the Islamic (terrorist) threat, poverty and the lack of education of great masses of population, AIDS and other diseases. But let us remember the situation, for example, in 1943, sixty years ago. Europe was under Hitler’s heel, the USSR, though heroically resisting, was living under the Stalinist yoke. America was not so strong, and the world war was raging. Was it easier and better than now? The forces of democracy have coped with it, saved the civilized society and nowadays both the Nazism and the communism have almost sunk into oblivion. That is why we can hope for the ultimate triumph of the democratic system and the secular humanism all over the world. The necessary conditions for that are the presence of historical memory and the development of science. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0058 = VG** |
| **Interview** |  |
| Q3 | **Professor Ginzburg, welcome to the Nobel e-Museum. We are very happy to have you here. You have a very long career in science behind you, I would say, so my first question would be what is your most exciting memory of life in science?** |
|  | Vitaly Ginzburg: It is difficult to answer because it is many exciting stories. A lot of things, especially I would like to stress how the life of anybody – I’m no exception – is something like a very small boat on the sea. I give here a lot of examples of how my boat can turn around. For instance, one example. During the war we … I am from 1940 working in the Lebedev Physical Institute of Science of USSR, it was Soviet Union and now Russia, so forty-three years I worked in the same institute. During the war we were in evacuation in Kazan. Kazan at that time was not on the river Volga but five or six kilometres from Volga, afterwards they made a dam and now Kazan … It is not important. |
| Q22 | **Do you regret this, that you worked with the hydrogen bomb?** |
|  | Vitaly Ginzburg: I do not regret because it is very amusing because … First, we understood nothing. We suppose, we don’t understand the Stalinist bandit and suppose that all the harsh thing is connected, not closely connected with the regime, even I. My wife was in exile at this moment, in prison before. And nevertheless I suppose that this bandit is not so bad that somebody else make his atrocities. The other question … and we don’t understood … [Only] now I understand that Stalin, if he have a bomb first he can destroy all mankind. I was admitted to work in this project in the beginning when it was an open problem. Possible to do. Then Sakharov and I proposed how to do. In 1950 Sakharov was sent to the so called Sarov, Arzamas-16, the place where the bomb was built. But I was, because I was not good enough, have not sent there. In fact after this I have absolutely no connection with this military. I never seen the bomb, never seen the explosion, never seen anything. |
| Q3 | **You wrote, “We were prisoners of the system, but we were happy.” What did you mean?** |
|  | Vitaly Ginzburg: Happy? This is strange. I wouldn’t say we were happy. The fact is that … Where you read this? The prisoner of system and happy. I don’t remember this. Of course, you see every person somehow adapt to the situation. We were happy. Many people were starving. Many people were wounded. Many people were killed. We were happy in the sense that we can do work which we like with the exception of this military work as I mentioned to you. Also, for my part I have absolutely nothing to do with armament etc, because I was not allowed to go to the real military centre. I also work at physics. Without this I have something to eat, I have very bad flat and not starving. It was possibly enough to be happy. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0059** |
| **Biographical** | I was born in Camberwell, South London, on the 26th of March, 1938. I am told I only made it into the world on the date in question by seven minutes, thereby exhibiting at this early stage the tendency to procrastination which I fear has characterized much of my subsequent career. Not a great deal is known about my ancestry beyond a few generations, though one tidbit of information which has been passed down is that an ancestor on my father’s side served for a time as a cook on Nelson’s flagship, the Victory (it is unknown whether he was actually present at the battle of Trafalgar). As far as we can trace them, my father’s forebears were village cobblers in a small village in Hampshire, though his father broke with this tradition to become a greengrocer; my father would tell me how he used to ride with him to buy vegetables at the Covent Garden market in London. My mother’s parents were both of Irish stock; her father had emigrated to England and worked as a clerk in the naval dockyard in Chatham. My maternal grandmother, who survived into her eighties (and my twenties), was a remarkable person; sent out to domestic service at the age of twelve, she eventually married my grandfather and raised a large family, then in her late sixties emigrated to Australia to join her daughter and son-in-law, and finally returned to the UK for her last years. She was a very warm person, and I still have very fond memories both of my meetings with her (unfortunately few in her last years, because of the geography) and of the many letters she wrote me; since she had had no formal education, she simply wrote down on paper exactly what she would have said to you in person, and to read a letter from her was like having her stand in the room with you. It is interesting that that kind of spontaneity in the written word, which seemed to have been lost forever under the influence of universal secondary education, is now returning in the guise of e-mail.  My father and mother were each the first in their families to receive a university education; they met and became engaged while students at the Institute of Education at the University of London, but were unable to get married for some years because my father had to care for his own mother and siblings. He worked as a secondary-school (high-school) teacher of physics, chemistry and mathematics. My mother also taught secondary-school mathematics for a time, but had to give this up when I was born. I was eventually followed by two sisters, Clare and Judith, and two brothers, Terence and Paul (both now deceased). My parents were both Catholics (in my mother’s case ancestrally, in my father’s I believe because his own father had converted), so that we children were brought up in that faith, making us members of a small and somewhat embattled minority in the England of those days. Although I ceased to be a practicing Catholic in my early twenties, I still wonder from time to time how far the experience, in childhood and adolescence, of maintaining and defending, sometimes in public and in the face of some ridicule, beliefs and attitudes not shared by the vast majority of my compatriots may have influenced my subsequent attitude to physics and indeed to life in general (which, at least as regards the former, is I suspect sometimes regarded by my colleagues as reflecting a degree of iconoclasm verging on counter-suggestibility).  Soon after I was born, my parents bought a house in Upper Norwood, just outside the southern boundary of London proper (but well within the London conurbation). However, when I was eighteen months old, war broke out and we were “evacuated” to Englefield Green, a small village in Surrey on the edge of the great park of Windsor Palace, where we stayed for the duration of the war. The village was fortunate to escape the bombs that fell on towns all around (including Slough, perhaps in response to John Betjeman’s couplet!) and my war-related memories are relatively few: barrage balloons tethered above the Thames, lying in bed at night listening to the German “doodle-bugs” droning overhead (and praying they would not cut out immediately above our house), and the day I was “officially” informed that there was a war on (and decided that at no cost should my sister Clare, a year younger than me, find out about it). When in later life I read the memoirs of people who lived through those same years in the ravaged cities of continental Europe or Asia, or even through the Blitz in London, I realize how extraordinarily lucky we were.  Although Englefield Green is only a few miles outside the western borders of the Greater London conurbation, it is (or was!) in some sense in the countryside, and many of my memories of those years are of long walks through the fields and parks surrounding the village. However, one of my main activities, and the one which gained me most notoriety among our neighbors, was digging deep holes, I think on occasion deeper than my height at the time, in our front garden (not the easiest activity in a soil which was basically London clay). Exactly what motivated me to do this, and what deep insights it reveals into my psyche, is something I have never figured out, but it was perhaps loosely connected with my then choice of future career: after a brief flirtation with the idea of becoming a railway signalman, I decided firmly that I wanted to be an explorer (you have to remember that in those days there were still odd patches of the planet which had never been trodden by human feet, nor mapped by GPS). In the end I suppose that, like many academics, I achieved this ambition though in a more abstract way.  After the end of the war, we returned to the Upper Norwood house and lived there until 1950; my father taught at a school in north-east London (unfortunately a long commute) and my mother by that time had her hands full looking after what were eventually five of us children. I attended the local Catholic elementary (grade) school, and later, following a successful performance in the “eleven-plus” examination which I took rather earlier than most, transferred to the College of the Sacred Heart in Wimbledon (a “grammar”, i.e. state-financed academic-stream, school). This required a slightly complicated commute (bus, train, then another bus), which had the advantage that I could get a fair amount of reading done on the journey to and fro. (A few years ago, while on a brief return visit to the UK, I had for a reason I forget, to repeat exactly this journey and got completely lost – such have been the changes in the transportation system of southwest London in 50 years). Although I took part in the required physical sports at school, in particular in long-distance running, my main recreation in those years was chess, an interest I kept throughout my teens and into my Oxford years (I had a brief moment of glory when some years later, I was picked for the English team to compete against Scotland, Wales, and Ireland in the (under-16) Glorney Cup). Another practice I developed in those years was to take long hikes or cycle rides alone into the countryside surrounding southwest London; in later years I kept up this habit in much more untamed terrain, sometimes steering by compass, in thick mist, over many miles of trackless Welsh or Scottish mountainside. (Since I often did this alone and did not always leave details of my itinerary, and there are plenty of accidents in those areas even in good weather, I am perhaps lucky still to be here!)  In 1949 my father, who had become increasingly unhappy with the long commute to his job in north-east London, obtained a job teaching physics and chemistry at Beaumont College, a school run by the Jesuits near Windsor. This was a “public” school in the British sense of the word (i.e., “private” in the US sense) and we could certainly not have afforded the fees for even one of us three boys to attend it; however, my father negotiated as part of his conditions of service that all three of us could go to Beaumont free of charge, so we did. At the same time we moved to a large, rambling and somewhat broken-down house in Staines, on the far western edge of the London conurbation, which remained our family home until I was well into my thirties; this house was a delight to my father, who in another life would have been a handyman, but probably less so to my mother who had to endure the consequences of all the things he had not yet got around to fixing. One of the prime attractions, at least for us children, was the large garden and in particular the huge chestnut tree, in which we built many camps and “forts”.  Even within the somewhat esoteric world of British public schools, Beaumont was a rather unusual place. One feature which is, to my knowledge, unique to Jesuit schools, is that while the teaching staff includes both laymen, such as my father, and priests, those who are priests typically have not been school teachers for all or even most of their careers: they may have been for example parish priests, missionaries or even university teachers, and be doing the teaching job only for a few years before going back to one of those occupations. Also, in the case of “boarding” schools such as Beaumont, it is common for priests who are retired to live on the premises, whether or not they have been teachers there. As I will relate, this circumstance had totally unforeseen consequences for my career.  Not long after my transfer to Beaumont, at the age of thirteen, I had to make what under normal circumstances would have amounted to an irrevocable decision about my career in life. I imagine that to anyone reading this 50 years later, in the context of a European (even British) or North American school system, this will seem quite incredible; but in those days the degree of “channeling” even in progressive schools in Britain (and Beaumont was far from progressive!) was extreme: you had to choose at an early age (normally 15, but in my case 13) between specialization in classics, modern languages, mathematics and science. That choice then essentially determined which kinds of degree you could take at university (assuming you went), and that in turn put severe constraints on the kind of job you could reasonably apply for. At Beaumont, the unspoken tradition was that if you showed any signs of academic ability, as I apparently did, and had no strong predilection to the contrary, you were automatically channeled into the stream which was deemed most academically prestigious, namely the classics stream (Latin and Greek languages and literature). Remarkably, my father, despite his own training and employment, never exerted the least pressure on me to take the science option (which was generally regarded, for reasons which are unclear to me, but were probably a reflection of a common attitude in Britain at that time, as the least prestigious of all). So I indeed ended up, at the age of thirteen, committed to spending most of my school years on the classics, with only a small fraction available for other subjects such as English, history, or mathematics. My memories of this classics education are mixed: on the one hand, I still recall the soul-destroying discipline of the “gender rhymes”, jingles which we were required to memorize to remind us of the correct gender of Latin nouns, and fragments of which still 50 years later seem to take up kilobytes of storage space in my brain which I am sure could be put to better use (“. and common are to either sex, *artifex* and *opifex* …”). On the other hand, some of my teachers were enthusiastic and inspiring, and introduced me at an early stage to the poetry of Catullus and Horace and the historical analysis of Thucydides.  Although I by and large enjoyed the academic side of life at Beaumont and prospered at it, in most other respects I was a decided misfit: Almost all my fellow-pupils were fee-paying, which almost automatically meant that they enjoyed, at home, a lifestyle of which we had no experience; except in my last two years I was one of a small minority of “day-boys” (that is, I commuted daily from home rather than residing at the school throughout the term); and, perhaps most importantly, because of my academic precocity I was placed in classes with boys who were mostly a couple of years older than me. As a result, the five years I spent there, while not unhappy, do not stand out in retrospect as a particularly joyful period of my life.  However, at least two things happened during those years which were to have, in different ways, very happy long-term consequences. The first was that, for a reason I cannot now recall (I think it may have been to help my recovery from an illness which had kept me in bed for a month) my father decided to send me on a mountaineering course in Snowdonia. Unlike the rolling hills of Surrey and Berkshire which I had explored on my solitary hikes, the mountains around Llanberis were the real thing, craggy, mostly pathless and, in bad weather, decidedly not to be trifled with; I fell in love with them at first sight, and mountaineering in all its forms, from easy strolls to technically relatively demanding rock-climbing, has been a major passion of my life ever since.  The second serendipitous event actually occurred after I had competed for, and obtained, a scholarship to Balliol College, Oxford, in December 1954. I had to wait a few months before taking up the scholarship, and in those days there were no nationally organized programs to employ people in my position in voluntary service overseas, etc., so I somewhat reluctantly stayed at school. Also living at the school was a retired priest, Fr. Charles O’Hara, who had at one period of his career been a university teacher of mathematics and had in fact written a textbook on projective geometry. One day he ran into me in the corridor and said to me: “You seem to have a lot of time on your hands. Well, I have a lot of time on my hands. Why don’t you come along to my room for a couple of hours a week, and I’ll show you some interesting things in modern mathematics?” Now, at that point I had only the minimal mathematics required for the “O-level” examination (though I don’t recall clearly, I think it did not even include differential calculus, and certainly not the integral variety), and I had absolutely no reason to think that I would ever in my career need anything more sophisticated. However, I indeed had time to kill, Fr. O’Hara seemed enthusiastic, and perhaps to humor him as much as anything, I agreed to his proposal. So for the rest of those two terms he gave me, for a couple of hours a week, a sort of Cook’s tour of ideas in modern mathematics, involving concepts such as groups, rings, and fields which my O-level education had never even grazed and which I found quite fascinating. Even more importantly in retrospect, he actively encouraged me to do a few simple exercises involving those concepts, and to my initial surprise I found I could in fact do them without great difficulty. After the end of the school year I had other things on my mind and promptly forgot all the details of what he had taught me; but this was to be the first of a series of fortunate accidents that helped to shape my eventual career.  In early October of 1955 I went up to Oxford to take up my scholarship at Balliol, with the intention of reading (majoring in) the degree technically known as Literae Humaniores, and informally as Greats (on which more below). For me, coming as I did from a school background which even by the standards of those days was somewhat restrictive and conservative, entering university was like stepping through a door and finding myself in a completely new world. I still have idyllic memories of my first two weeks in Oxford, with the autumn colors in the parks at their peak and the scent in the air of infinite possibilities, both intellectual and social. Although of course that initial sense of excitement didn’t last forever, by and large my nine years at Oxford, as undergraduate and graduate, were very happy indeed; not only did I thoroughly enjoy and prosper at my academic work, but I made a variety of friends from all over the world, especially from south and east Asia, with many of whom I have kept in lifelong contact. On the sports side, I had done some sculling at school, and initially rowed bow in the Balliol third eight. However, it was not long before the coach noticed that I was two stone lighter than the then coxswain (“cox”) and interchanged us, and thereafter my destiny was fixed; I eventually rose to be cox of the first eight, and while I did not exactly steer them to victory (in fact, in the year of my coxswainship we did, formally, just about as badly as it is possible to do!), I nevertheless feel we put up a good fight. (Incidentally, I should have no such career possibility today: one consequence of the gender-integration of the Oxford colleges which took place in the 70’s is that the coxes of college eights are now, for obvious reasons, almost invariably female).  The Oxford Greats degree is one of those many British institutions which need to be understood in historical rather than logical terms. It takes four years (12 trimesters), and for the first five trimesters one studies Greek and Latin languages and literature, thus making it the obvious choice of degree for anyone who, like me, has specialized in the classics at school. For the last seven trimesters one studies in parallel “ancient”, that is Greek and Roman, literature and philosophy; the philosophy side of the degree has only a relatively small “ancient” component (Plato and Aristotle), and is (or was in my day) mostly centered in the analytic and mostly Anglo-Saxon tradition (Descartes, Locke, Berkeley, Hume, Russell, Wittgenstein … and in more contemporary terms Ayer, Ryle, Austin …). I thoroughly enjoyed all the components of the course; I found particularly rewarding the individual (or sometimes two-person) tutorials characteristic of the “Oxbridge” system, in which I could discuss with my tutors, at least prima facie on equal terms, the most recent journal articles on the origins of coinage in the ancient Mediterranean or the concept of machine consciousness. (One disappointment I encountered on eventually switching to physics is that in that subject, as I suspect in most of the “hard” sciences, such cutting-edge discussions are not realistically possible at the undergraduate level).  I am often asked whether and how my Greats training has been useful to me in my subsequent career in physics. To that question I have a joking answer, namely that unlike (apparently) some of my physics colleagues I at least know the difference between the Greek letters j and y! However, there is a serious answer: I certainly do feel the philosophy component of the degree, at least, has helped to shape the way at which I look at the world and in particular at the problems of physics. This is not something which is easy to quantify or make concrete; I have never undergone a course of psycho-analysis, but I imagine that anyone who does so can never again look at the world in quite the same light, and I think that the same is true for the kind of rigorous course of analytic philosophy which I went through at Oxford. Another analogy might be with the feeling I experienced later when I learned to speak Japanese with a reasonable degree of fluency; it is as if one has learned to use a muscle that one did not know one had. At any rate I have never for a moment regretted the years I spent on this degree.  Towards the end of my third year in Oxford it gradually began to dawn on me that I could not go on being a student for ever and must start looking for gainful employment. Not being endowed with a great deal of initiative, I first looked around to see what careers previous Greats students at Balliol had chosen. With few exceptions, it seemed that they had either gone into the British civil service or become teachers, at university or high-school level, of one of the subjects they had studied (classics, ancient history or philosophy). As regarded the first option, it did not take me long to figure out that even were I to pass the examination for the civil service (something which was by no means a foregone conclusion, since it tested qualities rather different from the ones which had advantaged me in a purely academic context), a career there was likely to be both uncongenial to me and of dubious benefit to the British public. So I started contemplating a career in academia; and since the subject which I had enjoyed most, and performed best in, was philosophy, this seemed the natural choice.  But the more I thought about the prospect of doing a doctorate in philosophy and eventually obtaining a university lectureship in that subject, the more I realized that in my bones I just did not want to do it. Why not? It never occurred to me – as it no doubt would have to someone with more imagination – that maybe I was not actually cut out for an academic career at all. Rather I asked myself what exactly it was about philosophy as such which deterred me from doing it professionally for life. And I eventually came up with the answer that it was because what counted as good or bad work in philosophy – at least as it was practiced in Oxford at that time – seemed to depend so strongly on the precise nuances of one’s wording (something, incidentally, which I suspect may have subtly disadvantaged non-native English speakers in that discipline); there seemed to be no objective criterion of what was correct or not, or even what was good work or bad, and I felt in my bones that it was just such a criterion that I needed if I was going to pursue an academic career. I did indeed briefly consider the possibility of going into pure mathematics, but rejected it on the grounds that in mathematics, almost by the definition of the subject, to be wrong means you are stupid: I wanted the possibility of being wrong without being stupid – of being wrong, if you like, for interesting and nontrivial reasons. Physics seemed to fill that bill, and while I had zero formal training in that subject, the confidence which I had acquired from Fr. O’Hara in advanced mathematics led me to believe that that aspect of the subject, at least, would not give me major difficulty. So, in the early summer of 1958 I took my courage in both hands and applied to do a second Oxford undergraduate degree, in physics, following the anticipated completion in spring 1959 of my Greats degree.  In those days in Britain to do a second undergraduate degree in anything, let alone in a subject in which one had no secondary-school experience, was practically unheard of, and I immediately faced several practical obstacles. In the first place, I had to persuade some college to accept me; secondly, I had to find some way of financing two more years of undergraduate education, and finally, since 1959 was known to be the last year of conscription in Britain, I had to persuade my draft board that my application to do a second degree was not just a ruse to avoid military service for ever (a consequence it in fact had). At this point I had another piece of luck: in October of 1957 engineers in the Soviet Union had propelled into space the first Sputnik, thus stealing a march on the West in what not just governments, but every layman could see was an extraordinarily important area of technology. Immediately the cry went up from politicians and the press: how have the Soviets managed to get ahead of us in this crucially important enterprise? And the answer was not long coming: It was because we have encouraged all our best brains to study useless subjects (such as classics) rather than useful ones (such as science and engineering and particularly physics). Immediately all sorts of scholarships became available for students in the arts who wished to transfer to science; and while I did not in the end need to apply for any of these, I think that the general shift in cultural attitudes which they reflected was an enormous psychological boost to me in making the switch, and in particular may have been the crucial element in helping my prospective tutors to convince the draft board that they could dispense with my services on the parade ground.  The two people who played the most indispensable role in helping me make the transition are David Brink and Michael Baker. In those days most Oxford colleges did not have a tutor in theoretical physics as such, but Balliol had recently appointed David in this role, and since I initially applied to Balliol it was he who had to take a decision on whether I was or was not a reasonable prospect for a degree in physics, despite my lack of background. He asked me to read various sections of that beautiful book, “What is mathematics?” by Courant and Robbins in the summer vacation between my third and fourth years, and when I returned in October tested me on them; on the basis of my performance he recommended that Balliol should accept me for a second degree. However, by that time I had applied somewhat speculatively to Merton, where Michael Baker was a tutor in physics, for a Domus scholarship. These scholarships were normally given to students undertaking a postgraduate degree, but in my case the Fellows generously made an exception. Thus, I ended up doing my degree in physics at Merton; Michael was my principal tutor, but since he was an experimentalist and Merton did not at the time have a tutor in theoretical physics as such, I went to David for most of the more theoretically oriented parts of the course. In order to get a classified degree I had to complete the course in two years rather than the standard three, and this resulted in a certain degree of mental indigestion: I recall simultaneously struggling with old-fashioned problems involving rods and pulleys from Humphrey and Topping’s “Intermediate Mechanics” and with the properties of Hilbert space as set out in Mandl’s text on quantum mechanics.  The final examination at Oxford was in those days (maybe it still is) an ordeal unimaginable by those who have not experienced it: there was no continuous assessment at all, and one’s whole academic fate depended on a succession of closed-book three-hour written papers (in the case of Greats fourteen of them), one after the other, two per day, with only the weekend as a short respite. Not surprisingly, the population of the psychiatric wards of the local hospitals used to peak markedly before and during those periods. Actually, in my Greats degree I had thrived on the stress and come out with a straight first class, so I was probably over-confident going into the physics final exam. In the event this nearly turned into a complete disaster: the first paper was, I think, on thermodynamics, an area where problems notoriously require just the right trick to solve them, and after an hour and a half of the three hours I had got nowhere with the first problem I had tackled. I remember sitting at my desk in the exam room, my head figuratively if not literally in my hands, and contemplating the prospect of having dashed the hopes of all those who had supported and shown faith in me; it was one of the blackest moments of my life. In the end I pulled myself together and was able to answer one or two questions on that paper, as well as coping reasonably with the remaining papers; but when I was told that I was on the borderline between two (unspecified) classes and would therefore have to undergo a viva (oral exam) to decide between them, I was convinced that the two classes in question were a second and a third. Imagine my incredulity and, eventually, my delight when a few days after the viva, a letter arrived informing me that I had a first (and would therefore qualify more or less automatically for public funding for an advanced degree should I so chose). If nothing else, I think that experience has convinced me that no examination system, and certainly not the one I went through, can be an infallible measure even of purely academic ability.  I was now in a position to proceed to postgraduate research in physics. I had specialized on the theoretical side as an undergraduate, and my memories of the (rather small) amount of lab work I had had to do were not particularly rosy, so it seemed natural to apply for research on the theoretical side. But where, in which area and to whom? The first question answered itself easily, since I felt (perhaps wrongly) that no university other than Oxford itself was likely to appreciate my peculiar academic background. As to the second, the main areas of theoretical work at Oxford in those days were particle theory and condensed-matter (or, as it was known in those days, “solid-state”) physics. The time was early 1961, and the advice I got from the theorists I consulted was that the current state of particle theory was not very exciting (an opinion which had to be revised a few months later, when the concept of unitary symmetry burst on the scene); so I opted for the condensed-matter area. Finally, one person who was willing to overlook my unorthodox credentials was Dirk ter Haar, then a reader in theoretical physics and a fellow of Magdalen College; so I signed up for research under his supervision. As with all his students in that period, the tentatively assigned topic of my thesis was “Some Problems in the Theory of Many-Body Systems,” which left me a considerable degree of latitude.  Even by the standards of British universities in those days, Dirk’s supervisory style was somewhat unusual. He took a great interest in the personal welfare of his students and their families, and was meticulous in making sure they received adequate support; indeed, in the middle of my second year of research he encouraged me to apply for a Prize Fellowship (junior fellowship) at Magdalen. To my great surprise I was successful, I am sure in considerable measure thanks to his advocacy, and thereafter was able to enjoy a lifestyle rather more opulent than had been possible on the standard graduate studentship which had supported me earlier. On the other hand, Dirk’s method of supervising graduate research was to throw his students in at the deep end: by a few months into our relationship I had got the message that it was up to me not only to solve my thesis problem, once posed, but to find a viable problem in the first place. (I try to encourage my own graduate students to do the same, although I do not take Dirk’s extreme position of refusing, in effect, to make any suggestions at all). In the end my thesis work consisted of studies of two somewhat disconnected problems in the general area of liquid helium, one on higher-order phonon interaction processes in superfluid 4He and the other on the properties of dilute solutions of 4He in normal liquid 3He (a system which unfortunately turned out to be much less experimentally accessible than the other side of the phase diagram, dilute solutions of 3He in 4He). Although both halves resulted in publications, neither made much impact, and the only feature which in retrospect distinguishes my D. Phil. thesis in any way is that a small part of it (the acknowledgements) is, as permitted by the Oxford examination statutes and as a result of what our forebears would no doubt have described as a “conceit”, written in Latin.  One part of my graduate student experience which I have always felt was particularly valuable to me was the undergraduate teaching I did during this period. I had started this, I think as soon as I began graduate research, with a view to supplementing my rather subsistence-level studentship, but kept it up even after the Magdalen fellowship eliminated the need for this; as I recall, it was six hours per week of one-on-one or one-on-two individual tutorials in the undergraduate “theoretical option”, and I found I enjoyed it immensely and learned at least as much from it as from my formal research activities. Nowadays I always insist, as a matter of principle rather than financial economy, that my own graduate students spend at least one semester per year as teaching assistants.  In the spring of 1964, as I approached the three-year deadline for submission of my D. Phil. thesis, I began to think about postdoctoral work. In applying I had two criteria: the group in question should be a world-class center of excellence in many-body theory, and the environment should be as different as possible from Oxford (where I had by now spent nine years, more than a third of my life). So posed, the problem had an essentially unique solution, namely the group of David Pines at the University of Illinois at Urbana-Champaign, and it was there that I applied; I don’t think I even bothered applying anywhere else in those pre-information revolution days, the preparation of multiple applications was quite a daunting and time-consuming task. Despite this, I was quite surprised and gratified to be accepted. (David tells me that the letter Dirk wrote for me said in effect that I had a good training on the arts side but didn’t know any physics – a statement which in view of my highly compacted education in that subject was certainly true, but to which David’s reaction was “well, that’s something we can teach him!”). So I spent the period August 1964 – August 1965 at UIUC, and David and his colleagues ([John Bardeen](https://www.nobelprize.org/nobel_prizes/physics/laureates/1972/index.html), Gordon Baym, Leo Kadanoff and others) did indeed teach me a great deal. And it was indeed very different from Oxford …  From the academic point of view that year at UIUC was a turning-point in my career; as recounted in my Nobel lecture, I not only became interested in the (at the time hypothetical) superfluid phase of liquid 3He, but produced my first research (on [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html)-liquid effects in the superfluid phase) to make any real impact internationally. However, I did not find the physical environment congenial (it was only a few years since Dutch elm disease had wiped out more of the town’s mature trees, something which is an essential ingredient of the quality of life in small Midwestern towns), and I remember swearing an oath to myself, as I left at the end of the year, that I would never come back and live permanently in Champaign-Urbana – an oath which I was to break rather spectacularly twenty years later.  My Magdalen fellowship still had a couple of years to run, and the natural thing would have been to return to Oxford and spend them there. However, I had become increasingly interested in north-east Asia, and anxious to spend some time there before committing myself to the standard British academic career; and the Fellows of the college eventually consented, with considerable generosity, to allow me to spend a year of my fellowship in the group of Professor Takeo Matsubara at Kyoto University in Japan. This year at Kyoto was a marvelous experience for me. It was my first real experience of living and working in a foreign culture, and I tried to exploit it to the fullest: I lived in a standard Japanese student room, put a considerable amount of effort into learning the language and tried as far as possible to avoid using English or mingling very much with other foreigners. At this time this was not at all the typical behavior of Western visitors, and many years later I was told that it had caused considerable interest and speculation among some of my Japanese colleagues, who finally hit on what had to be the only logical explanation – that I must be a trainee CIA agent! Fortunately this opinion did not prevent me making many good friends during that year, and I have very warm memories of all the parties, mountain hikes and home visits we shared. Although the academic work I did during this year was overall not particularly remarkable, it included the paper on two-band superconductors which was to play a crucial role in my research on superfluid 3He a few years later.  After one more postdoctoral year which I spent in “roving” mode, spending time at Oxford, Harvard and Illinois, in the autumn of 1967 I took up a lectureship at the University of Sussex, where I was to spend the next fifteen years of my career. Sussex was one of the “new” universities founded in the fifties, and at that point was only a few years old, but had managed to attract among other things a lively group of theoretical physicists under the leadership of Roger Blin-Stoyle and a constellation of able low-temperature experimentalists headed by Douglas Brewer. However, what attracted me most was the liberal and collegial atmosphere both in the University as a whole and in the Physics Department, which actively encouraged people to explore their intellectual interests across traditional academic boundaries; quite a few of my colleagues, though trained in the traditional areas of physics, ended up spending much of their time on science policy, physics education and elsewhere. However, although I enjoyed this relaxed environment, I spent my first five years at Sussex mostly teaching the standard undergraduate physics courses and, in the time available for research, working on various problems in theoretical low-temperature physics, including some such as the possible “supersolid” phase of helium which appear to still be of interest nearly forty years later. Being without family attachments, I was able to spend a lot of time abroad during the vacations, and in particular had an extended stay at the Max-Planck-Institute in München and a couple of trips to winter schools in Karpacz, Poland (at the time something of a minor adventure, since the Iron Curtain was still firmly in place).  In the summer of 1972 there occurred the series of events which were to shape my research career for the next decade; I have recounted these in my Nobel lecture and will not repeat them here. At just about the same time an important event occurred on the personal front: I became engaged to Haruko Kinase, at that time an undergraduate student at Sussex, and we married in June 1972. (Since Haruko’s nationality was and is Japanese, and her undergraduate major was international relations, the family joke is that at least she passed the practical!). We had a sort of extended honeymoon in Japan, living in central Tokyo with Haruko’s parents while I worked in the group of Professor Yasushi Wada at the Hongo campus of Tokyo University. One spin-off from this stay was that I got to know Shin Takagi, who was in his final year as a graduate student in the Wada group, and was sufficiently impressed with him that I invited him to Sussex as a postdoc; he spent a couple of years there, and we collaborated extensively on superfluid 3He as well as having many conversations ranging over the whole of physics. Incidentally, if my early years in the extreme outer reaches of the London conurbation are not counted, the only large cities anywhere in the world where I have ever lived for more than a few months are Kyoto and Tokyo; I can think of a lot worse.  As this note has to be of finite length, I will speed up at this point and just review the important events in my life since 1974. Soon after our return from Japan, in early 1975, Haruko and I acquired a small house that we still own in the warren of streets above London Road station in central Brighton, and it was there (or more accurately on the twelfth floor of the nearby Royal Sussex County Hospital) that our daughter Asako was born in September 1978, and there that she spent the first five years of her life. After returning from Japan I spent several more very enjoyable years at Sussex, with various excursions including two one-semester trips in 1976 and 1977 to the University of Science and Technology in Kumasi, Ghana, with which Sussex had a teaching exchange arrangement. This was another interesting cultural shock; one unexpected aspect was that because of the generally relaxed atmosphere in Ghana, I had for the first time in many years a surfeit of free time, and I used it among other things to write a paper on nonlocal hidden-variables theories which I eventually published a full quarter-century later.  In the spring of 1982 I received, out of the blue, an offer from the University of Illinois at Urbana-Champaign of the MacArthur Chair with which the university had recently been endowed, and after the three of us had been across for a visit (where I observed, inter alia, that the trees had recovered from the blight of the early 60’s!) we decided to make the move. As I had already committed myself to an eight-month stay at Cornell in early 1983, we finally arrived in Urbana in the early fall of that year, and have been there ever since, so that Asako has in effect grown up as an American (she and I both eventually received U.S. citizenship, which we hold dual with that of the U.K., in the summer of 2002). Haruko eventually obtained a Ph.D. in cultural anthropology from the University of Illinois, and is currently doing research on the hospice system; Asako has graduated, also from UIUC, with a joint major in geography and chemistry. My own research interests have shifted away from superfluid 3He since around 1980 (although I still maintain an interest in some problems involving violent departures from equilibrium); I have worked inter alia on the low-temperature properties of glasses, high-temperature superconductivity, the BEC atomic gases and above all on the theory of experiments to test whether the formation of quantum mechanics will continue to describe the physical world as we push it up from the atomic level towards that of everyday life (a program for which my shorthand is “building [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/index.html)‘s Cat in the laboratory”). It is satisfying that this program, which when proposed twenty-five years ago met with considerable skepticism, seems in the last three or four years to have come to fruition, in the sense that several experimental groups have realized, in Josephson devices, quantum superpositions which can be legitimately regarded as of the “Schrödinger’s Cat” type.  When I look back on the chain of events which led to the research which the Nobel Committee has recognized, I realize how incredibly fortunate I have been – not just because of the intervention in my career of seemingly irrelevant events such as the propulsion of Sputnik into orbit in the fall of 1957, but because so many people were prepared to put their faith in me, and in particular my ability to make a successful career in physics, at a time when the evidence in favor of that proposition was nonexistent or perhaps even negative. I shall remain forever grateful. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0059 = AL** |
| **Interview** |  |
| Q5 | **Professor Alexei Abrikosov, and Tony Leggett, welcome to the Nobel e-Museum and also to this interview. We’re very happy to have you here. I’d like to ask you the first question. What are the sources of inspiration to you, as a scientist, Dr Leggett?** |
|  | Anthony J. Leggett: I think that’s a rather difficult question to answer, actually. I think in the scientific discovery, luck plays an enormous role, but I think one thing one can be fairly sure about is that if you’ve not been thinking about the problem continuously and perhaps even when you’re lying awake at night for some time, perhaps some weeks or even some months, then it’s unlikely that you’ll get the sudden flash of discovery that makes it work. |
| Q4 | **It’s a part of scientific research?** |
|  | Alexei A. Abrikosov: Yes. However, my ideas I just take from experiments.  Anthony J. Leggett: Yes, I would certainly agree with that. I always find that the main stimulus to theory is some curious experimental result that seems totally outrageous and unnatural. And one tries to understand it. |
| Q4 | **But there is one problem in your fields of research in super conductivity, I would say, that you can’t predict the super conductors that would work in high temperatures, like room temperature. What do you think about that?** |
|  | Alexei A. Abrikosov: You see, there was some experiment which was actually performed in the United States by an American physicist and a Russian visitor, which inspired these experiments where they tried to find high temperature super conductivity. They failed eventually. However, they inspired me to some extent, and therefore I even published some model, how high temperature super conductivity could be achieved. No such, actually, it was not achieved on that path but actually nobody tried to find it on that path, and so therefore I still have hopes that that is at least one of the good paths for searching high temperature super conductors. |
| Q4 | **And Dr Leggett, what about prediction?** |
|  | Anthony J. Leggett: Well, if you … there are about 100 elements known, if you consider a compound which involves six of these elements, then crudely speaking there are, let me think, a trillion such compounds. Nature has never made most of these compounds. We will certainly not be able to make most of these compounds in any reasonable time. Somewhere out there I would take a large bet that there are substances that will be super conducting at a room temperature. We just don’t know where they are in this immense space. Once we have a generally accepted theory of cuprate super conductivity, I think we may be in a much better position to go and look for them. |
| Q4 | **Yes. So you have to find something entirely new. Do you agree with that?** |
|  | Alexei A. Abrikosov: But with some idea.  Anthony J. Leggett: Well, I think, yes, I agree with part of that, at least. I would tend to, without wanting to express myself about whether we at the moment have a satisfactory theory or not, I would tend to agree with the belief that within the cuprate family it’s not very likely we’ll get much higher transition temperatures. What I do believe, however, is that there may be ways of understanding the cuprates, which will lead us to other classes of materials which might be room temperatures in the conductors. So I’m also optimistic. |
| Q20 | **So maybe, somehow you need the genius. I’d like to ask you what is it that makes some people do the discovery and others who work as hard as the discoverers don’t do that?** |
|  | Anthony J. Leggett: A large element of luck. Somehow, I suppose the people who do make big discoveries are ones who somehow manage to free themselves from conventional ways of thinking and to see the subject from a new perspective. But how you quantify that I wouldn’t know. |
| Q21 | **It’s also good luck if something doesn’t work as you expect, as I understand. What would you say about that?** |
|  | Anthony J. Leggett: Well, yes. Again, some of the most stimulating experiments, to a theorist, are those which don’t come out as you confidently expected them to. |
| Q4 | **You mean that the cat is both alive and dead?** |
|  | Anthony J. Leggett: Well, it’s difficult to express the result in classical terms, but if you take the interpretation of quantum mechanics seriously and you apply the same interpretation at the level of the cat as you do at the level of the atom, then you do seem to reach the conclusion that it is not definitely in one state or the other until observed. And that, of course, is the famous quantum measurement paradox or [Schrödinger](https://www.nobelprize.org/nobel_prizes/physics/laureates/1933/schrodinger-facts.html)‘s paradox. That’s a very different situation from what one normally gets in the sort of standard applications of super conductivity and super fluidity. |
| Q4 | **I understand. But what about quantum mechanics? Isn’t it bizarre that you have those super positions somehow, or whatever you call it, but somehow it doesn’t follow the logics.** |
|  | Alexei A. Abrikosov: I must say, I am in a sense much simpler. The existence of liquid helium that is actually at low temperatures and that it doesn’t solidify at ambient pressure is a quantum phenomenon. That is. It is a paradox. Such an object should not exist according to classical laws. And so, already here it starts. Anthony J. Leggett: Well, yes, I would agree. And I think that is one of number of cases one could quote in which one sees in one sense or other the macroscopic effects of quantum mechanics. But I’m including the difference between a liquid and a solid, as a macroscopic difference. But I do think there’s a big difference between this kind of case and the genuine Schrödinger’s cat kind of situation, which is one which we have not yet been really able to probe directly in experiments, although we’re working towards it. |
| Q4 | **There was a big problem even for, I would say, the most famous scientist in the world,**[**Albert Einstein**](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html)**; he could never accept quantum mechanics, because it was too bizarre for him. Do you think it is real, somehow?** |
|  | Anthony J. Leggett: I personally think it’s entirely possible that in the year 3000 we will still believe that quantum mechanics is the whole truth about the world. If we really do still believe it in the year 3000, then I think in some sense our attitude towards the physical world at the everyday level will be radically different from what it is today, because we will really have had to face up to this weirdness, which by that time I’m confident will have been amplified to the everyday level. I think it’s at least equally probable and perhaps more so, that as we go from the level of the atom to the level of the cat, we will find that somewhere along the line quantum mechanics breaks down and some new theory of which we can have at present no conception will take over. I am personally hopeful that it’s the second thing that happens. |
| Q4 | **The year 3000, so it’s almost 1000 years ahead. OK. What about quantum computers? It would be also …** |
|  | Alexei A. Abrikosov: What Tony was telling you, it was exactly about quantum computers. |
| Q4 | **That’s what you think?** |
|  | Anthony J. Leggett: Well, if quantum mechanics does describe the whole universe at all levels, then it seems, as far as I can see, that there is no reason in principle why one should not build a functioning quantum computer. I think, however, one may well find that the practical difficulties of doing that are just so enormous that in the end people will conclude that it just isn’t worth it. That although the price tag on a quantum computer that can factorise, say, a 500 digit number, is very large, it’s not infinite, and at some point people may just conclude that it isn’t worth the effort. |
| Q14 | **It will be too expensive, you mean?** |
|  | Anthony J. Leggett: Yes. Well, or just take too long and involve too many people etc, etc, yes. |
| Q4 | **Do you have any other guesses?** |
|  | Alexei A. Abrikosov: No, no. I don’t. Somehow, this topic, I never loved, and so therefore I always decide, I never go to conferences on quantum computing, and so on, so that’s not for me. That’s not for me. |
| Q4 | **It’s the weird, or too far from experiment, or why?** |
|  | Alexei A. Abrikosov: Well, it’s far from experiment, and from my point of view, it’s dull. I don’t know, my taste is so, I like objects, you know, I can see and I can feel them.  Anthony J. Leggett: I would think it’s probably fair to say that at least right now the challenge of quantum computing is not throwing up any very deep new conceptual questions. It’s a matter of in some sense engineering, and so whether it’s a matter of taste, whether you regard that as interesting or not. |
| Q14 | **I understand. So what is the challenge for the future, do you think, in your field?** |
|  | Alexei A. Abrikosov: In my field, first of all, if you speak like that, that we have many challenges, actually. And every time I am working on something and that is maybe a small problem from your point of view, but usually one should not divide the problems into small and large problems, because every small problem can become a large problem, or eventually, you know, develop into something. So therefore one must just, if one has a problem, one has to solve it. And that’s all. That is the main important thing. Of course, the general challenge is high temperature super conductivity, room temperature super conductivity, in my field at least, yes, but however, I understand very well that I am alone unable to solve it. Yes, and so it requires an effort of many people and for some time, yes, and experimental efforts, actually, not just theoretical attempts, yes? A theorist can give an idea where to search. However, he cannot predict that just this and this substance will be the one. No. No way. And so just when I was in Washington, there was such a session that was dedicated to 50 years since Eisenhower gave a talk at the United Nations about peaceful applications of atomic energy. So then I said there, I spoke about it, room temperature super conductivity, and I said that in order to reach that goal, and it is reachable, I am absolutely sure about that, then the funding system for science for this particular thing must be changed entirely, because it is a long term project and you cannot expect immediate, immediate success and you cannot even predict when that success will happen. However, if you conceive that topic is solvable and so on, so then, you must just give money for that, and people will do research, yeah, and that’s all, yeah? And then Ray Orbach, who is the head of basic energy sciences in the Department of Energy, so he said: I heard what you said, and I will think about that. And so he definitely has some positive thoughts about that. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0060** |
| **Biographical** | I was born in Washington, D.C. on October 14, 1914. My father was a photographer at the National Bureau of Standards. A self-educated man, he never finished high school, but, in his career at the National Bureau of Standards, he made many useful inventions, and eventually became chief of the Photographic Technology Section. His early influence led me in the direction of individual experimentation and designing my own apparatus. My mother, Ida Rogers Younger, was a native of the state of Virginia. She taught me to enjoy music, although she never succeeded in making me a performer. It was to please her that I spent several years as a choirboy, in spite of my inability to carry a tune. A bit later in life, I took pleasure in attending outdoor concerts at the Watergate, in the days before air traffic grew heavy enough to drown out the music.  My constant companion in childhood was my brother Warren, only 14 months my junior. Together we played street games on summer evening, paddled a canoe on the Potomac and, after my release from the choir, spent many weekends rifle-shooting with our father. In high school and college, I gathered a number of medals for marksmanship, but I have long since abandoned this activity, having concluded that the world would be a better place with fewer sharpshooters.  As my brother and I grew older, our interests diverged. He headed for a military career, while I became more interested in science. My father encouraged my interest, bringing me chemicals for my basement experiments, and helping me become a reasonably good photographer. My favorite reading matter was Smithsonian reports on many phases of science, obtained at my local branch library. Washington offered many educational opportunities for curious young minds.  I was educated in the Washington public schools, and attended the University of Maryland as a day student, graduating in 1938 with a degree in chemistry. After working for the Dow Chemical Company in Midland, Michigan for a year, I returned to the University of Maryland to take a Master’s degree, before going on to Yale to pursue a doctorate. In 1942, I received my Ph.D. in physical chemistry, and immediately entered the Army as a reserve officer. Most of my war years were spent at Dugway Proving Ground in Utah, observing chemical weapons tests and, in my spare hours, exploring and photographing the surrounding territory, which included the Great Salt Lake and geologic evidence for its much larger predecessor, Lake Bonneville.  Upon my discharge from the Army in 1945, I went to work at the Monsanto Chemical Company’s Mound Laboratory, in Miamisburg, Ohio, doing applied radiochemistry of interest to the Atomic Energy Commission. In the spring of 1948, I was able to join the newly created Brookhaven National Laboratory, which was dedicated to finding peaceful uses for atomic energy. In my first few months at Brookhaven, I lived at the Lindenmere, a summer hotel which had been leased by the laboratory to provide housing for new arrivals. It was there that I met my future wife, Anna Torrey, who was also employed at Brookhaven, in the Biology Department. Since this was a seaside community, I decided to build my own sailboat. This notion was viewed with scorn by most of my acquaintances but, with Anna’s help, I built a 21-foot sloop, the Halcyon, which gave our family many years of pleasure. Now in the hands of her third owner, the Halcyon still sails the Great South Bay. Anna and I were married in late 1948 and, over the next fifteen years, five children were born to us: Andrew, a senior scientist at the University of Chicago who studies meteorites to learn about stars and the early history of the solar system, lives in River Forest, Illinois; Martha Kumler, a private tutor of high school students, lives in Honeoye Falls, New York; Nancy Klemm, a homemaker and restorer of windows in old houses, lives in Webster Groves, Missouri; Roger, a mechanical technician working on the Relativistic Heavy Ion Collider at Brookhaven National Laboratory, lives in Center Moriches, New York; and Alan, an engineer with Boeing, lives in Seattle, Washington. Among them, they have given us eleven grandchildren. We have lived in the same house, in Blue Point, New York, for over fifty years.  My first act, on arriving at Brookhaven, was to report to the chairman of the Chemistry Department, Richard Dodson, and ask him what I was expected to do. To my surprise and delight, I was advised to go to the library, do some reading and choose a project of my own, whatever appealed to me. Thus began a long career of doing just what I wanted to do and getting paid for it. In the library, I read a 1948 review paper by H. R. Crane in *Reviews of Modern Physics* which led me to decide on an experiment in neutrino physics, a field in which little was known at the time, and which seemed well-suited to my background in physical chemistry.  In early experiments, I attempted to detect neutrinos from a reactor, using the chlorine-argon detection method suggested by Bruno Pontecorvo (in 1946). In this method, a 37Cl atom reacts with a neutrino to make an 37Ar atom. Argon is a noble gas and is easy to separate chemically from a large amount of chlorine-rich solvent. It is radioactive with a half-life of 35 days and can be counted with a gas-filled proportional counter. A first attempt, exposing a 1000-gallon tank of carbon tetrachloride at the Brookhaven Graphite Research Reactor, failed to detect any signal, as the neutrino flux at this reactor was too small to affect a target of this size. Furthermore, a reactor emits antineutrinos, and the Pontecorvo method only detects neutrinos. It was not known at that time that the two particles were not identical. Later, I built larger experiments, using one of the Savannah River reactors as the neutrino source. I eventually set a limit on the neutrino flux that was a factor of 20 below the antineutrino flux measured by [Reines](https://www.nobelprize.org/nobel_prizes/physics/laureates/1995/index.html) and Cowan in their elegant experiment that won Fred Reines his Nobel Prize.  Other early interests included the development, with Oliver Schaeffer, of a method of geological dating using 36Cl in surface rocks. With the later advent of accelerator mass spectrometry, this has become a useful tool in geochemistry, but our counting techniques were not sensitive enough to make the method work. We turned to measuring 36Cl in meteorites. Measuring the 36Cl radioactivity and the total accumulated decay product, 36Ar, in a meteorite allowed us to determine how long the meteorite had been exposed in space. Our interest in meteorite exposure ages continued for many years. We also worked on measuring cosmic-ray production of 37Ar and 39Ar in a variety of freshly fallen meteorites. Our greatest success in this work was with the Lost City meteorite. The track of this meteorite was photographed as it fell, allowing its orbit to be determined. Our measurement of radioactive argon isotopes allowed us to deduce the cosmic ray intensity gradient in the inner solar system. During the era of the moon landings, I was involved in measuring 37Ar, 39Ar, tritium and 222Rn in lunar rocks and in the lunar atmosphere (trapped in the rock boxes brought back by the astronauts). During processing of the Apollo 12 samples, one of the glove boxes in Houston leaked and I had the interesting experience of being quarantined with the astronauts and a few other unlucky scientists for two weeks until it was clear that we were not infected with any lunar diseases.  Following the Savannah River experiments, I began thinking about detecting neutrinos from the Sun. The first step was a pilot experiment located 2,300 feet underground in the Barberton Limestone Mine, near Akron, Ohio. Observing neutrinos from the Sun had the potential of testing the theory that the hydrogen-helium fusion reactions are the source of the Sun’s energy. In the 1950s, however, the proton-proton chain of reactions was believed to be the principal neutrino source, but this chain only emitted low-energy neutrinos, below the threshold of the chlorine-argon reaction.  A new measurement of the nuclear reaction 3He+4Heg7Be+g by Holmgren and Johnston in 1958, suggested that one of the terminal reactions in the proton-proton chain would produce energetic neutrinos which could be measured by the chlorine-argon radiochemical method. Encouraged by these developments, and with the support of the Brookhaven National Laboratory and the U.S. Atomic Energy Commission, I built a much larger experiment in the Homestake Gold Mine in Lead, South Dakota. The detector itself consisted of a 100,000-gallon tank filled with perchloroethylene, a solvent most commonly used for dry cleaning of clothing. The experiment was located nearly a mile underground, at the 4850 foot level of the mine. Initially, we observed no solar neutrino signal and expressed our results only as upper limits. Subsequent refinements in technique and, particularly, in counting methods, continued over the years, producing a solar neutrino signal approximately one-third of the expected flux from the standard solar model calculated by John Bahcall. This was the genesis of the so-called “solar neutrino problem”.  The solar neutrino problem caused great consternation among physicists and astrophysicists. My opinion in the early years was that something was wrong with the standard solar model; many physicists thought there was something wrong with my experiment. Years of measurements produced consistent answers and many tests showed that there were no problems with experimental procedures. Many distinguished physicists suggested explanations for the low solar neutrino flux that now seem fanciful. Trevor Pinch, a sociologist, made a study of how scientists responded to the solar neutrino problem. The disagreement between the measured solar neutrino flux and that predicted by the standard solar model was confirmed for energetic 8B neutrinos by the Kamiokande II experiment in the late 1980s and for the lower energy pp neutrinos by the gallium experiments GALLEX and SAGE in the middle 1990s. Only recently, observations at the Sudbury Neutrino Observatory (SNO) in the Inco Nickel Mine in Sudbury, Ontario, Canada, have indicated that, indeed, the total number of solar neutrinos emitted agrees with the standard solar model prediction, but that two-thirds of the neutrinos change in the course of their journey to the Earth into other flavors (m and t neutrinos), a phenomenon known as neutrino oscillation. Only electron neutrinos can be detected with the Cl-Ar radiochemical method.  I retired from Brookhaven in 1984, but wasn’t ready to give up measuring solar neutrinos, because I thought it important that the Homestake experiment measure the solar neutrino flux at the same time as new solar neutrino experiments. I transferred administration of the Homestake experiment to the University of Pennsylvania and have been a Research Professor there since that time. The experiment continued to measure the solar neutrino flux until the late 1990s, when the Homestake Mine ceased operating.  Meanwhile, to my surprise, a whole new field of neutrino physics has developed in directions I never imagined in the Homestake days. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0060 = RD** |
| **Interview** |  |
| Q9 | **Dr Giacconi, Dr Koshiba, Dr Raymond Davis and Andrew Davis, welcome to Stockholm and to this Nobel interview. Yesterday you received the Nobel Prize in Physics for this year 2002. The Nobel Prize gives you fame and it gives you money too. Do you feel happy? Dr Davis?** |
|  | Raymond Davis Jr: Same with me. My wife said, Why don’t we take all the children? and that will be the way we’ll spend our money. |
| Q21 | **I would like to say that the three of you share also another experience, I would say, of many years of work and trying to get and interbreed the signals from space. What you also share is the experience of not getting things right, maybe also the experience of getting accidents. Did you ever think about just quitting the field? Dr Giacconi?** |
|  | Riccardo Giacconi: Not at all. Can I expand a little on this? I used to be asked to give early morning breakfast talks to the donors and sponsors of the Hopkins University. I used to wake them in the morning asking them the question what does it mean if the Hubble telescope, upon being on the launch pad, blows up? What does it mean? My answer was that the point of it all was not this piece of metal and glass but what we were learning in doing it. Learning about our own field. Learning about being able to work together. At the end of this process, whether it blew up or not – of course much better if it didn’t – but this of course woke them up in the morning, this prospect. But basically we had changed and we had changed in such a way, we learn enormously, and it was easy to be the next one. Progress I think, we tend of overemphasise the stuff which is a result of the hard work, but there is a tremendous amount of learning that goes in doing this and in preparing for this and that will remain no matter what happens. No, I never doubted that I would go on. |
| Q21 | **Do you mean that you learn even more if things go wrong?** |
|  | Riccardo Giacconi: Sometimes. It’s not obvious that the lessons are then properly implemented but yes, you do.  Andrew Davis: Sometimes not such a fun way to learn.  Riccardo Giacconi: But it’s good because it’s a reality check. In many activities in human life there is no reality check. One can claim he is the greatest. Here Nature is a kind and abundant mother, but it’s also a hard taskmaster – that is if you don’t do it right, it just won’t happen. |
| Q2 | **Dr Davis, you were working for 30 years trying to measure neutrinos from the sun and never getting it right, I would say. How could you persist?** |
|  | Raymond Davis Jr: First off, it’s funding in science. You have to get enough money to do it right. I found that not too hard to do because the funding agencies have been very good to the laboratory that I live in and that’s a very important factor. |
| Q21 | **But you’ve also been heavily criticised, for example as a chemist by your colleagues physicists. How did you communicate?** |
|  | Raymond Davis Jr: They’re the same. Most of my experiments are really using chemicals and physics and … The other thing, I did a lot of work underground, so I wanted to find someone to dig a hole for me. Quite deep. You go to a mine and talk to the management and they say yes, you can do that. |
| Q4 | **You went to the gold mine in South Dakota?** |
|  | Raymond Davis Jr: Yes. In looking for neutrinos the way I did, you really need that. The company has to help and dig a little for us. |
| Q10 | **Have you ever visited your father in the mine?** |
|  | Andrew Davis: Yes, I visited many years ago. Before I answer that I thought I would say a little bit more also about the experiment. You were saying do you have difficulty perhaps being wrong. In the end of course it was not wrong. The experiment was right and the solar model was right, it was the neutrinos which in some sense are misbehaving or at least not behaving in the way people had expected them to. In the early days of your experiment and the challenge of physicists and theoreticians to you to say there must be something wrong with this step, that made your experiment better because you did more tests. You never think of giving up at that stage. You continue to believe, know from your own knowledge that it’s correct, and you try to follow up on that and make the experiment better. Do more tests to convince other people that you’re right. That’s how it works. In my case I visited the mine in 1968 when I was 18 years old. The whole family drove across the country and we visited the mine on the way. I’m the oldest of five children. I was the only one over 18 at the time so I was the only one permitted in the mine. My mother was not permitted in the mine because at that time women were not permitted in the mine, it was considered to be unlucky. Of course today there are women miners working, so things have changed. |
| Q9 | **I would like to continue a little about being a scientist. I would say that this is the thing, that you never quitted in spite of all the difficulties. This leads to the Nobel Prize somehow. Dr Giacconi, you mentioned before that one of your favourite lectures is the story of the white whale of Moby Dick. Is that a metaphor for science?** |
|  | Riccardo Giacconi: I think that we do science because we must and we want to. |
| Q22 | **What do you mean by must?** |
|  | Riccardo Giacconi: We can’t help it. We’re interested. We are lucky we hit upon a problem which really holds our own interest, and then we wish to carry out the work. One of the greatest difficulties is persuading somebody else which normally is required to give you funding, ‘somebody else’ means an agency, one or the other. For instance, in my case when I started off, I tried to convince the National Aeronautics Space Administration that I wanted to go and look at stars in x-rays and they thought this would not be interesting. Then I had to go to the Air Force and ask if they could be interested. They at the time were interested in studying the moon perhaps, so that’s why my proposal emphasised the moon because this was interesting for a sponsor. Basically, I was doing what I wanted to do. Then misfortune, criticism and so forth …  While you internalise a lot of this but basically you are doing what you want and that’s a fantastic reward, to be allowed to do in life what you want to do. It’s very difficult to reflect you, and adversity in a sense of what? Maybe one could give up, but it never occurred to me that I would. The adversity was just something that, particularly in space programme, you have to become accustomed to the fact that there may be failure. Do you stop? Not at all. You go on, you try to figure out a better way, you try to figure out some way to put in more disaster proof approaches and so on, but I haven’t heard anybody really involved that would quit. What makes people quit I think, is when they absolutely cannot convince their peers that what they want to do is rational. Therefore, they have great difficulties in getting support because of peer review system and so forth, particularly in the United States. Since we are sensitive people just like anybody else you tend to internalise that as a self-criticism. Maybe I don’t know what I’m doing, and at that point you’re lost. |
| Q17 | **What about the story of Moby Dick?** |
|  | Riccardo Giacconi: *Moby Dick* is only a funny story. When we launched the satellite there was a question of what should be the name and we had been very impressed with the name that the British gave to their own little satellite which was called Aerial. Aerial is nice because it gives you an idea of a live spirit, England. You don’t have to say more, you know it’s a British satellite. We were wondering what was the equivalent and some of my colleagues suggested Pequot. Now Pequot happens to be a Massachusetts Indian tribe. That was also the name of the ship of Captain Ahab and they saw some similarities between my behaviour and that captain Ahab. |
| Q21 | **Chasing the white whale?** |
|  | Riccardo Giacconi: Chasing a dream, notwithstanding the difficulties and Nature. The white whale is evil so you’re chasing evil. Then this name was vetoed because congress would have objected and the environmentalists about chasing whales and the congress, particularly of the United States, would have objected in chasing white whales which would have been a wasted enterprise. We never got called Pequot but I always have kept in my mind the Moby Dick story because it’s reliving the myth of Prometheus, it’s been picked up by Dante in Ulysses, in the *Divine Comedy*. Then I think Melville … That was just a retelling of the myth in puritanical New England. I found it interesting. |
| Q2 | **I have another question. You two especially, Dr Giacconi and Dr Koshiba, you are leaders of big experimental groups, there are several hundred people, or eve thousands maybe, working together. What is the input or the role of individual creativity in comparison with the work in science of the group?** |
|  | Masatoshi Koshiba: You ask me a very difficult question. I don’t know how to explain it. |
| Q18 | **How important is the individual in science?** |
|  | Masatoshi Koshiba: Individual is very important. There’s no doubt about it. At the same time if you are carrying out a large experiment you do also need a good collaboration spirit. The only thing you can achieve or acquire this is that you get trusted by your colleagues. How? I don’t know. You just trust your colleagues and they trust you. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0061** |
| **Biographical** | |  |  | | --- | --- | | Name: | KOSHIBA, Masatoshi. | | Date/Place of Birth: | September 19, 1926/Toyohashi city, Aichi Pref., Japan. | | Nationality: | Japanese. | | Marital status: | Married to Kyoko KATO on October 5, 1959, in Tokyo. | | Permanent address: | 4-11-7 Shimoigusa, Suginami, Tokyo 167-0022 Japan. Tel/Fax: 81-3-3396-6868, e-mail: mkoshiba@icepp.s.u-tokyo.ac.jp. | |  |  | | **Education** |  | | Mar. 1951: | Graduated from University of Tokyo, physics major. | | Apr. ’51 to Mar. ’53: | Graduate School, University of Tokyo. | | Sep. ’53 to Jun. ’55: | Graduate School, University of Rochester, Rochester, N.Y. Received Ph.D in physics: Thesis on Ultra-High- Energy Phenomena in Cosmic Rays. | |  |  | | **Academic appointments** | | | Jul. ’55 to Feb. ’58: | Research Associate, Department of Physics, University of Chicago. | | Mar. ’58 to Oct. ’63: | Associate Professor, Institute of Nuclear Study, University of Tokyo. | | Nov. ’59 to Aug. ’62: | on leave from the above, Senior Research Associate with the honorary rank of Associate Professor and the Acting Director, Laboratory of High Energy Physics and Cosmic Radiation, Department of Physics, University of Chicago. | | Nov. ’63 to Feb. ’70: | Associate Professor, Department of Physics, Faculty of Science, University of Tokyo. | | Mar. ’70 to Mar. ’87: | Professor in the same institution as above. June. ’74 to Mar. ’76; Director, Laboratory of High Energy Physics. Apr. ’76 to Mar. ’84; Director Laboratory for International Collaboration on Elementary Particle Physics. Apr. ’84 to Mar. ’87; Director, International Center for Elementary Particle Physics. | | Apr. ’87 to Aug. ’87: | Visiting Professor at DESY and University of Hamburg. | | Aug. ’87 to Mar. ’97: | Professor, Tokai University. Sep. ’87 to Aug. ’88; Guest Professor, CERN, Sep. ’89 to Dec. ’89; Distinguished Visiting Professor, University of Chicago. Feb. ’90; Regent Lecturer, University of California, Riverside, Jan. to Mar. ’94; Sherman Fairchild Distinguished Scholar, California Institute of Technology. Apr. ’95 to Mar. ’97; Director, Washington Liaison Office, Japan Society for Promotion of Science. Sep. ’96 to Mar. ’97; Distinguished Visiting Scholar, George Washington University. | | Jul. ’98 to Aug. ’99: | Alexander von Humboldt Preisträger staying at DESY in Hamburg, Max-Planck-Institut für Kernphysik in Heidelberg, and Max-Planck Institut für Extraterrestrische Physik in Garching. | | Present | Councilor, International Center for Elementary particle Physics, The University of Tokyo. | | **Important publications** | “Observation of a Neutrino Burst from the Supernova SN1987a” Phys. Rev. Lett., 58 (1987) 1490. “Results from One Thousand Days of Real Time, Directional Solar Neutrino Data; Kamiokande-II Collaboration”, Phys. Rev. Lett., 65 (1990) 1297. | |  |  |  |  |  | | --- | --- | | **Honors and Prizes** | | | Aug. ’85: | Das Grosse Verdienstskreuz from the President of Federal Republic of Germany. | | Dec. ’87: | The Nishina Prize from the Nishina Foundation. | | Jan. ’88: | The ASAHI Prize from the ASAHI Press. | | Nov. ’88: | The Order of Culture by the Japanese Government. | | Jun. ’89: | The Academy Award from the Academy of Japan. | | Jun. ’89: | The Bruno Rossi Award from the American Physical Society. | | Aug. ’96: | The Special Prize from the European Physical Society. | | Mar. ’97: | The Alexander von Humboldt Prize from the Humboldt Foundation. | | Jun. ’97: | The Fujiwara Prize from the Fujiwara Science Foundation. | | Nov. ’97: | The Order of Cultural Merit conferred by The Emperor of Japan in person. | | Jan. ’99: | The second ASAHI Prize from the ASAHI Press. | | Jan. ’99: | The Diploma di Perfezionamento honoris causa in Fisica from The Scuola Normale Superiole, Pisa, Italy. | | Jul. ’99: | Doktor der Naturwissenschaften ehrenhalber from Hamburg University. | | May ’00: | Rochester’s Distinguished Scholar Award from the University of Rochester. | | May ’00: | The Wolf Prize from The State President of Israel. | | Jun. ’00: | Citation by the Town of Kamioka. | | Aug. ’00: | Citation by the Governor of Gifu Prefecture. | | Apr. ’02: | Panofsky Prize from American Physical Society. | | Dec. ’02: | Nobel Prize in Physics. | | Dec. ’02: | Member, The Japan Academy. | |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0061 = MK** |
| **Interview** |  |
| Q9 | **Dr Giacconi, Dr Koshiba, Dr Raymond Davis and Andrew Davis, welcome to Stockholm and to this Nobel interview. Yesterday you received the Nobel Prize in Physics for this year 2002. The Nobel Prize gives you fame and it gives you money too. Do you feel happy? Dr Koshiba** |
|  | Masatoshi Koshiba: I felt the same. I also bought my granddaughters. They were happy and I was happy too. |
| Q21 | **Do you mean that you learn even more if things go wrong?** |
|  | Masatoshi Koshiba: It is true that I did have difficulties from time to time in my experiment. I never give up. I think about it over and over again until I find some solution. This is the way I have been doing my work. |
| Q2 | **You two especially, Dr Giacconi and Dr Koshiba, you are leaders of big experimental groups, there are several hundred people, or eve thousands maybe, working together. What is the input or the role of individual creativity in comparison with the work in science of the group?** |
|  | Masatoshi Koshiba: You ask me a very difficult question. I don’t know how to explain it. |
| Q18 | **How important is the individual in science?** |
|  | Masatoshi Koshiba: Individual is very important. There’s no doubt about it. At the same time if you are carrying out a large experiment you do also need a good collaboration spirit. The only thing you can achieve or acquire this is that you get trusted by your colleagues. How? I don’t know. You just trust your colleagues and they trust you. |
| Q18 | **There is this spirit in science that science goes its own way somehow, that scientists can be replaced. Something is in the air. If I don’t do this maybe somebody else will do it. What do you say about that Dr Koshiba?** |
|  | Masatoshi Koshiba: Science is a special type of recognition and by its nature it is a common asset of the entire human being. Therefore, if I didn’t build the detector and detect super nova neutrinos. Well, a super nova happens every 30 years in the average galaxy but in our own galaxy the previous one was more than 300 years ago. However eventually other people will think of detecting such neutrinos and then find it – there is no doubt about it. When it comes to different type of recognition, like when you are listening to the music you like, then you and the recognised subject there is no separation between the two. You and music are just one entity. You enjoy it, you don’t analyse it, you don’t study it. You just feel it, this is the type of recognition different from scientific recognition. For instance, I used to say if Mozart didn’t make this particular music there would be nobody who can produce the same thing. |
| Q15 | **This is another way of putting the question of what you are doing as a scientist. Are you studying the nature as it is or are you just testing your morals?** |
|  | Masatoshi Koshiba: You’re my spokesman.  Riccardo Giacconi: He does that to me. He did that to me once before. Then he’s very tough on me when I don’t give the right answer. I know this trick. I have seen this trick. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0062** |
| **Biographical** | In Memory of Marc Antonio Giacconi and dedicated to my wife, Mirella, and to our daughters, Anna and Guia.  **I. Early years** I was born in Genoa, Italy, on October 6, 1931, but I spent most of my life until 1956 in Milano. I was the only child. My mother, Elsa Canni Giacconi, was a teacher of Mathematics and Physics at the high school level. She was the co-author of many textbooks on geometry which were widely adopted in Italy. She held that God made geometry. My father, Antonio Giacconi, owned a small business. He had a knack of seeing historical developments clearly and to perceive when the King was naked. He was an anti-fascist and suffered for it. My mother and father were legally separated when I was eight years old.  I experienced World War II as a young teenager. I was sent to live with my aunts, Giulia and Elisa Canni, in Cremona following the 1942 bombings of Milano. The son of Giulia, Giovanni Benini, of my same age, became the only brother I ever had. I had access to a large library, including classics, and I read copiously.  I returned to Milano in 1944 and lived there through 1956. My schooling was not completely conventional. I went to a German kindergarten, then first year elementary school in a Military College, then third year in Genoa, then again third year in Milano. I cut school frequently in Genoa and I was a discipline problem in Milano. I had my best learning in Cremona. I jumped last year of high school directly to the University of Milano. I obtained the doctorate in Physics in four years. Outside of school I loved climbing, hiking, and skiing. I traveled throughout Europe and I enlisted for two weeks as a trimmer on an English trawler out of Grimsby.  **II. Education** Up to and including university my interaction with the educational system was always difficult. In high school I loved to point out the mistakes by our mathematics teachers. At the university, I had difficulties following the heavy burden of lectures although I was able to pass all tests with relatively high grades. My salvation was to start doing research in the very first year of university. At the University of Milano there was at the time an active group of cosmic ray researchers interested in studying muons, lambda particles, and proton interactions. A very kind and bright young professor, Antonio Mura, put me to work doing literature searches and summaries, while Carlo Succi put me in the laboratory building power supplies with surplus U.S. parts and building and operating cloud chambers.  This training was in stark contrast to the pedantic approach of many of the older teachers. The lectures in physics, chemistry, physical chemistry, and theory were of little use to me. I still loved geometry, analysis, and physics as I saw it from a researcher’s point of view.  I did my thesis work on the development of nuclear interactions by protons in the lead plates of a cloud chamber at the laboratory of Testa Grigia (3500 m). It took me two years to collect 80 proton interactions. I prepared a modest thesis confirming [Fermi](https://www.nobelprize.org/nobel_prizes/physics/laureates/1938/index.html)‘s model and I was finally out of formal schooling. I was hired as an assistant professor in the Physics Department and just before this time I met Giuseppe Occhialini. He was an extraordinary figure in Milano. His work was at an extremely high level and widely recognized internationally.  To my knowledge he never gave a lecture. He took a liking to me and I to him. I was able to follow his multiplexed way of thinking and talking, and I was able to moderate some of his flights of imagination by sober use of first principles. He approved of my choice of working with cloud chambers, though most of the young people worked with emulsions, with the remark that at least I would learn plumbing. He suggested I go the United States to work with R.W. Thompson for whom he and I had the most profound esteem. I obtained a Fulbright Fellowship and sailed for the U.S. in 1956 and have lived here until now, except for a seven-year period (1992-1999) in Munich.  **III. Fellowships** I spent 1956 to 1958 in Bloomington, Indiana, working on the analysis of data previously obtained by Thompson and on the construction of a new and bigger cloud chamber for cosmic ray research. Thompson was a painstaking experimentalist as well as brilliant in data analysis and theory. He was the closest in the U.S. to the ideal of the Italian school of physics as embodied by Fermi, who was both a great experimentalist and theoretical thinker. Thompson had committed a blunder in research when young and this fact haunted him for the rest of his life. He never received the recognition he deserved for his discovery of the qo mass and this troubled him greatly.  It became quickly apparent that Thompson’s group was not the place for me. The search for the anti-lambda particle was unsuccessful and the new cloud chamber would take 10 years to build. I moved to Princeton University in 1958 to work in G. Reynolds’ laboratory. There I conducted research in m mesons and carried out an unsuccessful search for a new type of particle whose discovery had been claimed by Russian scientists. This search was a collaboration between Fred Hendel (a senior Austrian scientist), Herbert Gursky (a post doc), and myself. We built equipment, worked like fiends, analyzed data, and declared failure. In the meanwhile I had learned a lot about scintillation counters and image intensifiers to be used for elementary particle research at the then-envisaged Princeton-Penn accelerator.  This put me in contact with the MIT group led by Herbert Bridge. Also with American Science and Engineering (AS&E), led by Martin Annis, an excosmic ray physicist. Both Bridge and Annis had been students of Bruno Rossi of MIT, who was chairman of the Board of AS&E.  My fellowship at Princeton expired in 1959. I went to visit Occhialini at CERN. He seemed to be in one of his emotional downturns seeing conspirators everywhere. CERN felt like an impersonal huge machine and offered me no prospects. I was therefore quite happy to receive an offer from AS&E to initiate for the 28-man corporation a program of space sciences. I joined the Corporation in September 1959 and I started a serious career in science.  **IV. Outlook and personal considerations** Cosmic ray research as a tool to study elementary particles was ending just as I was doing my thesis. The work by Thompson was destined to a dead end. The MIT cosmic ray research group led by Rossi had decided to abandon almost completely cosmic ray research and to start utilizing space to study plasmas and gamma rays from celestial objects. In particular, W. Kraushaar and George Clark carried out one of the first surveys in gamma rays.  For me the problem was much more personal. I felt I had not learned any useful skills while working in Milano, Indiana, and Princeton. I desperately wanted some kind of permanent position where I could learn a trade and, moreover, I felt I owed this to my family. I had married Mirella, whom I had known in Milano since the age of 16. Guia and Anna, our two daughters, needed stability and a home. The influence of Mirella on my life has been greater than that of any other person. She brought love, calm, and stability where none had existed before. She created a home for us full of beauty and tenderness. She is brighter than I am, translated many books for the MIT press, and has an uncompromising view of reality. Through thick and thin we are still together today.  **V. Early years at AS&E** Given the task of creating a space program for the company, I started discussions with George Clark, an MIT professor and consultant and shareholder in AS&E. We discussed the possibility of searching for the ratio between alpha particles and protons in the trapped radiation belts just recently discovered by Van Allen. Another suggestion came from Bruno Rossi at a party in his house. He reported discussions at the Space Science Board of the National Academy of Sciences about the potential for x-ray astronomy and suggested AS&E might undertake it. I have always been grateful for that initial suggestion.  1959 to 1962 were among the most productive years of my life. I was involved in classified research: 19 rocket payloads, six satellite payloads, one entire satellite, and an aircraft payload, as well as four rocket payloads for geophysical research. Also in this period I produced instruments for alpha – proton ratio experiments, as well as the initial development of the x-ray telescope and the first few flights of rocket payloads for x-ray astronomy. This was a tremendous challenge. My group at AS&E went from 3 people (myself and two technicians) to 70 people in two and a half years. Frank Paolini and Herbert Gursky joined the group. Frank was a great instrumentalist. Without his contributions the successful 1962 x-ray payload could not have happened. Herbert Gursky, though quite valiant in the laboratory and in the field (he actually launched the June 12, 1962, rocket), was always more of a natural scientist. The fundamental steps in the dawn of x-ray astronomy remain in my mind: the first brief review, theoretical and experimental, of x-ray astronomy (1960), the invention of the x-ray telescope (1960), and the 1963 plan for xray astronomy by Gursky and me. This plan laid out a program of experiments which went from rockets to UHURU, “Einstein,” and Chandra. We thought then it could be done in five years, but it was not accomplished until the year 2000.  Much of the history of the development of x-ray astronomy has been told by Richard Hirsh, by Wally Tucker and me, and by others in books and articles and I will not repeat it here. But my scientific life took an unexpected turn after 1980 and I will devote some space to my growing interest in the direction of scientific enterprises outside of x-ray astronomy.  **VI. Center for astrophysics** I had decided by 1973 that I actually loved astronomy, at least as I was practicing it. The success of UHURU gave the members of my group (Gorenstein, Gursky, Kellog, Murray, Schreier, Tananbaum, Tucker, and Van Speybroeck) a feeling of awe and gratitude at the richness and beauty of nature. We had won the NASA contract to build the Large Orbiting X-Ray Observatory (1970-1973). The contract was canceled due to cost overruns in the NASA Viking Program in 1973 and restarted the same year for what became “Einstein,” at one-half the size. We felt that we wanted to operate “Einstein” as a national observatory open to astronomers of all disciplines. AS&E did not seem to be the right place to do this. Thus the move to Harvard (where I was named a full professor) with eight of my group. We were looking for a closer involvement with the rest of astronomy and we thought the Center for Astrophysics (CfA) might provide the opportunity.  As it turned out, hardly anybody at CfA cared about what we were doing and there was less support for research than we had at AS&E. (In retrospect I have often wondered if we could have been successful at all had we started our work at Harvard). When I proposed to NASA an x-ray astronomy institute to direct the construction and operation of the “Einstein” successor in 1976 (then called AXAF and now Chandra), there was little support from CfA itself, where the institute would have been located. The proposal for AXAF was written by Harvey Tananbaum and me in 1976 and started the process which led to Chandra. Harvey was the project scientist for UHURU, the scientific manager for “Einstein,” and took on the leadership role in Chandra. His contributions to x-ray astronomy have never been sufficiently recognized. It took perseverance over almost 20 years to turn Chandra into reality. For me the delay between conception and execution was becoming too long. Also, after the glory of discoveries with UHURU, “Einstein” seemed relatively tame. I was ready for a change, which came with the unsought offer by Margaret Burbidge to become the first director of the Space Telescope Science Institute (STScI).  **VII. Hubble space telescope** The effort at CfA to make x-ray astronomy useful to astronomers of all disciplines spurred us to provide the user with calibrated data out of a software pipeline. The development of end-to-end data management systems stems from that beginning. As the first Director of STScI, I was able to transfer my methodology for doing science in the planning and execution of science operations of Hubble.  Every aspect of the mission was examined by myself and the distinguished staff we were able to attract to the Institute. Ethan Schreier, my comrade of UHURU and “Einstein” days, and Rodger Doxey brought the physics-oriented mentality of x-ray astronomers to the new project.  Although we were quite ignorant in optical astronomy, we quickly found that operational planning for Hubble was a disaster. Guide stars could not be found on the fly as planned, the telescope could not point to planets, simultaneous reception of data from two instruments could not be achieved, and on and on.  Even more serious was the lack of tools to schedule the complex operations of Hubble and the absence of a data reduction system capable of ingesting the very high data rates. Both were accomplished by STScI. This meant developing models for the instrument defined by parameters measured on the ground and continuously verified in orbit, determining modes of instrument operations, and defining calibration routines. We also developed a software pipeline capable of analyzing data in real time and constructed an archive of calibrated data suitable for reuse by scientists other than those who built the instruments or used the Hubble through the competitive research program. We instituted, with NASA’s consent, data analysis grants and the Hubble Fellowship program. We developed an outstanding outreach program to reach the general public as well as colleagues and students.  We took responsibility for Hubble beyond the construction of glass and metal to turn it into an outstanding scientific tool.  It would be impossible to credit all of the scientists involved in creating and running the STScI and to name only a few seems unfair to the others. Many of them have continued at the Institute until today and many have gone on to prestigious positions at outstanding research institutions.  In 1991 my son Marc died in an automobile accident. STScI, Hubble, and Baltimore were continued and painful reminders of devastating grief. When the offer came to become Director General of the European Southern Observatory (ESO), Mirella and I jumped at the opportunity.  **VIII. ESO and the VLT** When I joined ESO in January 1993, ESO was beginning to execute the Very Large Telescope (VLT) program. Quite a bit of progress had already occurred but the program was of such a size as to equal eight times the yearly budget of ESO and was 30 times larger than the previously built New Technology Telescope.  Massimo Tarenghi, an excellent scientist and an energetic manager, needed support from the rest of ESO to carry the project through. To help him in his task we had to fully reorganize ESO and introduce modern management techniques suitable for large programs. This was reasonably straightforward. More difficult was the introduction at ESO of the concept of a single observatory in which quality rather than quantity mattered and the introduction of end-toend software systems, calibration, and archiving, whose validity was proven on Hubble but had not yet been used in ground-based optical astronomy.  VLT became a machine to do science in which efficiency of operations and ability to use the data were as important as telescope and instrumentation performance.  Keck had the fortune of being the first to provide a 10 meter class telescope and reaped the early reward. But in a certain sense, it is operated as an old telescope for use by a restricted community. The success of VLT has placed European optical ground-based astronomy in the position to compete worldwide.  Toward the end of my stay at ESO we initiated a new cooperative program with the United States and Canada to build a large submillimeter and millimeter wave array of antennas to be placed in the Atacama Desert in Northern Chile. At the expiration of my tenure as Director General in 1999, I returned to the United States where now, as President of Associated Universities, Inc., I am working with the National Radio Astronomy Observatory as the Executive for the North American portion of the project, called the Atacama Large Millimeter Array (ALMA). The same principles and the same methods used in “Einstein,” Hubble, VLT, and Chandra are being utilized. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0062 = RC** |
| **Interview** |  |
| Q9 | **Dr Giacconi, Dr Koshiba, Dr Raymond Davis and Andrew Davis, welcome to Stockholm and to this Nobel interview. Yesterday you received the Nobel Prize in Physics for this year 2002. The Nobel Prize gives you fame and it gives you money too. Do you feel happy? Dr Giacconi.** |
|  | Riccardo Giacconi: All I can tell you is we had a wonderful time yesterday and at 7.30 in the morning I was woken up by the grandchildren before they had breakfast and then after they had breakfast because they were leaving. Having them here was perhaps one of the very happy things I could do for them and made me happy. |
| Q12 | **The family makes you happy?** |
|  | Riccardo Giacconi: Very much so. |
| Q21 | **I would like to say that the three of you share also another experience, I would say, of many years of work and trying to get and interbreed the signals from space. What you also share is the experience of not getting things right, maybe also the experience of getting accidents. Did you ever think about just quitting the field? Dr Giacconi?** |
|  | Riccardo Giacconi: Not at all. Can I expand a little on this? I used to be asked to give early morning breakfast talks to the donors and sponsors of the Hopkins University. I used to wake them in the morning asking them the question what does it mean if the Hubble telescope, upon being on the launch pad, blows up? What does it mean? My answer was that the point of it all was not this piece of metal and glass but what we were learning in doing it. Learning about our own field. Learning about being able to work together. At the end of this process, whether it blew up or not – of course much better if it didn’t – but this of course woke them up in the morning, this prospect. But basically we had changed and we had changed in such a way, we learn enormously, and it was easy to be the next one. Progress I think, we tend of overemphasise the stuff which is a result of the hard work, but there is a tremendous amount of learning that goes in doing this and in preparing for this and that will remain no matter what happens. No, I never doubted that I would go on. |
| Q21 | **Do you mean that you learn even more if things go wrong?** |
|  | Riccardo Giacconi: Sometimes. It’s not obvious that the lessons are then properly implemented but yes, you do.  Andrew Davis: Sometimes not such a fun way to learn.  Riccardo Giacconi: But it’s good because it’s a reality check. In many activities in human life there is no reality check. One can claim he is the greatest. Here Nature is a kind and abundant mother, but it’s also a hard taskmaster – that is if you don’t do it right, it just won’t happen. |
| Q9 | **I would like to continue a little about being a scientist. I would say that this is the thing, that you never quitted in spite of all the difficulties. This leads to the Nobel Prize somehow. Dr Giacconi, you mentioned before that one of your favourite lectures is the story of the white whale of Moby Dick. Is that a metaphor for science?** |
|  | Riccardo Giacconi: I think that we do science because we must and we want to. |
| Q22 | **What do you mean by must?** |
|  | Riccardo Giacconi: We can’t help it. We’re interested. We are lucky we hit upon a problem which really holds our own interest, and then we wish to carry out the work. One of the greatest difficulties is persuading somebody else which normally is required to give you funding, ‘somebody else’ means an agency, one or the other. For instance, in my case when I started off, I tried to convince the National Aeronautics Space Administration that I wanted to go and look at stars in x-rays and they thought this would not be interesting. Then I had to go to the Air Force and ask if they could be interested. They at the time were interested in studying the moon perhaps, so that’s why my proposal emphasised the moon because this was interesting for a sponsor. Basically, I was doing what I wanted to do. Then misfortune, criticism and so forth …  While you internalise a lot of this but basically you are doing what you want and that’s a fantastic reward, to be allowed to do in life what you want to do. It’s very difficult to reflect you, and adversity in a sense of what? Maybe one could give up, but it never occurred to me that I would. The adversity was just something that, particularly in space programme, you have to become accustomed to the fact that there may be failure. Do you stop? Not at all. You go on, you try to figure out a better way, you try to figure out some way to put in more disaster proof approaches and so on, but I haven’t heard anybody really involved that would quit. What makes people quit I think, is when they absolutely cannot convince their peers that what they want to do is rational. Therefore, they have great difficulties in getting support because of peer review system and so forth, particularly in the United States. Since we are sensitive people just like anybody else you tend to internalise that as a self-criticism. Maybe I don’t know what I’m doing, and at that point you’re lost. |
| Q17 | **What about the story of Moby Dick?** |
|  | Riccardo Giacconi: *Moby Dick* is only a funny story. When we launched the satellite there was a question of what should be the name and we had been very impressed with the name that the British gave to their own little satellite which was called Aerial. Aerial is nice because it gives you an idea of a live spirit, England. You don’t have to say more, you know it’s a British satellite. We were wondering what was the equivalent and some of my colleagues suggested Pequot. Now Pequot happens to be a Massachusetts Indian tribe. That was also the name of the ship of Captain Ahab and they saw some similarities between my behaviour and that captain Ahab. |
| Q21 | **Chasing the white whale?** |
|  | Riccardo Giacconi: Chasing a dream, notwithstanding the difficulties and Nature. The white whale is evil so you’re chasing evil. Then this name was vetoed because congress would have objected and the environmentalists about chasing whales and the congress, particularly of the United States, would have objected in chasing white whales which would have been a wasted enterprise. We never got called Pequot but I always have kept in my mind the Moby Dick story because it’s reliving the myth of Prometheus, it’s been picked up by Dante in Ulysses, in the *Divine Comedy*. Then I think Melville … That was just a retelling of the myth in puritanical New England. I found it interesting. |
| Q2 | **What would you say Dr Giacconi? You have also a big collective to lead.** |
|  | Riccardo Giacconi: We are after all working in the United States so some of the things I’m going to say are fairly common, at least in management experience in the United States. One thing is I think that when you are involved in big enterprises it becomes a little bit different than being in a faculty. If you ever been to a faculty meeting there are 30 people and each one has his own ideas, and we rarely agree on anything. Therefore, if you are asking for a recipe for action this is very tricky. You can’t get action. When you are involved in major programmes and particularly in space where there is a deadline the problem is not only to have creativity by individuals but have that combined with discipline. You can’t have discipline. Discipline in science doesn’t work. These are creative minds they can’t be told what to do. What I found works very well is we work like a band of brothers. Meaning that somebody is the recognised leader, but he is the leader because he has ideas and because he’s contributing leadership. If he doesn’t somebody else in the wolf pack will take over. It’s not quite as wolf type image that I suggest because for instance I can say with fair conscience that the Hubble space telescope institute, which was created from the ground, no major decision of any kind, technical or managerial was taken without reaching consensus. I don’t mean vote, I mean consensus. We would talk until we were blue in the face so that we could agree. However, once we had agreed on a technical approach or managerial approach then there was an implied pact that we would carry out this decision even if we had disagreed originally with all our might, in all our loyalty.  In order to do any of this you need to achieve a level of communication and truth. That is hard to achieve. I was very accustomed as a leader of this group of scientists being told I was stupid, and that was perfectly OK. In fact, the only real bad thing was if somebody thought so and didn’t tell me, because that would have been really dangerous to our progress. I was able to do that when I was young at American Science and Engineering where we did the discovery work and then the work on UHURU, at Harvard and then space telescope. Later on I became a little bit more imperious because I’m getting a little impatient, I didn’t have the time to go through all of this and building up this through a period of education. That’s the way to do it. I see no conflict between feeling fully contributing and allowed to create as an individual and to recognise that you need to work cooperatively. When that works those are moments in human life which are very rare. There is a great sense of privilege in belonging to such a group. The young generals of Alexander must have felt that way. |
| Q2 | **Do you agree with that Dr Giacconi? That individual scientists don’t matter as much as individual artists.** |
|  | Riccardo Giacconi: We have had a running discussion with Professor Koshiba who obviously has thought about this longer perhaps than I have, but I do not. I do not agree to this, but I don’t have a very good way to express it, so I can only express it by stating some things. For example, let me think a moment of Kepler, the astronomer. Kepler had the following problem that he was thinking about the motion of the planets. He was very careful in interpreting Tycho Brahe data and he tried to express regular laws. However, there was a fundamental problem in his mind. He wasn’t obvious how the planets would be dragged in the sky according to these laws. What was the actual mechanism? There was an Italian scientist not quite as famous who had solved the problem by saying that the planets were inhabited by living beings who would fly like swarms flying around the sun in formation. Now Kepler didn’t accept that at all, thank God. However, he had no idea of gravitational laws. For him to say that there was a law of distance whereby the sun could influence the motion of the planet was like magic. It was worse magic than what he suggested which was that there would be magnetic brooms pushing the planets along in their orbit. At least it was a physical explanation.  Now the jump of going to that from that to recognising the physical laws that regulates this motion to me is a creative jump. The question that I really have been asking myself more since I’ve heard this concern of Professor Koshiba was the following. It is quite true that presumably we are living in one world and if we knew all about it, all aspects of it, then we would have to agree how it works. However, it’s not clear to me that for instance during the period in which you have no idea at all of probability, quantum theory, you really are conceiving the world in the same way as you would if you have a different theoretical outlook. Whether that would be part of reality that would remain hidden, or unimportant, or not interpreted to you. All I’m saying is very simple. Does language determine our thinking in a fundamental way because then certain different discoveries can be interpreted in a linear way? We are progressing one on top of the other but basically we are within a confined civilisation, a certain type of language. Is there some other aspect that we can never appreciate if we continue in that language? That’s why it would be interesting to find an alien civilisation. I’m sure that it would work in the same world. Would they really conceive of it in the same manner? I don’t know this. I want to think about it more, but it seems to me that maybe not.  Finally, one should say that I did feel as a creative scientist all my life and to be told that I’m not I don’t like it, psychologically, but he may still be right. But I was trying to put enough of a thought about it to say, well this example I’m using of the aliens is that obviously the aliens should ultimately come to the same conclusion. Now they could have had infra-red vision, they could have had x-ray vision or whatever. At that point would they come up with the same world view that we have? I don’t know. I can’t answer. |
| Q15 | **Are you studying the nature as it is or are you just testing your morals?** |
|  | Masatoshi Koshiba: You’re my spokesman.  Riccardo Giacconi: He does that to me. He did that to me once before. Then he’s very tough on me when I don’t give the right answer. I know this trick. I have seen this trick. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0063** |
| **Biographical** | I was born in Palo Alto, California in 1961. My parents were completing graduate degrees at Stanford. Two years later we moved to Cambridge, Massachusetts, the city I consider to be my hometown. My father was a professor of civil engineering at MIT, and my mother taught high school English. The family, including my younger brother and sister, accompanied my father on sabbatical years to Berkeley, California and Lisbon, Portugal. These were wonderful experiences for me and no doubt they are in part to blame for my lifelong love of travel.  My mother taught me to read when I was still quite young, and at least in my memory I passed the majority of my childhood reading. My head was always bubbling over with facts and it seems to me this had little to do with my paying close attention in school and more to do with my voracious and omnivorous reading habits. Indeed in elementary school I often kept my desktop slightly open and affected an alert-looking pose that still allowed me to peek into the desk where I kept open my latest book, as interesting as it was irrelevant to the academic subject at hand. Every so often my hand slipped surreptitiously into the desk to turn the page. In the intervening three decades I have spent plenty of time lecturing in front of a classroom of my own, and in retrospect I realize I was seldom fooling anyone. Most of my teachers probably found I made less trouble if they let me read.  Some nights, especially in the early summer when the late evening light kept my west-facing bedroom from getting very dark, I had trouble falling asleep at my appointed bedtime. My parents probably felt that reading me a story was a little redundant, but on occasion my father would come in and suggest to me a “problem” to think about. Stewing over these problems was supposed to help me go to sleep. It never did that, but it did get me in the lifelong habit of thinking about technical issues at all sorts of random moments in my daily life, and not only (or even primarily) during scheduled “thinking time.” Some of my father’s bedtime problems I now recognize as classic physics brainteasers. A man driving a van full of beehives comes to a bridge. The combined weight of the truck, bees, and beehives barely exceeds the safety limit of the bridge. The driver comes up with the idea of banging on the side of the van, so that all the bees swarm out of the hive and fly around in the back of the van. Does the fact that the bees are now all airborne make the truck light enough to safely cross the bridge? Other problems were exercises in mental estimation. If you hold out your thumb, at arms length, you can just about cover the moon with your thumb. The moon is a quarter of a million miles away. How big is it?  The 1970s, the decade of my teenage years, was a transitional period in American youth culture. It was already past the peak of the era when science-minded kids built radios, model airplanes, rockets – things of that sort. But it was certainly well before the heyday of computers and video games. I was partly old-fashioned and partly modern. I certainly remember building model rockets. It was fun to watch the rocket blast into the air, suspenseful to wonder if the parachute would open to bring the rocket safely back. I didn’t really enjoy the assembling the model kits very much, and usually I couldn’t be bothered to paint the thing, or even to stick on the decals. A more vivid memory for me was designing a model of my own. Besides the store-bought kits, the Estes Model Rocketry company in those days also sold by mail various sizes of cardboard tubing, balsa-wood sheets, nosecones, and gun-powder rocket engines. Estes also published a terrific little booklet full of quantitative design tips. A key issue in rocket design is to make sure that the center of mass is well forward from the fins, lest the rocket be aerodynamically unstable. My father showed me how (after a candidate design was laid out on graph paper) to calculate the center of mass of the assembly based on the masses and distribution of the component parts. I designed an over-sized, under-powered, clunky sort of rocket. I didn’t care how high it would go – I wanted it to rise slowly enough that I could watch to see if its orientation wobbled during the flight. On its maiden flight it lifted off the ground with all the ponderousness of a Saturn V, rising steady and true but rolling slightly about its long axis (had I glued the fins on crooked?) as it gained altitude. The engine burn completed, and then the parachute popped and my creation drifted with the wind to land on the roof of a schoolhouse. My parents suggested I go on Monday morning to ask the school’s janitor to retrieve my rocket, but this I was too shy to do.  My freshman year of high school I joined the chess and math clubs. The clubs met after school in the computer-instruction classroom, under the loose supervision of a genial polymath with the unlikely name of Mr. Wisdom. Between rounds of speed chess I read enough of a programming manual to teach myself to write programs on the school’s DEC mainframe in the language Basic. For several months I was really captivated with this new activity. The exercises in the Basic manual seemed pretty tedious so I invented a few projects for myself, including a program to generate word puzzles for the math club newsletter. After a semester or so, my infatuation with computers burnt out as quickly as it had begun. Not enough substance there to sustain interest, I felt. This episode is probably the basis for my lifelong distaste for “computers for computers’ sake” – it’s a kids’ game, I think. A second legacy of my brief childhood infatuation with computers was a life-long secret preference for programming in Basic, although during my years of apprenticeship in other scientists’ labs I was compelled to learn both C and Fortran. When eventually I had the opportunity to establish a lab of my own, one of my first acts as a young principal investigator was to write a program to output a precisely timed sequence of electronic pulses to control the lasers and magnetic fields in what was to become the first successful Bose-Einstein condensation apparatus. Of course, I wrote the program in Basic!  Some of my classes in high school were pretty interesting and I benefited from having several very intelligent and inspiring teachers. Among these were John Samp, a physics teacher, and JoAnn Walther, an English teacher. After the Nobel Prize announcement, I got back in touch with them and was delighted to learn that they are still (as of 2001) teaching at my old high school.  Just before my final year of high school, my brother, sister and I moved with my mother to San Francisco. I spent my last year of high school there, at Lowell High School. Lowell High was a so-called “magnet school,” drawing academically inclined students from all over the city. My fellow students there were very smart, but the really novel thing was that they actually seemed to put a lot of effort into their school work. By the end of my first semester there, I began to get into that habit as well. Something else new at Lowell was that it was “cool” to excel at school, at least among the Asian kids with whom I mostly hung out. Without the transitional year at Lowell, my first year as an undergraduate at Stanford would have been a horrible shock.  The truth is that first year at Stanford was a shock anyway, although not for academic reasons. Everyone was beautiful, self-confident, self-satisfied. Later I moved into a student-run, co-op house and felt more at home in that “alternative” residential atmosphere. It was there I met my future wife, Celeste Landry, although our lives took us separate ways for many years and we were not to marry until more than ten years later.  My first job in physics was as a “scanner” at the Stanford Linear Accelerator Center. As a freshman I needed to earn a little money and I was looking for a way to learn about science at the same time. The advertised hourly wage was unusually high for a campus job, which should have been a danger sign. On my first day on the job, a postdoc spent 30 minutes or so showing me how to call up symbolic representations of an endless series of archived detector “events,” for display on a graphics terminal. There was a particular kind of rare event I was to look for – I can’t remember now exactly what it was – characterized by a certain precise number of photons, of muons, etc. The postdoc explained to me how to distinguish different sorts of particles on the basis of the amounts of energy they deposited in various sorts of detectors, spark chambers, calorimeters, what have you. When I recognized a promising event, I was to flag it by pressing a certain key on the terminal, and, “pop”, another event would come up on the screen for my consideration. After my 30-minute training period was up, the educational part of the job (and incidentally the part of the job involving any human interaction) was essentially finished. I could come in whenever I wanted, work as many hours as I wanted. The money was great but towards the end of the third mind-numbing afternoon of staring at the graphics terminal I realized my sanity was at risk. I decided to quit right then and there, and wandered around the data center looking for someone to notify of my decision. There were plenty of people buzzing around the room, but no one looked familiar. It occurred to me that, after the original 30-minute training period, I had never again seen the postdoc who had taught me the tricks of the high-energy physics trade. Finally I just wandered out of the building, never to return. Over the course of my three afternoons I had worked my way through hundreds of stored events, and flagged four of them as promising candidates. Is it possible those four events eventually got my postdoc a nice assistant professor position at the University of Chicago? One can always wonder!  Meanwhile, I was taking freshman physics with Blas Cabrera, then only in his second year as a professor, and eventually I worked up the nerve to approach him after class. Did he have a position in his lab for an undergraduate? He did! I started off building some data acquisition electronics for a scanning magnetometer, sharing a lab bench with a fellow undergraduate, Charlie Marcus. For the remainder of my years at Stanford I worked afternoons and summers for low-temperature physics groups on campus. I really enjoyed this experience, and it was these jobs, more than anything else, that persuaded me to pursue a career in scientific research.  Roughly halfway through my undergraduate years, I began to worry that my future was choosing me, instead of the other way around. Time seemed to be accelerating. Had I really already completed nearly two years of college? I was taking lots of science classes, spending lots more time in physics labs, and was doing well there. In a little more than a year, the most natural thing for me to do would be to apply to physics graduate school. Doubtless I would be admitted, and then – zoom – off I would go into a pre-defined future as a scientific researcher. It seemed somehow too pat, too canned. When was it that I actually got to decide the course of my own future life? Perhaps I would be happier pursuing something a little more explicitly intellectual than physics. Maybe a return to my first love, of books, was in order. I had been studying Mandarin Chinese for a quarter or two. I took a great interest in politics. Couldn’t I put together some sort of future with all that in mind? The first thing I needed was to buy a little time to think it over, lest I be out the door with a degree before I knew what had happened. A Stanford program called Volunteers in Asia seemed to offer me that time. So the summer following my second year of college, I went off to the YMCA in Taichung, Taiwan, to teach conversational English. The work was pleasant and not very hard; I had a lot of time to read and to think and to study Chinese. Six months after that, I left Taiwan, first for Hong Kong and then for mainland China, where I spent another three months studying still more Chinese and generally kicking around the country.  Travel provided many interesting experiences, but perhaps the most useful lesson I learned was that I really had no proficiency for learning the thousands of characters of the written Chinese language. It is not that my memory is generally poor. I am very good at remembering the lyrics to popular songs. A single line from a popular song probably represents about as many bits of information as a single Chinese character. If I could have displaced the one set of information with the other, I would have had no problem storing in my brain the 5000 characters necessary for advanced Chinese literacy. As it was, I realized choosing the study of Chinese literature as my life’s work was probably a mistake. Conversely, I came to realize that being good at something is hardly a reason to avoid doing it.  I returned to Stanford with much more of a sense of purpose. I continued to take elective courses in such topics as poetry and political science, but I allowed myself to enjoy my physics courses and my work in the labs. My last two years at Stanford I worked for the gyroscope-based general relativity experiment of Francis Everett and co-workers, with my final year’s work growing into an honors project. Everett was the titular advisor of my honors thesis, but I worked more closely with John Turneaure, a research professor. The gyroscope relativity experiment needed data on the low-temperature adsorption properties of helium on various technical materials such as OFHC copper, fused quartz and so on. I inherited a recently abandoned apparatus and was told to extend the range of temperatures and go beyond monolayer coverage. I went to see John for advice as needed, but other than that I was left to work alone. No doubt I wasted a lot of time reinventing the wheel, but I loved the sensation of “having my own lab.”  For graduate school I returned to Cambridge. In the spring of 1985, shopping around for a graduate school and a research project, I met Dave Pritchard at MIT. He spun me a wonderful yarn: by very precisely measuring the mass difference between the helium-3 and tritium, one can determine the total amount of energy released in the beta decay of tritium. Combine this mass measurement with a determination (no big deal, Dave implied) of the endpoint of the beta-ray spectrum, and one has measured the rest mass of the electron neutrino! There were hints, in those days, that the neutrino might have a rest mass as large as ten eV, a value of cosmological significance. Think of it, Dave said: working with two or three other students on a bench-top experiment, one might just find the missing dark mass and close the universe! It sounded awfully good to me. It still does, as I retell it today.  Thus in the fall of 1985 I joined Dave’s single-ion cyclotron resonance experiment. The idea was to trap a single ion in a Penning trap, measure its cyclotron frequency to great accuracy, then swap in a different species of ion and do a comparison measurement. The ratio of cyclotron frequencies should be just the inverse of the ratio of masses. Two graduate students, Robert Weisskoff and Bob Flanagan, and a postdoc, Greg Lafyatis, had the apparatus designed and largely assembled by the time I arrived, but we didn’t succeed in trapping and detecting single ions until three years later. The work got to be pretty frustrating and when at last one morning Robert finally acquired the definitive signal from a single ion, he said “That is that.” By that afternoon he had begun writing his thesis and he did not return to the ion lab again. A new graduate student Kevin Boyce had recently joined the group and the two of us spent a couple of years learning how to make precision measurements on the single ions.  It is hard to overstate how much I learned from Dave Pritchard over my five years as a graduate student. He was seldom in the lab, but he ate lunch with us students several days a week, and held regular progress meetings as well. Meeting with Dave could be a fairly overwhelming experience. He frequently was in a sort of quizmaster mode, in which he peppered his student with questions. “How big is this effect? You don’t know? That’s fine, but why don’t you estimate it for me then? No, don’t offer to go away and think about it – work it out right now, out loud, for the benefit of all of us here.” His quiz sessions could be aggravating or even intimidating, but in the end I found them to be great training. Dave liked to show us how widely disparate effects in quantum and classical physics could be understood with the same basic and rather small set of ideas such as resonance, adiabaticity, stationary points, dressed states, entropy and so on. To this day I have ambitions of designing a course called “The Seven Most Useful Ideas in Physics,” that would somehow condense and codify the Pritchardian wisdom. Thus it was that when my five years of grad school were over, while I had come nowhere near to finding the Universe’s missing mass, I still felt enthused enough about physics research to proceed on to a postdoc.  There are relatively few experiments in atomic physics these days that don’t involve the use of a laser. One major shortcoming in my graduate education in preparing me for a career in atomic physics research was that I had not learned any laser techniques. I felt my postdoctoral job had better fill in that lacuna. Looking for a postdoc job, I made the usual rounds, visiting Yale, Stanford, Bell Labs, Gaithersburg, and so on. Laser cooling was in its heyday in 1990, and as I traveled around I saw all the major programs. I was a little daunted by the size and complexity of the experiments, and worried also that maybe all the really interesting experiments had already been done. Finally, I went out to Boulder to give a talk to [Dave Wineland](https://www.nobelprize.org/nobel_prizes/physics/laureates/2012/)‘s group in NIST labs. Dave Wineland was and is one of the towering figures in ion trapping, so I felt a little foolish, earnestly describing to his group my modest contribution, but I soldiered on through my talk. No job offer was forthcoming, but as luck would have it, in the audience was a former Wineland-group postdoc, Sarah Gilbert. Sarah called her husband, Carl Wieman, who was looking to hire a postdoc, and suggested that he invite me to make the one kilometer trek from NIST labs over to JILA, on the University of Colorado campus, to visit his lab. At this time the main focus of Carl’s research was on precision measurements of parity violation in cesium, but my attention was immediately drawn to his smaller, laser cooling experiment. In contrast to the other laser cooling experiments I had seen, which took up the better part of a room, Carl’s experiment could have fit on a card table. Using diode lasers instead of Ar+-pumped dye lasers, and using a tiny little vapor cell instead of an atomic beam machine, the whole experiment seemed accessible and compact, even cute. There was just one graduate student working on the project, and this impressed me as well – if a single student could make it work, how hard could it be? (It would be almost a year later before I realized that Chris Monroe was not exactly an average graduate student!) It was clear to me that during a two-year postdoc I could learn how to make a fun little laser-cooling set up like Carl’s, and, looking ahead, it also seemed to me that I could duplicate such an experiment as an assistant professor without much trouble. It would be sufficiently easy to constract that that I would have energy, time and money left over to use the cold atoms in turn to study something else; I would not be compelled to catch up with the established major AMO groups that were studying the cooling process itself.  With an offer from Carl in my pocket, I went back to Cambridge to write up my dissertation. While considering the offer, I began to think for the first time of attempting to see Bose-Einstein condensation (BEC). BEC was a natural thing for atomic physics student at MIT to think about: occupying the office next to Dave Pritchard was Dan Kleppner, co-leader (with Tom Greytak) of one of the major groups attempting to see BEC in spin-polarized hydrogen. The idea of BEC was in the air, and I had seen a number of talks on the topic. Just a year earlier the MIT BEC group had dramatically succeeded in implementing evaporative cooling out of a magnetic trap, a clever idea due to Harold Hess. The MIT hydrogen experiment was daunting in its size and complexity, whereas it seemed to me that if one took as one’s starting point the relatively tractable vapor-cell, laser-cooling technology that Wieman was using, it wouldn’t be so much of a stretch to imagine souping it up into an apparatus capable of evaporatively cooling to BEC. So I decided to head off to Boulder for a couple of years.  After accepting Carl’s offer I postponed actually moving to Boulder for three months while my then girlfriend finished her PhD as well. In the meantime I took a very short-term postdoctoral position working with Joel Parks at the Rowland Institute, helping him design and build a Paul trap for ionized atomic clusters.  In October of 1990 I arrived in Boulder. I found working with Carl to be a very congenial experience. Carl and I share very similar tastes in what makes for an interesting physics experiment, and I was happy to assimilate a fraction of his seemingly endless bag of technological ideas. Carl taught me to decide what part of the experimental apparatus really mattered, and then to spare no effort improving that part. Conversely, Carl emphasized that one needs to recognize where “good enough” was indeed good enough, and to waste no time worrying about it. I learned from Carl’s student, Chris Monroe, as well. I had always been reluctant to mess with the innards of a store-bought piece of equipment, lest I break something. Chris’ ever-fearless attitude was, if that gizmo isn’t doing what we need it to do now, how much worse off will we be even if we *do* break it? As my two-year postdoctoral appointment wound up, Carl, Chris and I had essentially defined what needed to be done to make BEC with the hybridized method of laser cooling followed by magnetic trapping and evaporative cooling.  During those early years in Boulder, I spent a lot of time trying to imagine what a Bose-Einstein condensate would be like, if we could ever make one. Would it be superfluid, like liquid helium? Would it be coherent, like a laser? What do “superfluid” and “coherent” really mean? I understood these words in the context of the experiments the words had been invented to describe, or at least I thought I did, but it seemed to me that to understand how these words applied to a dramatically different physical system, one had to have a much deeper understanding. Superfluidity and lasing were two of my favorite topics in physics, but each was surrounded by a vast thicket of lore and literature. It was hard to step off of the well-worn paths through these thickets, hard for a newcomer to get a fresh look at the underlying phenomena. If one could make a gas-phase condensate, one would have a less brambled system against which to test one’s own physical intuition. Meditations along these lines converted me from BEC dabbler to true believer.  It was with some zealotry, then, that I took the “hybrid cooling to BEC” pitch on the road in 1992, in an effort to find a faculty job. Berkeley and MIT did not bite, but I had offers from Haverford College, University of Virginia and JILA/NIST. The environment at JILA for doing AMO research was so strong, I decided to accept their offer and remain, against the advice of several people who pointed out the potential risks of remaining in the shadow of my postdoctoral advisor. As it turned out, over the years Carl was to be extremely fair in the sharing of credit, and I have never regretted my decision to stay at JILA.  The scientific developments from 1990 to 1995 leading to BEC are discussed in the companion article. In the mid-1990s I ran a secondary research project in parallel with my BEC effort. The idea was to extend the techniques of laser cooling into solid-state systems. We never got it to work. In the end, my sunny optimism was trumped by my complete lack of training in solidstate spectroscopy. As it turned out, a group at Los Alamos National Labs has since successfully cooled a solid using a related experimental approach. Also in the mid-90s, Dana Anderson and I began a project to construct waveguides for matter waves. Our first successes were based on hollow glass fibers, but our ongoing collaboration now focuses on guiding atoms with the magnetic fields from lithographically patterned wires. The bulk of my group’s research efforts over the last seven years has focused on elucidating the properties of BEC. With every passing year, BEC proves that it still has surprises left for us. Most lately my group has been pursuing studies of quantized vortices in BEC and of spin-waves in ultra-cold atoms. This latter work required us to retreat back above the BEC transition temperature! (Although we are still comfortably within a millionth of a degree of absolute zero.)  I have been very fortunate over the years in the graduate students and postdocs who have come to work in my lab. Their hard work, talent and creativity have made me look good. I have been fortunate also to live in a society that values scientific research, and is willing to support people to do it.  In 1993, Celeste Landry and I rekindled an old romance and we were married in January of 1995, in the Stanford Faculty Club. At the time of our wedding, I had upcoming professional travel to the ICOLS conference in Capri scheduled for June, and we planned to delay our honeymoon until then. Just two weeks before the ICOLS conference, the BEC experiment finally succeeded. In beautiful Capri, with lovely Celeste, I felt on top of the world.  The next year I experienced a still keener pleasure, attending the birth of our daughter, Eliza. Her younger sister, Sophia, arrived in 1998. The four of us live in an old brick house in the shade of two large silver maples in central Boulder. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0063 = EC** |
| **Interview** |  |
| Q6 | **Eric Cornell, Carl Wieman and Wolfgang Ketterle welcome to this Nobel interview. You have been in Stockholm now for a few days and yesterday you got the Nobel Prize and this was for your discovery of Bose-Einstein condensate – the coldest piece of material in the whole universe. Do you still remember the day of this discovery, Eric?** |
|  | Eric Cornell: Very very clearly. It’s etched in my memory. The excitement of the people in the lab, the images coming up on our computerised camera. Very vividly. |
| Q22 | **No secrets?** |
|  | Wolfgang Ketterle: Small secrets. It got the best out of us.  Eric Cornell: It really focuses the mind. It’s often emphasised as a negative aspect: Oh, we’re fighting together when we should be cooperating. But in fact in the long run there is cooperation. We publish ideas. We exchange. But momentarily when it’s focused on the idea of making something work I really think it does in fact provide a set of rules to the game that as Wolfgang says really bring out ones best.  Carl Wieman: I tend to think that it doesn’t make people particularly smarter but it does make you, just like competition in any other area, it makes people just try that extra harder. The students work with a little more effort and are little less likely to go off skiing. |
| Q21 | **Blood, sweat and tears … This expression, can one apply it to research? Which of these is the most adequate?** |
|  | Eric Cornell: Our chambers are different from Wolfgang’s. Our experiments are done in glass chambers and if one isn’t careful they break and in fact blood is not out of the question in these circumstances. The /- – -/ of materials these come naturally. |
| Q4 | **So it does exist at some level at least? (Bose-Einstein condensate)** |
|  | Carl Wieman: For real atoms, yes.  Eric Cornell: At a great many laboratories, it’s one of the things that adds to the excitement of the field is that a lot of people have moved into exploring different directions. There are more than 30 laboratories all around the world, on four continents, that can make the Bose-Einstein condensation and there have been 5,000 papers published in this field.  Carl Wieman: An astronomical amount.  Eric Cornell: It’s all of these people thinking about it, bringing new angles to the problem. That adds to the turmoil.  Carl Wieman: And that adds to its significance in physics. If it was just us that had been working on this we would never have a Nobel Prize today. |
| Q15 | **But when you see those equations and laws the physicists used to say that they also see the beauty in mathematics or the beauty of this coldest piece of matter. Can you see it too?** |
|  | Eric Cornell: Physicists like everyone else come in different philosophical strengths. There are people who take a more aesthetic approach to their work. People who take a more practical approach. People who take a more competitive approach. I think that oftentimes the physicists who have the most aesthetic take on things they’re very quotable. They talk about I see God of the stars and so on. I don’t say that I don’t see God in the stars but in fact I don’t think of my daily work as a search for God or even for something which is lovely. Instead I’m looking for something which is interesting. Something I can get my fingers on which somehow has broader implications.  Carl Wieman: I think we’re all looking for something that is new and different but that doesn’t mean it’s the grand picture.  Wolfgang Ketterle: I think I feel we show the beauty of nature and especially the experiments we have been involved in, they have been very beautiful and I think they have inspired other people.  Eric Cornell: I don’t disagree.  Wolfgang Ketterle: They have offered new glimpses into the quantum world in an almost emotional beautiful way. I think I agree with you that beauty is not a goal in itself but when we try to do the impactful important physics it means we don’t try to go for the little details, we try to go for some sort of bigger things, things which are really new and if we explain that well in an elegant way it shows the beauty.  Carl Wieman: I think Bose-Einstein condensation is something that does not exist in nature or certainly not with that. Nature is millions times too hot. It owes its existence to Einstein noticing these equations, had these strange properties, and then developing all these decades of learning to control things. How the atoms interact. And now these extraordinarily low temperatures could meet and now it’s formed, so now there’s this totally new form of matter whose existence only came because of physicists understanding nature and realising that it could exist and that making it exists. Somehow there is something grand about that, no matter how you figure it. This came out of simply human intellectual effort.  Eric Cornell: I think something which adds to that is that in turn through the efforts of human kind going through this extreme conditions, as Carl says, really beyond the conventional natural limits, we see this effect of quantum mechanics. The underlying equations which usually govern matter on the tiniest scale within the atom. We see quantum mechanics written large over the entire sample something the size you can almost touch. We see the laws of nature amplified in this way, very much by having gone beyond what lives in the natural world. It’s a strange paradox. |
| Q14 | **I have a final question: what kind of reward would you expect from the next years of your research, next years in science? What is the question that you would like to approach now?** |
|  | Eric Cornell: I’m sorry I didn’t understand the question.  Wolfgang Ketterle:  think what we are going to do next year is pretty much already on the drawing board because the experiments which are being planned and experiments which are in focus. That would also get us into some technicalities. What are the goals for next year? Of course there is more exploration of Bose-Einstein condensation. |
| Q10 | **So you let people be driven by intuition?** |
|  | Eric Cornell: When you see it there, when you sense it, it’s exactly that, you have to let them go with it.  Carl Wieman: That’s the ideal to bring them to the level that they’re now taking off on their own and that’s when you stand back.  Wolfgang Ketterle: We’re not just doing research we are educating and guiding the next generation of scientists. And it’s just a wonderful feeling for me to observe how those young scientists become mature, become independent. I just want to motivate and encourage them to do that. It pays back. It pays back in their later life as a researcher, but it already pays back now because they are the people who make discoveries and drive the research. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0064** |
| **Biographical** | I was born on October 21, 1957, in Heidelberg, a small town in Germany with a charming old city and a famous castle. My parents had come to Heidelberg after the second world war, when many people relocated within Germany searching for better economic opportunities. My mother’s parents were farmers in Silesia, which has now become part of Poland. My father grew up in Memmingen, a small city in the southern part of Germany, where his parents had a canteen.  I enjoyed a childhood of stability and peace, in contrast to my parents who had grown up amidst the conflicts of war. When I was three, we moved from Heidelberg to the village (now city) of Eppelheim, three miles away, where my parents still live. I grew up with an older brother (Günter, 15 months older) and a younger sister (Monika, three and a half years younger).  My parents worked hard to provide security and prosperity for our family. My father first joined an oil and coal distribution company as an apprentice and retired as a director. My mother ran the household and cared for the children; later, she managed a small business distributing first-aid products. In our family, work was not regarded as sheer necessity, but as a defining feature and rewarding aspect of life.  My parents supported all our interests in music, sports and sciences. As they hadn’t been exposed to many of these activities themselves, they did not steer us in certain directions, but rather observed our interests and then reinforced and supported them. That may be one of the reasons why my brother and sister are successful in quite different areas: finance and education.  My explorations of the technical world started with Legos, with which I was quite creative in constructing moving objects with the basic building blocks that were then available. (Legos have become much more fancy since then!) I remember playing with electricity kits, doing repairs of household appliances, and using my father’s power tools for woodworking projects. Explorations into chemistry were done in our basement, sometimes with friends, and my parents must have had quite a bit of confidence in my abilities when they allowed me to experiment with explosive mixtures. (I was quite impressed when such a mixture was able to melt metal.) Other projects included taking old radios and a TV set apart and combining a portable radio and a vacuum tube audio amplifier to create stereo sound. I was interested in learning more about electronics, but I was disappointed that the electronic kits explained only how to put the parts together, not how they really worked. So although I explored technology and science as a child, I didn’t penetrate very deeply, partially because nobody guided me, and partially because I spent a lot of time on school and sports.  I attended elementary school in Eppelheim and Heidelberg, and then grammar school at the Bunsengymnasium in Heidelberg. My science classes didn’t involve laboratories and the variety of projects and science fairs which my children now enjoy at their schools, but they were instructive and kindled my interest. There was one mathematics teacher, Albrecht Strobel, who was inspirational. He challenged me with special problems, and tried to teach the class to approach mathematical problems in a playful rather than formal spirit. Science and mathematics did not require much of an effort for me, but I worked hard to get the highest grades in languages and other subjects. As a result, I was the best student in my class.  As a student, I liked to play soccer and basketball, and I also enjoyed trying out the various disciplines within track and field. My focus became longdistance running, but I competed occasionally in pole-vaulting. There was a year when I ran five times a week, but my talent was limited; I was occasionally well placed at town-wide events, but never in regional competitions. Still, I have maintained a passion for endurance sports until the present day. When I was around thirty, I met my own personal challenge and finished a few marathons under three hours, and I have completed many long bicycle tours.  It was clear to me early on that after high school I would go to university to study either physics, mathematics, or computer sciences. I decided on physics, as I thought it would combine the real world and mathematics. At that point, I lacked a clear idea of what modern physics was about, but my initial enthusiasm survived when I learnt more about my chosen area.  Before starting university, I received a fellowship from Studienstiftung des Deutschen Volkes (German National Merit Foundation). Part of the fellowship was the opportunity to attend special summer schools. The summer schools took place in the Italian Alps and introduced me to the beauty of mountains and to hiking. It has been one of my favorite activities ever since. Most importantly, those schools brought together a select group of highly talented students. The interactions with them reinforced my motivation to set high goals in life for myself.  In 1976, I entered the University of Heidelberg, my home town. (This traditional university was founded in 1386 as the second German university.) It was clear to me from the outset, however, that after passing the pre-diploma (intermediate exam) in two years time I would transfer to another university and leave my parents’ house. My choice was the Technical University in Munich because Munich is one of the most attractive towns in Germany and because the Technical University is excellent in many different subfields. At this stage, I wasn’t certain what I would specialize in, and had only a diffuse notion of my current field, atomic physics.  Starting an independent life in a new town was a formative experience. The proximity to the Alps was an invitation to go hiking in the summer and skiing in the winter, and I loved both the small and big theaters in Munich and its arts museums. I also became involved with the peace movement and a group working on third world issues.  . One of the electrons had a blinking light built into it. Such humorous mortar boards are a German academic tradition.  At the end of my diploma studies, I was very interested in theoretical physics and did my diploma thesis on spin relaxation in disordered materials under the supervision of Prof. Wolfgang Götze. I learnt a great deal from his lucidity in analyzing problems and how he obtained physical insight from mathematical solutions. The thesis project took one year and at the end I found myself at a crossroads. Up to this point I had been focusing on purely academic problems, and now wanted to gain experience with applied physics and how it connected with problems of the real world. Therefore, for my PhD. I chose an experimental project, trace analysis of semiconductors using laser spectroscopy. This project was supervised by Prof. Herbert Walther and Dr. Hartmut Figger at the Max-Planck Institute for Quantum Optics in Garching. After one year, it became clear that this project was too difficult to be carried out within the existing infrastructure. Since I didn’t want to start over from scratch, I continued my Ph.D. in the same laboratory, and focused on the basic spectroscopy of small molecules. We generated excited neutral molecules by charge transfer to a mass-selected ion beam. This gave us much higher selectivity in observing certain molecules than the usual discharges, and we obtained almost pure fluorescence spectra of triatomic hydrogen.  Towards the end of my PhD, I applied the same method to helium hydride and observed the first discrete spectra of this molecule confirming its existence. Helium hydride is the simplest heteronuclear molecule (besides HD), yet its spectrum had not been observed. I remember my excitement when I produced helium hydride for the first time and rotated the grating of the monochromator used to record its spectrum, and there was light almost everywhere in the visible and near infrared spectral regions. In the next few months, I would decipher some of those spectra and obtain first values for the bond lengths and vibrational frequencies of this molecule. It was exciting to determine the basic properties of a new molecule, like in the old days when molecular spectroscopy was established. After earning my PhD, I stayed at the Max-Planck Institute as a postdoc, working on laser excitation of Rydberg states of triatomic hydrogen and helium hydride. I also succeeded in analyzing all the emission spectra of helium hydride, which I had discovered during my Ph.D. The analysis of the spectra was complicated because the rotation of the molecule leads to a break-down of the Born-Oppenheimer approximation known as L-uncoupling. As a result, different electronic states are mixed. In addition, the spectrum was perturbed by interactions between s, p, and d states. Several electronic states and their perturbations had to be simulated together, before the calculated spectra started to show some similarity with the data. I regard the solution of this puzzle as one of the most challenging pieces of work I have done.  Even before finishing my PhD I already knew that I would not stay in molecular spectroscopy. I either wanted to work in a more fundamental area of physics, or focus on an area which was directly related to the needs of society. Another option was going into industry, and I had several interviews and job offers. In the end, I decided to pursue applied research in a university setting, maintaining at least some of the freedom of academic research. I joined the group of Prof. Jürgen Wolfrum at the University of Heidelberg. There, I worked in physical chemistry, focusing on combustion diagnostics with lasers. Molecular spectroscopy, in which I was an expert by this time, was used to measure temperature and molecular concentrations in a flame. One of my main projects was carried out in collaboration with the Volkswagen company. We had to transport a truckload of lasers and equipment to an engine test stand at the plant and encountered problems such as soot on the windows of the transparent engine and optics dripping with oil coming from a Diesel engine.  Changing fields was a crucial experience for me. Amazed to see how much of what I had learnt before could be applied within the new field, I realized that general skills are much more important than specific knowledge. I thought it would take a long time before I became productive in my new environment, but within months, graduate students who had been working in this area for much longer came and sought my advice and leadership. This experience gave me the self-confidence to venture into new areas, and provided the impetus for my later decision to come to the United States and start once again in a new field.  When your work is directly related to cleaner and more efficient combustion, you can easily convince non-scientists of the relevance of your research. I enjoyed this, as well as the interactions with industry and engineers. However, I began to miss something, the quest for pure knowledge and the pursuit of goals which are only vaguely defined and change as the research progresses. I thus realized that my place would be in basic research.  At 32 years, I decided to change fields once more. I wanted to switch to an area of fundamental physics where I could apply some of my knowledge in optics and spectroscopy and thus identified the field of cold atoms as the most promising area. At this time, my assessment was that the field of laser cooling and trapping had reached its peak, but there was still enough to be done. I didn’t anticipate that the best was still to come.  Leaving a long-term position in Germany and taking a short-term postdoc position in the U.S. was a risk for myself and my family. However, the time in Heidelberg in combustion research had helped me to discover what I really wanted to do, and also strengthened my self-confidence. I was willing to take risks which I wouldn’t have taken a few years earlier. By talking to people and browsing through conference proceedings, I identified the leading groups in the field and sent out applications. I was pleased that I received two offers, despite my lack of experience with cold atoms. In the spring of 1990, I joined Dave Pritchard’s group at MIT.  During the first year at MIT, I was supported by a fellowship of DAAD (Deutscher Akademischer Auslandsdienst). It is a great tradition in Germany to support scientific study abroad, but unfortunately such a tradition does not exist in the U.S. Going abroad means more than just immersing yourself in a new culture. It also means that you free yourself from your previous environment and have the opportunity to change and redefine yourself. As a foreigner in a new area of research, I didn’t feel bound to a certain tradition and could develop my own personal style – in lab work, giving talks, and discussions within the group. At MIT, where half of the graduate students are foreigners, there is no prejudice, but rather a tolerance and appreciation for unconventional ideas and styles of work.  I also found a unique atmosphere in Dave Pritchard’s group. Until then, I had worked in two rather large German groups. Dave’s group was smaller, the interactions with him and within the group were very informal; and exciting science was pursued in an atmosphere of comradeship. Dave’s knowledge of the field was enormous. During discussions, he could answer almost any question that came up, or immediately make an estimate whether a phenomenon was observable or not. Initially, I felt both intimidated and challenged by his scientific prowess, but soon we became more equal partners. This was the beginning of a wonderful collaboration that continues until the present day. Some account of it is given in the written version of my Nobel lecture.  Towards the end of my PhD studies, in 1986, I married Gabriele Sauer, whom I had known since my high school years. We had three wonderful children, Jonas, born in 1986; Johanna in 1988; and Holger in 1992, who continue to surprise me with their developing talents and personalities; they enrich my life every day. My wife and I were very different and grew apart over the years. In 2001, we separated, two months before it was announced that I was awarded the Nobel prize. Despite some difficulties, the last year has been full of joy, and the Nobel ceremonies have added extra glamour, bringing together my family, friends and colleagues. It is those people to whom I am most grateful, and they have instilled in me a passion for life and a critical, but always optimistic perspective for the future. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0064 = WK** |
| **Interview** |  |
| Q6 | **Wolfgang, you were working at another laboratory in parallel and also chasing the Bose-**[**Einstein**](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/)**condensate. Do you remember when you got the news from Boulder?** |
|  | Wolfgang Ketterle: I remember that day. I guess I was at MIT and probably Dan Kleppner was informed over the telephone and he shared it with us. We were quite impressed. It’s always nice as a scientist to see that science moves on. On the other hand, we were the tightest competitors and for several years we had put all our efforts, invested all our resources on the goal of achieving Bose-Einstein condensation. We were just about to get ready for the next round of experiments and I remember there was feverish activity in the lab because we knew we were ready to observe it. If you have worked for something for several years, if the final result is obtained within a few days or weeks this is almost simultaneously and we were running our experiments feverishly and we thought we would also have a shot at it. |
| Q4 | **When did you arrive at the condensate?** |
|  | Wolfgang Ketterle: We arrived at the condensate in a memorable night September 30th about four months later. In between it was a very traumatic time because for a few days or weeks we thought we had the experiment ready. We knew now of course that there would be a signal, it can be observed and we had everything ready to observe it, but somehow we had some problems, we couldn’t get the atoms cold enough. Then we tried in the following weeks to implement some improvements and unfortunately we lost the /- – -/ vacuum which is a major disaster and it takes weeks or a couple of months to get it back. We were ready to respond but we couldn’t. We had to wait for pumps to evacuate the chamber, to clean the surfaces, and it took a long time. Then we were ready to go and then in the next – I don’t remember – days or weeks of experimentation it happened on this night. |
| Q4 | **This is why you’re sitting here, I understand. How important is competition for research?** |
|  | Wolfgang Ketterle: It was absolutely crucial. I have to say this competition was for me a scientific competition at its best. There was cause for realisation. I learned from things you did. We were inspired and people were working harder. |
| Q22 | **No secrets?** |
|  | Wolfgang Ketterle: Small secrets. It got the best out of us.  Eric Cornell: It really focuses the mind. It’s often emphasised as a negative aspect: Oh, we’re fighting together when we should be cooperating. But in fact in the long run there is cooperation. We publish ideas. We exchange. But momentarily when it’s focused on the idea of making something work I really think it does in fact provide a set of rules to the game that as Wolfgang says really bring out ones best.  Carl Wieman: I tend to think that it doesn’t make people particularly smarter but it does make you, just like competition in any other area, it makes people just try that extra harder. The students work with a little more effort and are little less likely to go off skiing. |
| Q4 | **Why was it so important to get this Bose-Einstein condensate? Was this existing already in the minds of physicists and in science?** |
|  | Carl Wieman: But there’s a big difference between existing in the mind and being there. A nice dinner is very different whether you think about it or whether you can eat it.  Wolfgang Ketterle: Once you get to into the world then nature tells you what the properties are. There were some ideas how the Bose condensate would behave but some of those ideas were controversial. And if you can produce it and you can do real experiments then you learn something about nature. It’s important to do things to create things and not just speculate or have theories about it.  Carl Wieman: Another side of this is it didn’t have to exist. It existed for an ideal system. Whether it could exist with real atoms in a real world, that was a very much open question. |
| Q18 | **This adds something to physics and physics adds something to other humans that are not physicists. How do you view the role of physics as a part of science in shaping our world view, answering the eternal questions of humans?** |
|  | Wolfgang Ketterle: There are definitely questions of humans that go beyond physics and they are treated in biology, in philosophy and in religion.  Eric Cornell: And in literature.  Wolfgang Ketterle: But what physics does it attempts to describe the world around us in terms of physical laws, particles, their interactions. What happens if you put many particles together and form different forms of matter? At this level of complexity, if you put many particles together things happen that you can’t really imagine by just knowing what the constituents are. There is enormous richness and new phenomena emerging if you put many particles together. This is the category in which the discoveries about Bose-Einstein condensates and the properties fit in. |
| Q18 | **It wasn’t necessary – but you did. Einstein’s work for almost 100 years ago it has shaken the way people lood at the world and the universe around them. Do you think there are questions in physics, and answers of course, that can play the same role now?** |
|  | Carl Wieman: Absolutely. One of the big issues of the last few years is for example what’s the material in the universe. Physicists have now discovered and have quite convincing evidence that most of the matter in the universe isn’t the stuff we see, for example. That has to revolutionise our view of our existence. It means somewhere all around us most of what’s there is something entirely different than what we’re used to. That’s just one small example from the last few years.  Wolfgang Ketterle: Physics has not lost anything of its excitement. There are fundamental questions to be addressed and there are new questions being discovered. |
| Q8 | **Which questions?** |
|  | Wolfgang Ketterle: The fundamental questions about particle physics. There are fundamental questions in cosmology. You just mentioned, Carl, that we don’t really know what is the major constituent of the universe.  Carl Wieman: But at the same time I think that physicists would like to maintain that they’re pursuing great questions. I don’t actually believe that. I think they’re mostly tinkering, like to manipulate control, tinker with things and that opens up – like with Bose condensation – opens up questions and then they say Yes, I really was, after answering this great question. A lot of the time it’s not exactly playing but it’s not pursuing great goals. It’s pursuing more modest questions.  Wolfgang Ketterle: But I assume this has always been like that and if scientists make discoveries which we now regard as very fundamental – when [Max Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) explained the black body law – I think the general implication of those discoveries was not even imagined by the people who did it.  Carl Wieman: Sure. Both Einstein’s prediction of Bose-Einstein condensation was clearly that, he was playing with these things: Look at this. I’ve got these cohesions that describe matter, if I add this idea of Bose to that what happens. Oh this happens. It goes to a funny solution. I wonder what that means. |
| Q15 | **But when you see those equations and laws the physicists used to say that they also see the beauty in mathematics or the beauty of this coldest piece of matter. Can you see it too?** |
|  | Eric Cornell: Physicists like everyone else come in different philosophical strengths. There are people who take a more aesthetic approach to their work. People who take a more practical approach. People who take a more competitive approach. I think that oftentimes the physicists who have the most aesthetic take on things they’re very quotable. They talk about I see God of the stars and so on. I don’t say that I don’t see God in the stars but in fact I don’t think of my daily work as a search for God or even for something which is lovely. Instead I’m looking for something which is interesting. Something I can get my fingers on which somehow has broader implications.  Carl Wieman: I think we’re all looking for something that is new and different but that doesn’t mean it’s the grand picture.  Wolfgang Ketterle: I think I feel we show the beauty of nature and especially the experiments we have been involved in, they have been very beautiful and I think they have inspired other people.  Eric Cornell: I don’t disagree.  Wolfgang Ketterle: They have offered new glimpses into the quantum world in an almost emotional beautiful way. I think I agree with you that beauty is not a goal in itself but when we try to do the impactful important physics it means we don’t try to go for the little details, we try to go for some sort of bigger things, things which are really new and if we explain that well in an elegant way it shows the beauty.  Carl Wieman: I think Bose-Einstein condensation is something that does not exist in nature or certainly not with that. Nature is millions times too hot. It owes its existence to Einstein noticing these equations, had these strange properties, and then developing all these decades of learning to control things. How the atoms interact. And now these extraordinarily low temperatures could meet and now it’s formed, so now there’s this totally new form of matter whose existence only came because of physicists understanding nature and realising that it could exist and that making it exists. Somehow there is something grand about that, no matter how you figure it. This came out of simply human intellectual effort.  Eric Cornell: I think something which adds to that is that in turn through the efforts of human kind going through this extreme conditions, as Carl says, really beyond the conventional natural limits, we see this effect of quantum mechanics. The underlying equations which usually govern matter on the tiniest scale within the atom. We see quantum mechanics written large over the entire sample something the size you can almost touch. We see the laws of nature amplified in this way, very much by having gone beyond what lives in the natural world. It’s a strange paradox. |
| Q3 | **In some way you see the link between the smallest things to the very large. The Nobel Prize is regarded as the highest award in the world of science. You are quite young people, you have a long future in front of you. How will you find the motivation to continue with your science?** |
|  | Carl Wieman: The first thing you have to realise is I don’t think any of us started in this with that goal. Our goal wasn’t winning the Nobel Prize. It was interesting science. The science hasn’t gone away just because you get the Nobel Prize. Maybe some of the time to work on it has. At least for me and I think it’s true, although Eric and Wolfgang can answer for themselves, but it really hasn’t changed anything about the motivation.  Wolfgang Ketterle: I have to say myself after the discovery of Bose-Einstein condensation in -95 and a year later we showed that condensates are coherent just one single wave. This was very traumatic. I had to say to myself that we may not be able to repeat that in my lifetime because it was a unique combination of there was something to be discovered, I was just ready with my team, I had an apparatus which could be used for that. Such breakthroughs in science are so unique or so that it would be presumptuous to assume that you could repeat that. What I expect for me is just to do good science, to do the very best science I could. I was quite happy with what we did in the last few years exploring the properties of a condensate. Things went very well. It wasn’t as dramatic as those discoveries but that’s what I want to continue. I’m not expecting for myself to do another major discovery of that kind because maybe I’m trying but I am not putting myself under pressure. |
| Q14 | **I have a final question: what kind of reward would you expect from the next years of your research, next years in science? What is the question that you would like to approach now?** |
|  | Eric Cornell: I’m sorry I didn’t understand the question.  Wolfgang Ketterle:  think what we are going to do next year is pretty much already on the drawing board because the experiments which are being planned and experiments which are in focus. That would also get us into some technicalities. What are the goals for next year? Of course there is more exploration of Bose-Einstein condensation. |
| Q14 | **My last question would be what kind of science would you like to do now? What is the question that you would like to have answered in the coming years?** |
|  | Carl Wieman: As a purely personal view I have to say that I’m somewhat older than Wolfgang or Eric and have done several other things before Bose-Einstein condensation. I actually, although I still plan to keep working on Bose-Einstein condensation because I see lots of interesting experiments to do there, I see the next few years getting a lot more involved in something that I’ve been spending quite a bit of time on which is general science education. I have a feeling that the Nobel Prize will actually push me more in that direction and it will give me more opportunities.  Wolfgang Ketterle: I have a very active research group exploiting and exploring this new form of matter in different directions. We hope to discover new properties of the condensate or demonstrate that the condensate offers even more profound and deeper views into the quantum world. On the other hand we try to use the condensate to build very sensitive sensors. These are two directions which are both exciting and I’m hoping that we have new results in the next year. |
| Q10 | **So you let people be driven by intuition?** |
|  | Eric Cornell: When you see it there, when you sense it, it’s exactly that, you have to let them go with it.  Carl Wieman: That’s the ideal to bring them to the level that they’re now taking off on their own and that’s when you stand back.  Wolfgang Ketterle: We’re not just doing research we are educating and guiding the next generation of scientists. And it’s just a wonderful feeling for me to observe how those young scientists become mature, become independent. I just want to motivate and encourage them to do that. It pays back. It pays back in their later life as a researcher, but it already pays back now because they are the people who make discoveries and drive the research. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0065** |
| **Biographical** | I was born on March 26, 1951 in the small town of Corvallis, Oregon. A number of years earlier my newly wed parents N. Orr and Alison Wieman, like somewhat belated pioneers, had driven their decrepit car across the country to settle deep in the forests of the Oregon coastal range. My father began working in the lumber industry and during most of my childhood he worked as a sawyer in a sawmill. I was the fourth of five children. Most of my childhood was spent in the woods of Oregon where lumber was the sole industry. Probably some of my spirit of independence came from growing up far from other houses and towns. The nearest tiny store was always many miles away over unpaved mountain roads. Some of my earliest childhood memories are of the long school bus rides that my siblings and I used to take over those winding roads to go to school.  Much of my youth was spent wandering around in the forests of towering Douglas fir trees. I also spend much of my time reading and picking fruit and fir cones to earn spending money. Every Saturday my family would make a long expedition to the nearest town to do the week’s worth of shopping. A stop at the public library was always part of these trips. Although I was unaware of it at the time, my parents must have made special arrangements for their children to use the library since we lived far outside the region it was supposed to serve. The librarians would also overlook the normal five-book limit and allow me to check out a large pile of books each week that I would then eagerly devour. That experience has left me with a profound appreciation for the value of public libraries. At the time I was quite envious that my friends had televisions while we did not, but in retrospect I am very grateful that I spent this time reading instead of watching TV.  I went to primary school (up to grade 6) at Kings Valley grade school. It was a tiny rural school that had expanded from one to three rooms shortly before I enrolled. For the seventh grade I had to take the much longer bus ride (almost interminable for an impatient 13 year old!) to the small town of Philomath. My young idealistic teachers in mathematics and science there had a significant influence on me. I particularly remember my science teacher, Ron Tobias, who was just starting his first teaching job. I am sure that Philomath 7th grade, with all its children of loggers and farm workers, for whom education was not a particularly high priority, must have been a very tough job for a young teacher. At times I could sense hints of his frustration. However Mr. Tobias did a great deal to kindle my interest in science with his enthusiasm and knowledge. I still remember his explanations (far better than any of the material from my college courses!) of the structures of atoms in the periodic table and how these structures determined the various chemical properties and molecular reactions.  After 7th grade my parents moved to Corvallis (home of Oregon State University) so that my siblings and I could both avoid the long bus rides and take advantage of the better school system offered by this “big city” of 25,000. It was a heady day for me when we moved into a house that had a central heating system instead of just a wood stove and had an actual paved street out front! Although I was never a very sociable child, Corvallis provided me with somewhat more comfortable companions. My intellectual interests and the liberal political attitudes of my parents were always somewhat at odds with the leanings most of my previous rural classmates, but I fit in better with the children of faculty at OSU. I became close friends with a very smart boy, Brook Firey, whose father Bill was a Professor of mathematics. One summer Bill gave Brook and I our own private course in geometry. It was a rewarding and eye-opening experience to get a glimpse of the richness of mathematics, even elementary geometry, as viewed by a true mathematician. And of course, at that age, I did not realize there was anything unusual about a University professor spending a few hours each day to provide personal instruction to two fourteen year olds.  Brook and I also spent many hours engrossed in all sorts of projects constructing and investigating things. I think that much of my talent and enjoyment at improvising solutions to experimental problems goes back to those homebuilt projects. In this regard my older brother Howard also inspired me; he was always tinkering with machines and building astonishingly elaborate toys for his younger siblings. Carrying out these individual projects also developed in me a good sense of self-reliance and a sense when a piece of improvised apparatus was likely (or unlikely) to be adequate. This sense is one that I often see missing in students whose education has been confined to formal instruction.  During high school I was a good student, but never quite at the top of the class. I mastered the material, but was usually a little too independent to do precisely what the teacher wanted, and so was never considered among the very best students. Usually the worse the teacher (at least according to me), the lower was my standing. Although always interested in science, my most memorable classes were in literature and writing. From 7th through 10th grade I was a passionate chess player, spending hours a day on it. I traveled all over Oregon and occasionally to nearby states to play in tournaments. I was highly ranked in the northwest US among my age group, but at the ripe old age of 16 decided to “retire” to spend my time in more productive activities. Those activities were studying and playing tennis.  My high school grades, although not outstanding, were good enough to get me accepted into MIT. From what I now know about college admissions, I suspect my admission was considerably helped by their being intrigued to have a student who had spent much of his life literally in the wilds of Oregon. My accomplishments as a competitive chess and tennis player may also have helped. Although it may seem surprising that a boy from the woods of Oregon would aspire to go to MIT, my family always had a strong interest in education. Both my parents graduated from college and had come from welleducated families. My grandfather Henry Wieman was a rather well known Professor of Theology at the University of Chicago. Out my four siblings there are two Ph.D.’s, including a successful nuclear physicist, as well as a high-level software engineer.  My tendency to intensely pursue a particular activity to the exclusion of everything else was and is one of my most notable strengths and weaknesses. After “retiring” from chess, my focus turned to tennis. That continued after I went to MIT, and I played intercollegiately my freshman year. I also learned to play squash rackets and took to it so naturally that I was quickly at the top of the freshman intercollegiate squash team. My squash career was notable in that I can claim to have lost to some of the best players in the country, including one future national champion. Unfortunately my rather fierce competitive drive exceeded my limited physical capacities, and after surviving several minor injuries caused by throwing myself into walls and such, by the end of my freshman year I had seriously damaged my right elbow from excessive practice at squash and tennis. After several unsuccessful treatments, I then switched to playing left handed, and by early in my second year of college was starting to again be competitive in both sports at the intercollegiate level. At that point I developed serious elbow problems in my left arm, and reluctantly came to the conclusion that at age 19, it was time for my second “retirement”. It was only then I turned my full attention to physics.  As one might imagine, going from the woods of Oregon to MIT was quite a culture shock. I did not do particularly well in classes my freshman year, but I greatly enjoyed an informal freshman seminar on physics that I had with Professor Al Hill. He was a gruff but kindly old faculty member. Although I had a general interest in physics at least since seventh grade, particularly the behavior of light and atoms, I was not totally convinced when I started at MIT that I wanted to go into physics. However, after this seminar and its casual far ranging discussions about physics, Al Hill encouraged me and suggested that I should get involved in research. I discussed this with my freshman advisor who was Daniel Kleppner, and he took me on to work in his laboratory my first summer of college. This was a dramatic change from my employment the previous summer. Then, just out of high school, I had worked in the lumber mill, “pulling on the green chain”. As the green lumber came out of the mill on a large conveyor chain, my job was to pull it off and stack it in the appropriate pile. This was an exhausting job that gave me a clear taste of what real labor was like. Every now and then when I am fed up with some aspect of my job as an academic, it is useful to reflect on that summer on the green chain to remind myself how well off I am compared to all those people who spend their lives doing *real* work.  I quickly became deeply engaged in research as an undergraduate and continued to work in Dan Kleppner’s research group until I left for graduate school. I found this much more interesting and educational than taking courses, and quickly adopted a philosophy of taking as few courses as possible. Since I never did terribly well in most normal courses anyway, particularly ones that had exams, this worked out well. I was actually remarkably successful at avoiding courses, helped in large part by the events of the times. The last few weeks (and the dreaded final exams) of my freshman year were canceled because of massive protests over the Vietnam War, and during the following years there were many opportunities to participate in experiments in various sorts of alternatives to normal classes. I took full advantage of all of these alternatives, and their rather lax requirements and oversight. I also spent countless hours discussing physics with the graduate students and postdocs (notably Dave Pritchard) in Dan Kleppner’s research group. My education as a physicist came largely from my work in Dan’s lab and these interactions with him and his group. I also spent much time in physics discussions with an informal seminar group (the “physics family”) run by Rai Weiss and Al Hill.  In spite of (or because of) this unorthodox education, I ended up far more enthusiastic about physics than most of my classmates, as well as having a much better grasp of many basic concepts such as quantum mechanics. Of course I was considerably weaker in the formal solving of problems, and I still have not learned much of the standard material of the undergraduate curriculum. However, when I needed to know some material, I was completely comfortable with going out and learning it myself in a way that I discovered was not typical for my classmates. My undergraduate experience has always left me deeply suspicious of the claims of those who say a student cannot become a physicist without being required to take courses covering a whole list of specific topics. I have had a pretty successful career in optics and atomic physics without having a course in either, for example. Some may argue that this could only work because I was an extraordinary student, and the more typical student must be required to take a formal curriculum with a large number of courses and exams. However, it might be noted that before obtaining this unusual “education” there was little to indicate that I was anything special as a physics student. So one could equally well argue that it was not me that was exceptional, but rather the education I received. Perhaps if far more students learned physics in the haphazard way that I did, many more of them might turn out as motivated and successful as I have been.  I did become extremely involved in research as an undergraduate. Through a chain of circumstances, helped out no doubt by my enthusiasm and willingness to put in long hours of work, I ended up with my own lab and my own experiment. This involved the construction and use of a tunable dye laser, which at that time was a very new and exciting device. This was the beginning of Kleppner’s group moving into the use of lasers to study atomic physics. I spent my time blasting atoms with a dye laser tuned to the atomic resonance line and looking carefully at what happened. To a large extent much of my subsequent career has been variations on this basic theme. After spending many very late nights by myself taking data in the lab and showering every day at the athletic center after exercising, I started to wonder why I was paying all that money, of which I had little, to rent a dormitory room I almost never saw. So driven by my involvement in the research and a desire to save money, I actually moved into my lab. After about a half a year, living in the lab got pretty old, and so I moved into a normal apartment, but the story of my being so devoted to experimental physics that I actually lived in the lab has tended to follow me ever since.  My work with dye lasers made me aware of the exciting developments in narrowband dye lasers and their applications to atomic spectroscopy being done by Ted Hänsch. That, along with the far superior weather and more relaxed academic atmosphere, convinced me to move from MIT to Stanford for graduate school. At Stanford I resisted the natural temptation to immediately jump into laser spectroscopy again, and so I spent a year looking fairly carefully into all of the different faculty and research areas in the department. However, in the end I concluded that working on laser spectroscopy with Hänsch was the best option. I began working with his group as they were developing a very high power narrowband dye laser for exciting the 1S-2S transition in hydrogen. Ted was a new enthusiastic young professor, the technology and the experiment were new and exciting, and because of my previous background I was able to become thoroughly involved in the experiment almost immediately. It was a fun time, made more so by the fact that we soon observed the H 1S-2S transition and used it to measure the Lamb shift of the 1S state. For my thesis work I then went on to develop the technique of polarization spectroscopy and built the first single mode continuous wave dye laser at 480 nm to further improve the 1S Lamb shift measurement and greatly improve the determination of the 1S-2S isotope shift.  As I neared completion of my Ph.D., I became interested in the subject of parity violation in atoms. This was predicted by the theory of electroweak unification, but had not been seen. It seemed like the natural next step to my thesis work in that it was using precision spectroscopy of atoms to test fundamental physics. But rather than further test QED in atoms, which by then I was ready to accept as being confirmed as well as ever need be, the parity violation work was looking for new physics in atoms that went beyond QED and was far from certain. It offered the possibility of using atomic physics to do important elementary particle physics. I took a position as an assistant research scientist at University of Michigan to pursue these studies. I joined Bill Williams’ ongoing experiment to measure this parity violation in atomic hydrogen using microwave spectroscopy. Shortly after I arrived at Michigan I found that the research scientist position I had taken was not the research faculty position that I had expected. It had all the disadvantages of a regular postdoc, but none of the advantages in that there was not sufficient research money in the grant to cover my salary, so that I had also had to teach, and I had to be responsible for much of the administration of the research group. However, I threw myself into the experiment and worked extremely hard, and my position was converted into a regular assistant professor position after a couple of years. Shortly after this I developed a somewhat different formulation for how to describe atomic parity violation experiments. This allowed me to see clearly how to compare the sensitivities of a large variety of different experimental approaches. At that same time I was also becoming increasingly disillusioned with the hydrogen experiment, and my new formulation made it clear to me that a quite different approach, using laser spectroscopy of cesium, would have a far better chance of success.  For a variety of reasons I chose to pursue the cesium experiment on my own, after first receiving assurances from the department chair that this was a suitable activity. Unfortunately, my abandoning of the hydrogen experiment to pursue my own atomic parity violation experiment lead to considerable friction with senior faculty and general strife within the department. As a young assistant professor naïve in departmental politics I was quite vulnerable, and had a difficult time during my subsequent years at Michigan. However, during that time, Sarah Gilbert and I were able to get funding from Research Corporation and then NSF and used it to thoroughly develop a novel experimental approach for measuring atomic PV in cesium. Sarah was a graduate student that I had met soon after I arrived at Michigan. We then worked intensively with graduate students Rich Watts and Charlie Noecker to implement this difficult experiment. By 1984 we had made sufficient progress to indicate the viability of our approach, and this attracted an offer of a faculty position at the University of Colorado in Boulder. I eagerly accepted the offer.  The year 1984 was a very active one for me. First, Sarah Gilbert completed her Ph.D., and I accepted the job at Colorado. In late August we then packed up the entire lab into the back of a rental truck along with all the personal furniture of the graduate students, and then Sarah, Rich, Charlie, Charlie’s girlfriend, and I set out on a modern day pioneer caravan across the Great Plains to Boulder, Colorado. After quickly unpacking the truck, Sarah and I then left to fly out to Oregon for our wedding. We had been anticipating this since shortly after we met, but we had delayed until after Sarah finished her degree. We then returned to Boulder to start our new jobs and a new lab.  In the supportive environment of JILA and the Department of Physics in Boulder, along with lots of very hard work, the four of us, Sarah, Rich, Charlie, and myself, were able to make rapid progress and in less than a year completed our first measurement of parity violation in cesium. As with all my experiments, I had started out wildly optimistic as to the difficulty and time required for this experiment, and it was both a tremendous relief and a tremendous satisfaction when it succeeded. It was the best measurement of atomic parity violation at a time when this subject was being pursued by a number of notable atomic physics groups. Our result established both to myself and the rest of the world that I would have a career as research physicist; something that had sunk into considerable doubt during my seven years of meager accomplishments at Michigan. Shortly after the success of the PV experiment I was given tenure and promoted to Full Professor at Colorado. During the subsequent 15 years, my group has carried out two further generations of this long and difficulty experiment with ever improving accuracy.  I would be remiss if I failed to mention the tremendous benefits that I have gained in my career by having a wife who is a very talented and intelligent physicist (as well as being a wonderful person of course). Shortly after we arrived in Boulder, Sarah took a job at the NIST Boulder labs where she has worked ever since. We worked together on the PV experiments, and still collaborate on an occasional small project. Talking with Sarah about physics has always provided me with countless inspirations for new ideas, and has revealed critical flaws that I had overlooked in twice as many bad ideas. She also can understand and share in my obsession with the research and its occasional extraordinary demands. Finally, her ruthless editing has greatly improved the writing of nearly all of my papers. When we are not working, Sarah and I can usually be found running or hiking on the trails of the Boulder Mountain Parks. We can also occasionally (but not frequently enough) be found at our house on the central Oregon coast.  The development of the diode laser technology that was needed for the third generation of the parity violation experiment led to my involvement with laser cooling and trapping and ultimately BEC. Originally in about 1984 Rich Watts and I were simply looking for something fun and easy to do with the diode laser technology we had developed for the PV experiment, as a respite from the very long hard grind of that project. This resulted in our slowing atoms using lasers that were about 1% of the cost of what was used for previous work by Hall and Phillips. I then became increasingly interested in laser cooling and trapping. Initially my work on it focused largely on developing it as a useful technology for doing other atomic physics, but then I became more involved in studying the novel behavior of atoms at the unprecedented temperatures we could achieve. In the process of those studies I worked with an undergraduate Bill Swann to invent the vapor-cell MOT to replace the traditional atomic beam loading of optical traps. This provided a means to trap atoms using only inexpensive diode lasers and a small glass cell, which was a dramatic advance towards making laser trapping a simple and widely useable technology.  To me personally, this reduction in the cost and complication offered the opportunity to explore a variety of speculative directions involving laser cooled atoms with relatively little risk, since the cost and effort was now quite modest. One such quick experiment was to switch the laser cooled and trapped atoms to a magnetic trap in order to avoid the limits we had discovered were imposed by the photons in the optical trap. This worked so easily and so well – we obtained trapped atoms about 100 times colder than had been achieved previously, with a corresponding enhancement in phase space density – that it inspired me to pursue goals grander than just better trapping and cooling technology; namely, the attainment of BEC by further cooling in the magnetic trap. Fortunately Eric Cornell joined me at just that time (1990) to pursue the goal of BEC. Ours turned out to be an extraordinarily friendly and effective partnership that has continued up to the present. Our pursuit of BEC is now well-documented history.  Over the past several years I have become increasingly involved with trying to improve undergraduate physics education and have been balancing my time between that and my research. I have been examining alternative curricula and learning about the research in physics education as to how students do and do not learn. A particular concern has been improving how physics is taught to students who are not planning to become physicists, in the hope of one day making physics understandable, useful, and interesting to a large fraction of the population. My efforts have ranged from working with national organizations pursuing widespread change in undergraduate physics education to developing useful innovations in the individual courses that I teach. Because of my particular concerns, these courses have lately been large introductory courses primarily for nonscience students. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0065 = CW** |
| **Interview** |  |
| Q6 | **Can you describe the feeling of seeing this big thing?** |
|  | Carl Wieman: The initial feeling was of course a lot of excitement trying to get this … But there was also considerable nervousness in it. What we saw looked so much like the best we had hoped for, it’s often the case you fooled you. When things work better than you almost can expect then oftentimes it means you’re fooling yourself. I remember forcing myself to say Wait a minute, don’t get too excited here. You could be just fooling yourselves. Let’s think hard about what extra tests we can do. How can we check this? We can’t get too wrapped up in until you are absolutely certain. |
| Q22 | **No secrets?** |
|  | Wolfgang Ketterle: Small secrets. It got the best out of us.  Eric Cornell: It really focuses the mind. It’s often emphasised as a negative aspect: Oh, we’re fighting together when we should be cooperating. But in fact in the long run there is cooperation. We publish ideas. We exchange. But momentarily when it’s focused on the idea of making something work I really think it does in fact provide a set of rules to the game that as Wolfgang says really bring out ones best.  Carl Wieman: I tend to think that it doesn’t make people particularly smarter but it does make you, just like competition in any other area, it makes people just try that extra harder. The students work with a little more effort and are little less likely to go off skiing. |
| Q2 | **I understand when you arrived to the discovery that people were working through years because nobody was lucky enough to get it before you. I understand also that they had lots of failures behind them, so you must also have had it. How can you psychologically overcome all those failures you meet in your life as a scientist?** |
|  | Carl Wieman: I always think that one of the most important aspects for being a good scientist is the capacity to have a large threshold for frustration because it’s not just the big experiments that run for a couple of years and then fail. Sometimes the previous work you referred to, it’s been working … it’s polarised hydrogen, they were failures in that they didn’t achieve Bose-Einstein condensation, but they weren’t failures in that no results came out. They came out with results showing paths that don’t work and we learned a great deal from those experiments. That’s failure in one sense but success on another.  Eric Cornell: More success than failure, I would say. |
| Q4 | **Why was it so important to get this Bose-Einstein condensate? Was this existing already in the minds of physicists and in science?** |
|  | Carl Wieman: But there’s a big difference between existing in the mind and being there. A nice dinner is very different whether you think about it or whether you can eat it.  Wolfgang Ketterle: Once you get to into the world then nature tells you what the properties are. There were some ideas how the Bose condensate would behave but some of those ideas were controversial. And if you can produce it and you can do real experiments then you learn something about nature. It’s important to do things to create things and not just speculate or have theories about it.  Carl Wieman: Another side of this is it didn’t have to exist. It existed for an ideal system. Whether it could exist with real atoms in a real world, that was a very much open question. |
| Q18 | **It wasn’t necessary – but you did. Einstein’s work for almost 100 years ago it has shaken the way people lood at the world and the universe around them. Do you think there are questions in physics, and answers of course, that can play the same role now?** |
|  | Carl Wieman: Absolutely. One of the big issues of the last few years is for example what’s the material in the universe. Physicists have now discovered and have quite convincing evidence that most of the matter in the universe isn’t the stuff we see, for example. That has to revolutionise our view of our existence. It means somewhere all around us most of what’s there is something entirely different than what we’re used to. That’s just one small example from the last few years.  Wolfgang Ketterle: Physics has not lost anything of its excitement. There are fundamental questions to be addressed and there are new questions being discovered. |
| Q18 | **Which questions?** |
|  | Wolfgang Ketterle: The fundamental questions about particle physics. There are fundamental questions in cosmology. You just mentioned, Carl, that we don’t really know what is the major constituent of the universe.  Carl Wieman: But at the same time I think that physicists would like to maintain that they’re pursuing great questions. I don’t actually believe that. I think they’re mostly tinkering, like to manipulate control, tinker with things and that opens up – like with Bose condensation – opens up questions and then they say Yes, I really was, after answering this great question. A lot of the time it’s not exactly playing but it’s not pursuing great goals. It’s pursuing more modest questions.  Wolfgang Ketterle: But I assume this has always been like that and if scientists make discoveries which we now regard as very fundamental – when [Max Planck](https://www.nobelprize.org/prizes/physics/1918/planck/facts/) explained the black body law – I think the general implication of those discoveries was not even imagined by the people who did it.  Carl Wieman: Sure. Both Einstein’s prediction of Bose-Einstein condensation was clearly that, he was playing with these things: Look at this. I’ve got these cohesions that describe matter, if I add this idea of Bose to that what happens. Oh this happens. It goes to a funny solution. I wonder what that means. |
| Q15 | **But when you see those equations and laws the physicists used to say that they also see the beauty in mathematics or the beauty of this coldest piece of matter. Can you see it too?** |
|  | Eric Cornell: Physicists like everyone else come in different philosophical strengths. There are people who take a more aesthetic approach to their work. People who take a more practical approach. People who take a more competitive approach. I think that oftentimes the physicists who have the most aesthetic take on things they’re very quotable. They talk about I see God of the stars and so on. I don’t say that I don’t see God in the stars but in fact I don’t think of my daily work as a search for God or even for something which is lovely. Instead I’m looking for something which is interesting. Something I can get my fingers on which somehow has broader implications.  Carl Wieman: I think we’re all looking for something that is new and different but that doesn’t mean it’s the grand picture.  Wolfgang Ketterle: I think I feel we show the beauty of nature and especially the experiments we have been involved in, they have been very beautiful and I think they have inspired other people.  Eric Cornell: I don’t disagree.  Wolfgang Ketterle: They have offered new glimpses into the quantum world in an almost emotional beautiful way. I think I agree with you that beauty is not a goal in itself but when we try to do the impactful important physics it means we don’t try to go for the little details, we try to go for some sort of bigger things, things which are really new and if we explain that well in an elegant way it shows the beauty.  Carl Wieman: I think Bose-Einstein condensation is something that does not exist in nature or certainly not with that. Nature is millions times too hot. It owes its existence to Einstein noticing these equations, had these strange properties, and then developing all these decades of learning to control things. How the atoms interact. And now these extraordinarily low temperatures could meet and now it’s formed, so now there’s this totally new form of matter whose existence only came because of physicists understanding nature and realising that it could exist and that making it exists. Somehow there is something grand about that, no matter how you figure it. This came out of simply human intellectual effort.  Eric Cornell: I think something which adds to that is that in turn through the efforts of human kind going through this extreme conditions, as Carl says, really beyond the conventional natural limits, we see this effect of quantum mechanics. The underlying equations which usually govern matter on the tiniest scale within the atom. We see quantum mechanics written large over the entire sample something the size you can almost touch. We see the laws of nature amplified in this way, very much by having gone beyond what lives in the natural world. It’s a strange paradox. |
| Q3 | **In some way you see the link between the smallest things to the very large. The Nobel Prize is regarded as the highest award in the world of science. You are quite young people, you have a long future in front of you. How will you find the motivation to continue with your science?** |
|  | Carl Wieman: The first thing you have to realise is I don’t think any of us started in this with that goal. Our goal wasn’t winning the Nobel Prize. It was interesting science. The science hasn’t gone away just because you get the Nobel Prize. Maybe some of the time to work on it has. At least for me and I think it’s true, although Eric and Wolfgang can answer for themselves, but it really hasn’t changed anything about the motivation.  Wolfgang Ketterle: I have to say myself after the discovery of Bose-Einstein condensation in -95 and a year later we showed that condensates are coherent just one single wave. This was very traumatic. I had to say to myself that we may not be able to repeat that in my lifetime because it was a unique combination of there was something to be discovered, I was just ready with my team, I had an apparatus which could be used for that. Such breakthroughs in science are so unique or so that it would be presumptuous to assume that you could repeat that. What I expect for me is just to do good science, to do the very best science I could. I was quite happy with what we did in the last few years exploring the properties of a condensate. Things went very well. It wasn’t as dramatic as those discoveries but that’s what I want to continue. I’m not expecting for myself to do another major discovery of that kind because maybe I’m trying but I am not putting myself under pressure. |
| Q14 | **My last question would be what kind of science would you like to do now? What is the question that you would like to have answered in the coming years?** |
|  | Carl Wieman: As a purely personal view I have to say that I’m somewhat older than Wolfgang or Eric and have done several other things before Bose-Einstein condensation. I actually, although I still plan to keep working on Bose-Einstein condensation because I see lots of interesting experiments to do there, I see the next few years getting a lot more involved in something that I’ve been spending quite a bit of time on which is general science education. I have a feeling that the Nobel Prize will actually push me more in that direction and it will give me more opportunities.  Wolfgang Ketterle: I have a very active research group exploiting and exploring this new form of matter in different directions. We hope to discover new properties of the condensate or demonstrate that the condensate offers even more profound and deeper views into the quantum world. On the other hand we try to use the condensate to build very sensitive sensors. These are two directions which are both exciting and I’m hoping that we have new results in the next year. |
| Q10 | **So you let people be driven by intuition?** |
|  | Eric Cornell: When you see it there, when you sense it, it’s exactly that, you have to let them go with it.  Carl Wieman: That’s the ideal to bring them to the level that they’re now taking off on their own and that’s when you stand back.  Wolfgang Ketterle: We’re not just doing research we are educating and guiding the next generation of scientists. And it’s just a wonderful feeling for me to observe how those young scientists become mature, become independent. I just want to motivate and encourage them to do that. It pays back. It pays back in their later life as a researcher, but it already pays back now because they are the people who make discoveries and drive the research. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0066** |
| **Biographical** | Life goes on surprisingly fast. It seems to happen a short time ago that I would attend anniversary celebrations in honour of noted physicists, my teachers who to my mind looked quite old. But at the present time, I myself have recently marked the 70th birthday.  My parents, Ivan Karpovich, and Anna Vladimirovna, had been Byelorussia born and raised. At the age of eighteen my father arrived in Saint Petersburg, in the year 1912. In his early hard years, he had been a docker, an errand boy and consequently got a job as a worker at the “Lessner” plant (later the Karl Marx Plant).  During World War I, he was a brave hussar, a non-commissioned officer of the Life Guards, a holder of the St. George Order. In September 1917, my father joined the Bolshevik party and retained his adherence to the socialist and communist principles to the end of his life.  In childhood, my brother and I “with a sinking heart” used to listen to father’s stories about the civil war and his military career. We learned how the formerly non-commissioned officer had been appointed to take command of a cavalry regiment in Red Army. Father also used to tell us about his meetings with revolutionary leaders: V.I. Lenin, L.D. Trotsky, B.E. Dumenko, “comrade Andrey” (A. Solts) who always put his apartment in the “Embankment House” at my father’s disposal while we stayed in Moscow. Father graduated from the Industrial Academy in 1935 and since then destiny was throwing us all over the country: Stalingrad, Novosibirsk, Barnaul, Syas’stroy in the environs of Leningrad, Turinsk (Sverdlovsk region), where we lived throughout the war time, and eventually the Minsk-city lying in ruins after the war. Dad was given a new assignment as director of a factory, joint enterprises (corporation of enterprises), later director of a trust. Mother headed a public organization of housewives; worked as a librarian and always remained our close friend while bringing us up without discouraging words. As a result of being so-called “director’s boys”, my brother and I tried to behave ourselves and to act in the way that people thought was correct and proper both at school and in public.  Learning was easy to me, and dependable defender, my elder brother Marx, made my existence cloudless at school and outdoors as well. Marx had graduated high school on June 21, 1941 (next day the Nazi invasion started) in the town of Syas’stroy and shortly after that we left for the Urals to Turinsk city as Dad had been assigned there to a post of director of a newly-built gunpowder cellulose factory (at the time referred to as factory No. 3). My elder brother, who was seventeen years old then, joined the Urals Industrial Institute (the Energy Faculty). The young student considered the problem of energy to be of cardinal importance for the future. But not long did he study at the Institute. He decided to defend his Motherland and to fight against fascists at the front line.  He passed Stalingrad, Kharkov, the Kursk battle. Having recovered after heavy head injury he was sent to the Army in the Field again. That was so called “another Stalingrad”, i.e., the Korsun-Shevchenko battle, where in his 20 years was shot down a Guard junior lieutenant Marx Ivanovich Alferov, my elder brother who remained of 20 years forever.  In October 1943, on the way to front from a hospital he spent 3 days with us in Sverdlovsk. I often look back and reflect on those three days; on his description of the war, his youthful enthusiasm and faith in the power of science, technology and human intelligence.  In the post-war particular situation I attended an only boy’s school in the destroyed Minsk-city, and was lucky in having an excellent physics teacher there Yakov Borisovich Meltserson. He delivered lectures on physics for us, rather naughty boys, and we were sitting quiet and listened attentively. The teacher loved physics devotedly and had a gift of making our imagination work. His explanation of the cathode oscilloscope operation and talk on radar systems greatly impressed me. When finishing the school I took his advice which institution to choose for education and that was a celebrated Ul’yanov Electrotechnical Institute in Leningrad (abbreviated to LETI).  Many of systematic studies in electronics and radio engineering that had been performed there made significant contributions into the electronics industry. As for me, it was my good fortune to meet my first supervisor there. Theoretical courses of studies were easy enough for me. It was the laboratory research that attracted me. Being a third-year student, I began to work in a laboratory of vacuum processes. My first investigations were directed by a research associate N.N. Sozina who studied semiconductor photodetectors. Since that time, half a century ago, semiconductors have become main objects of my scientific interests. A book “The Electroconductivity of Semiconductors” by F.F. Volkenshtein, which had been written in Leningrad (during the time of Leningrad’s siege) was my Textbook then. My graduation thesis was devoted to the problem of obtaining the thin films and investigating the photoconductivity of bismuth telluride compounds.  In December 1952, I graduated from the Institute and was offered by my supervisor N.N. Sozina to stay in the LETI to continue my study. But I dreamed of working at the Physico-Technical Institute that had been founded by Abram Fedorovich Ioffe. His book “Fundamentals of Modern Physics” was a manual for me. Happily, three vacancies for graduates had been given to us by Ioffe’s Institute. One of them fell to my lot. My joy was boundless. And may be it is this lucky distribution that has determined my happy scientific career.  In the letter to my parents, then residing in the Minsk-city, I wrote about my lucky chance. I did not know that Academician Ioffe was dismissed and left the Institute of which the director he had been for thirty years.  I recall my first day at the Physico-Technical Institute on January 30, 1953. I was introduced to my new supervisor, V.M. Tuchkevich, head of a subdivision. It was a very important problem to be solved by our not very big team: creation of germanium diodes and triodes (transistors) on p-n junctions.  The Physico-Technical Institute, being regarded on today’s scale, was not a big one. I was given an Institute pass No. 429, i.e., the total amount of employees was as high as the above mentioned number; most of famous physicists of the Physico-Technical Institute moved to Moscow (to I.V. Kurchatov’s, and newly-built atomic centers). Semiconductors elite followed A.F. Ioffe in order to work under his supervision in a recently organized semiconductor laboratory belonging to the Presidium of the Academy of Sciences of the USSR. In the Physico-Technical Institute there retained only D.N. Nasledov, B.T. Kolomiets and V.M. Tuchkevich as representatives of the old generation of physicists who formerly dealt with semiconductors.  Academician A.P. Komar was after A.E. Ioffe on charge of the Physico-Technical Institute. The new director’s attitude to his predecessor was not quite correct but as to the restoration and development of the Institute, his strategy was O.K. Of utmost importance was the support of works on the creation of new semiconductor electronics, space investigations (gas dynamics of high velocities and high temperature protective coatings; development of the light isotope separation methods for the hydrogen weapon (under the guidance of B.P. Konstantinov).  Studies of fundamental problems of physics, both theoretical and experimental ones, were encouraged too: just in this time experimental discovery of exciton was done (E.F. Gross), it was formulated the principles of a kinetic theory of strength (S.N. Zhurkov), development of the pioneering works on physics of atomic collisions were initiated (V.M. Dukel’skii, N.V. Fedorenko).  Both the director of the Institute (A.P. Komar) and the deputy director (D.N. Nasledov) understood the importance of drawing the interests of young people to science. It was a practice then to welcome newcomers at the highest level. In this way many renowned Russian scientists started their work, among them were present members of the Academy of Sciences, B.P. Zakharchenya, A.A. Kaplyanskii, E.P. Mazets, V.V Afrosimov and others.  I remember my first attendance of the seminar on semiconductors at the Physico-Technical Institute in February 1953 as one of the most impressive events I have ever experienced. That was a brilliant report delivered by E.F. Gross about the discovery of the exciton. The sensation I experienced then could not be compared to anything. I was stunned by the talk on the birth of a discovery in the area of science to which I myself had got the access.  Yet the main thing was everyday experimental work in the laboratory. Since that time I have been keeping, as a most precious thing, my laboratory daily report book that contains notes of mine about the creation of the first soviet p-n junction transistor on the 5th of March, 1953. And now, when recalling that time I cannot help feeling proud of what we had accomplished. We comprised a team of very young people. Under the guidance of V.M. Tuchkevich we succeeded in working out principles of the technology and the metrics of transistor electronics. Below are the names of researchers who had been working in our small laboratory: A.A. Lebedev, a Leningrad University graduate – the growth and doping of perfect germanium single crystals; Zh.I. Alferov – the preparation of transistors, their parameters being at the level of the best world samples; A.I. Uvarov and S. M. Ruvkin – the creation of a precise metrics of germanium single crystals and transistors; N.S. Yakovchuk, a graduate of the Faculty of Radio Engineering of the Leningrad Electrical Technical Institute – designing transistor-based circuits.  As early as in May 1953, the first Soviet transistor receivers were shown to the “top authorities”. That work, of which the performers had been working with passion peculiar to their young hearts and with utmost sense of responsibility, exerted a great influence upon me. While quickly and effectively progressing as a scientist, I began to comprehend the significance of the technology not only for electronic devices, but in basic research work too, in regard with notorious “minor” details and sporadic results. And it is since then that I prefer to analyze experimental result proceeding from “simple” general laws prior to putting forward sophisticated explanations.  In subsequent years, our team of researchers at the Physico-Technical institute expanded considerably and in a very short time the first Soviet germanium power rectifiers were created alongside with germanium photodiodes and silicon also were being carried out then.  In the month of May 1958, Anatolii Petrovic Alexandrov (later the President of the Academy of Sciences of the USSR) asked our team of working out a special semiconductor device for the first Soviet atomic submarine. That required a perfectly new technology and in addition to – another construction of germanium rectifiers, which had been done in a record short space of time. In the month of October, these devices were mounted on a submarine. I was a junior research associate at the Institute then, and was somewhat surprised by a telephone call from the first Vice-Chairman of the Government of the USSR, Dmitrii Fedorovich Ustinov, who asked me of fortnight reduction of the term. There was no getting away from that: I directly moved in the laboratory premises and settled there but, of course, the request was fulfilled that was my first State Order, which I had been decorated with then and which I valued very much.  In 1961, I read my candidate degree thesis that had been mainly devoted to working out and investigating of power germanium and partially silicon rectifiers. Occurrence of Soviet power semiconductor electronics became possible as a result of those works. Of great importance there, in the sense of a scientific, purely physical standpoint, had been a conclusion drawn by me that in p-i-n, p-n-n semiconductor homostructures under working current densities (for most of semiconductor devices), the current had been determined by recombination in heavily doped p- and n (n+)-regions while the recombination contribution in the middle i(n)-region of a homostructure was not the determining one: so, as soon as the first work on semiconductor lasers had appeared, it was natural for me to consider the advantages of employing in lasers the double heterostructure of p-i-n (p-n-n+, n-n-p+) type. The idea was formulated by us shortly after the appearance of the first work of R. Hall with co-workers, which described a semiconductor laser based on a GaAs homo-p-n-structure.  To realize principal advantages of heterostructures appeared to be possible only after obtaining of AlxGal-xAs heterostructures. We did that and it turned out that we had been only one month ahead in relation to American researches from IBM.  When we began investigating heterostructures, I used to convince my young colleagues, that we were not one and only group of scientists in the world who understood the significance of the concept that the semiconductor physics and electronics would be developing on the basis of HETERO-, rather than HOMO-structures. Indeed, since 1968 we entered an era of a strong competition and the first of all were three laboratories of the biggest American companies: Bell Telephone, IBM and RCA.  In 1967, while on a short trip to UK, I visited STL laboratories in Harlow. They were well equipped and the experimental base was excellent but English colleagues only discussed theoretical aspects of the heterostructures physics; they did not find experimental study of heterostructures to be promising then. In London I had some time for sightseeing and shopping. I bought there Wedding gifts to my fiancee Tamara Darskaya. As soon as I returned to Leningrad, we celebrated our wedding in a splendid restaurant “Krysha” (the Roof) in the Grand Hotel “Europe”.  Tamara was a daughter of a very popular actor of Voronezh Theater of Musical Comedy. Tamara worked then in the environs of Moscow at a big Space Enterprise under the guidance of Academician V.P. Glushko. She wonderfully combined incompatible beauty with cleverness and common sense and was always very kind toward her close friends. It was time of repeated weekly flights to Moscow. Holding a position of a Senior Research Associate at the Physico-Technical Institute, I could afford that. Leningrad-Moscow flight occurred in an hour time and the cost of a ticket to the TU-104 plane was as low as 11 rubles (about 15 US dollars). Nevertheless, after half a year shuttling between the two cities Tamara had moved to Leningrad.  In 1968-1969, we virtually realized all the ideas on control the electron and light fluxes in classical heterostructures based on the arsenid gallium-arsenid aluminum system. Apart from fundamental results that were quite new and important efficient one-side injection, the “superinjection” effect, diagonal tunneling, electron and optical confinement in a double heterostructure (which in a short while became the main element in studying the low-dimensional electron gas in semiconductors), we succeeded in employing principal benefits of heterostructure applications in devices, i.e., lasers, LEDs, solar cells, dynistors and transistors. Of utmost importance was, beyond doubts, the making of low threshold room temperature operating lasers on a double heterostructure (DHS) that had been suggested by us as far back as 1963. The approach developed by M.B. Panish and I. Hayashi (Bell Telephone) as well as by H. Kressel (RCA) was different from that of ours since they offered to use in lasers a single p-AlGaAs-p-GaAs heterostructure, which made their approach rather limited. A possibility of obtaining an efficient injection in the heterojunction seemed doubtful to them and, in spite of the fact that potential advantages of DHS had been recognized.  In August 1969, I first time visited the USA; my paper that I read there at the International Conference on Luminescence in Newarc (State of Delaver) was devoted to AlGaAs-based DHS low threshold room temperature lasers and produced an impression of an exploded bomb on American colleagues. Professor Ya. Pankov from RCA, who just shortly before my reading the paper had explained to me that they had not got a permission for my visiting their laboratory, as soon as I concluded my speech told me that the permission had been received. I could not help enjoying my refusal explaining that now I had been invited by that moment to attend IBM and Bell Telephone laboratories.  My seminar in the Bell followed by the looking over the laboratories and discussions with researches clearly revealed to me our merits and demerits of our progress in my laboratory. I believe that the soon commenced emulation for being the first in getting the continuous wave operation of laser at the room temperature was at that time a rare example of an open and friendly competition between laboratories belonging to the antagonistic Great Powers. We won the competition overtaking by a month Panish’s group in Bell Telephone. Significance of obtaining the continuous wave regime had the connection first and foremost with working out an optical fiber with low losses as well as the creation of our DHS lasers, which resulted in appearance and rapid development of optical fiber communication.  In the winter 1970-1971 and spring 1971, I spent six months in the USA working in laboratory of semiconductor devices at the University of Illinois together with Prof. Nick Holonyak. We met at the first time in 1967, when he visited my laboratory at the Physico-Technical Institute. Prof. Nick Holonyak, who is one of the founders of semiconductor optoelectronics, the inventor of the first visible semiconductor laser and LED became my closest friend. Now over 33 years we have discussed all semiconductor physics and electronics problems, political and life aspects and our interaction (visits, letters, seminars, telephone conversations) played very important role in our work and life.  In 1971, I became a recipient of the USA Franklin’s Institute gold medal for DHS laser works. Being my first international award, it was of particular value to me. There are Soviet physicists besides me who have been given the Franklin’s Institute gold medals too: Academician P.L. Kapitsa in 1944; Academician N.N. Bogolubov in 1974; Academician A.D. Sakharov in 1981. I consider it a big honour to belong to such a company!  An AlxGal-xAs system of lattice-matched heterostructures, which in practice seemed to be a lucky exception, was infinitely expanded on the basis of multi-component solid solutions, first theoretically and later on experimentally (InGaAsP is the most convincing example).  Heterostructure-based solar cells were created by us as far back as 1970. And when American scientists published their early works, our solar batteries have been already mounted on the satellites (sputniks) and their industrial production was in full swing. The cells, when being employed in space, proved their efficiency. For many years they have been operating on the “MIR” skylab and in spite of the fact that forecasts of a substantial decrease of the value of one watt of the electrical power have not been justified so far, the most effective energy source in space is, nevertheless, a set of solar cells on heterostructures of III-V compounds.  In 1972, my pupils-colleagues and I were awarded the Lenin’s Prize – the highest scientific Prize in the USSR. Our gladness regrettably was not cloudless. For some formal and obscure reasons we lost from the list of nominees R.F. Kazarinov and E.L. Portnoi.  On the day of the prize award I was in Moscow and called home, to Leningrad. But the telephone did not answer. Then, I called my parents (they have been living in Leningrad since 1963) and gladly told my father that I had been given the Lenin’s Prize. But my father replied – And so what- Our grandson is born today! In my lucky 1972 year, in addition to the prestigious prize I was elected a member of the Academy of Sciences. But the happiest day was that of Vanya Alferov’s birth.  Studies of superlattices and quantum wells were rapidly promoted in the West and afterwards in this country soon resulted in coming into being of a new area of the quantum physics of solid: the physics of low-dimensional electron systems. In this regard, studies of zero-dimensional structures – so-called “quantum dots” – form the summit of the above mentioned works. Gratifying is the circumstance that the Ioffe Institute today, while going through the hard times, remains the world leader in this area of physics. Works of the second and third generation of my students, those being well-known P.S. Kop’ev, N.N. Ledentsov, V.M. Ustinov, S.V. Ivanov have won general recognition nowadays. N.N. Ledentsov has become the youngest corresponding member of the Russian Academy of Sciences.  In 1987, I was elected director of the Ioffe Institute, in 1989, president of the Leningrad Scientific Center of the Academy of Sciences of the USSR; and in April 1990, Vice-President of the Academy of Sciences of the USSR. Afterwards, I was reelected and hold all these posts now within the Russian Academy of Sciences.  In the first years of my presidency and directorship we succeeded in remarkable scaling up research activity in our unique (for all the world) Academy of Sciences. We have also developed effective collaboration with Universities and Educational Institutions. The Physico-Technical Special Secondary School attached to Ioffe’s Physico-Technical Institute had been opened at that time; ongoing was the process of creation of specialized University chairs: the first one, that of Optoelectronics was organized in the Electrotechnical University, (formerly the LETI) as far back as in 1973. On the basis of both then existing and newly organized chairs a Physicotechnical faculty was set up in the Polytechnical Institute in 1988.  A great contribution into the above mentioned system makes the Scientific Educational Center that has been built by the Physico-Technical Institute and incorporates school boys, students and scientists in a magnificent edifice, which can be called “The Palace of Knowledge”.  Still, throughout the years passed, of greatest importance has been so far the existence of our Academy of Sciences as a unique both scientific and educational structure in Russia. The Academy faced the menace of abolition in the twenties as “an inheritance from the tsarist regime”. It faced the menace of abolition in the nineties as “an inheritance from the totalitarian Soviet regime”. To insure its safety I gave my consent to be a member of the Russian Parliament (a deputy of State Duma) in 1995. President Yu.S. Osipov and Vice-Presidents, Academicians and Corresponding Members, doctors and candidates of sciences, senior and junior research associates, lab-assistants and mechanics took a firm stand on this kind of situation. For the saving of the Academy of Sciences, we made compromises with the power but never with the conscience.  All that had been made by human beings, in principle, was made due to Science. And if our country’s choice is to be a Great Power, Russia will be the great power not because of the nuclear potential, not because of faith in God or president, or western investments but thanks to the labor of the nation, faith in Knowledge and Science and thanks to the maintenance and development of scientific potential and education.  When I was a little boy of ten, I have read a wonderful book “Two Captains” (by V. Kaverin). In essence, in my life I have been following the principle that was peculiar the main character of that book: “One should make efforts and search for. And having obtained whatever the purpose, to make efforts again”.  Of great importance here is to know what you are struggling for. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0066 = ZA** |
| **Interview** |  |
| Q9 | **Zhores Alferov, Jack Kilby and Herbert Kroemer, welcome to the Nobel e-museum and to this conversation, and congratulations to the Nobel Prize in Physics that you received just three days ago. You have changed the world, the world will never be the same again, can we agree on that? Zhores?** |
|  | Zhores Alferov: No, I just was saying that the second half of our century was really the time when the information technology was developed so much, and /- – -/ just very, very frequently that we have now a post-industrial or informational society. It happened first of all due to one of the great discoveries just after the war, the discovery of the transistor, the invention of transistor. Then of course the discovery of laser-maser principles and of course the work of Jack Kilby, the invention of integrated circuits just absolutely changed the situation in computer science and information technology. I think that Herbert Kroemer and I just added to that the development of new kinds of materials, semi-conductor heterostructures which permit to control absolutely a new way that electron and light fluxes inside of crystals. This gives a lot of new possibilities for high-speed electronics and it was created real optoelectronics by development of heterostructural lasers ILD. But I also should like to mention the name of Nick Holonyak who did very strong contribution to conventional at first p-n junctions visible lasers and pn-junction LEDs.  Herbert Kroemer: Yes, he was the first, we had actually …  Zhores Alferov: Yes, he was the first who created the visible LED and the visible lasers.  Herbert Kroemer: Yes, he’s a good friend, a good friend of both of ours.  Zhores Alferov: Yes. |
| Q4 | **I see, you two worked in parallel, you were in St Petersburg or Leningrad and you were in the US on the same problem?** |
|  | Zhores Alferov: Initially not knowing of each other. There is, we are working in parallel but it’s also there was some difference because Herbert Kroemer mostly was doing theoretical work and described some principles, makes the proposals. I did also proposal of the double heterostructure lasers and some other devices based on the heterostructures, but I am an experimentalist and first of all I started with the idea of the double heterostructure laser came to my mind, I first of all started to do that, to realise that. I was working at first just with technicians and engineers and the group would grow up, and we realised our ideas first of all, lasers and solar cells and heterostructure /- – -/ and other devices. But I should like to add to the comments of Herbert Kroemer, that It’s really he and I started with the proposals and consideration from the practical point of view, how to improve well known devices like transistor and then laser for instance, but what we did not expect it really, that from this heterostructure research just appear the new region of absolutely fundamental research which we call now low dimensional electro structures. It became possible only due to development of the heterostructures and successes in the technology of new heterostructures, so It’s always connected by this way, the basic fundamental results frequently came out from practical considerations, and vice versa.  Herbert Kroemer: Yes, but because they went beyond the original motivation, that where they created a new application ‒ this pendulum idea is a very good one. In a very real sense this has been true for science and technology for a very, very long time, that one creates the other, it’s a …  Jack Kilby: Although perhaps we are not very many solid-state physicists in the world, almost none before the invention of the transistor, a few thereafter. Today people working on integrated circuit problems are frequently solid-state physicists, so it has expanded the field and I suppose will continue to do so.  Herbert Kroemer: For example, if you look at the meetings of the American Physical Society, the annual meetings, the biggest single meeting is the March meeting which is, well, they now call it condensed matter physics but it’s really solid-state physics, dominated by solid state physics, so this is the biggest single area of activity for physicists as a profession.  Zhores Alferov: Right now if you look for instance for semi-conductor physic international conference which is going every two years, if you look to this conference for instance 20, 30 years ago there were just a few talks about heterostructures. If you look now, it’s practically 2/3 of the conference is the heterostructures and the devices and applications and so on.  Herbert Kroemer: It simply has become a universal ingredient in solid state technology, it’s a tool without which we could no longer do it.  Zhores Alferov: And then solid-state physics also.  Herbert Kroemer: Yes.  Jack Kilby: All the early work on transistors was done by people with very disparate backgrounds: chemists, physicists, electrical engineers. One of the best I knew had a degree in paper making, and all of these people were pulled into the activity and began to develop a common vocabulary and interest.  Zhores Alferov: I knew something about this very important discovery, transistor inventions in 1947, at Bell Telephone. I knew that from John Bardeen, who frequently visited the Ioffe Institute, and I met him the first time in 1960 at the Prague conference and also Nick Holonyak as a first pupil of John. He told me a lot, and what was very interesting in my opinion, that time it was a group created at the Bell Telephone in 1945, [Bill Shockley](https://www.nobelprize.org/prizes/physics/1956/shockley/facts/), [Walter Brattain](https://www.nobelprize.org/prizes/physics/1956/brattain/facts/), [John Bardeen](https://www.nobelprize.org/prizes/physics/1956/bardeen/facts/), Gerald Pearson, Gibney and a few technicians. Mervin Kelly, I think, was the vice president of the Bell Telephone, and he formulated as one of the tasks of this group just to check quantum theory for solid state, for condenser for solid state materials. I will be happy if now the vice president of the industrial companies, everywhere, in the United States, in Russia, would be so clever that put the task in an industrial laboratory to check some new principles in physics.  Herbert Kroemer: That was possible under Kelly at Bell Telephone Laboratories, I don’t think It’s possible anymore. |
| Q18 | **But the institutes as the name of your institute says, is Physical Technical, this is a bridge between science and industry?** |
|  | Zhores Alferov: Yes, because it’s a bridge between basic research and applications, and of course because I could mention Ioffe, when he founded our institute in 1918 during the civil war time, he was clever enough that physics in the 21stcentury is background for technology, so he from the beginning considered the physical technical institute it was the name of our institute and he was very clever person. He founded simultaneously physical mechanical department of the polytechnical institute. Maybe it was just another example, MIT and later Caltech, the universities where physicists educated with very good understanding in engineering problems and engineers educated with very good basic background in physics and mathematics. These physical technical universities we can call them Caltech, MIT, Physical Technical institute in Moscow is a university, in polytechnical institutes were the places where the new generation of engineers and physicists were trained. |
| Q10 | **What about your corporation? Was it mostly cooperation or competition?** |
|  | Herbert Kroemer: I think it was competition, right?  Zhores Alferov: Yes.  Herbert Kroemer: But friendly competition.  Zhores Alferov: It was absolutely friendly competition, and sometimes it was just /- – -/.  Herbert Kroemer: Yes.  Zhores Alferov: I think in general the example of the work in heterostructures, because we had very strong competitions with Bell Telephone Laboratories, with some other laboratories, but it was during the Cold War time, end of 1960s, beginning of 70s, but we invited each other to the laboratories, we organised joint seminars, so it was just an example, I think. The physicists in general gave the example how to live in this complicated world together and enticing the American and Russian physicists during the World War II, during the Cold War ‒ during the World War II we were just on the same front ‒ but during the Cold War also gives the example for friendly competition and to show that we are representatives of science and we are together.  Herbert Kroemer: Yes.  Jack Kilby: I think another important effect was that as the field matured, as work became more expensive, in the early days you could literally build transistors in your garage and all the equipment that was required to do so. Today a modern semiconductor flat costs several billion dollars and it’s desirable for the industrial companies to share the development costs of those things. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0067** |
| **Biographical** | I was born on August 25, 1928 in Weimar, Germany. My father was a civil servant working for the city administration of my home town; my mother was a classical German “Hausfrau.” Both came from simple skilled-craftsmen families. Neither had a high-school education, but there was never any doubt that they wanted to have their children obtain the best education they could afford. My mother, in particular, pushed relentlessly for top performance in school: simply doing well was not enough. Fortunately, I breezed through 12 years of school almost effortlessly, not once requiring help with homework from my parents.  Despite their insistence on excellence, my parents never pushed me in any particular academic direction; I was completely free to follow my inclinations, which ran towards math, physics, and chemistry. When I finally told my parents that I wanted to study physics, my father merely wondered what that is, and whether I could make a living with it. I certainly could become a physics teacher at a High School, or “Gymnasium,” a thoroughly respectable profession.  I did have one major problem in school, though: Discipline! I was often bored, and entertained myself in various disruptive ways. A frequent punishment was an entry into the “Klassenbuch,” the daily class ledger. These entries were considered a very serious matter, and if I had not been excellent academically, I would have risked being expelled. Once, after I had again been entered as having disturbed the class, the teacher who had overall responsibility for the class – Dr. Edith Richter, whom I adored – asked me in great exasperation: “Why again?” I told her that I had been bored, whereupon she exploded: “Mr. Kroemer, one of the purposes of a higher education is that you learn to be bored gracefully.” I will never forget that outburst – nor have I ever really learned to be bored gracefully.  Another teacher – Willibald Wimmer – had his own clever way of handling me. Before the end of the war, he had been an instructor at a local engineering college, ending up teaching math and physics at our high school. He was used to dealing with more mature students, and he treated us as adults. I was way ahead of the curriculum in math, and kept showing off. Worse, I taught some of my classmates math “thricks,” that were not part of the curriculum. So, Mr. Wimmer made a “treaty” with me: While he could not excuse me from attending class, I was guaranteed a top grade without being required to turn in the homework assignments, and was permitted to do whatever I wanted to do during the hour, *provided* I kept absolutely quiet – except when explicitly asked to speak up. Both of us kept that treaty.  Mr. Wimmer also became our physics teacher, a subject about which he clearly knew little more than what was in the textbook. Realizing that I was deeply into physics, he simply enlisted me and one other student to help him in lecture preparations, like setting up what apparatus had survived the war. Once I even was asked to present the lecture myself, with him sitting in the front row and enjoying the show. It was a wonderful experience.  Having graduated from the gymnasium in 1947, 1 was accepted as a physics student at the University of Jena, where I fell under the spell of the great Friedrich Hund, the most brilliant lecturer I ever encountered. The joy did not last long. In early 1948 the political suppression in East Germany became very severe, especially at rebellious universities like Jena. Every week, some of my fellow students had suddenly disappeared, and you never knew whether they had fled to the West, or had ended up in the German branch of Stalin’s Gulag, like the uranium mines near the Czech border. During the Berlin airlift, I was in Berlin as a summer student at the Siemens company, and I decided to go West via one of the empty airlift return flights.  From Berlin, I had written to several west German universities for admission, including Göttingen, but did not receive a reply before leaving Berlin (they had turned me down). I followed the advice of one of my Jena professors “why don’t you give my greetings to Professor König in Göttingen.” König told me that physics admissions were closed, but he passed me on for what was ostensibly just a friendly chat to Professor Richard Becker and his alter-ego assistant, Dr. Günther Leibfried. They in turn passed me on to [Wolfgang Paul](https://www.nobelprize.org/nobel_prizes/physics/laureates/1989/index.html) (Nobel 1989), and I think also to Robert Pohl. It soon dawned on me that this was not just a friendly social chat with people who had nothing better to do, but a thorough examination. I remember one of the questions Paul asked me: “You know that a mirror interchanges left and right? -Then why doesn’t it interchange top and bottom?” In the end, I was returned to Becker, who told me that two of the students who had been admitted were not coming, and a meeting was scheduled for the next day to select who would get the two openings. A few days later I received a postcard that I had been accepted.  Post-war Göttingen. was – intellectually – a wonderfully stimulating place. I was attracted to one of the younger instructors – “Privatdozent” Dr. Hellwege – who offered a so-called Proseminar, where pre-research students would present papers assigned to them, and I participated in this for several semesters in a row. Once, the famous Fritz Houtermans visited Hellwege, and sat in on several of the presentations, including mine. I presented someone’s data that yielded a reasonable straight line on a double-log plot, and proudly claimed a power law for the data. Houtermans was not impressed: “On a double-log plot, my grandmother fits on a straight line.” I keep quoting Houtermans’ grandmother to my own students. Eventually, I signed up with Hellwege for a Diploma Thesis, which would probably have led to an experimental study of the optical spectra of some rare-earth salts. But Hellwege had a long waiting list, and in the meantime, Professor Fritz Sauter – a refugee who had found a temporary home as a guest in Becker’s Institute for Theoretical Physics – offered me a theoretical Diploma Thesis, based on a talk that I had given in one of his seminars. Hellwege suggested that I accept Sauter’s offer: “You will be finished with him before you can start with me.” So I became a theorist.  The diploma thesis was an extension of a 1939 paper by Shockley on the nature of surface states in one-dimensional potentials. As one of the elaborations, I looked at the interface between two different periodic potentials, which confronted me for the first time with what we would today call the band offsets at heterojunctions.  There was another early encounter with heterojunctions while working under Sauter. We made a field trip to the AEG research laboratories in Belecke, a small town in Westphalia. There, a Dr. Poganski gave a beautiful demonstration that the selenium rectifier was not a Schottky barrier, but a p-n junction between p-type selenium and n-type CdSe, a true heterojunction – although that term did not exist yet. This must have had an at least sub-conscious influence on me: when I later started thinking about heterojunctions in earnest, the question whether such things could actually exist as real devices had an obvious answer: Of course!  While working on my diploma, I gave another colloquium talk under Sauter, reporting on the famous Bardeen/Brattain paper “Physical Principles Involved in Transistor Action” (or some title like that). At the end I made some suggestion about some open questions raised by the authors. Sauter was intrigued and suggested that as a possible Ph.D. topic. Sometime later, he came into my office and told me to stop further work on my Diploma thesis, and to simply write up what I had done so far. When I protested, he insisted that it was time to move on to the real thing, the Ph.D. dissertation.  I had thus come into contact with one of Sauter’s strong beliefs, apparently dating back to the tradition of the 20s: that degrees should not be awarded on the basis of having “served time,” but were basically certificates that the recipient had proven capable of executing creative work independently, and no longer required supervision. In fact, he clearly preferred quick dissertations. As a result, I received my Ph.D. before my 24th birthday, fast even for a theorist: Wonderful!  The Ph.D. dissertation involved what we would today call *hot-electron effects*, in the collector space-charge layer of the then-new transistor. The idea was simple. Almost nothing was known about the energy band structure of Ge, but someone’s theoretical estimates suggested – quite incorrectly – very narrow bands, especially for the valence band. In this case, if the field was strong enough, any holes in the valence might undergo what we now call Bloch oscillations. A few lines of algebra suggested that, for a given current density, the traveling hole concentration would increase with increasing field (“Staueffekt”), leading to strong space charge effects. The influence of these space charges on the current-voltage characteristics of point contact diodes and transistors formed the main body of the dissertation.  My algebra also implied a decrease of electron drift velocity with increasing field, implying a negative differential conductivity. Knowing nothing about electrical circuit theory, I was unaware how useful such a phenomenon could be, until [Shockley](https://www.nobelprize.org/nobel_prizes/physics/laureates/1956/index.html) pointed it out to me in a personal discussion two years later.  But it became clear soon that my dissertation was unrelated to reality. My assumptions about the band structure and about an energy-independent mean free path had been invalid, and after the discovery of avalanche breakdown it became obvious that the huge fields required for Bloch oscillations in a bulk semiconductor could never be reached. Twenty years later, after the pathbreaking work of [Esaki](https://www.nobelprize.org/nobel_prizes/physics/laureates/1973/index.html) and Tsu on negative differential conductivity in superlattices, I realized that I had in fact anticipated their basic physics, albeit in a more primitive form: What was not possible in bulk semiconductors, appeared to become possible in superlattices with their much longer period.  Back to Sauter. He was not interested in closely supervising his students; he simply watched what they were doing on their own initiative. Still, he had a tremendous influence on me in matters of methodology. Whenever I came to him with a pure physics idea, he would invariably say, with slight sarcasm: “But Mr. Kroemer, you ought to be able to formulate this mathematically! ” If I came to him with a math formulation, I would get, in a similar tone: “But Mr. Kroemer, that is just math, what is the physics?” After a few encounters of this kind, you got the idea: You had to be able to go back and forth with ease. Yet, in the last analysis, concepts took priority over formalism, the latter was simply an (indispensable) means to an end.  This set of priorities clearly showed, and it had a profound influence on me. As a student of Sommerfeld, Sauter was a superb mathematician himself. But he detested it when people were showing off their math skills by using math that was more advanced than necessary for the problem at hand. To the contrary: You were expected to show how simple you could make it. Because he was a great expert on Bessel functions, I once felt compelled to put, into the draft of my dissertation, an ad-hoc problem that required Bessel functions. He was not amused: “This has no business here; you just put it in to impress me. Take it out!”  Richard Becker had exactly the same attitude (the two were close friends), and I later encountered it again in Shockley. Under influences such as these, I never developed into a “hard-core Theorist with a capital T,” but became basically a conceptualist who remained acutely aware of his limitations as a formalist, and whose personal role model was [Niels Bohr](https://www.nobelprize.org/nobel_prizes/physics/laureates/1922/index.html) more than anybody else amongst the Greats of Physics.  The German 1952 job market for theoretical physicists was all but nonexistent. New university positions were not created, and there were plenty of more senior people waiting to occupy any vacancies that might open up. So I never even considered a university career. The situation in industry was hardly any better. As luck would have it, the small semiconductor research group at the Central Telecommunications Laboratory (FTZ) of the German postal service was looking for a “house theorist” who knew semiconductor theory, and I got the job. My duties were simple. I had to be available for whatever theoretical questions anybody had, and also take an active role by poking my nose into the work of my experimentalist and technologist colleagues, to look on my own for topics to which I could contribute – provided I would never touch any equipment. Every week or two, I had to give a talk of 1 to 2 hours to the group, on any subject of my choosing of which I thought that the group should be taught about it. Other than that, I was left completely free to pick whatever problems I felt were worth tackling. So I had become a “professor” of sorts after all, teaching a small but highly motivated “class.” From day-l I was forced to learn to communicate, not with other theorists, but with experimentalists and technologists. It was a fascinating challenge, with a range of topics far beyond what I myself had learned in Göttingen, very often going beyond physics, into metallurgy, chemistry, and electrical engineering.  Of course I ceased to be a “real” theoretical physicist – if I ever was one. Call me an Applied Theorist if you want. However, the awareness of doing something truly useful helped overcome the uneasy feelings over ending a theorist career as soon as it had begun. By hindsight, maybe it wasn’t such a bad career move after all!  As my research topic at the FTZ, I picked the problem of the severe frequency limitations of the new transistors – and what one might be able to do about them. It was this problem that led directly to heterostructure ideas. In a 1954 publication of mine there are a couple of paragraphs outlining in a rudimentary form the first ideas for what was later to be called the heterostructure bipolar transistor, or HBT. I proposed both a transistor with a graded gap throughout the base, and the simpler form of just a wide-gap emitter. The rest is history. This history is described in some detail in my Nobel Lecture, so I will give here only the highlights.  Some time afterjoining RCA Laboratories in Princeton, NJ, in 1954, I returned to heterojunctions. I actually tried – unsuccessfully – to build some HBTs with a Ge/Si alloy emitter on a Ge base. But my principal contributions to the field were two theoretical papers. One of these, in the *RCA Review*, is essentially unknown to this day, but it clearly spelled out the concept of *quasielectric fields*, which I considered the fundamental design principle for all heterostructures.  The final step came in 1963, while I worked at Varian Associates in Palo Alto, CA. A colleague – Dr. Sol Miller – gave a research colloquium on the new semiconductor diode laser. He reported that experts had concluded that it was fundamentally impossible to achieve a steady-state population inversion at room temperature, because the injected carriers would diffuse out at the opposite side of the junction too rapidly. I immediately protested: “But that’s a pile of … ; all you have to do is give the outer regions a wider energy gap.” I wrote up the idea and submitted the paper to *Applied Physics Letters*, where it was rejected. I was talked into not fighting the rejection, but to submit it to the *Proceedings of the IEEE*, where it was published, but ignored. I also wrote a patent, which is probably a better paper than the one in *Proc. IEEE*.  Then came the final irony: I was refused resources to work on the new kind of laser, on the grounds that there could not possibly be any applications for it. By a coincidence, the Gunn effect had just been discovered, and having a long-standing interest in hot-electron negative-resistance effects, I worked on the Gunn effect for the next ten years, and did not participate in the final technological realization of the laser.  I left Varian in 1966, and in 1968 joined the University of Colorado. There I eventually returned to heterostructures, and in the early-70s tackled the theory of band offsets together with my student Bill Frensley – now at UT Dallas – who worked out the first ab-initio theory of the band offsets. Shortly afterwards – now at UCSB – I developed a powerful method to determine band offsets experimentally, by capacitance-voltage profiling *through* the hetero-interface.  In the late-70s, I returned to the device that had started it all, the HBT. The technology developments that had made possible the DH laser offered great promise also for the HBT, and I became a strong advocate of developing the full potential of that device.  In addition to heterostructures, I have worked on numerous other semiconductor topics, be it in physics, materials, devices, or technology. Second only to heterostructures has been a continuing interest in hot-electron negative-resistance effects, dating back to my Ph.D. dissertation. I already mentioned the work on the Gunn effect, but there was more. During my RCA years, I had come up with a crazy scheme to obtain a negative resistance perpendicular to a strong bias field, by drawing on the fact that some of the heavy holes in Ge have negative transverse effective masses – that is, *perpendicular* to their velocity. Experimentally, it was another failure, but conceptually, I found it extraordinarily stimulating. So did others, and it earned me a great deal of early notoriety. Today, I am back to one of the sins of my youth: to the superlattice Bloch oscillator, an exciting combination of heterostructures and hot electron physics.  At the opposite end from hot electrons has been recent work on superconducting weak links in which a degenerately modulation-doped InAs/AlSb quantum well acts as a ballistic coupling medium between superconducting Nb electrodes. They exhibit some utterly delightful large discrepancies between experiment and accepted theory.  There are numerous additional topics scattered throughout my career. I have basically been an opportunist – and not at all ashamed of it. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0067 = HK** |
| **Interview** |  |
| Q9 | **Zhores Alferov, Jack Kilby and Herbert Kroemer, welcome to the Nobel e-museum and to this conversation, and congratulations to the Nobel Prize in Physics that you received just three days ago. You have changed the world, the world will never be the same again, can we agree on that? Zhores?** |
|  | Zhores Alferov: No, I just was saying that the second half of our century was really the time when the information technology was developed so much, and /- – -/ just very, very frequently that we have now a post-industrial or informational society. It happened first of all due to one of the great discoveries just after the war, the discovery of the transistor, the invention of transistor. Then of course the discovery of laser-maser principles and of course the work of Jack Kilby, the invention of integrated circuits just absolutely changed the situation in computer science and information technology. I think that Herbert Kroemer and I just added to that the development of new kinds of materials, semi-conductor heterostructures which permit to control absolutely a new way that electron and light fluxes inside of crystals. This gives a lot of new possibilities for high-speed electronics and it was created real optoelectronics by development of heterostructural lasers ILD. But I also should like to mention the name of Nick Holonyak who did very strong contribution to conventional at first p-n junctions visible lasers and pn-junction LEDs.  Herbert Kroemer: Yes, he was the first, we had actually …  Zhores Alferov: Yes, he was the first who created the visible LED and the visible lasers.  Herbert Kroemer: Yes, he’s a good friend, a good friend of both of ours.  Zhores Alferov: Yes. |
| Q14 | **I would say that we find ourselves in an ever-accelerating circle of smaller and smaller electronics and bigger and bigger economy, and everybody feels like everything is going faster and faster. Do you see an end to all this? Herbert?** |
|  | Jack Kilby: Perhaps it is going faster but some of these things have taken quite a long time to materialise, the integrated circuit is 40 years old, Dr Kroemer and Alferov’s work is about that old, so when you’re sitting and waiting and watching it, no, it doesn’t seem quite so fast.  Herbert Kroemer: Of course you do not really anticipate what it will lead to, I think the development has not … You cannot predict what the applications will be. One of the reasons it took so long is because it took a while until the full significance of all of our work became clear. If it had become clear right at the beginning … Well, there was no way it could become clear right at the beginning, in fact I was not allowed to work on the double heterostructural laser because … |
| Q14 | **If you look at the chip it is at least 40 years old or about 40 years old, so somehow the life expectancy for the chip, maybe It’s like 40 years or 50 ‒ that’s what I mean by coming to the end. What will be after this?** |
|  | Herbert Kroemer: I certainly get asked this question very often and I frankly refuse to make predictions. One of the reasons I refuse to make predictions, is because I believe the progress in science and technology is an opportunistic one rather than a deterministic one- Particularly when you’re confronted with situations where the discoveries themselves create their own applications, until such time that we can predict the discoveries we cannot predict where it will go, where the applications will go and so I try to be a little bit modest about that one. But I am convinced that things will not come to a stop, I just do not know in which direction it will go. |
| Q14 | **Do you think that there is a scientific limit for development, for making electronics smaller?** |
|  | Herbert Kroemer: Ultimately, once you get below the size of the atom you’re in trouble.  Jack Kilby: The very factor I think is the decrease in cost. In 1958 Texas Instruments sold a single, not very good, silicone transistor for 10 dollars, today you can buy several hundred million much better transistors for that price. Nothing else has ever decreased in cost at that rate, so it’s tremendously opened up the field and permitted new applications. |
| Q18 | **So it’s the applications and the economy which is the driving force behind the development maybe?** |
|  | Herbert Kroemer: Sometimes it is and sometimes it isn’t, certainly, although I was lead to the heterostructure ideas by an applications question. Once the basic idea had emerged the order turned around, and suddenly there was a basic idea and you were now trying to apply this basic idea to things that were very different from when it got started. I became involved in the heterostructure concept by wanting to make transistors faster and that did in fact happen, but then I realised that the same principles that speed up the transistor could also be applied for light emitting devices and then the flow was from science to application. It’s like a pendulum, it goes back and forth from science to applications, science creates application, applications stimulate new science, and you cannot say it’s one way or the other. |
| Q4 | **I see, you two worked in parallel, you were in St Petersburg or Leningrad and you were in the US on the same problem?** |
|  | Zhores Alferov: Initially not knowing of each other. There is, we are working in parallel but it’s also there was some difference because Herbert Kroemer mostly was doing theoretical work and described some principles, makes the proposals. I did also proposal of the double heterostructure lasers and some other devices based on the heterostructures, but I am an experimentalist and first of all I started with the idea of the double heterostructure laser came to my mind, I first of all started to do that, to realise that. I was working at first just with technicians and engineers and the group would grow up, and we realised our ideas first of all, lasers and solar cells and heterostructure /- – -/ and other devices. But I should like to add to the comments of Herbert Kroemer, that It’s really he and I started with the proposals and consideration from the practical point of view, how to improve well known devices like transistor and then laser for instance, but what we did not expect it really, that from this heterostructure research just appear the new region of absolutely fundamental research which we call now low dimensional electro structures. It became possible only due to development of the heterostructures and successes in the technology of new heterostructures, so It’s always connected by this way, the basic fundamental results frequently came out from practical considerations, and vice versa.  Herbert Kroemer: Yes, but because they went beyond the original motivation, that where they created a new application ‒ this pendulum idea is a very good one. In a very real sense this has been true for science and technology for a very, very long time, that one creates the other, it’s a …  Jack Kilby: Although perhaps we are not very many solid-state physicists in the world, almost none before the invention of the transistor, a few thereafter. Today people working on integrated circuit problems are frequently solid-state physicists, so it has expanded the field and I suppose will continue to do so.  Herbert Kroemer: For example, if you look at the meetings of the American Physical Society, the annual meetings, the biggest single meeting is the March meeting which is, well, they now call it condensed matter physics but it’s really solid-state physics, dominated by solid state physics, so this is the biggest single area of activity for physicists as a profession.  Zhores Alferov: Right now if you look for instance for semi-conductor physic international conference which is going every two years, if you look to this conference for instance 20, 30 years ago there were just a few talks about heterostructures. If you look now, it’s practically 2/3 of the conference is the heterostructures and the devices and applications and so on.  Herbert Kroemer: It simply has become a universal ingredient in solid state technology, it’s a tool without which we could no longer do it.  Zhores Alferov: And then solid-state physics also.  Herbert Kroemer: Yes.  Jack Kilby: All the early work on transistors was done by people with very disparate backgrounds: chemists, physicists, electrical engineers. One of the best I knew had a degree in paper making, and all of these people were pulled into the activity and began to develop a common vocabulary and interest.  Zhores Alferov: I knew something about this very important discovery, transistor inventions in 1947, at Bell Telephone. I knew that from John Bardeen, who frequently visited the Ioffe Institute, and I met him the first time in 1960 at the Prague conference and also Nick Holonyak as a first pupil of John. He told me a lot, and what was very interesting in my opinion, that time it was a group created at the Bell Telephone in 1945, [Bill Shockley](https://www.nobelprize.org/prizes/physics/1956/shockley/facts/), [Walter Brattain](https://www.nobelprize.org/prizes/physics/1956/brattain/facts/), [John Bardeen](https://www.nobelprize.org/prizes/physics/1956/bardeen/facts/), Gerald Pearson, Gibney and a few technicians. Mervin Kelly, I think, was the vice president of the Bell Telephone, and he formulated as one of the tasks of this group just to check quantum theory for solid state, for condenser for solid state materials. I will be happy if now the vice president of the industrial companies, everywhere, in the United States, in Russia, would be so clever that put the task in an industrial laboratory to check some new principles in physics.  Herbert Kroemer: That was possible under Kelly at Bell Telephone Laboratories, I don’t think It’s possible anymore.  **No, so most of the research is going on in the academia so to say?**  Herbert Kroemer: I would say in solids, in our field, the research with a capital R has moved from industry to the universities whereas when I came to the United States it was the other way around. I came to the United States in 1954, I then went back once but came again, and I went to an industry laboratory. There was very little going on at the universities … |
| Q10 | **What about your corporation? Was it mostly cooperation or competition?** |
|  | Herbert Kroemer: I think it was competition, right?  Zhores Alferov: Yes.  Herbert Kroemer: But friendly competition.  Zhores Alferov: It was absolutely friendly competition, and sometimes it was just /- – -/.  Herbert Kroemer: Yes.  Zhores Alferov: I think in general the example of the work in heterostructures, because we had very strong competitions with Bell Telephone Laboratories, with some other laboratories, but it was during the Cold War time, end of 1960s, beginning of 70s, but we invited each other to the laboratories, we organised joint seminars, so it was just an example, I think. The physicists in general gave the example how to live in this complicated world together and enticing the American and Russian physicists during the World War II, during the Cold War ‒ during the World War II we were just on the same front ‒ but during the Cold War also gives the example for friendly competition and to show that we are representatives of science and we are together.  Herbert Kroemer: Yes.  Jack Kilby: I think another important effect was that as the field matured, as work became more expensive, in the early days you could literally build transistors in your garage and all the equipment that was required to do so. Today a modern semiconductor flat costs several billion dollars and it’s desirable for the industrial companies to share the development costs of those things. |

|  |  |
| --- | --- |
| **Physics\_2024-2000** | |
| **ID** | **0068** |
| **Biographical** | The Nobel Committee has asked me to discuss my life story, so I guess I should begin at the beginning.  I was born in 1923 in Great Bend, Kansas, which got its name because the town was built at the spot where the Arkansas River bends in the middle of the state. I grew up among the industrious descendents of the western settlers of the American Great Plains.  My father ran a small electric company that had customers scattered across the rural western part of Kansas. While I was in high school, a huge ice storm knocked down most of the poles that carried the telephone and electric power lines. My father worked with amateur radio operators to communicate with areas where customers had lost their power and phone service.  My dad’s goal was to do whatever it took to run his business and to help people, but I thought that amateur radio was a fascinating subject. It sparked my interest in electronics, and that’s when I decided that this field was something I wanted to pursue.  I also was a fan of broadcast radio. In the 1940s, I especially enjoyed listening to Big Band music. Even today, there’s a radio station in Dallas that plays this kind of music, and with a little luck, I don’t have to listen to much else.  After high school, I studied electrical engineering at the University of Illinois. Most of my classes were in electrical power, but because of my childhood interest in electronics, I also took some vacuum tube engineering physics classes.  I graduated in 1947, just one year before Bell Labs announced the invention of the transistor. It meant that my vacuum?tube classes were about to be come obsolete, but it offered great opportunities to put my physics studies to good use.  In line with the interests that occupied my thoughts in Great Bend, I hired on with an electronics manufacturer in Milwaukee, Wisconsin, that made parts for radios, televisions and hearing aids.  While in Milwaukee, I took evening classes at the University of Wisconsin towards a master’s degree in electrical engineering. Working and going to school at the same time presents some challenges, but it can be done and its well worth the effort.  In 1958, my wife and I moved to Dallas, Texas, when I took job with Texas Instruments. TI was the only company that agreed to let me work on electronic component miniaturization more or less full time, and it turned out to be a great fit.  After proving that integrated circuits were possible, I headed teams that built the first military systems and the first computer incorporating integrated circuits. I also worked on teams that invented the handheld calculator and the thermal printer, which was used in portable data terminals.  In 1970 I took a leave of absence from TI to do some independent work. While on leave, one of the things I worked on was how to apply silicon technology to help generate electrical power from sunlight.  From 1978 to 1984, I spent much of my time as a Distinguished Professor of Electrical Engineering at Texas A&M University. The “distinguished” part is in the eye of the beholder, and I really didn’t do much “professing.” However, I did have a rewarding time doing research and working with students and faculty on various projects.  I officially retired from TI in the 1980s, but I have maintained a significant involvement with the company that continues to this day.  Along the way, I’ve been honored to receive awards such as the National Medal of Science and to be inducted into the National Inventors Hall of Fame. Seeing your name alongside the likes of Henry Ford, Thomas Edison, and the Wright Brothers is a very humbling experience, and I appreciate these and the other honors very much.  Receiving the Nobel Prize in Physics was a completely unexpected, yet very pleasant surprise. I had to start my pot of coffee very early the morning I received the news that I had been chosen.  It’s gratifying to see the committee recognize applied physics, since the award is typically given for basic research. I do think there’s a symbiosis as the application of basic research often provides tools that then enhance the process of basic research. Certainly, the integrated circuit is a good example of that. Whether the research is applied or basic, we all “stand upon the shoulders of giants,” as Isaac Newton said. I’m grateful to the innovative thinkers who came before me, and I admire the innovators who have followed.  Four decades of hindsight is perhaps a unique experience among those who have been awarded the Nobel Prize in Physics. As I noted in my lecture, there were various efforts to solve the electronic miniaturization problem at the time I invented the integrated circuit. Humankind eventually would have solved the matter, but I had the fortunate experience of being the first person with the right idea and the right resources available at the right time in history.  I would like to mention another right person at the right time, namely Robert Noyce, a contemporary of mine who worked at Fairchild Semiconductor. While Robert and I followed our own paths, we worked hard together to achieve commercial acceptance for integrated circuits. If he were still living, I have no doubt we would have shared this prize.  Now that I am retired, I still occasionally consult on various industry and government projects, mostly in the area of semiconductors. I also serve on the board of directors of a company or two.  People often ask me what I’m proud of, and, of course, the integrated circuit is at the top of the list. I’m also proud of my wonderful family. I have two daughters and five granddaughters, so you could say that the Kilbys specialized in girls.  I’ve reached the age where young people frequently ask for my advice. All I can really say is that electronics is a fascinating field that I continue to find fulfilling. The field is still growing rapidly, and the opportunities that are ahead are at least as great as they were when I graduated from college. My advice is to get involved and get started. |
| **Autobiography** |  |
| **Podcast** |  |
| **Telephone**  **interview** | **0068 = JK** |
| **Interview** |  |
| Q14 | **I would say that we find ourselves in an ever-accelerating circle of smaller and smaller electronics and bigger and bigger economy, and everybody feels like everything is going faster and faster. Do you see an end to all this? Herbert?** |
|  | Jack Kilby: Perhaps it is going faster but some of these things have taken quite a long time to materialise, the integrated circuit is 40 years old, Dr Kroemer and Alferov’s work is about that old, so when you’re sitting and waiting and watching it, no, it doesn’t seem quite so fast.  Herbert Kroemer: Of course you do not really anticipate what it will lead to, I think the development has not … You cannot predict what the applications will be. One of the reasons it took so long is because it took a while until the full significance of all of our work became clear. If it had become clear right at the beginning … Well, there was no way it could become clear right at the beginning, in fact I was not allowed to work on the double heterostructural laser because … |
| Q14 | **Do you think that there is a scientific limit for development, for making electronics smaller?** |
|  | Herbert Kroemer: Ultimately, once you get below the size of the atom you’re in trouble.  Jack Kilby: The very factor I think is the decrease in cost. In 1958 Texas Instruments sold a single, not very good, silicone transistor for 10 dollars, today you can buy several hundred million much better transistors for that price. Nothing else has ever decreased in cost at that rate, so it’s tremendously opened up the field and permitted new applications. |
| Q4 | **The thing that forced you to make your innovation as I understand was something called the “tyranny of numbers” can you explain that?** |
|  | Jack Kilby: That phrase came about because people could visualise electronic equipment that would be useful if it could be built, but the existing technology made it too expensive, too bulky and too unreliable just due to the very large number of parts – the tyranny ‒ and it was a very apt phrase for the time. |
| Q4 | **I see, you two worked in parallel, you were in St Petersburg or Leningrad and you were in the US on the same problem?** |
|  | Zhores Alferov: Initially not knowing of each other. There is, we are working in parallel but it’s also there was some difference because Herbert Kroemer mostly was doing theoretical work and described some principles, makes the proposals. I did also proposal of the double heterostructure lasers and some other devices based on the heterostructures, but I am an experimentalist and first of all I started with the idea of the double heterostructure laser came to my mind, I first of all started to do that, to realise that. I was working at first just with technicians and engineers and the group would grow up, and we realised our ideas first of all, lasers and solar cells and heterostructure /- – -/ and other devices. But I should like to add to the comments of Herbert Kroemer, that It’s really he and I started with the proposals and consideration from the practical point of view, how to improve well known devices like transistor and then laser for instance, but what we did not expect it really, that from this heterostructure research just appear the new region of absolutely fundamental research which we call now low dimensional electro structures. It became possible only due to development of the heterostructures and successes in the technology of new heterostructures, so It’s always connected by this way, the basic fundamental results frequently came out from practical considerations, and vice versa.  Herbert Kroemer: Yes, but because they went beyond the original motivation, that where they created a new application ‒ this pendulum idea is a very good one. In a very real sense this has been true for science and technology for a very, very long time, that one creates the other, it’s a …  Jack Kilby: Although perhaps we are not very many solid-state physicists in the world, almost none before the invention of the transistor, a few thereafter. Today people working on integrated circuit problems are frequently solid-state physicists, so it has expanded the field and I suppose will continue to do so.  Herbert Kroemer: For example, if you look at the meetings of the American Physical Society, the annual meetings, the biggest single meeting is the March meeting which is, well, they now call it condensed matter physics but it’s really solid-state physics, dominated by solid state physics, so this is the biggest single area of activity for physicists as a profession.  Zhores Alferov: Right now if you look for instance for semi-conductor physic international conference which is going every two years, if you look to this conference for instance 20, 30 years ago there were just a few talks about heterostructures. If you look now, it’s practically 2/3 of the conference is the heterostructures and the devices and applications and so on.  Herbert Kroemer: It simply has become a universal ingredient in solid state technology, it’s a tool without which we could no longer do it.  Zhores Alferov: And then solid-state physics also.  Herbert Kroemer: Yes.  Jack Kilby: All the early work on transistors was done by people with very disparate backgrounds: chemists, physicists, electrical engineers. One of the best I knew had a degree in paper making, and all of these people were pulled into the activity and began to develop a common vocabulary and interest.  Zhores Alferov: I knew something about this very important discovery, transistor inventions in 1947, at Bell Telephone. I knew that from John Bardeen, who frequently visited the Ioffe Institute, and I met him the first time in 1960 at the Prague conference and also Nick Holonyak as a first pupil of John. He told me a lot, and what was very interesting in my opinion, that time it was a group created at the Bell Telephone in 1945, [Bill Shockley](https://www.nobelprize.org/prizes/physics/1956/shockley/facts/), [Walter Brattain](https://www.nobelprize.org/prizes/physics/1956/brattain/facts/), [John Bardeen](https://www.nobelprize.org/prizes/physics/1956/bardeen/facts/), Gerald Pearson, Gibney and a few technicians. Mervin Kelly, I think, was the vice president of the Bell Telephone, and he formulated as one of the tasks of this group just to check quantum theory for solid state, for condenser for solid state materials. I will be happy if now the vice president of the industrial companies, everywhere, in the United States, in Russia, would be so clever that put the task in an industrial laboratory to check some new principles in physics.  Herbert Kroemer: That was possible under Kelly at Bell Telephone Laboratories, I don’t think It’s possible anymore. |
| Q10 | **What about your corporation? Was it mostly cooperation or competition?** |
|  | Herbert Kroemer: I think it was competition, right?  Zhores Alferov: Yes.  Herbert Kroemer: But friendly competition.  Zhores Alferov: It was absolutely friendly competition, and sometimes it was just /- – -/.  Herbert Kroemer: Yes.  Zhores Alferov: I think in general the example of the work in heterostructures, because we had very strong competitions with Bell Telephone Laboratories, with some other laboratories, but it was during the Cold War time, end of 1960s, beginning of 70s, but we invited each other to the laboratories, we organised joint seminars, so it was just an example, I think. The physicists in general gave the example how to live in this complicated world together and enticing the American and Russian physicists during the World War II, during the Cold War ‒ during the World War II we were just on the same front ‒ but during the Cold War also gives the example for friendly competition and to show that we are representatives of science and we are together.  Herbert Kroemer: Yes.  Jack Kilby: I think another important effect was that as the field matured, as work became more expensive, in the early days you could literally build transistors in your garage and all the equipment that was required to do so. Today a modern semiconductor flat costs several billion dollars and it’s desirable for the industrial companies to share the development costs of those things. |