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| Podcast | “Asking is hard. Once you realise there’s an interesting question to develop answers to, it is even harder.” Growing up in Istanbul, Turkey, shaped Daron Acemoglu’s life and career in many ways. It sparked his interest in politics and social sciences and led to a research career investigating the differences in prosperity between nations.  Today Acemoglu is exploring the future of AI and how we can use it in the best possible way. In a conversation with Adam Smith he discusses his thoughts on the topic as well as sharing his advice for young researchers, including how to decide which research question to go for.  This conversation was published on 5 June, 2025. Podcast host AdDaron Acemoglu: One thing I tell my students is don’t listen to comments too much because if you listen to every comment that you receive from every economist and every seminar, then you are just going to be at the very average of everybody’s opinion, which is not an original place to be.  Adam Smith: I particularly like hearing Daron Acemoglu say that because of the story that his longtime collaborator [James Robinson](https://www.nobelprize.org/prizes/economic-sciences/2024/robinson/facts/) tells about the way they met. Robinson was giving a seminar at London School of Economics, and apparently Daron Acemoglu was sitting there in the front row asking endless questions and making innumerable comments. It was annoying Robinson. But afterwards they went out to dinner and became firm friends. Anyway, it’s absolutely fascinating to listen to Daron Acemoglu talk about his life, work and motivations. So please do join me for this conversation.  Karin Svensson: This is Nobel Prize Conversations, and our guest is Daron Acemoglu, recipient of the 2024 prize in economic sciences. He was awarded for studies of how institutions are formed and affect prosperity. He shared the prize with [Simon Johnson](https://www.nobelprize.org/prizes/economic-sciences/2024/johnson/facts/)and James Robinson. Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramon Areces. Daron Acemoglu is an institute professor at MIT and faculty co-director of MIT’s ‘Shaping the Future of Work Initiative’. He talks to Adam about who he thinks we should fear, the most powerful AI or powerful tech companies. Also, who’s missing from the table as we decide the future of AI in our lives. But first, how the current state of the world is giving our laureate the chance to test his theories in real time.  Smith: You were awarded the prize for your work, highlighting the importance of inclusive institutions. You are currently working on the future of democracy and our relationship with technology and what a time to be awarded the prize working on those things.  Acemoglu: Absolutely. Did they time it?  Adam Smith: I wonder if they did. Your banquet speech in particular that you gave at the Nobel Banquet, it was really a call to arms for people to wake up and get interested and if they weren’t paying attention to what was going on around us.  Acemoglu: Absolutely. Yes. I hope it was. Humanity has never seen corporations as powerful as the tech companies. Now those companies are choosing the direction of another extremely powerful technology. Whatever your view on other things may be. I think most people have an instinctive agreement with Lord Acton when he says absolute power corrupts. So we are in such a situation.  Smith: What’s it like right at the moment? We are recording this in late February. These are difficult times in the US for people like you.  Acemoglu: I would say that the US has not experienced, at least in my opinion, such difficult times for centuries. So these are indeed very formative moments. This is the kind of thing that we had in mind when James Robinson and I coined the term critical junctures in our book ‘Why Nations Fail’. Events during such episodes really place countries or even the world on different trajectories, and the world may look very different in 50 years time depending on what transpires today. I think a lot of the questions are institutional in nature.  Smith: Of course, you famously worked with natural experiments, the natural experiment of colonialism, and here you have a enormous natural experiment happening right in front of you.  Acemoglu: It might take a couple of years or a decade for social scientists to get the measure of this and write papers on this.  Smith: Yes. But in some ways, what a fabulous opportunity for study, what an opportunity to see things unsolved.  Acemoglu: Yes, that’s the thing. When you’re in the middle of it, you don’t see it as an opportunity for studying it, but you see it as a threat to your way of life and to what the world will hold.  Smith: It must be quite difficult to keep emotion out of it.  Acemoglu: Yes, I think it is. I think if I were younger it would’ve been perhaps even harder. But even now I find it difficult to be objective sometimes because these are things that will touch me. It’ll touch the lives of my children and lives of everybody around us.  Smith: In your banquet speech, you mentioned in particular the threats to democracy and the threats of big tech and the threat plus the opportunity of AI. If we take those apart, in terms of democracy and I know you’re working on this with James Robinson, I think you have a book coming up.  Acemoglu: Coming up would be amazingly generous. It’s like more like a twinkle in the eye. Yes.  Smith: I like the idea of twinkle in the eye. I’ve spoken to both James Robinson and Simon Johnson, and I think twinkle in the eye might well describe what happens between you when you sort of catch on to an idea that you’re going to follow up. Is that right? I like the idea of the kind of way you jointly find something that excites you both or all three of you.  Acemoglu: Correct. Yes.  Smith: Can you talk a little bit about how you feel democracy is under threatened? Also, how you feel it might need to be redefined?  Acemoglu: I think it’s one of the least well understood questions of social science. In many ways democracies have been more successful than authoritarian regimes over the last 70-80 years. But if you look at support for democracy, people’s satisfaction with democracy, it’s at an all time low. In other ways, there are more alarming signs. Especially the young are very polarised and do not want to compromise at all on pretty much anything, which is the kiss of death for democracy. Where we’re going to go from here is very unclear, and I think it’s a sort of an overdetermined problem. It is true; I think democracy has underperformed relative to the aspirations it set. it is true social media and other mass communication tools have complicated cross-cutting conversations. It is true that there have been disruptive changes due to technology and globalization over the last 40 years that could have downstream effects on democracy and trust in institutions. There are all sorts of factors at play here. We also suspect, I think many people do suspect, that many of these factors whatever their role are going to intensify over the next decade or so, more automation, more isolation and online existence, more echo chambers, more propaganda, more polarisation. Where will that leave us?  Smith: In a way, democracy fails in a truly polarised situation because you get this sort of situation we have now in the states where the slight majority feel that they’re getting what they want, perhaps, and the rest of the people feel they’re absolutely not getting what they want.  Acemoglu: Absolutely, 100%. But it’s not the fact that there’s a slight majority in favour of one party or another. That’s always been the case. The question is, once one party comes to power, how willing are they to compromise and how those who are out of power and those who are in power liaise and form agreements and make compromises. I think that’s the part that has become increasingly difficult as a result of polarisation and as a result of tribalisation, meaning that you see the other party as immortal enemy rather than just having some legitimate differences in ideology or policy. I think our very online existence has changed the way that we interact with our broader social networks and the way that we get information. All sorts of things have really complicated the nitty gritty of democratic politics.  Smith: I guess this will be the substance of the book you’re working on, but do you have any kind of suggestion of a formula for how we begin to reverse this?  Acemoglu: Not a silver bullet, but my belief (and that’s not the heart of the book but some part of the book) is that you have to identify the pathways, why some democratic coalitions work, strengthen them and also show that democracy delivers in terms of its higher level promises, like shared prosperity, good public services, voice to people. I think there are clear failures in the ability of democracy to have been able to do that over the last several decades. Pretty much everywhere in the world, but especially in the United States and parts of Western Europe. In some sense, for a long time it looked like Nordic countries were avoiding this. But now the Nordic countries are also subject to the same trends.  Smith: I suppose in the mention of shared prosperity takes us perhaps into the realms of technology because there is a general belief held by many that somehow technology is the silver bullet.  Acemoglu: Oh you wouldn’t believe it! If you come to the US you’ll see that much more intensely. The US is a deeply techno optimistic country.  Smith: What does the historical record say? You’ve studied this in detail, especially in your book ‘Power and Progress’. What is it that we know about technology and shared prosperity?  Acemoglu: I think the history says the world is complicated context, nature of technologies, how they are used, how they are developed. They all matter. But one thing is very clear that there isn’t a universal law that good technologies will give us good outcomes. There are many instances in which better communication technologies have led to worse information. There are many cases in which technologies that have increased productivity have deepened poverty, reduced wages and increased inequality. So really the details of how technology is developed, who is empowered, what regulatory and other countervailing forces there are in society matter greatly.  Smith: Do you think that there’s a general understanding of how technology can lead to general prosperity?  Acemoglu: Yes, there is. I think economists have that mostly right; that if you’re gonna get general prosperity from technological changes, it must work largely via the labour market. Meaning improving labor market fortunes, wages and employment opportunities for people. The mechanism for that in economics is quite clear and sometimes works quite well, which is that when technology improves our capabilities, firms demand more labour that then pushes up wages and then workers doing various different tasks get higher wages.  Smith: The fear now is that we are not going in that direction. Technology is taking us in a different direction where there are fewer jobs.  Acemoglu: Yes. The fear now is that, but it’s not just this time is different. It’s always been so. The pathways that I have just summarised are not universal. There are many reasons why they may not work. This is the heart of the theory of the power and progress book that I co-wrote, which Simon Johnson. First of all, it may well be the case that labour markets are not competitive and higher labour demand doesn’t translate into higher wages. For example, think of cotton production in late 18th century United States, US South, where the cotton gin and other improvements led to huge productivity in cotton production. But because most of the workers who did that cotton production were slaves, there were no forces to ensure that they shared in the vast fortunes that people made. In fact, their living conditions and their real wages probably worsened during the intensification of that plantation economy. There are many other examples like this. In addition, and I think this is an even more fundamental point. The presumption that greater capabilities via technology will necessarily lead to more labour demand from firms, meaning that firms would want to hire more workers, is simply not true. It depends on what type of technologies we’re speaking of. If technologies are focused on automation, meaning increasing productivity by eliminating labour and substituting cheaper machinery or algorithms for them, then productivity will grow. Our capabilities will grow, our output will grow, but demand for labour will decline because we have fewer users for labour. That’s what happened in the early phases of the industrial revolution that started in England in the middle of the 18th century. That’s what happened over the last few decades in the US for example, with digitalisation and AI promises to go down the same path.  Acemoglu: We have to automate work, but at the same time make plumbers better plumbers, electricians better electricians, educators better educators, and create new occupations that we can’t even dream of at the moment. That’s not something like mana from heaven that immediately descends because we declare, we have powerful AI that comes out of a vision, a desire to develop tools to make labour more productive. That’s what’s missing. It is in that context that we need more inclusion. I argue, for example, if labour was at the table, then labour wouldn’t be enthusiastic about automation and more automation. They would say, ‘Well, can we not use these tools for making more workers more productive and more important for the production process?’ That’s the pathway to get to a more more balanced portfolio of technologies.  Smith: It is basically a collective imagination or tapping into the collective imagination about the sort of society we want.  Acemoglu: Absolutely. That’s the first step. That’s why I think conversations like that are important because most people don’t even realise that there is a socially beneficial and technically feasible different direction of AI that would give such better outcomes. That’s the collective imagination. But imagining isn’t enough. Different people have different imaginations, different people have different incentives. In the current environment, it’s not your imagination or mine that matter, but it’s what the tech bosses desire. That’s the sense in which, if we can bring more people to a table that might even the scales when it comes to whose interests are going to be favoured with these decisions.  Smith: So much of this debate seems to be centered in the huge rich countries. If you’re listening to this conversation from somewhere that isn’t a rich country, that doesn’t have a big stake in the development of these technologies, what can you do?  Acemoglu: First of all, most of the discussion (including in my book and in my writing) focuses on that precisely because we are in the pitiful state where there isn’t a voice from the developing countries. That is both because of omission and commission. There are no international organisations that speak for the developing world. That’s the commission. We haven’t really helped them have that voice and omission on their part that very few of the emerging economies actually prioritise this. If India, Mexico, Brazil, Turkey, Indonesia prioritised this and wanted to have a voice and they were speaking in unison, their voices would have a better chance of being heard. But they’re not, they’re focused on much more mundane, shorter term political issues.  Acemoglu: Necessities. Sometimes they’re all authoritarian paths and everything. But the other thing is that actually there is a lot of commonality between what is good for the American worker and what would be good for India. Why? Because if the direction is between automation on the one hand and more pro worker things that amplify the skills and expertise of workers of different backgrounds helps them perform more sophisticated or newer tasks, especially enables opportunities for lower education workers. That’s the one that would reduce inequality and increase real wages in the United States and help contribute to shared prosperity. That’s the one that would also help India, Indonesia, Mexico etc because these countries are abundant in middle and low skill labour and any development path that doesn’t rely on that abundant factor is not going to work.  Svensson: Adam, you’ve mentioned the book ‘Power and Progress’ that Daron Acemoglu wrote together with James Robinson a couple of times during this conversation, which was the main takeaway for you from reading that book?  Smith: Essentially the same point that he was making in his banquet speech during Nobel week, which is that it’s blatantly ridiculous to assume that a few top executives at the world’s leading tech companies should be responsible for deciding how we’re gonna coexist with technology in the future.  Svensson: Someone had to say it.  Smith: Yes. He’s not alone in saying it. But the book uses lots of examples to emphasise that there isn’t a correlation necessarily between new technologies and a rise in living standards, that very often things go in the opposite direction for a while. After time things begin to improve. It tends to be (the book argues) that people need to get involved much more broadly in working out how a technology can benefit society more generally.  Svensson: Can you give an example of that?  Smith: Yes, for instance, close to home for me is the industrial revolution in Britain and in a section of the book called Less Pay for More Work they analyse how the initial long lasting effect of the industrial revolution in the UK was to make working conditions worse for everybody. It was only after human creativity was allowed to come into it, and people began to have more input into how to coexist with these new technologies and how the special things that humans bring could flourish together with these technologies that things began to improve. That’s the story again and again. It’s not about automation, it’s about finding ways to use automation to make other things flourish.  Svensson: It seems like there’s a lot of work ahead of us.  Smith: Yes. But I think they think it’s a hopeful message that there are lots of models to follow for how to do this. But it takes everybody to get involved in the conversation and to move away from this model of just assuming that tech profits are somehow gonna lead to progress.  Svensson: That’s sort of been an axiom of economics, hasn’t it? That the free markets will solve everything.  Smith: Maybe, I don’t want to put words into Daron Acemoglu’s mouth, but you might say the free market combined with human creativity is the best path forward.  Svensson: What was he awarded the economics prize for?  Smith: He, Simon Johnson and James Robinson I suppose broadly looked at history and applied maths. They took this natural experiment in economics of the period of colonialisation and studied how countries had fared in the wake of being colonised. Broadly they showed that countries that had had slightly more attention paid to them by the colonial power in building institutions and making the apparatus of state did better in the end than countries where the colonial power had simply exploited the resources of the country and not built infrastructure there. Their conclusion was that building inclusive institutions can contribute to the prosperity of a country.  Svensson: Daron Acemoglu himself has got an interesting background, sort of ties into all this.  Smith: Yes, indeed. He was born in Turkey, he studied in the UK and then he ended up in the United States. That’s so common among laureates that they have these paths of migration and have benefited from different inputs along the way. That’s constantly emphasised when you talk to laureates, that either by moving or just by talking, it’s so important to expose yourself to other points of view.  Svensson: Particularly growing up in Turkey during a specific time also had an influence, didn’t it?  Smith: Actually, yes. Let’s listen to him talk about how growing up in Istanbul sparked an interest in politics and economics.  Acemoglu: Turkey was going through a variety of political problems in the aftermath of a military coup and a controlled transition to democracy, a lot of political repression and people being imprisoned for their political views. It was also undergoing a lot of economic problems, high unemployment, high inflation, low growth. Just idly, I was wondering whether the two were related. That’s what I wanted to study and that’s what I wanted to go into economics. Little did I realise that that’s not what economics was about for the most part, but I remained anchored to those questions even as I became interested in other parts of economics. Then I returned to them during the second half of my PhD work. That’s become my passion as well.  Smith: As a teenager seeing this around you, were you an activist or was it more of an intellectual question at that stage?  Acemoglu: I was interested in politics and became involved a little bit in politics, but then I found that involvement not very rewarding for a variety of reasons. It became more of a thought process rather than an activity process.  Smith: Then you decided to go and study in York in the UK rather than studying in Turkey?  Acemoglu: Yes. What a crazy idea, right?  Smith: Weather wise, it’s quite a decision.  Acemoglu: Yes. It’s much warmer than where I am right now. The gradient is not pointing in the right direction in terms of weather. Where are you from?  Smith: I’m from London.  Acemoglu: A little better.  Smith: Not much of a big difference.  Acemoglu: York is beautiful. I have only good things to say about York.  Smith: Still the question is, if you’re deciding to leave Istanbul and Turkey. What led you there?  Acemoglu: I decided to leave Istanbul and Turkey because I wanted to study these issues and I didn’t think the university’s in Turkey were offering the best academic environment. Secondly, I wasn’t happy about the political atmosphere and lack of freedom of speech. That was the impetus for me leaving. So both the academic and the personal. And then, where was I going to go? Essentially there were two attractors. One being France because I had gone to a French school but the little I knew didn’t fill me with great confidence that the education I would get in France, except perhaps in the Grand Ecole where it was harder to get into coming from Turkey would be excellent. The second option was the US which even then it was clear that was just like the center of gravity for a lot of academic thing but it was very far from Turkey. It was just one step too many. So the UK ended up as like the compromise solution.  Smith: That makes sense. Did it fulfill your hopes?  Acemoglu: Yes, I was very lucky that I ended up at York. I always tell people I learned all the economics I’ve been using for the last 30 some years at York. Don’t tell my LSE Friends and teachers. No, it was great. I became really passionate about economics and it was a great environment.  Smith: When speaking to James Robinson about your meeting at LSE, he said what you’ve shared in common was a realisation that at the end of your PhDs at LSE, you still didn’t really know anything. I love that. One instruction you’re given at the beginning of your PhD is you’re supposed to become the world expert in this topic by the end of three years or whatever. But how lovely to recognise that not only you’re not the world expert, there’s a lot more to find out.  Acemoglu: I would still say that would’ve been true as a general statement, but it was also a specific statement that when it came to understanding the causes of big growth differences or big differences in prosperity or economic development, we didn’t know much because we didn’t focus on the right questions.  Smith: What’s your secret for finding the right question?  Acemoglu: Luck more than anything else. I think you have to be a little irreverent. If your gut instinct tells you to ask certain things that are not asked, don’t shy away from them. But other than that, your gut instinct is mostly luck.  Smith: As you mentioned, your journey has taken you from Turkey to the UK to the US and other places in between. But what it hasn’t done is taken you back to Turkey. I just wondered, given that Turkey sort of ignited your interest, whether that is something that plays on you, that you are a product of Turkey.  Acemoglu: Meaning going back to Turkey in terms of living there or going back to Turkey to study Turkey?  Smith: To study Turkey and to participate in the academic circles of Turkey.  Acemoglu: I am not completely detached from the academic circles in Turkey. I have written one or two pieces about Turkey and I follow Turkey. But in some level, being close to somewhere is both an advantage and a disadvantage. When you’re closer, you understand the details, but it’s also perhaps sometimes harder to abstract from those details. I often find myself applying theories and conceptual frameworks that I have developed, sometimes inspired by my experience in Turkey, but developed much more generally at the world level or more for the US or Western Europe, or for other emerging economies. Then I apply it back to Turkey rather than sort of do an in-depth study of Turkey. But one day, if I have enough time, which probably will never happen, I might do the latter too.  Smith: But I suppose one thing that interests me is the outside perspective. Of course you’re not an outsider in the US in any way anymore.  Acemoglu: Oh, I’m in some ways.  Smith: It is so important again and again, this migration of mind, this exchange of populations that goes on, especially within academic circles and particularly important now again at this particular moment in time, it is good to emphasise that and talk about it.  Acemoglu: What’s important is the traveling of ideas and new ideas and new perspectives. If that happened, would people stay put in their place like it was during the Renaissance? That will be fine too. But often it’s easier when people travel too.  Smith: What defines whether something’s worth sticking with for you?  Acemoglu: No formula. Sometimes you stick with things that don’t pay off. You have to be prepared for that as well. That’s where the stubbornness comes in. One thing I tell my students is don’t listen to comments too much because if you listen to every comment that you receive from every economist and every seminar, then you’re just going to be at the very average of everybody’s opinion, which is not an original place to be.  Smith: It needs a lot of confidence to keep going.  Acemoglu: If you think about it that way, if you step back and say, ‘Oh, well everybody disagrees with me, but I believe in myself’, that would require a lot of confidence. But if you approach it playfully and say, ‘I want to explore this’, that could be more realistic.  Smith: One of the things that characterises you is you ask questions and you have always asked questions. That does require a boldness. It requires an interest.  Acemoglu: Yes. I think I asked too many questions as a kid too.  Smith: Did anybody tell you so?  Acemoglu: Yes, they did. Especially when they didn’t know the answers.  Smith: Yes, exactly.  Acemoglu: But adults don’t like it.  Smith: Yes, exactly. But still, it is something that people find hard to really be bold enough to keep asking.  Acemoglu: Asking is hard. Once you realise there’s an interesting question to develop answers to, it is even harder. That’s where you need a lot of work. I love writing articles and I love that I have written books, but writing books is a lot of work. That’s because you have to develop answers (at least provisional answers) to some big questions in a multi-dimensional and multi-faceted way. That’s a lot of work.  Smith: It’s a lot of work, it’s a different audience and it’s a different style completely.  Acemoglu: I like the style. The thing that’s hard with books is you don’t see the end of the tunnel. You have to start working hard, recognising you might need to work hard for another two years. It’s not like an article where you say, ‘Okay, I know I’m going to write the introduction and then everything else is ready’. That’s easier.  Smith: Is that why you write with people?  Acemoglu: Yes. I think research is already solitary. Writing books is even more solitary.  Smith: But learning to share ideas with people is not, for some people it comes very naturally. But to have these close relationships that you do as you write a book together, batting ideas off each other, that’s a partial description. But how would you describe those relationships?  Acemoglu: Yes, that’s exactly it. But I had decades of relationship to these people before starting to write a book. We had written several papers, so that helps. I think it would be much harder to write a book with somebody you haven’t collaborated with before. I think sharing has to be natural and organic, and you can only do that if you know the other person well enough.  Smith: Any prescription for what to do about AI? It was the subject of a much debate during Nobel Week. You had amongst the laureates, rather different views of what to worry about with AI.  Acemoglu: Yes. I think some thought there was nothing to worry about it. Some thought it was super intelligence that was oncoming, that should be the real worry. I worried about the things that not so super intelligent AI can do in the hands of whoever controls it at the moment.  Smith: Do you worry about super intelligent AI? Or is that just something too far away?  Acemoglu: I’m not in the middle of developing the large language models and the associated generative AI tools. I keep hearing from people at the center and at the periphery of those efforts that AGI is on the horizon. I cannot completely ignore it. On the other hand, instinctively I still don’t believe it. That’s because my understanding of human intelligence is sufficiently multifaceted. That one simple architecture based on the foundation models and next word prediction and souped up with chain of reasoning or other sort of tricks of machine learning, doesn’t seem like it could have the multifaceted aspect of human intelligence. But the proof in the pudding, we’ll see. But in the meantime, I think we have a lot of other things to worry about. Even if we are headed towards super intelligence, I would still worry about the things that I’m worrying about right now for the simple reason that before we get to super intelligence where AI itself can do damage to us, there’s going to be a pretty lengthy period, years at least, where AI tools are super productive and super powerful. Even if that’s super intelligent and somebody controls them and that person controlling them can do a lot of damage to us.  Smith: The question of what it means to be human living with AI works across the spectrum from…  Acemoglu: It does. But AI is a tool in the hands of some people that can be used against other people directly or indirectly, is always present. That’s why I started by saying, you have to worry about the fact that these tools are in the hands of the most powerful corporations humanity has ever seen. If you have immediate size corporation that has a very powerful technology in its hands, that company is subject to a lot of checks, regulations and barriers. Not so with the richest companies in the world.  Smith: Do you think in some ways the conversation about super intelligent general AI is obscuring the conversation about the current threats from AI?  Acemoglu: 100%. Yes, that’s what I’ve been arguing. That’s why I don’t want to get engaged so much with the super intelligent, because the moment you get engaged (I may be wrong, I may be right) but that’s not the conversation that I think we should be having right now, because before we get there, we could do untold damage.  Smith: Oh dear. That’s rather a doom scenario to end on.  Acemoglu: Yes. But it’s not a hopeless scenario because part of what I’m saying also is that there is a direction of AI that’s much more beneficial. We can use AI to provide greater privacy, better communication possibilities, better ways for people to protect their data, to uncover lies that they are being told. We can create AI that makes workers more productive rather than just automate their work. There are directions, it’s just that we’re not heading there. That’s the pessimism or that’s the gloomy aspect. If you wanted a Cinderella story where the good is going to triumph over evil ultimately. There’s no guarantee.  Smith: I suppose, as you said in your banquet speech, it’s up to all of us.  Acemoglu: Yes, exactly. That’s the message I would like to convey.  Smith: Good. Thank you very much indeed.  Acemoglu: Thank you, Adam. This is a great conversation.  Svensson: You just heard Nobel Prize Conversations. If you’d like to learn more about Daron Acemoglu, 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 like how economic sciences laureates often seem to work in pairs and packs, why not listen to our earlier episodes with 2022 laureates, [Douglas Diamond](https://www.nobelprize.org/prizes/economic-sciences/2022/diamond/podcast/) and [Philip Dybvig](https://www.nobelprize.org/prizes/economic-sciences/2022/dybvig/podcast/). You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| Telephone  interview | 0800=DA  Daron Acemoglu: Hi.  Adam Smith: Hi, is this Daron?  DA: Yes, it’s me. Is it Adam Smith?  AS: It is. This is Adam Smith. Hi.  DA: Wonderful. Great talking to you. Very wonderful.  AS: Many, many congratulations.  DA: Thank you. It’s amazing news.  AS: I imagine that you are pretty busy most of the time, but today must be just extraordinary,  DA: A lot of phone calls, that’s right.  AS: Where were you when the news reached you?  DA: I’m actually in Athens right now. I had just given a talk and I had some press interviews, and then I went to my hotel room. I was sitting on the balcony. Then I got an e-mail from Per Krusell asking for my phone number. So that’s how I got some idea that whether this is about something, and then the office called me.  AS: Nice news, and I imagine a nice view to go along with it.  DA: Yes exactly. It was a great view of the Riviera, although by the time I was talking to them, I was inside my room, concentrating, and I was like, what are they gonna say?  AS: Who was the first person you told?  DA: I couldn’t tell my wife, because she’s asleep in Boston. I called her after the press conference, but she was still asleep. I just talked to her now.  AS: She must be happy.  DA: Yes, she’s happy. Delighted.  AS: Of course. People have spoken about you being awarded the prize for quite a time, so, you must have thought this moment may come.  DA: You know, you never dream of such thing. Or you can dream, but you never expect such things, let’s say. So it’s a wonderful event.  AS: You have worked on the institutional drivers of prosperity and the differences between rich and poor nations, and that makes us stop and think about those differences. And are you amazed by just how large the difference is between the rich and poor nations?  DA: I am of course amazed. That’s why I started working on these topics. Once I started looking at the data and reading what other social scientists were already working on in the 1990’s, as I was finishing my dissertation at the LSE, I just got so interested in these topics because if a country is 50% richer than another one, you might say, well, perhaps that’s natural. They have some resources or some other advantages, but there’s nothing natural about 30-, 40-, 50- fold differences in income per capita in a globalised, connected world.  AS: Is there anything one can say about why some countries are trapped in poverty or seem to be trapped?  DA: Essentially, the way that in my work with Jim Robinson and with Simon Johnson, the way that I like to sort of break that down is we can try to understand that via sort of proximate causes of economic development. Differences in education, differences in efficiency with which you use things, differences in the amount of machinery you have and some other important factors, but then you go one layer down, and that’s where we think that institutional factors are the most dominant. Of course, other things influence human capital, other things influence efficiency. But institutions, especially your broad institutional trajectory over time is a major determinant.  Then you of course have to ask about what it is that makes countries end up with bad versus good institutions, and why do they stick with those institutions? That’s some of the issues that I try to explore in my work. Trying to model the choice of democracy and why dictatorships or other bad institutions survive, and also look in my work with Simon Johnson and Jim Robinson at the colonial origins of these institutional differences, because the colonial experiment, which started 500 years or so ago, was really a transformative one for about half of or more than half of the world. It really changed deeply their institutional trajectories.  Moreover, it’s not just like one size fits all. There was so much variation within the colonial world and the types of institutions that took root for a variety of reasons. So we really wanted to understand and analyse these. The one factor that at first we focused on, although later we looked at other things like population density and other things.  But the one factor was the disease environment, because that was rather exogenous to Europeans and rather stark because they did not have immunity to some diseases. And we tried to explore the pathways that went from the disease environment facing Europeans to how that affected their early colonization efforts, and how that ended, that led to very different institutional trajectories, which then persisted and shaped a variety of economic incentives throughout the 19th and 20th centuries.  AS: It’s a natural experiment, which proved to be such fruitful territory to explore, didn’t it?  DA: Thank you, thank you for saying that.  AS: One crucial thing that comes out of it, and which you’ve very much worked on, is how institution building can function most efficiently.  DA: Correct.  AS: The conclusion one would like to draw, especially in today’s world where democracy is talked about so much, is that democracy is an absolutely key thing to institute. But it’s obviously a bit simplistic.  DA: It is simplistic in the following sense that you cannot institute democracy from above. It’s a very difficult process. And democracy is not the only dimension of institutions that matters. Although my work does emphasise that democracy by itself matters as well. But the problem is really alive today in the industrialised world, where you would think, and many political scientists have claimed that democracy is safe and secure and would never be under threat. It is under threat. Support for democracy is at an all time low, not just in the US, although really jarringly in the US but not just in the US, throughout the western world.  So democracy is not easy to make work because democracy is about democratic citizenship. It’s about consensus, it’s about communication. It’s about accepting defeat, making compromises, talking and understanding the other side. All of those things are always difficult. They become more difficult during times of turbulence, which, you know, we are living through and they become harder when the infrastructure, for example, the communication infrastructure, makes this sort of democratic citizenship harder, which I think is not the only factor. But social media has certainly played that role.  AS: Yes. Your recent work, especially with Simon Johnson on the role of technology, who controls it, who benefits from what it brings, plays into this absolutely. Picking up on a theme that came up with last week’s prizes, on [Geoff Hinton](https://www.nobelprize.org/prizes/physics/2024/hinton/facts/) in particular, his fears of AI: what’s your greatest fear?  DA: I have so many, but I definitely fear for democracy. Because I am convinced that democracy is a pretty good system, considering the alternative, as [Winston Churchill](https://www.nobelprize.org/prizes/literature/1953/churchill/facts/) said, it is the best one that equips us to deal with the turbulent times. I am really worried about support for democracy and often I view it as a self-inflicted pain, that democracy and democratic parties have not always delivered on the promises of democracy, especially in terms of inclusivity, in terms of clean government, in terms of shared prosperity.  But I do also worry about AI, not in the way that Geoff Hinton does, not worried at all about super intelligent AI. I’m worried about dumb AI because I think there is great potential there. And if we don’t use it or if we use it in the wrong way, I think a) it will be lost potential. But even more importantly, I think if used the wrong way, it will be a major contributor to further inequality, further weakening of democracy with data collection and manipulation by some actors. And it would really contribute to, the emergence of a two-tier society, which I think we are already starting to suffer from.  AS: Thank you very much indeed. We will get to talk about all this greater length, once everything has died down a bit. I just wanted to close by asking about, you know, you work on the difference between different nations and you yourself are sort of concatenation of different nations.  DA: I am indeed.  AS: Armenian heritage, Turkish born now living in the States.  DA: Also educated in the UK, so quite a bit of mixture there.  AS: A lovely mix.  DA: I’m proud of all my heritages, and I’m delighted that I have been able to learn from many different experiences.  AS: Do you think you’ll get a chance to celebrate, or is it just going to be phone calls all day?  DA: Of course. I’m right now in Athens, I’ll be on my way to Boston tomorrow morning and I’ll celebrate with my family tomorrow evening.  AS: Lovely. Thank you very much indeed. Lovely speaking to you.  DA: Thank you. Bye, bye.  AS: Thank you, Daron. Bye, bye. |
| Interview |  |
| Q1 | Where does your passion for economic sciences come from? |
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| Q20 | How was your childhood in Istanbul? |
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| Q10 | What is your advice for young researchers? |
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| Q4 | Tell us about your motto “Swing for the fences.” |
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| Q11 | Have you encountered any failures or mistakes in your career? |
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| Q31 | Do you ever get imposter syndrome? How should one handle that? |
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| Q47 | What has contributed to the successful collaboration with your co-laureates? |
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| Q42 | Is diversity important in the field of economic sciences? |
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| Q5 | When do you get your best ideas? |
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| Q24 | How do you think humans should use AI? |
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| Q18 | How important is it to use science to address the issue of poverty? |
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| Q18 | What are the most urgent threats to democracy? |
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| Biographical |  |
| Autobiographical |  |
| Podcast | “With the great power to invent technology comes a great responsibility.” In this lively and energetic podcast conversation, economic sciences laureate Simon Johnson talks about how the past, future and present are interconnected, as well as how science fiction and history are intertwined. He comes to the conclusion that “science fiction is history in reverse or history is science fiction in reverse, whichever way you want to think about it.”  He also tells us about his family history and how his family was part of the steel industry in Sheffield, England. The industrial revolution is discussed as well as the responsibility that comes with inventing technology.  This conversation was published on 3 July, 2025. Podcast host Adam Smith is joined by Karin Svensson.  Simon Johnson: It’s not that history is predetermination or something that was done to you or your country 200 years ago, means you’ll always be rich or you’ll always be poor. But it does matter. It has had a long lasting effect. It’s pretty helpful to understand that context.  Adam Smith: Simon Johnson’s work points to the great importance of setting the correct foundations on which to build prosperity and such foundation to take great time and effort to put together, but they can be dismantled rather easily. In this conversation he goes on to talk about the real danger of that happening in especially his adopted country of the United States. He also talks about the way his background has shaped his approach to work and the importance of fantasy, how productive it can be to get out of your current way of thinking if you’re going to make progress. Please join me for this conversation with Simon Johnson. I hope you’ll enjoy it.  Karin Svensson: This is Nobel Prize Conversations and our guest is Simon Johnson, recipient of the 2024 prize in economic sciences. He was awarded for studies of how institutions are formed and affect prosperity. He shared the prize with [Daron Acemoglu](https://www.nobelprize.org/prizes/economic-sciences/2024/acemoglu/facts/) and [James Robinson](https://www.nobelprize.org/prizes/economic-sciences/2024/robinson/facts/). Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramon Areces. Simon Johnson is the Ronald A Kurtz professor of entrepreneurship at the MIT Sloan School of Management. He talks to Adam about why explaining science in simple terms doesn’t have to mean dumbing it down, why he’s turned to science fiction to solve the problems of tomorrow, and which fellow laureates he thinks will take home another three Nobel Prizes. But first they discuss what it’s like for Adam to share a name with a father of capitalism.  ohnson: The first call I got from Stockholm that was conveying this official information was actually from you. When I tell my audiences that Adam Smith called me to tell me I’d won the Nobel Prize in economics, most of my economic audiences just go wild with appreciation of applause. So well done.  Smith: Well, this ridiculous name is good for something then.  Johnson: When I was in Stockholm I learned that the biology profession, the distinguished team for instance, prank each other routinely on Nobel Day. They call each other, they put on fake Swedish accents and they pretend that somebody’s won a prize. It’s a little bit out of control from what I hear, but economists don’t do that yet. Let’s hope we don’t start something.  Smith: It may come. But yes, there’s a lot of questions for validation reasons. Occasionally I have been in the situation of getting through to somebody and having to break the news that it has just been announced in Stockholm, that they’ve been awarded the Nobel Prize. That actually makes for an awfully boring interview because of course all that happens then is the person keeps saying, who are you again? And tell me again. Once again, who was that? So from that moment to now, quite a lot has happened. You’ve had the experience of being, I suppose, pushed more into the limelight by the announcement. Also you’ve been to Sweden. How has it all been?  Johnson: Being in Stockholm was amazing. It felt like being treated like minor European royalty 150 years ago. A little bit frozen in time, but with this celebration of science and the delightful idea that economics is a science which most people around the world would put up some resistance to, but not in Stockholm, not in Sweden. That week we were riding in the cavalcade with the biologists, the physicists, the chemists and of course the winner of the literature prize, it was just a fantastic honour. My wife, my two daughters came, my brother-in-law and his family, I’ve written a couple of books with my brother-in-law actually, and a number of work colleagues, including my boss’s boss, the president of MIT, very graciously agreed to come with me, which was really fun for me and my family. I think she enjoyed it too. I think it was a bit of a holiday and a sort of appropriate outing for the president of MIT. It’s a very joyous, celebratory moment, not just for the individuals, but also for science, the scientific agenda. The idea of progress can be made through research and through carefully studying things in engagement with the world. That’s a lot of what MIT stands for too.  Smith: You like writing books, don’t you? I mean papers obviously as an academic. What is it about the book form that you particularly like?  Johnson: The book form allows you to tell a more complete story. Papers make you focus and drill down into a topic. But with a book you can be more expansive and the books that I’ve written with various coauthors, we really try very hard not to dumb it down. We are not taking anything out of the analysis and anything out of the economics, politics and other disciplines we’re drawing on. But we are taking it upon ourselves to communicate in language that everybody can understand, something everybody who reads a newspaper can potentially understand these books. I think more specialists should do that. I think when you only write for a technical audience and you only write for people who use the same language, all the same jargon that you do, it is too easy to fall into certain ways of thinking as well as speaking that don’t necessarily force you to confront all the realities that you face. My books, I take them on the road, I go talk to audiences, I talk to lots of different kinds of people and I’ve got to be ready to answer their questions. I think that’s an important thing for experts to do, experts of all kinds, not just in economics.  Smith: That’s very interesting, they famously quote [Rutherford](https://www.nobelprize.org/prizes/chemistry/1908/rutherford/facts/); “I think as saying that you didn’t really understand your work unless you could explain it to your tobacconist” as he put it back at the beginning of the 20th century. It’s an important test of your own understanding that you can tell it honestly to an audience who don’t know any of the jargon.  Johnson: While we’re quoting people who may or may not have actually said things, I think [Einstein](https://www.nobelprize.org/prizes/physics/1921/einstein/facts/) said, “If you can’t explain an idea in simple terms, either you don’t understand the idea or the idea is wrong” but perhaps he never met Rutherford’s tobacconist. So we could caveat that way.  Smith: Lovely picture of Einstein and Rutherford trying to explain things to each other in simple terms. Who knows how it would’ve gone?  Johnson: Probably a Tom Stoppard play in there somewhere.  Smith: One of the books you’ve written recently has been about technology and progress and how it connects with the fate of us all. AI was very much a theme in Stockholm this year. The physics prize, chemistry prize and to an extent your prize touched on it. There were wildly different views of AI expressed during Nobel week. Geoffrey Hinton was giving dire warnings about it, and the folks from Google DeepMind were giving a much more positive view of how AI would transform life. It’s a ridiculously big question, but do you have a position on what you think AI is going to do for us all?  Johnson: Yes, and actually I think the split that you’re picking up, the two sides of it are about right. The biology winners, by the way, who didn’t win it for AI, but I observed a conversation between them and [John Jumper](https://www.nobelprize.org/prizes/chemistry/2024/jumper/facts/) who won part of the chemistry prize, John Jumper being a co-inventor of AlphaFold 2, they were talking about how AlphaFold 2 will be used to transform biology research in profound ways. I understood nothing at all about the technicalities of that conversation, but I could see from the body language that this was an amazing connection. John Jumper is a physicist who just won the chemistry prize and is gonna transform biology. I told him, I’m looking for a bookie to take my bet that he’ll the first person ever to win three or maybe four Nobel Prizes because he’s only 38 years old. I think that the technological transformation piece that the DeepMind people, [Demis Hassabis](https://www.nobelprize.org/prizes/chemistry/2024/hassabis/facts/) and John Jumper are all about that’s real and compelling and is actually happening for drug discovery. At the same time, there is a dark sided downside. [Geoffrey Hinton](https://www.nobelprize.org/prizes/physics/2024/hinton/facts/) laid out the pretty apocalyptic version, which may also be true with a version that concerns me and Daron Acemoglu. We wrote about this in our book, ‘Power and Progress: Our Thousand-Year Struggle Over Technology and Prosperity’. There we asked what happens to jobs, what happens to good paying jobs. In our read of history, there have been episodes in which automation has been super exciting for people. People with power have wanted to adopt machines. They wiped out a ton of jobs and hammered the working people for a long time. There’s progress of a kind, but you don’t really share prosperity. I think there’s a real risk that AI could go down that road, but it could also go down the Hassabis Jumper positive road. Which way are we gonna go? Who decides? How do we know which road we’re on? That’s actually our take of where we are at this moment.  Smith: As I understand it, the who decides is the absolute key piece that who decides has to be inclusive, has to involve as large a section of society as possible in order to really realise the benefits of any technology. But in particular this one.  Johnson: That’s our view and our position and that’s the link to the research that won that Nobel Prize because we argue that if you take long historical experiences and you look at what happened within European colonisation, specifically places that received for various reasons, more inclusive institutions, and by the way, wasn’t inclusive for everyone, but it was inclusive for quite a lot of people. When that inclusion expanded over time, they got better economic outcomes, they got more development technology and that technology was more likely to help more people. Places that got really extractive institutions, a few people are super rich and they can tell everyone else what to do and force other people to do what they want. Those places have actually done less well in terms of innovation and prosperity. That is exactly the sort of mapping or the relationship to AI. Is AI something that is going to the development? It is going to include a lot of people or is it gonna be a few people using this technology to tell you what to do? I like to tell my audiences, I like economics, but I love science fiction. I read a lot of science fiction and with a lot of science fiction it’s one way or the other, right? You’ve got your dystopia with a few people in charge, the technology driving things, and then you have more positive outcomes. I think that the jury is out on that, Adam, honestly, but it won’t stay out for long.  Smith: You mentioned science fiction and your love of science fiction. That’s interesting. I suppose it raises the question of what part dreaming and imagination plays in your work. Are they connected? Is your love of science fiction part of what makes you good at doing your work?  Johnson: I like to think so. That would justify me reading a lot of science fiction novels. I think what I really like that’s linked to research is actually history. I love to read history and of course there’s a cliche, history is a different world, a different life because you go back and you look at what actually happened in the 1940s, the 1840s or the 1740s, and it’s absurd, right? From a modern perspective, how did they live like that? Those attitudes were unbelievable. When people look back at the 2020s from the future, they’re gonna say, well that was absurd. These people are absurd. If we can look forward into the future and imagine the future, that’s gonna be absurd to us too today, right? Where do we sit? How do we think about what’s gonna happen going forward? It’s not going to be like today, and this is actually why economists are so bad at forecasting the future because we’re extremely responsible, careful people. We don’t want our colleagues to think that we’re dilettantes. So we say, it will go a little bit this way, a little bit that way. History says it changes a lot, including profoundly at all kinds of levels of society and individuals and so on but which way is it going to go? Economics has no idea about that. Isaac Asimov on the other hand, he had some ideas.  Smith: If you are recommending science fiction to somebody, would you say read Asimov?  Johnson: I say to a lot of my audiences, if you’re only going to read one book after you’ve heard me speak, you should read my book ‘Power and Progress’ by me and Daron. But if you want to read a second book, you should read ‘Play a Piano’ by Kurt Vonnegut. Vonnegut’s first full length novel, which I think he wrote at the end of the 1940s, early 1950s, in which he imagined a world in where automation had taken over. There were two classes, the highly paid, highly educated engineer class and everybody else. Everybody else is, spoiler alert, but they’re not doing that well. What’s brilliant about it is Vonnegut basically took a theory of economics and a theory of technology and applied it to his read of modern American society and he rolled the clock forward, right? This is what people like Neil Stevenson or Kim Stanley Robinson do. They force themselves, or maybe they do it without any effort at all, they imagine an entire consistent world where the incentives match people’s actions, structures and institutions. That’s what the future will have. It may not be one of the futures that they imagine, but it’s the holistic interlocking piece, what economists will call general equilibrium. But we can’t even describe the modern economy in satisfactory general terms so we certainly can’t predict the future. But that’s what science fiction gives me. It’s history in reverse or history is science fiction in reverse, whichever way you want to think about it. I’m just fascinated by the profound changes that we live through and that we see around us and we never understand because every day is kind of like yesterday to us, but in fact we are wrapped up and participating in, I don’t think we’re participating in accelerating change. But since the 1750s, the rate of change has been very fast across a lot of societies. Since the industrial revolution hit us, we have continued to change our societies at a remarkable pace.  Smith: Absolutely. But the point you make that you don’t see the pace of change on a day-to-day basis, but if you stop and reflect and you’re of any certain age, you can see tremendous change has happened around you. I can absolutely see how it’s actually a good exercise to go into history or into the future and free your mind and allow yourself to think big about change before coming back to your own desk if you like, and trying to predict where we’re going.  Johnson: Absolutely. When we researched and decided that we really wanted to dig into the European colonial strategies, a lot of that was what were they thinking? What was the decision making? What did they know? What data were they looking at? The attitudes of those European colonisers are very alien to any modern person, and actually pretty abhorrent in many ways, but you can’t shrink away from confronting that and understanding that if you want to understand history and our view and our contribution to economics. Some people ask me this now, I say we brought a bit of history in and that’s what we did. We said, look, the stuff that happened a long time ago was very consequential then and those consequences have lasted through to today. It’s not that history is predetermination or something that was done to you or your country 200 years ago means you’ll always be rich or poor, but it does matter. It has had a long lasting effect. At least in terms of thinking about context and what your options are today, it’s pretty helpful to understand that context.  Smith: Was it your own background that made you interested in that? Because you grew up in Sheffield and in Thatcherism came in, there were profound changes going on around you. Were you a precocious enough young man to sort of see that policy institutions made a tremendous difference to the world and you needed to understand what was going on?  Johnson: No honestly. Life is lived forwards and understood backwards. Sure, I could tell you a story in which that happened and it is true that part of my family lived and worked in Sheffield for a hundred years. We made screws and there was a screw manufacturing company called Henry Johnson. My father inherited that company and he ran it for a while. He sold it then things got rough. The economy didn’t do well. I think pretty much everybody in my generation, in my immediate family, ended up leaving Sheffield to pursue opportunities elsewhere.  Smith: That must have been sad because Sheffield has such an extraordinary connection with the steel industry and all steel products. It must have been difficult for people to say goodbye to such a heritage.  Johnson: Yes, there was a lot of discussion in my family about industrial decline and the loss of entrepreneurship. My parents impressed upon me that I should never ever consider becoming an entrepreneur. I should become an accountant, get a professional qualification, and I’m now a professor of entrepreneurship at MIT. The irony around, but I’m not an entrepreneur, so perhaps in the end I followed their advice. When you win a Nobel Prize, they ask you to donate one artefact to the Nobel Prize Museum in Stockholm, a fantastic museum I really enjoyed visiting, not only because they’re hanging my picture up now. I gave them my grandfather’s passport, a special passport, that was issued in 1942 with his visa issued in May, 1942 to visit the United States. In May, 1942, the Americans had just entered the war. In December, 1941 the Pearl Harbour was attacked. My grandfather was one of the countries, the British leading experts on metallurgy, and he helped to run a big steelworks integrated steelworks in Sheffield. He came to the United States on a technical mission as leader of what was called the British Armor Mission, to consult with the Americans about how to build tanks. Here’s the key point, my father knew my grandfather however my grandfather died very shortly after I was born. I’ve looked into the historical record, my father and I are quite convinced that Cyril Daswell was his name, did not come to the United States to learn from the Americans because he’d been building tanks for a while and they had not. He came to teach the Americans what he knew because the British needed the Americans to learn and to scale. If you think about the history of technology and when American scientific leadership emerged, it was during this World War II period. Before 1940 the Americans won almost no Nobel Prizes in science. After 1945 they dominate, by far the leader in terms of winning prizes. There’s a switch and there’s a change. America was a strong engineering country. It was a manufacturing powerhouse. It was not leading the scientific frontier, that was Western Europe. But it changed during the war and it changed because of what the Americans did in the war. Afterwards they said, if we own the science and we own the commercialisation of science, we not only will do well in terms of productivity in the civilian economy, we’ll do very well in terms of national security. They pushed that very hard for 40 years and we still have a lot of positives from that experience. But my grandfather came to the United States at that moment and he participated in the moment when technology leadership was transferred from Western Europe to the United States. I’m sure he didn’t see it like that. There’s no way you would’ve understood it or could’ve appreciated it. But he was there present at the creation. I say to all my audiences in the United States in particular, I think with the great power to invent technology comes a great responsibility. What are you inventing? For whom are you inventing? ChatGPT was the software heard around the world within 24 hours, right? The tools you create for yourself when you’re the leader are the tools that everybody else in the world is going to be using. What’s your goal? What are you trying to achieve here? How are you aiming to improve human society? It’s not just an interesting question, I think it’s the central question of the day.  Smith: That passport, that symbol of exchange and what a family history you have of being involved in exchange of knowledge between the UK, Europe, and the US. How do you feel currently about the approach being taken in the US, your adopted country in particular, but I suppose around the world about retrenching and becoming more isolationist?  Johnson: I do think a lot of damage is being done to the scientific enterprise. We’ve have young scientists with promising careers being fired randomly, and they’re not going to be picked up by some private firm because they’re also worried about what’s going to happen to the economy. You’re destroying human capital and you’re handing a massive advantage to your geopolitical competitors. The Chinese in particular must be loving this. It’s a self-defeating foolish action by the Trump administration, the way they’re going about treating the scientific enterprise. We should be turning more science into technology, generating more good jobs and stronger national security. Instead, you can look at health, drones and at all manner of other technologies. That’s part of what my group does at MIT actually. The measures that have been taken so far in the first month, it’s first month by the Trump administration, are massively damaging to American national interests.  Smith: Do you see an end to it? Do you see just this continuing or do you see some kind of backlash, some sort of stopping of this?  Johnson: “Everything comes to an end” that’s what Voltaire said about the Roman Empire. It comes to an end no idea when. By the way, I was the chief of economy of the IMF and one of the first things they tell you there is never predict a date and a specific action or event in the same speech. It will end, but I won’t tell you when.  Smith: Okay, we’ll do another podcast. I get the “when” then.  Johnson: Exactly. Will there be a backlash though? I think is an interesting question. If you’d asked me a couple of years ago, I’d have said, sure, there’s a pendulum swings. People are more inclined to invest in science, less inclined. But I fear that we’ve run into this counter enlightenment retreat to medieval beliefs, which has been funnily enough, funny in a sad sense, compounded by the latest of technology, social media. That instead of reasoned debate and looking at the facts and trying to figure stuff out, people just shout at each other on the internet or over social media. The basis of democracy has to be deliberation and deliberation only makes sense if there’s a process through which you discover facts and sometimes the facts are wrong and sometimes the facts have to be revised and sometimes new facts arrive. We understand that’s part of the scientific process, but that entire process has become undermined and delegitimised in various ways in the United States. I don’t know if the pendulum swings back or when it swings back at the level of governmental action. I do think the private sector can do a lot by itself. I do think places like MIT need to step up and demonstrate more leadership and find partners to help us build better technologies for more people. But whether the federal government will come and join us in that enterprise in the future on the same scale as in the past, honestly, I don’t know.  Smith: I mean you have at MIT this shaping the future of work institute, which you could direct. That must surely be exactly the sort of institution that federal government should be interested in as you try to improve a lot of people throughout the country.  Johnson: That’s very nice you say so. I think that’s the case. I do have this very nice baseball cap that I’m trying on now, and I’ll send you one. It says pro worker AI because we’re all about pro worker AI. You’ll get one of those in the mail. But in terms of the government priorities and government support, no, in the technology sector, the big tech companies want to be left alone. This administration doesn’t wanna do anything at all that rhymes with regulation or anything. You’re going to get what the big technology companies want to give you and what comes out of this massive expensive race to build more powerful artificial intelligence. Whether or not that’s pro worker is not something that interests the tech companies or the federal government today.  Smith: It’s good that it interests somebody, good that it interests you. Drawing together two things – you talked about the big question. This is the big question about basically how the technologies we develop, the progress we make benefits humanity, which is pretty much what the Nobel Prize seeks to reward. Then back to that question of how you saw your life as you were growing up in Sheffield. Please talk more about the big question. What you think you are trying to do and what economics should be trying to do?  Johnson: Since you put it like that, I think that what we should focus on within economics and at places like MIT is how to deliver on shared prosperity. That’s something that industrial companies like the US did quite well and without too much special thought in the post 1945 period, but after the 1980s, you mentioned Mrs. Thatcher, of course in America we match her up with Ronald Reagan. The shift that took place in part due to their policies and in part due to some other factors, that shift moved us towards much more unequal outcomes in terms of economics, politics and the quality of people’s lives. That has, I think, undermined the legitimacy of democracy in places like the United States. People can reasonably say, look, you claim that you’re running policy for the interests of all of us, but me, my friends and my town have been left behind and we were crushed by automation or we were crushed by some version of globalisation and you did nothing to help us so why should we support the existing form of democratic institutions when it manifestly and for many decades has not actually done anything for us. I think trying to tilt ourselves back towards a more inclusive path for technology and all of economic policy is an important goal. But as you can see from the last round of elections in the United States and the current actions of the government, that there are alternative narratives put forward by various people that I think will not help the less well-off and less educated and they’ll only exacerbate the inequalities. But if you exacerbate the inequalities, you make people more angry and then you’re more likely to elect populace. There’s a potentially self-reinforcing loop that you can find yourself on and, don’t want to pick anyone in particular but Argentina for example, was a rich country in 1900, but it got itself into this populist loop and has never again been able to regain its previous level of relative prosperity after 120 years.  Svensson: Adam, Simon Johnson’s research seemed to be focused on the past, the present, and the future. That’s a pretty massive undertaking.  Smith: I suppose it is. I suppose it’s needed in his case to make sense of everything. I guess he’s trying to address fundamental question economics of how come things are as they are. In order to find that out, you have to go back into history. Also as he points out, he likes to, if you like, separate himself from the current situation to try and clear his mind and get a different perspective. He finds going into the future a good way of doing that.  Svensson: But why was he awarded the Nobel Prize?  Smith: He, James Robinson and Daron Acemoglu used the experience of colonialism as a natural experiment in economics and investigated what effect different ways that colonial powers handled countries had on their eventual outcomes in terms of their prosperity. Broadly they found that if the colonising paths had been more inclusive in the way they treated those countries and put a bit more into building the institutions of the country and I suppose caring for them, the outcome in the end was better.  Svensson: As in quite a lot of conversations that you have with economics prize winners, the other Adam Smith sort of looms over the conversation, despite having been dead for 235 years, why is he so omnipresent still?  Smith: He might have questioned whether he should be referred to as the other Adam Smith. But it’s fun because people seem to react to the name.  Svensson: Why is he important?  Smith: He’s taken by so many people to be the founding father of economics and the study of political economy. I suppose his belief in the power of the free market, in the power of assembly lines, this mix of choice and the support of self-interest and if you like, freedom very much mirror the way we think about society today, that wealth is built on the supply of goods and services to consumers. The more we consume the wealthier we are, it’s the model accepted in much of the world now, perhaps most of the world.  Svensson: Is it annoying to share a name like that with someone well known in this sphere?  Smith: What would I rather have been called Karl Marx? Honestly yes.  Svensson: Another thing that seems to entertain people is one of my favourite Nobel related phenomena, which is the IgNobel Prize. Is that something you enjoy as well?  Smith: Very much so. Aren’t they fun?  Svensson: We should explain to the listener what they are if they’ve managed to miss them.  Smith: Yes, it’s an award given annually for research that makes you laugh, which is a beautiful idea. It’s supposed to make you laugh and also make you think. They’re given in Cambridge, Massachusetts every year. Generally it’s prizes given for published research that’s entertaining. It’s experiments like watching the most viscous liquid in the world drip through a funnel. I think the prize was given after the seventh drop fell and after 30 years or something. They gave them a prize in fluid mechanics or a piece of research that showed that ostriches get more sexually aroused when humans are around. This is all published stuff.  Svensson: Is there a connection between the IgNobel Prize and the Nobel Prize?  Smith: Yes, for a start they often get Nobel Prize laureates to hand out the prizes at the ceremony. There has been one occasion when a Nobel Prize laureate has also been an IgNobel laureate. They were an IgNobel laureate first. It was Andre Geim who got the Nobel Prize in Physics in 2010 for the discovery of graphene, which is this thinnest form of carbon with amazing properties. 10 years before that he was awarded an IgNobel Prize for levitating a frog.  Svensson: It’s a good stepping stone then?  Smith: In his case, yes.  Svensson: But it says something about daring to be a bit ridiculous as well in terms of what kind of science you want to do.  Smith: That’s such an interesting point because I think that daring comes into a lot of exciting scientific thinking, and I’m sure in Andre Geim’s case that that playfulness and also bravery are important for being able to break barriers. You don’t sort of think of academic thinking as being daring, but if you can actually be brave enough to suggest doing things that everybody just says, oh, for goodness sake, no way. That’s daring. Just in the way that explorers are daring.  Svensson: You can’t be afraid to be awarded an IgNobel Prize then.  Smith: No. I bet secretly many Nobel Prize laureates would love to be awarded IgNobel Prize. But obviously it’s a high standard they have to achieve to get it because there’s a lot of fun stuff out there it turns out.  Svensson: Talking about entertaining, I’ve heard that Simon Johnson has a new sorter book in the works.  Smith: That’s right, yes. He’s working on a science fiction novel being a devotee of the genre. Indeed I asked him about it and this is what he told me.  Johnson: I am working on a science fiction novel, yes. Thank you for bringing that up. It’s great fun. It’s a science fiction thriller about history. I’m marrying my two major interests here and we’ll see how it goes. We’ll see if anybody can bring themselves to read it but I’m in great fun writing it.  Smith: Does it come naturally to write something that is entirely imaginative?  Johnson: I can write better when I’m in certain frames of mind. I find riding on planes to be quite easy. I have to step back from the responsibilities and let my mind float a little bit. I think reading all that history is helpful because I think I can find myself in a different world in a different moment. It is true. I sit in my MIT office where I am today, and there’s these pressing administrative, teaching and research tasks around me. I can’t think about the novel.  Smith: What inspired you to go down that path? Because it’s not as if you haven’t presumably got enough to be doing and to entertain you.  Johnson: It really came about when I was writing the last book ‘Power and Progress’. We were trying to document and write down what we thought AI was. I also teach a couple of courses at MIT that touch on AI. We have a lot of open-ended discussions with students. I just started to imagine what could happen if you take two or three more steps down the corridor that we’re sort of explaining to people. I found myself taking notes on my phone, standing waiting to board planes. There’s this liminal time when you’re waiting to be told you can board and just sort of standing there staring blankly at the countdown till boarding time. Then I started to feel a couple of characters emerging and voices, and they started to say things to me. I thought, that’s interesting, where did that come from? And then they started to talk to each other and I thought, well, I’ve never had that experience. Then I sat down on a plane that was taking off from San Francisco one day I was on the book tour presenting the nonfiction book. I thought I just open my laptop and work on the novel for a few minutes and then I’ll do some real work. Next thing I knew I was landing in Charlotte on the other coast of the United States. I’ve solved the problem of long distance air travel. I’m never bored on a plane again. Flight delay? Excellent. Let me open a laptop.  Smith: It seems to be connected with travel and airplanes, but how clever of you to find a way of using the interstices in your life.  Johnson: It wasn’t a deliberate strategy, Adam. It’s not that I thought, oh my goodness, I’m being unproductive. It was where the mind wanders at that time. I did think about writing the entire novel on my phone, which I believe some people have done, but I don’t think my couple tunnels would survive that. At some point I had to transfer it. But something liberating about just to write five words. What are those five words or ten words and I think that frees you a little bit when you’re thinking about the fiction side of things.  Smith: It reminds me of, there was a Nobel Prize laureate in medicine called [Oliver Smithies](https://www.nobelprize.org/prizes/medicine/2007/smithies/facts/), who was a kind of mad professor originally from Oxford.  Johnson: We’re all mad, Adam! Let’s be clear about that.  Smith: Okay, so very similar past you, started in Oxford, ended up in the east coast of the US and he did pretty sensible experiments five or six days a week. But he used to like to talk about Saturday afternoon experiments when you just let things rip and you just went mad. You’ve chucked a bit of this in there and a bit of that in there and just saw what happened. All sorts of interesting things came out of that, perhaps everyone could think about your novel writing as your Saturday afternoon experiments, just playing with ideas.  Johnson: Can you also win an IgNobel Prize? Because that could be, I need to readjust my career goals. It could be an IgNobel Prize. We do have a graphic novel underway based on ‘Power and Progress’. We think we might be the first Noble Prize winners to have produced a graphic novel. Not absolutely sure about that.  Smith: What’s the idea?  Johnson: It’s a version of ‘Power and Progress’, which is a 500 page heavyweight tome that is told in a lightweight, fun way with pictures. It’s not 500 pages, it’s 20 or 30 pages, but it’s just supposed to get the main messages across, to communicate it to outreach. We’re not planning to make money on it, we just want to reach more people. We’ll find a way to give it away free electronically. That’s the goal.  Johnson: I tell my audiences, the ones who are a bit more up on science fiction, that the question is which Neil Stevenson novel are we gonna live in? He wrote a famous book called ‘Snow Crash’, which is fairly dystopian on average. He wrote another book called ‘The Diamond Age’, in which he imagined that AI could be massively empowering to people and help elevate the education of relatively oppressed, underprivileged people. They could teach themselves using interactive software on a personal device. He wrote this before we had cell phones, before we had AI. We’re in the middle of it. It’s happening around us.  Smith: You’ve given tons of worrying food for thought there.  Johnson: I’m sure that if the other Adam Smith, the one who came before you, your namesake, was here and we’re writing as he wrote, I think his famous book was written in the 1770s. He would be absolutely fascinated and probably quite brilliant on the interplay of economics, politics and technology. The original Adam Smith launched economics. We are living in the world that he didn’t create it, but he imagined it and he described it and we’re all following in his footsteps. From my perspective, always a pleasure to talk with any Adam Smith.  Smith: He was quite optimistic about things, so he probably would’ve felt that this was a way to help humans flourish.  Johnson: Fair. Behind Adam Smith, when we look back, was Medievalism, a lot of restrictions, a lot of power of established interests, and the market was liberating. The market was allowing people to have transactions and to exchange ideas and to move goods in ways that are not previously been anticipated. He also began to see, although I don’t think he saw it that much, because it hadn’t happened that much, but the economies of scale could change everything. The cost of something that was good and you liked could be quite high to start with, but it would come down very fast when you built a bigger factory or a massive supply chain. On the one hand, we’ve achieved a lot more than Adam Smith could reasonably have imagined that we would achieve. On the other hand, it’s not worked out quite so well for everyone as he might have hoped or as he did hope I think. 250 years later we’re still trying to understand exactly what went right and why things continue to go wrong.  Smith: Thank you very much indeed. It’s been a huge pleasure.  Johnson: Very nice to speak with you, Adam, and I’m so glad that it was you who reached out to me and connected with me on Nobel Prize day. I will always remember that moment with great appreciation. Also your kindness when I questioned whether your name was really Adam Smith, you were very kind about that, I’m not sure I was entirely polite because I was still a bit in shock. Thank you very much for that. Thank you.  Svensson: You just heard Nobel Prize Conversations. If you’d like to learn more about Simon Johnson, 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 laureate with a passion for creative writing, listen to our earlier episode with chemistry laureate, poet and author [Joachim Frank](https://www.nobelprize.org/prizes/chemistry/2017/frank/podcast/). You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| Telephone  interview | 0801=SJ  Simon Johnson: Hello?  Adam Smith: Oh, hello, am I speaking with Simon Johnson?  SJ: Yes.  AS: Hi, my name is Adam Smith. Presumably you’ve heard the news just announced in Stockholm a few minutes ago,  SJ: A few minutes ago. Yes. I didn’t hear from the academy. I just saw text messages from people congratulating me, which is an interesting way to learn.  AS: Well, may I add my congratulations.  SJ: Thank you.  AS: I suppose one has to ask what your first thoughts are on being awarded the prize?  SJ: I’m surprised and delighted.  AS: The committee highlighted the link between the setting up of inclusive political institutions and prosperity. Does your work point to a way out of poverty for the poorest nations, do you think?  SJ: Well, and that’s certainly something we’ve worked on a lot and tried to answer. I don’t think there are any easy answers because so much of that poverty is unfortunately the result of longstanding institutional arrangements, political and economic. So there are some very big burdens to overcome.  AS: Sticking with this inclusiveness, your recent work that you’ve published together with [Daron Acemoglu](https://www.nobelprize.org/prizes/economic-sciences/2024/acemoglu/facts/), the book from last year, *Power and Progress*, on who controls technology and who benefits from it, seems to point in the same direction. That one key to getting things right is to make sure that institutions of participatory and that the way that we approach technology has to be participatory. Is that true to say?  SJ: Yes. I think we see that book as exactly developing that theme. And I think you put it very well. Controlling technology, particularly if you’re talking about and thinking about new technology, technology at the frontier, who makes those decisions is very important. It reflects inclusivity, and can affect it as well.  AS: I suppose it all hinges around the idea of letting humans flourish to their best extent, talent being spread so broadly around, it’s good to let it flourish everywhere.  SJ: Yes. I think that’s a very good way of saying it. I think a way Adam Smith – the original Adam Smith – might have said it.  AS: I suppose another potential reading of your work is that democracy matters, and I suppose that is a message which people might find particularly important to the moment, given the threats to democracy that people perceive around the world.  SJ: Yes, absolutely. Democracy, true, genuine, inclusive democracy matters very clearly.  AS: I think we’ll let you deal with your day, which is, I know, going to be impossible. Congratulations.  SJ: Thank you.  AS: Thank you. Bye. |
| Interview |  |
| Q20 | What was your childhood like and how did it shape you? |
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| Q1 | What do you enjoy most about the field of economic sciences? |
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| Q25 | How important is it to use scientific research to address some of our time’s greatest challenges? |
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| Q10 | What are the most important qualities to be an economist or a researcher? |
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| Q41 | How did your collaboration with your co-laureates Daron Acemoglu and James Robinson develop? |
|  |  |
| Q24 | How do you know when you’ve discovered a good idea? |
|  |  |
| Q11 | How do you continue moving forward when confronted with challenges? |
|  |  |
| Q10 | What advice would you give to an up-and-coming researcher or economist? |
|  |  |
| Q10 | Can you tell us about the importance of being a teacher and mentor? |
|  |  |
| Q3 | Has there been anyone in particular who has mentored or inspired you? |
|  |  |
| Q36 | How has being an avid reader been important to your work? |
|  |  |
| Q28 | Do you have a favourite book? |
|  |  |
| Q5 | What other interests have shaped your career and outlook? |
|  |  |
| Q17 | When and where do you get your best ideas? |
|  |  |
| Q47 | As a tennis player, do you see similarities between tennis and the scientific process? |
|  |  |
| Q4 | What motivation drives your tremendous sense of curiosity? |
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| ID | 0802 |
| Biographical |  |
| Autobiographical |  |
| Podcast | “I just think a book can change your life.” 2024 economic sciences laureate James Robinson loves books – he has about 10 000 at home. Growing up without a TV, he and his mother spent evenings discussing news from the newspaper and reading. It’s no wonder his interest for social sciences, politics and economic sciences was sparked at a young age.  As well as delving into his thoughts on literature and reading, Robinson shares his opinions on field work in this wide-ranging conversation. For Robinson field work provides an opportunity to gain a deeper understanding of why problems exist and how they can be solved.  This conversation was published on 22 May, 2025. Podcast host Adam Smith is joined by Karin Svensson.  James Robinson: I tell my students, I never made a career decision in my life. I tell people it’s like a calling. It’s like being a priest or an artist. That’s not a rational decision. It’s just like a passion. It’s just that’s how you want to live your life.  Adam Smith: When we think about what makes laureates like James Robinson different, one thing that comes to mind is the fact that they really want to know. It’s not just that they’re niggled by questions, but the question that just won’t go away needs answering, and that drives them to dig deeper and deeper and recognise their own lack of understanding. They just have to keep exploring. That sense of an almost unforgiving question, which just won’t go away, seems to be what has driven James Robinson in his research journey that has taken him to so many countries around the world. Please join me for this podcast as I try to explore that journey he’s taken to investigate this most fundamental question of why some countries are prosperous and other countries are poor.  Karin Svensson: This is Nobel Prize Conversations, and our guest is James Robinson, recipient of the 2024 Prize in economic sciences. He was awarded for studies of how institutions are formed and affect prosperity. He shared the prize with [Simon Johnson](https://www.nobelprize.org/prizes/economic-sciences/2024/johnson/facts/)and [Daron Acemoglu](https://www.nobelprize.org/prizes/economic-sciences/2024/acemoglu/facts/). Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces. James Robinson is the Richard L. Pearson Professor of Global Conflict Studies and professor at the Harris School of Public Policy at the University of Chicago. In this conversation, he talks to Adam about how poverty is a solvable problem, how he approaches his research with a passion of an artist, and the one-time heckler who became a good friend and a co-laureate. First, let’s go back to that early morning in 2024 when he received the call from Stockholm.  Smith: When I spoke to you in October, your wife had just told you, you got to get up because she’d heard the news that you’d been awarded the prize in economic sciences. I was wondering whether getting up was a good idea, whether the deluge that has hit you since that moment has been worth it?  Robinson: No, absolutely. It is a bit overwhelming. I don’t really have the technology. I tried to keep my life very simple so I can get on with the things that I love. I’m not really used to coping with so many requests, but I shouldn’t really complain because it’s fantastic recognition and so many people engaging with the research and asking about the research and fascinated by the research. That’s pretty exciting, really.  Smith: I read a quote from you that said, you need to disrupt yourself all the time. I guess this wasn’t the sort of disruption you meant when you said that.  Robinson: I don’t know actually, it might be. Something like this, it’s a positive shock. It’s a moment to kind of rethink all sorts of things that you’re doing and prioritising. Negative shocks and positive shocks can all be good for disrupting you.  Smith: It’s an interesting moment to be disrupted geopolitically, especially for those in America, and be thrust center stage, because suddenly I suppose you are more in the public eye than you would’ve been had this not happened in October. How do you take this moment?  Robinson: Yes, I think it’s very interesting from a social science perspective. I spent most of my career studying poor countries. When Daron Acemoglu and I first started working together, we focused on these questions of how politics makes it so difficult to adopt a kind of good policies in developing countries. There was always this assumption that somehow developed countries like the United States. We’re not prone to many of these political syndromes that caused disaster in Nigeria, Columbia or whatever. But now we see they’re just as prone and the politics of developing countries can be just as perverse from an economic point of view. I think what you see at the moment is a completely irrational economic policy making. President Trump is advocating policies that no serious economist would ever argue in favour of. I think it’s pretty disruptive of the paradigm. Developed economics is overwhelmed by this idea that we just need to be generating better information about policies, what policies work, it completely ignoring politics whatsoever. Mainstream public finance in economics completely ignores politics and it assumes that the government is trying to maximise social welfare. The language that economists love is that there’s a benevolent social planner. Does anyone really think that President Trump and Elon Musk are acting like benevolent social planners? I think it’s a wake up call for the economics profession, and for us, it’s a challenge to think about how the ideas that we’ve developed studying primarily poor countries actually apply to countries like the United States or other developed countries, where many of the issues seem different.  Smith: In some ways it’s an unwanted but rather useful natural experiment going on.  Robinson: Yes, it’s unwanted in the sense that it’s rather challenging to be living through it. That being said, for someone who works in poor countries and has done for decades, the challenges that people have to cope with every day in Nigeria, Columbia or the Congo, nothing is like that is happening here. People survive somehow. I’m not feeling too sorry for myself given what I know about other people’s lives in the world. But it is pretty disturbing to see such irrational things happening and such shortsighted things happening. What I find astonishing is the risks people are willing to take with the institutions that this country has built over hundreds of years. They seem to have no consciousness that this could all dissolve and collapse. That’s a bit frightening.  Smith: Precisely. So much of your work has pointed to the importance of institutions and institution building and to see this disruption must be very disheartening in some ways.  Robinson: Yes, it’s disheartening. But I think it’s very revealing about what keeps these institutions in place and what do we have to do to get collective support for these institutions? How you can take these things for granted and not really understand how important they are for your lives? I think this current attempt to basically deinstitutionalise the state, that’s what’s happening. Deinstitutionalise the federal state seems to be happening in a complete vacuum of understanding of these issues, which I find frightening and whether this project will actually be implemented or whether it can be derailed or whether President Trump actually has the kind of attention span necessary. I do think if you look at serious deinstitutionalisation of the state, say in Venezuela in the last 30 years or Zimbabwe or Argentina in the 1940s and 50s, it takes a lot of planning and persistence to deinstitutionalise the state. Usually you need the military on your side also, which I don’t think is true in this country.  Svensson: Adam, the picture that he paints here seems pretty grim.  Smith: Yes. But perhaps there’s a chin of light in the fact that the establishment of institutions which then lead to prosperity, which is one of the things he worked on, takes a long time. One might hope that it takes a long time to dismantle such institutions, not as long as to build them, but it does take some time and perhaps there isn’t enough time to put it all apart. Obviously he feels it’s a very challenging and dangerous time.  Svensson: Why was he awarded the prize in economic sciences?  Smith: I suppose you could say for asking one of the most fundamental questions in economics and coming to something of an answer about it, which is why some people rich and some people poor, he and his colleagues saw this opportunity to look at the historical record, study countries that had been colonised and see how they had turned out. Then to look for possible reasons for the way that things had turned out. They found some particular aspects of the way that colonising paths had treated countries that gave them clues to the fact that those countries that had built really inclusive institutions that involved collective decision making seemed in the end to fair better than those that had gone in and just taken all the goodness out of a country that they could get without building up any institutions to maintain the more or less democratic running of that country. Broadly it was that finding, I suppose you could say, the connection between politics, prosperity and how essential politics is to prosperity of a country.  Svensson: James Robinson is a bit unusual in the field of economics since he does a lot of his research on the ground in Colombia, Congo and other countries. You’ve also visited many countries in the global south for your work, and I’m wondering why is it important to have this approach?  Smith: Yes. I think it’s incredibly important to be out there in the field, but I would draw no parallel between his in depth visits to these countries and our fleeting educational visits. However, there is a perhaps something that applies to both, which is that you really don’t know what the situation is until you’re on the ground. It’s easy enough to sit at your desk in Chicago as he says, and think you know all the answers. But when you get there, it’s not quite as you thought it would be. It’s easy to get trapped in your own bubble of thinking and understand the world. He found again and again that things work rather differently from the way that was expected. So not just to search the records of these places, but also understand what makes them tick. It’s really important to be out there.  Svensson: It’s really easy to get chopped in your own bubble and to just sort of view the world from your own set of circumstances I think.  Smith: I’m sure I do it all the time. I’m sure we all do it all the time. There was one fairly awful example of that actually, that suddenly springs to mind that we were visiting a lovely group of young engineers in a country where it wasn’t clear that they were going to be able to make their careers and really realise their potential in that country. So they were wrestling with this awful question of should you leave and do better or should you stay and support your country and make your career there? They were talking to a laureate and asking this question that people all over the world always ask, which is how do you cope with failure? How do you cope with everything suddenly just going wrong? He gave an answer which really illustrated that he perhaps hadn’t left his bubble behind. He said, ‘yeah, when this happens to me, I always take a few days off and I go and relax in my vineyard’, which is lovely. But it wasn’t quite an option for these young engineers.  Svensson: I wonder how many people that is an option for.  Smith: It depends how you define a vineyard. My small square of, garden of London perhaps?  Svensson: Maybe that’s the takeaway for these students as well to sort of find your own vineyard.  Smith: If only I’d said that at the time. That would’ve been good.  Svensson: What did you say at the time?  Smith: I don’t know. I think we all just looked a bit baffled. The thing about that comment was it really did illustrate just how there can be a real gulf between one’s understanding of the world and the people you’re talking to. It’s really good to be reminded of that.  Svensson: Especially when you talk about young people in these countries. There is this really sort of tragic element of the untapped potential from these countries. How do you think about that?  Smith: I’m so struck by that everywhere I go. In a way it’s very sad, but in a way it’s very exciting and lovely. If you think about where science sort of comes from in the world when you’re listening to the news and stuff, most of it seems to come from rich nations, but there’s no shortage of brilliant young artists and brilliant young people, full stop. I asked James Robinson why he decided to go out into the world in the first place, let’s listen to his answer.  Robinson: I think it mostly stemmed from the recognition of my own ignorance, to be honest with you. I was always terribly curious about the world, fascinated by the world and about the differences between different societies and how to think about that and what the consequences were. I didn’t know how to engage with that. It just so happened that I engaged with it through economics. I guess it could have been something different. I started off studying political science at the London School of Economics, and I switched to economics because I had a very inspiring professor, Miko Omashimo. It seemed that in economics there was a machine which would give you answers to questions. I didn’t see that machine in political science. I tend to get very obsessed with things when I get my teeth into them. So I went along this path of sort of trying to master economics and understand economics and then when I decompressed from that, when I finished my dissertation, I realised I still don’t understand anything. I know all this mathematics and I know how to solve models and whatever, but actually that doesn’t tell you anything about what I’m interested in. Daron and I, when we met in 1992, we had exactly the same kind of impulse, like we thought in exactly the same way, which is, we know how to write papers, solve models and publish articles and stuff, but we don’t know anything about what we are really interested in. We’re not studying the real issues.  Smith: It’s fascinating that you weren’t satisfied by kind of going through the form of academia if you like. The what needs to get done in order to make a career in academia that you saw you had to dig down deeper.  Robinson: I don’t think we’ve ever thought of it as a career. I’m not sure. I tell my students I never made a career decision in my life. I’ve never made a rational choice about anything about jobs. I tell people it’s like a calling, it’s like being a priest or an artist. I have friends in England who are artists and that’s not a rational decision. It’s just like a passion. It’s just that’s how you want to live your life and you don’t really think about money, careers or whatever. I think we’ve been very fortunate. People have enjoyed and got excited about what we’ve done and we’ve been successful professionally. But honestly I never really thought about that. You just sort of throw yourself into it.  Smith: What a dream way to live, just to ask questions and find people who will support you to do so. Tell me about your first meeting with Daron.  Robinson: I was giving a job talk at the London School of Economics in March, 1992. He was a PhD student, so he was the sort of star student. He was sitting in the front row of my seminar kind of interrupting me, saying, ‘No, if you change assumption four, that result wouldn’t go through’. I was thinking, ‘Gosh, who is this? This chap is so irritating.’ Then Kevin Roberts, who was the chair of the search committee, he sort of comes up to me at the end of the seminar and says, ‘Oh, let me introduce you to Daron Acemoglu, he’s going to come for dinner’. I’m thinking, ‘Oh my gosh, seriously, this guy’s coming for dinner’. Then we walked out of the St. Clements building, I think it’s been demolished now at London School of Economics. He looked at me and he said, have you read this paper by Norton Weingast? I had read that paper, it turned out. We walked off to Covent Garden talking about this paper by Norton Weingast, which was about the economic implications of the glorious revolution in 1688 in England. We sat next to each other at dinner, and I guess we sort of hit it off at a personal level and we realised that we had lots of interests in common. Then I went off to Australia to teach at the University of Melbourne, and email had just disseminated itself in the world in the summer of 1992 so we started talking. Then he got hired by MIT and I came back to the US. We discussed and talked, then we started thinking about ideas and writing papers and that’s how it happened. We just found that we had a lot in common.  Smith: It continues. It never stops. You don’t run out of new directions to explore together.  Robinson: Oh gosh, no. There’s just so many things to understand, so many things to read about, so many things to think through. Absolutely not.  Smith: Obviously there’s a trade off between doing things alone and doing things in collaboration with somebody else and thinking jointly. How do you view that with him?  Robinson: It can be very lonely working on your own. I think collaborating is fantastic in sort of the solidarity and you get stuck on something and somebody else understands how to solve that or what the answer is. It’s not a coincidence that many major works of philosophy are written as dialogues. Plato’s Republic, people are discussing, you discuss ideas, you deliberate. That’s the way you understand things. It’s a collective experience. I think this idea of going off into the Sinai for 40 days and 40 nights and coming back with ideas. That’s not how things work in intellectual life. I think you just get enormous stimulation from other people and different perspectives. Plus different people are good at different things. That’s the truth of the matter. You allocate tasks, you have a division of labour, you do different things. One of the reasons why Daron, Simon and I worked so well was we are very good at different things, but we’re very good at collaborating. We get on very well to personal level. We’re really good at sort of like, okay, you do that, I’ll do this. There’s no egos involved. No one’s trying to take credit for anything. Everything is collective. That’s great. That’s very fun that that it’s a collaborative. I’ve worked a lot with all sorts of people, with students and I’m writing a book on my own at the moment, which is actually fun. But the collaboration is very important and stimulating.  Smith: There are these Nobel Prize laureates in medicine, [Joseph Goldstein](https://www.nobelprize.org/prizes/medicine/1985/goldstein/facts/) and [Mike Brown](https://www.nobelprize.org/prizes/medicine/1985/brown/facts/), who famously have been a very strong collaborative partnership in their field for several decades. They preach the idea that collaboration is a fantastic way to go. They say exactly what you say, that it’s really important that you don’t have egos and there’s equality. They go so far as to ensure equality by, for instance, accepting invitations to talk one after another so that nobody gets ahead, if you see what I mean. They’ve set up a structure to make sure it works and they co-supervise their groups. It’s a fascinating and perhaps under-researched area how productive collaborative teams can be.  Robinson: Yes. I don’t think I’ve ever worried about somebody getting ahead, honestly. I never worried about it.  Smith: Back to this idea of going out into the world, you are so active in going and visiting places and being on the ground and listening to people. That’s obviously an extraordinarily important part of your work.  Robinson: Yes, because for me, one of the problems with, it’s not just economics, economics is a particularly kind of egregious example but political science is very similar. There’s this sort of idea that we kind of have this theory that explains everything. Like Newton’s laws of gravitational motion that we have forces equal to mass times acceleration and force is equal to mass times acceleration in Chicago, Lagos, Bogota, Bangalore, whatever it is. Where are the humans? Where are the people? Where are the societies? Where is the history? I just found that that theory, social science is not like that actually. You can have all the data you like and do all the statistical analysis you like, but how do you interpret the results? To interpret the results you need to know about people’s motivation, about the societies and the context. I have a talk I give called ‘What I learned from doing field work’, which is mostly just about all the stupid ideas I had. You can sit in your office, have an idea, write a mathematical model down and you can get some data and test it, but it’s very solitary to go into the field and like ask people does that make any sense whatsoever? I find it just incredibly exciting and fascinating. It’s been very productive. I think just generating ideas, insights, motivation, there should be a lot more of that I think. Astonishingly enough, the professional incentives and economics to do that are basically zero. Most economists, even in development economics astonishingly enough, think that this is a complete waste of time doing things like that because they think they understand everything. They have this general theory that basically explains everything. You could go collect data and whatever, but the idea of like talking to people or even sort of the idea that you don’t have the right theory or maybe you have the wrong assumptions – that never occurs to people. I think in economics you’re just taught, here’s this wonderful thing and that’s it.  Smith: Strange that that should be, when you say it, it seems so self-evident that you should be out there talking to people. But it’s funny how one can miss the self-evident.  Robinson: That’s not what physicists do. That’s the model that economists have taken as the ultimate theory. Why not be like the physicists? But social science is not like that, which is something that Max Weber understood a long time ago, Weber thought that social scientists actually had an advantage over natural sciences because we could kind of understand motivation. We could question ourselves and think about why that had happened or why people had done that. We had a sort of consciousness of social phenomena that physicists didn’t have, but everyone ignored him.  Smith: What is it that you think is the take home message (if it’s possible to say that) about what does build prosperity as opposed to poverty in societies?  Robinson: I always think the take home message simply is that it’s about humans themselves, it’s about people and the types of societies and institutions they build. This is not about geography. It’s about how individuals themselves shape their societies. This is the big story about homo sapiens. There’s 9,000 species of ants. When ants got to Canada, Canada’s a sort of rocky and inhospitable place, they speciated to be better adapted to the Canadian environment. When homo sapiens got to Canada, they invented igloos and a taste for seal blubber and ice fishing. We are so adaptable. We create technologies, different ways of living, different cuisines and different social structures. There’s commonalities in the human condition but I think that sort of diversity is really about the history of humanity. It is not about succumbing to geographical constraints, it’s about overcoming them. I think that’s a very kind of optimistic message in the sense that the poverty anywhere is sort of solvable by people. But that doesn’t mean it’s easy of course to kind of construct institutions that achieve that or kind of solve the political problems or social problems that stop those institutions being created. I think if you start from that perspective of this is really about the types of societies that humans build or embedded in history and it’s about a political process of constructing different institutions. I think that’s the place to start. That’s the main message. Of course, that sounds very vague. What we did was to put some sort of structure on why do you see these patterns in the world in terms of why is it that nevertheless, some societies systematically seem to have got worse institutions. There we pointed to these historical phenomena like colonialism and the way the slave trade influenced systematically the institutions across many different societies. But that’s not to say that people are trapped in the history. They can reinvent themselves as many societies have.  Smith: As well as the building of inclusive institutions that you stress the importance of, you’d also use this term creative destruction. What do you mean by that?  Robinson: I think that what you see with innovation or the creation of new institutions or the creation of new technologies is that they often have very large distributional consequences in terms of income or wealth, but also political power. In fact, we point to the political consequences actually as being kind of more disruptive to society than the economic consequences. That makes it difficult to innovate because some people are going to lose in that process and they may have incentives to oppose it. It’s sort of disruptive. Institutional change is disruptive, technological change is disruptive. That’s something you have to understand. That creates lots of political challenges to actually moving a society ahead. It’s something that you have to think through if you want to help a developing country. If you look at any successful country, like we tell this story and why nations fail about the history of Britain, the British were very good at devising ways of making sure that creative destruction didn’t create a lot of opposition to the transformation of the economy. Actually Deng Xiaoping did the same thing. The same thing happened in China in the 1980s, a lot of the reform and the transition was designed so that the creative destruction was not going to induce a massive backlash from society.  Smith: Do you think that that now is particularly challenging with some of the technologies that are coming through, some of the disruptive technologies that are appearing on the horizon right now, and the rise of communication platforms that allow people to worry about them more?  Robinson: At the moment, there’s so much uncertainty. It’s not evident exactly who the losers are. Typically for creative destruction to have political consequences, it has to be clear who’s going to be losing who the beneficiaries are and who are the losers. I think it’s so uncertain actually the consequences of this innovation. It’s not quite clear who’s going to be losing from this and who’s going to be benefiting from it. No one can quite figure that out. If chatGPT is going to replace academics and we’re all going to be doing research by pressing buttons, or it’s going to complement and increase our productivity. There’s different views on that. Or if we’re all going to be replaced by robots. I think potentially yes. But I think we’re all thinking that through still, at least I am.  Smith: Given the focus of many of the prizes of October on AI, do you have any particular view on what the dangers of AI?  Robinson: I thought that was fascinating in the sense that the differences of opinion ran the gamut from mankind couldn’t fly so he invented the airplane. Like it was sort of like the toaster. What’s not to like about toasters or airplanes to Hinton scenario is that we’re all basically in 10 years time will be living in a scene from the Matrix with Keanu Reeves and robots will be running the world. But he thought that was just fine. It was just evolution playing itself out. In between with the economists saying, hold on a second, neither is inevitable. It all depends on incentives and what we do with it. I do think, and this is something that Daron and Simon have written about much more than me, I was trying to make the point in Sweden, especially in the BBC Nobel Minds, that the places where I work, there’s basically no electricity, let alone internet. The idea that somehow AI is going to have these kind of tectonic effects on Nigerian or Congolese societies are a little sort of ludicrous. That’s not the world I live or work in.  Smith: It is useful to have a reality check once in a while.  Robinson: Leaving that issue aside, the issue of the millions and millions of poor people in the world who are worrying about where their next meal is coming from rather than whether they’re gonna be replaced by chatGPT, I think it’s exactly right that there’s enormous social consequences of these technologies and nobody’s thinking it through. The tech billionaires don’t care at all. It’s obvious they don’t think about that and they don’t care about it. The US government doesn’t care about it or at least the next four years, it doesn’t care about it. That’s pretty frightening, I think. I think there is an enormously important policy agenda to start thinking about the social consequences of the way these things work, the way artificial intelligence work, what’s its likely impact on employment and wages and there’s nothing inevitable about that. It can work in different ways. It seems like the Nobel Foundation is in a very good position to sort of precipitate that discussion actually, because now you have so many people who are knowledgeable in a kind of definitive way on this topic. To start trying to get people to pay attention to that topic and start thinking about it. I don’t think there’s a magic wand to it. There’s not a magic wand solution to this, but it needs to be discussed. We need to start coming up with real answers to some of these very uncertain issues of the social consequences of artificial intelligence.  Smith: In a way, capitalise on the tremendous amount of worry and activity there is in this space without it being collated into conversations between people that are mostly constructive. It’s a very good point. I wanted to ask a little bit about your childhood because you came out asking very good questions, and that takes some path as a child to learn to be a good questioner.  Robinson: Yes, that’s an interesting question. My father was a sort of itinerant engineer. He worked most of his life overseas. He was a scientist. He loved science. We lived overseas when I was a kid. My mother was a teacher. She was very sort of political, very immersed in social issues, injustices and female liberation. When I was a teenager, my father was working overseas and we were back in England, every evening I would read the Guardian newspaper and discuss it with my mother. We would talk about politics and society. We didn’t have a television. My mother thought that watching television was the biggest waste of time. She used to say, ‘What are you going to say on your deathbed? Oh, I wished I watched more television.’ We didn’t watch television, we read books. We talked about what was going on in society. I think that was very important for me.  Smith: Dare I say, she must have been absolutely delighted with her teenager who read the newspaper and discussed it with her. It’s not a given that you would agree that that was a good way to spend the evening, it’s lovely that you did.  Robinson: Yes, and I was fascinated by history. I was obsessed with history and reading books. I didn’t go to a fancy school. I just went to the local government school. But I had some inspiring teachers and I just loved reading. We had a house full of books. My mother couldn’t afford buying books. My parents were both very working class people from the north of England. She was the first person in her extended family who ever finished high school. She didn’t go to university, she went to teacher training college. For her, education was everything. It was the way out of poverty. It was the way out of the north of England. It was the way out of working class life. So education and books – that was the world I grew up in. I just found it fascinating. I was just curious about everything. Even now when I buy a book, I buy a lot of books, I just think a book can change your life. You open a book and it’s just so exciting, what’s going to be in there. I still have that sense of excitement all these years later.  Smith: There is a thrill of the potential of it, isn’t there?  Robinson: Absolutely. I try to communicate to students that’s a life journey. Just so many things out there that are interesting to read. There’s just such amazing stuff.  Smith: It sounds like you are constantly reminded, as most of us are not, of the privilege of just being able to walk in and buy a book and have it on the shelf.  Robinson: Completely. I have about 10,000 books at home, so that’s my world.  Smith: You must be an ordered person to know where they all are.  Robinson: Pretty organised yes.  Smith: When you travel to many places where people are not so privileged, again, education can be the key, but it doesn’t necessarily lead in the same direction because the institutions are not there to kind of realise the potential. How do you feel about that side of things? It’s something that I think about a lot as we travel the world with laureates and meet people who are brilliant everywhere, but so many times the opportunity to be brilliant in the country and really have an effect is limited.  Robinson: Absolutely. I think that’s why there was a survey recently in Nigeria asking people, what are their motivations for saving money? I think the second biggest motivation was leaving Nigeria. I always say that the one sentence version, or maybe the two words version of world poverty is wasted talent. There is so much wasted talent in these places. It’s true, you can get a great education at the University of Nigeria, but what do you do with it? You can’t get a job if you don’t have the right contacts or you don’t know the right people in the right places. I think that’s right. I think a sort of simple minded, oh yes, you need to invest in education. Yes, of course you do, but other things have to move. Other institutions have to move. You need to create the opportunities and the social mobility that kind of allows that to flourish. It’s an interesting fact that there’s work by Diego Gambetta, who’s a sociologist at Oxford about suicide bombing and also Islamic radicalism. It turns out that suicide bombers and radicals are extremely highly educated. In fact, most suicide bombers have degrees in engineering. Why engineering? That’s an interesting question, but they’re very highly educated because they’re the ones who are most frustrated by the institutional context. I think that’s a glaring example. I was lucky because I was getting all that education, but I was living in Britain and I had access and opportunities. I didn’t suffer from discrimination or marginalisation so I could take that education and I could run with it, but that’s not true in Nigeria or Congo.  Smith: It’s a insurmountably big problem to fix in a conversation for sure. But in your own sphere, as you interact with social scientists around the world, do you think that your profession is doing enough to bring social scientists from elsewhere into the mainstream? I think of the fact that you for instance are based at the University of Chicago, and the University of Chicago has about a third of the prizes in economic sciences to its name. There are centers of excellence, clearly, but are they embracing the rest of the world?  Robinson: Yes, I think that’s not a coincidence. The University of Chicago is a very intellectual place compared to some of the places I’ve taught, without mentioning names. It’s just a place where intellectual life is more important than anything else. It’s really something special. I think that’s the fundamental reason that there’s so many Nobel Prize laureates here. But, the answer to the other question is no. I taught in Columbia and South America every summer for 28 years, in Bogota at the University of the Andes. Now I kind of coordinate a part of a collaborative PhD program in economics with many African universities, which is run out of Nairobi, an institution called the African Economic Research Consortium. You do your best, but the problem is sort of massive. What I see with my philosophy of social science, we desperately need more non-Western people at the table doing research, setting the agenda. One of the most kind of challenging things is that western academic and social science paradigms are so kind of hegemonic that if you are a young person in Africa or India, wherever it is, you feel you have to conform to succeed, to be taken seriously. That means that you can’t be yourself. You can’t express your own understanding of the world and your society you are living in. For that to happen it needs confidence. It needs a critical mass. That’s such a big challenge there. Working in Africa, you teach Africans and Africans don’t tell you what they know. They think, ‘Okay, I have to do this if I want to succeed, I want to impress them.’ No, there’s always this moment when you’re teaching where the kind of blinkers come off people’s eyes and they’re like, ‘What? Seriously?’ I can talk about Maasai society, I can talk about like my own people. And they’re like, ‘Oh my God, I didn’t know I could do that.’ That’s so exciting when that happens. But oh my gosh, there’s so much work to do there.  Smith: Your new book is called ‘Wealth in People’. That’s a lovely title. I guess that refers back in part to this idea that there is just this enormous talent pool out there.  Robinson: All of my African friends speak five or six languages. Africa is so cosmopolitan, everyone wants to know everybody else and make connections to people. They don’t care about what language you speak or your ethnicity. Africans are just so cosmopolita. Then think about the English people, we couldn’t even stay in Brexit, for heaven’s sake. That’s a completely out of date model of society. This is a 19th century nationalism. The world has moved on from that. You’re never going to survive with that attitude in this globalised world. For me, the Africans have what it takes to flourish in the globalised world and the British people certainly don’t.  Smith: How wonderful. Especially given that they say the three biggest cities in the world are going to be in Africa by the end of the century, don’t they?  Robinson: Cairo, Lagos and Kinshasa, I guess. I don’t know about Dar el Salam maybe.  Smith: This has never happened before in one of these conversations, but just last night, my 19-year-old happened to be asking me, ‘Why is it that African societies have somehow been slower to achieve wealth than places in other parts of the world?’ You have just begun to answer that question for me after the feeble answer I gave him.  Robinson: Yes, the book is about that. I don’t want the book to be centrally about that because I want to emphasise how fantastic Africa is, there is doom and gloom. That’s so inconsistent with my own personal experience in Africa. Africa’s just a very exciting, fascinating place, and I want to communicate that also. But of course, it’s also important to understand that issue of African poverty. The way I think about that is this sort of notion of wealth in people led Africans to organise their societies in ways that made it very vulnerable to the expansion of mercantile capitalism to European colonialism. That wasn’t planned, it wasn’t anticipated, but nevertheless, it led to kind of chaos for several centuries in Africa with very negative economic effects. But the future doesn’t have to be like the past. If you think about China as a sort of fascinating comparison. In 1978 China had been a disaster for 200 years. There’d been state collapse, civil war, communist revolution, warlism. There was a great leap forward. There was chaos for 200 years in China. There was economic collapse, famine, you name it. But then China bounced back. Underneath that, there was all sorts of potential that you’d never have noticed, if you didn’t know how to think properly about the society. For me, Africa is like that. There’s been chaos. It’s been more than 200 years, you could even say 500 years. But that doesn’t mean the future has to be like those 500 years, honestly. That’s part of the agenda with the book too.  Smith: Underneath it all is this very important questioning of what’s going on. Asking the question that is beyond just the obvious surface questions of why institutions aren’t working. Trying to understand. It must be so exciting. Digging out just little pieces of evidence.  Robinson: Yes, modular self-doubt.  Smith: Yes, of course. Much of that. Anyway, what a joy to talk to you. Thank you very much indeed for taking time.  Robinson: My pleasure.  Svensson: You just heard Nobel Prize Conversations. If you’d like to learn more about James Robinson, 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 economist who has made groundbreaking contributions to our understanding of poverty and development, listen to our earlier episode with economic sciences laureate [Esther Duflo](https://www.nobelprize.org/prizes/economic-sciences/2019/duflo/podcast/). You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| Telephone  interview | 0802=JR  James A. Robinson: Hello?  Adam Smith: Hello, is this James Robinson?  JR: Yes. Speaking.  AS: Hi, this is Adam Smith from the website of the Nobel Prize.  JR: Okay. Yes. Thanks for calling. I got your message. Sorry, my phone was switched off.  AS: Not at all. Not at all. How did you learn about the prize?  JR: My wife woke me up. One of her friends texted her and she woke me up and told me.  AS: It’s a nice wake up message. What did she actually say?  JR: “You’ve won the Nobel Prize,” I think. No, no, I think she said, “Get up. You need to get up.” [Laughs] I think that’s what she said. And I said, “Why?” She said, “You’ve won the Nobel Prize.”  AS: That’ll get you out of bed, I guess. And then I suppose there hasn’t been a second’s rest since then?  JR: No. The journalists from the University of Chicago came round at 6:30, so I just had time to get in the shower and make some coffee. Yes.  AS: You spend so much time in South America, Sub-Saharan Africa. I suppose the news could just as easily have caught you there.  JR: If it would have been in the summer, you would have caught me there, but yeah, I’m teaching at the moment, so I’m here in Chicago. Yes.  AS: So what can you say broadly about why there is such a disparity between poorer nations and the rest of the world?  JR: Well, I think that’s an outcome of a long historical process. But in our work, what we’ve identified are these institutional and political differences between developed and underdeveloped countries, and the way in which there’s enormous differences in how inclusive the societies are in terms of the opportunities and incentives they give people. So, you know, our work is focused on trying to show how these different institutional structures create poverty or prosperity.  AS: Your work has looked at the sources of those institutional structures, and especially their roots in colonialism. But do you understand, in a way, the intractability of the system, how difficult it is to change? Is that something that is …  JR: Well, that’s something we have looked at. I think, you know, there’s enormous persistence in the world, but you also see examples of transition from, what we call extractive to inclusive institutions, that countries that manage to change. I mean, look, every prosperous country today, historically, was extractive. Think of the United States, you know think of the history of slavery and the expropriation of indigenous people. And there’s a lot of extraction historically in this country. And there was a struggle to create more inclusive institutions and a more inclusive society. And that’s true of, you know, it’s true of my own country, of Britain. And so, there have been transitions and we’ve tried to study those too.  AS: I mean, I know it’s ridiculous to try and summarise things in just a phrase or two, but what would, what would be the key for societies in thinking about changing from being extractive to inclusive?  JR: The key to that transition is just really collective action by citizens, by people who suffer under extractive institutions and inequality and marginalisation. So I think if you look at that history, think about this, think about the civil rights movement. That’s a fantastic example of people getting organised collectively to fight for their rights, to fight against extractive economic and political institutions in the United States in the 1950’s and 1960’s.  AS: So it’s really, it’s a bottom up change then.  JR: I think that’s right. I don’t think, you know, inclusive institutions are not created by well-meaning elites. They’re created by people who fight for their rights and fight for a different vision of society.  AS: Nicely said. I just wanted to ask you very briefly about your relationship with your co-laureates. You obviously all have a very close working relationship. What is it that makes it go so well?  JR: Well, we’re all very close friends. I think we’re all good at different things, and we all respect each other very much. And we just have fun talking about ideas and thinking about the world.  AS: Yeah. I know that all sorts is going on around you. Thank you very much indeed. Many, many congratulations. And thanks for talking to me.  JR: Okay. Thank you. Thank you very much.  AS: Bye.  JR: Thank you. |
| Interview |  |
| Q1 | How did your upbringing shape you? |
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| Q1 | What do you enjoy most about economic sciences and research? |
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| Q8 | How did your collaboration with co-laureates Daron Acemoglu and Simon Johnson begin? |
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| Q8 | How important is collaboration? |
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| Q31 | Have you encountered any failures? |
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| Q29 | What advice would you give a young researcher? |
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| Q24 | How important is field work? |
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| Q17 | What qualities do economists or researchers need? |
|  |  |
| Q56 | How important is it for economists to address the world’s biggest challenges? |
|  |  |
| Q44 | How do you spend your spare time? |
|  |  |
| Q35 | When or where do you get your best ideas? |
|  |  |
| Q17 | How do you maintain your curiosity? |
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| Biographical | The Economist as Detective I have always wanted to be a detective. As a young child in New York City, I was determined to uncover the secrets of the mummies at the Museum of Natural History. I grew up in the Parkchester section of the Bronx, New York with my parents and older sister, Judy. I was a happy and vivacious child (Fig. 1) filled with curiosity. My mother became an early childhood teacher when I was about five years old (typical of many in her cohort) and later was an assistant principal at Public School 105 in the Bronx. My father was a middle manager, eventually at Burlington Industries, where he was in charge of data processing, programmed an IBM 360, and supervised a bevy of key-punch operators.  I took advantage of much that NYC had to offer – young people’s concerts at Carnegie Hall with Lenny Bernstein, ice skating at Wollman Memorial, folk music in Greenwich Village, window shopping, and of course, the museums. But when I encountered Paul de Kruif’s *Microbe Hunters* in junior high school, I realized a new calling – bacteriology.1 I would be a micro hunter myself.  I had the good fortune of attending the Bronx High School of Science, where I studied the subject that had, long before, brought fame to [Koch](https://www.nobelprize.org/prizes/medicine/1905/koch/facts/), Pasteur, Lister, and others who sought the causes of infectious disease. But it wasn’t fame I craved; it was the thrill of discovery.  In the summer of my junior year in high school, I took an undergraduate bacteriology course at Cornell University as part of a program sponsored by the U.S. National Science Foundation. (Fig. 2) A year later I entered Cornell as an undergraduate with the objective of majoring in microbiology. But college introduced me to other subjects about which I knew too little and to new worlds to discover.  In my sophomore year at Cornell, I encountered Alfred (Fred) Kahn, whose utter delight in using economics to uncover hidden truths did for economics what de Kruif’s stories had done for microbiology.2 I had found my new major. I abandoned my microscope and moved my detective work to libraries, dusty archives, and, much later, large electronic data sets.  After earning my B.A. in economics at Cornell, I entered graduate school at the University of Chicago to continue my study of industrial organization and regulation begun with Kahn. It was almost pure luck to have chosen Chicago, and I don’t know what led me to believe that it would have been a great place to study and to live. Hyde Park was, in fact, was a lousy place to live in 1967; it was unsafe and had few amenities. But it was the very best place to begin graduate work.  The greatest economists were in Chicago – [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/), [George Stigler](https://www.nobelprize.org/prizes/economic-sciences/1982/stigler/facts/), [Gary Becker](https://www.nobelprize.org/prizes/economic-sciences/1992/becker/facts/) (a year later), Arnold Harberger, [Robert Fogel](https://www.nobelprize.org/prizes/economic-sciences/1993/fogel/facts/), Lester Telser, Deirdre McCloskey, Zvi Griliches, [Ronald Coase](https://www.nobelprize.org/prizes/economic-sciences/1991/coase/facts/), [Robert Mundell](https://www.nobelprize.org/prizes/economic-sciences/1999/mundell/facts/), Gregg Lewis, Harry Johnson, among others – and they taught with religious zeal.3 I did exams in both industrial organization and labor economics, although I ultimately wrote in the field of economic history.  My first project as an economic historian was my dissertation. It began as a term-paper in a course taught by Robert Fogel and concerned the role of slavery in the urban and industrial development of the antebellum South. Fogel strongly encouraged me to expand it into my dissertation, although it was several years later that I would discover his broader interests in the history of slavery. I was persuaded to write a dissertation in economic history and to call myself an economic historian.  I had loved growing up in NYC, but I had always known something was missing. As a child, I longed for nature and animals. Graduate school gave me the chance to learn the joys of hiking, backpacking, birds, flowers, and, most importantly, dogs. I got my first Golden Retriever, Kelso, in 1970. I also learned to drive, since as a New Yorker I hadn’t. It may seem odd that I discovered mountaineering in Chicago, but the Rockies were just a (long) day’s drive away, and I was fortunate to find a group of rock climbers, backpackers, and naturalists at the University of Chicago to show me the ropes.  After graduate school, I took an assistant professor position at the University of Wisconsin–Madison largely because it was a great place to learn more economic history. It had the editors of the two major U.S. journals in the field and several other economic historians. I had no clear vision of my future.  I continued to work on the economic history of the South: the Civil War, emancipation, the post-bellum era, and the role of slavery in the labor force participation of Black women after emancipation. It was a heady period in the field of economic history (see Goldin 1995a), a great time to be doing economic history and to be studying the economics of the American South and the history of Black Americans. But, looking back, I was searching for something of deeper personal interest.  As one of my teachers, Ronald Coase, has noted about his own experiences: “I came to realize where I had been going only after I arrived.”4  When I began at Wisconsin, I taught a class of around 250+ students, including most on the football team, in an enormous room with clanking radiators and a blackboard I could hardly reach. I used an overhead projector. It was a drafty room, and I could hold down either my mini-skirt or the slides. I had never taught before and, looking back, I find it astounding that I was entrusted with this large and potentially unruly class.  Despite my inexperience, I was a highly effective teacher of the large economics principles and intermediate theory courses at Wisconsin. And I continued teaching these courses at Princeton and at the University of Pennsylvania.  I left Wisconsin two years after I began as an assistant professor in part because of an incident involving Kelso. I brought her to my office, because I had to walk through a wooded area to get my car and I worked late. One day, I was handed a violation (written on a traffic ticket) by a Wisconsin state trooper who came to my office wearing the characteristic wide-brimmed hat and high leather boots of the state police and carrying two revolvers on his hips (not intended for me or my dog Kelso). He was enforcing a little-known late nineteenth century law that banned animals (like pigs, not Golden Retrievers) from entering state office buildings. Why the law was used against me and Kelso had much to do with a recent history at UW of violent anti-war demonstrations and the fact that the animal control officer thought I was a student. I soon moved to Princeton University where Kelso was welcome and I was an assistant professor.  After I moved to Princeton, my work began to focus on family decisions in the late nineteenth and early twentieth centuries about who worked and went to school, when children left home, where the family resided, and so on. The work sustained me for some time, but around the late 1970s I realized that something was missing. I was slighting the family member who would undergo the most profound change over the long run – the wife and mother. I neglected her because the sources had. Women were in the data when young and single and often when widowed. But their stories were faintly heard after they married, for they were often not producing goods and services in sectors that were, or would be, included in GNP.  In 1979, I moved to the University of Pennsylvania from Princeton (both institutions, by the way, generously allowed Kelso to be in my office). I had been denied tenure at Princeton. But I was not surprised since there was little interest in my work. Unpardonably to us today, I was never asked to give a seminar when I was at Princeton and never even gave one when I interviewed for the position in 1971 (I turned down an offer to go to Wisconsin) or in 1973, when I received the position again. Economic history was apparently not valued in the department until recently, with the hiring of two of my former students. I went to the University of Pennsylvania as an associate professor. It was a department and a university that greatly valued economic history and had Richard Easterlin, a distinguished economic historian, and Sam Preston, a renowned demographer and historian. I was granted tenure at Penn around 1983 and became a full professor soon after.  Around the same time that I moved to Penn, the National Bureau of Economic Research (NBER), founded in New York City in 1920, was transformed by Martin Feldstein and opened an office in Cambridge, MA. My life would also be altered by that move. Feldstein was a visionary, and he reconfigured the NBER to be an “umbrella” organization that would have members in various fields, publish working papers, and run conferences. He asked Bob Fogel to be the director of the Development of the American Economy (DAE) program. Fogel asked Bob Gallman and me to be members of an executive committee that would determine the research included in the DAE program. Since the NBER had been founded to create the statistical foundation of the U.S. economy, we decided that I would accomplish that for the female work force. I knew it would be a story of importance, relevant to the current period, and a project for which my detective work would pay off. I also knew that I was the one to do it.  Women’s role in the American labor force was unfolding before me, and I had personally experienced many of the changes I would be studying. Yet I would come to realize that change was neither as precipitous nor as recent as most thought.  The central question I posed was: why did the female labor force expand at certain times and for certain cohorts? What had caused married women to increase their paid market participation rate from around 5% to 70% across the twentieth century? I first had to track the expansion in every possible way. I began by assembling as much data as I could find in easily accessible sources. I pushed the project forward in time (to the present) and backward (to the 1790s), and tackled various topics in turn, producing series or estimates on the labor force by age, marital status, race, and ethnicity. I also produced series on earnings, work experience, and “wage discrimination,” among others.  When I began the NBER project on women in the labor force I thought I could find all the data I needed in published census documents. I soon discovered that even twentieth century data were imperfect.  Modern labor force constructs were not used prior to 1940. Rather, individuals were asked about their “gainful employment.” If a woman worked 25 weeks out of the year or fewer, she might not have answered that she had an occupation. I became more aware of the fact that the bounds of market work omitted many women who labored in their homes as family-business workers, boardinghouse keepers, family-farm laborers, and piece-rate workers. There was also the nagging question of whether the social norms of the day meant that married women gave census-takers socially accepted answers rather than the factually correct ones.  I needed retrospective work histories predating modern collections, data on how much time women spent in the labor force over the year, and information on the “hidden market work” of married women, among other statistics. I needed to know the truth about the female labor force from 1790 to the present. What would Sherlock Holmes (“there is nothing like first-hand evidence”) have done? Off I went to the National Archives, Washington, D.C., in search of micro-level surveys.  I look back on my years as an economist with no sense that there have been watersheds related to appointments, promotions, fellowships, honors, acceptances. I do, however, remember the precise moment that I found documents at the National Archives containing information on whether firms hired married women; surveys covering the labor market histories of women during World War II; and questionnaires given to a large fraction of women in the college class of 1957. I remember the “eurekas” I quietly exclaimed when my model or framework took on life and began to “talk back” to me.  The National Archives today is a tightly controlled place. No one is allowed into the stacks except the official searchers. But when I went to the National Archives in 1981 and requested information about the Women’s Bureau records, I soon discovered that the relevant “finding aid,” as the archival organizing tool is called, was vastly incomplete. The searcher invited me into the stacks where I was able to rifle through the boxes. I soon found a gold mine of original surveys.  One of the surveys concerned office workers and their firms and covered a host of industries across various cities in 1939. Information for the firms included data on the education, earnings, and work experience of their office workers, among other mundane variables. But the second page of the survey contained remarkable questions – whether the firm “discriminated on the basis of race,” if “there were any jobs for which the firm would not hire a woman (a man),” whether “married women were not hired and single women were fired when they married,” and if “married men were paid more than single men.” Without any anti-discrimination legislation, managers answered the questions candidly.  The Women’s Bureau boxes yielded five data sets for my project. One allowed me to produce work histories for women before 1940, another exposed much about the extent of and reasons for “marriage bars” in the 1920s and 1930s, and one enabled estimates of “wage discrimination” in 1939. I had, indeed, located much of what I had gone to the National Archives to find. But what was I to do about the earlier decades, when the sources were far less abundant and less quantitative?  Once again, I first tapped the obvious – the census of manufactures. I also discovered that all major and many minor cities had extensive city and business directories dating back to the late eighteenth century. (One can think of these as pre-telephone phone books.) I used these documents to find the “hidden market work” of married women when family businesses were significant. The occupation of a husband would have been listed. But if he suffered an untimely death (there were many during epidemics), his widow might have been listed in a subsequent directory. If an innkeeper’s widow was listed as an innkeeper, a reasonable presumption would be that she did the same the previous year, when her husband was alive, but she was “hidden” and not listed.  I was closing in on a full and rich story to tell about the evolution of the female labor force. But I was still lacking evidence on the role of World War II. What happened to Rosie and her compatriots? I wasn’t certain, but I knew that I couldn’t delay my book to find the answer. I spent the academic year 1987/1988 at the Princeton Industrial Relations Section writing the book (Goldin 1990). I continued to pursue data leads on the 1940s.  I was aware that the economist Gladys Palmer had worked from the 1930s to the 1950s at the University of Pennsylvania (my employer at the time) on surveys concerning unemployment and labor force mobility. My colleagues in the sociology department believed that many of the original surveys were in boxes in the McNeil Building where I worked. I finally located the boxes, literally, right under my nose in a nearby closet. The boxes contained thousands of surveys of working women (and men) in 1951 and their complete work histories back to 1940. I had discovered the treasure for which I had been searching. Combined with other information that serendipitously showed up, I was able to piece together a more complete history of the female labor force during and just after World War II, although I wasn’t able to include the findings in my book.  How did I collect all these data? Much of it was collected the old-fashioned way – by hand and often by me. The forms tell stories, and I listen.  After *Understanding the Gender Gap* was published, I moved from the University of Pennsylvania to Harvard. I greatly enjoyed my decade at Penn. I was a member of many “graduate groups” that put me in close contact with historians and demographers, and with other female professors at Penn. But the Harvard economics department was better. I knew that Larry Katz was there, and I suspected that I would learn a lot from him (I was right). Another development was that a year before I moved to Cambridge MA, I took over from Fogel as the director of the NBER Development of the American Economy program. I remained in that position for 28 years and then became co-director of the NBER Gender in the Economy working group.  When I moved to Harvard, in 1990, I decided to explore a new area of research – the economic history of education. And since changes in the education of the labor force alter the earnings of workers, the project would also be about economic inequality, a topic I had worked on with Robert Margo when we explored the “great compression” of the 1940s.  A rising star in labor economics – Larry Katz – was also working on economic inequality. His knowledge of the recent period and mine of the historical era eventually produced a large number of papers on long-run economic inequality and technological change. That work eventually became our book *The Race between Education and Technology* (2008). We also became a couple and ultimately married.  As in my research on the female labor force, the readily available data on economic inequality were deficient since information on education and income were not collected by the federal government until 1940. But I discovered the 1915 Iowa State Census, the first and only U.S. census to inquire about education, earnings, religion, ethnicity, and property values. Katz and I began a project to digitize a large sample of those data.  Only by entering hundreds of observations myself did I discover important details about education on the eve of the great expansion of U.S. high schools. The latent demand for high schools was revealed in the excessive number of years youths were spending in common schools in rural areas. I might not have discovered this important point had I not read the schedules myself.  The project began when I was on leave at The Brookings Institution and Larry was on leave as the first chief economist at the Department of Labor. By scouring various educational data sources at the federal and state levels, I pieced together the state-level data on the expansion of secondary schools, known as the “high school movement.” I soon discovered that the growth from 1910 to 1940 was exceptionally rapid in certain parts of the nation. The leading states formed an “educational belt,” running from parts of New England to the central portion of the Plains states to the Mountain states and on to the Pacific. These were rich places, relatively homogeneous by income, race, religion, and ethnic origin, primarily non-manufacturing, and in non-southern states. The proportion of their population that graduated from high schools in 1925 was almost double that in the rest of the nation.  I then studied the impacts of these large and sudden shifts in the supply of educated labor on the wage structure. The premium to ordinary white-collar office workers collapsed by the early 1920s. Relative to production workers, the clerk, stenographer, typist, secretary, and bookkeeper saw their real wages fall as the supply of high school graduates vastly expanded. Various high-technology industries were introduced in the period, including electrical machinery, aircraft, non-ferrous metals, chemicals, and paints. I turned my attention to whether these industries were hiring disproportionate numbers of high school graduates as blue-collar workers. The answer was that they were. Education endowed workers with valuable cognitive skills, and firms in particular industries prized these abilities. The industries that were more willing to pay for the higher priced blue-collar workers were more capital intensive, newer, and higher-tech.  At the same time that I was working on the education, technological change, and economic inequality project, I was also working on gender issues. No matter how similar men and women had become with regard to their education and occupations, and no matter how much weaker were social norms, gender differences in the economy and society remained.  When I arrived at Harvard in 1990 I was given a chance to create my own courses. I gave one on “Work and Family.” In crafting that class, I began to realize that college educated women in the U.S. had greatly altered their ambitions and achievements across the twentieth century and formed five distinct cohorts. That was the beginning of my book *Career and Family: Women’s Century-Long Journey toward Equity* (2021), which took shape across next three decades.  I also completed important parts of the history of long-term change in the economic role of women by writing a paper (Goldin 1995b) on the U-shaped hypothesis of female market participation that was implicit in my 1990 book. Correctly measured, women’s participation generally decreases as markets develop and as work leaves the home. Their employment later increases as their value in the market begins to exceed that in the home. I also wrote a draft of “A Pollution Theory of Discrimination,” which took another two decades to become an article (Goldin 2014). It is among my favorite pieces because it is a combination of economic theory and deep historical work and resolves the question why and how men discriminate against women when they have no social and personal animosity toward them.  Among my most-cited papers is one on blind auditions for orchestral positions, which involved placing a curtain or screen between the contender and the jury (Goldin and Rouse 2000). The late Justice Ruth Bader Ginsburg cited the work in interviews, speeches, as well as in a dissent.  How the project came about is yet another detective story. In 1991, I learned of the use of the “blind” in orchestral auditions. Cecilia Rouse (who was, until recently, the chair of the U.S. Council of Economic Advisors) was then a Harvard economics graduate student. In the NBER office kitchen one day, we discussed whether we could evaluate the role of blind auditions in orchestral hiring. We could get roster data. But could we get the actual audition records? That seemed impossible, and it almost was.  After a year of trying to get any top orchestra to answer our phone calls and faxes, the orchestral manager of the NY Philharmonic invited us to use their archival records. Most of the top eleven orchestras followed in the next few years. We traveled across the U.S. from orchestra to orchestra and copied audition lists. We worked in wood-lined archives and we worked in dusty attics and trailers. The paper took almost a decade to research and write. It also was also the first paper I ever wrote with one other female co-author. I have had many male co-authors before that paper and after. But changes in the gender mix of economists enabled me to find exceptional female economists to work with (e.g., Marcella Alsan, Sari Kerr, Adriana Lleras-Muney and Claudia Olivetti).  The late 1960s and early 1970s were pivotal for women. I have termed the changes during that period the “quiet revolution.” Some of the changes, I thought, occurred because of advances in birth control technology. The history of the development of the birth control pill was fascinating as were the state laws that constrained its sale to single, young women. It made for great history and personal stories, but economists require identification of causal statements. I realized that the laws that constrained the sale and distribution of contraceptives changed in somewhat random ways. In my work with Katz (2002), we used this random variation to identify the impact of the pill on the age at first marriage and the entry of college graduate women into a host of professional and advanced degree programs.  To understand women’s relative progress and their earnings across a host of professions, I needed detailed data not available in usual sources. Katz and I ran the Harvard and Beyond survey. We worked with Marianne Bertrand to survey business school graduates. I obtained confidential data from pharmacy and veterinary organizations, and I used extensive restricted data on doctors and lawyers. The evidence seemed clear: women began their careers with earnings that were fairly similar to men’s, but they did relatively less well as they began their families. The progression was particularly severe for occupations that were “greedy” and disproportionately rewarded long, variable, and demanding hours.  My American Economic Association presidential address, “A Grand Gender Convergence: Its Last Chapter,” showed that earnings differences between men and women were due more to the difficult choices that couples make in a labor market of greedy jobs than to factors that often get the most attention, including bias and discrimination.  I am often asked what it is like to be a woman and to work in a field still dominated by men. I have been the first female economist to be offered or to have achieved tenure at several major universities. I don’t find that distinctive because many brilliant and determined women came before me. I do know that there are inequities and misinformation in the field of economics that deter women from majoring in the field. When I was president of the American Economic Association in 2013, I decided to figure out why women were just 27 to 33 percent of undergraduate majors in economics. I began the Undergraduate Women in Economics (UWE) Challenge, a randomized controlled trial (RCT) funded by the Alfred P. Sloan Foundation to help attract women to a field that provides lucrative and sustaining careers and helps us understand pressing issues, such as those in economic development, health, education, and social mobility.  I began life as an inquisitive, joyous child in the nation’s largest metropolis. Yet I knew something was lacking for me. I found the missing element when I discovered the Rockies, the Wind Rivers, and the Cascades and engaged in long backpacking trips through rugged terrain, carrying all my gear and food, taking risks in numerous ways, and trekking with my dog Kelso (and other companions). I continue to be a naturalist, a birder, and a dog trainer. Kelso lived an amazing (almost) 16 years. I eventually got Prairie, who died at eight from cancer. I currently have Pika who is active at 13.5 years and is a therapy dog at a local nursing home. I began competition work (obedience, rally, scenting) with Prairie and continued with Pika (Fig. 3), who holds titles in competitive scenting. I am delighted that the Prize committee and graphic designer depicted me with a magnifying glass, a Sherlock Holmes’ hat, and a Golden Retriever (Fig. 4). That is how I would like to be remembered: as an economist detective with an obedient dog at her side.  1. *Microbe Hunters*, first published in 1926, remains in print and on Kindle, as well as in Audible Audiobook format. Other laureates have also pointed to its influence, including Earl W. Sutherland, awarded the 1971 Nobel Prize in Physiology or Medicine.  2. Kahn was an expert in regulation and deregulated the airlines when he was chair of the U.S. Civil Aeronautics Board (1977/78). He began teaching at Cornell in 1947.  3. In that group Friedman, Stigler, Becker, Fogel, Coase, and Mundell are Laureates in Economic Sciences.  4. R.H. Coase, “My Evolution as an Economist,” (1994), unpublished version of a lecture in the “Lives of the Laureates” series, given at Trinity University, San Antonio, Texas on April 12, 1994. |
| Autobiographical |  |
| Podcast | “I think we all have doubts about what we’re doing. I wouldn’t call that failure. It’s a sense that we question our own work” There are many roads one can take in life. But to what extent will your life choices decide what kind of person you become? Listen to our podcast conversation with economist and laureate Claudia Goldin, as we discuss the choices that brought her to this moment in time.  Our podcast host Adam Smith, who meets Goldin in the year after she received the prize in economic science, also talks with her about the definition of a good teacher and Goldin’s pioneering research in women’s labour market.  This conversation was published on 6 June, 2024. The host of this podcast is nobelprize.org’s Adam Smith, joined by Clare Brilliant.  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.  Claudia Goldin: Robert Fogel was so fascinating and so interested in figuring out why things happen. People make these subjects special, and I know that every day when I walk into a classroom that I am representing a field. If I do it well, I will convince people. If I don’t do it well, they’re going to walk away.  Adam Smith: You can truly hear Claudia Goldin’s commitment and sense of responsibility listening to her there. That question she’s posing why things happen is absolutely fundamental to making progress. The way she often phrases it of why are things as they are, is the starting point for everything. I think there are probably far too few people looking at the world and asking, how did it come to be like this? Because that is surely absolutely key to working out how to make things better. I suppose the public celebration around Claudia Goldin’s prize was in part because people recognised the importance of that historical perspective in studying questions that are of burning importance today. Please join me for this conversation with Claudia Goldin.  Clare Brilliant: This is Nobel Prize conversations. Our guest is Claudia Goldin, the 2023 laureate in economic sciences. She was awarded the prize for having advanced our understanding of women’s labour market outcomes. Your host is Adam Smith, chief scientific officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces. Claudia Goldin is the Henry Lee professor of economics and holds the Li and Deletto professorship of Arts and Sciences at Harvard University. She talks to Adam from her office in busy Cambridge, Massachusetts, as we hear them discuss channeling a preacher’s fervor in her classroom, how she takes care of her students long after graduation, and narrowing her academic interests down from practically everything to economic history. But first she tells us about her golden retriever and how the nose truly knows.  Smith: Pika is a champion, right?  Goldin: He’s titled in scenting, he’s not a breed champion. Scenting is the sport version of what we see in the real world in bomb detection, airport security, and so on. Whereas tracking is the sport version of search and rescue. When they do scenting as a sport, you train them on three essential oils. In the US we use birch, clove and anis, and these scents are then hidden in various places, outdoors, indoors, on vehicles, etc. The dogs have to find the scent. They have to find the source of the scent. Then when you train a dog to do bomb work or to do airport security, all you’re doing is you’re switching the scent from birch or CL or anis. You’re adding a scent. You’re saying now you’re supposed to find money, the scent of money, you’re supposed to find prosciutto, you’re supposed to find any type of food.  Smith: Have you ever found a practical use for what Pika can do?  Goldin: It turns out that Pika not only has great scenting ability, but he can also sent cancers, which is bizarre in other dogs. I’ve never tried him on anything else. But he has scented two cancers on two different dogs.  Smith: That’s extraordinary.  Goldin: I know he also scent UTIs. He’s very good at scenting UTIs on female dogs.  Smith: Presumably this isn’t a skill that he alone possesses.  Goldin: No, it’s not. But he’s just very affected by whatever proteins there are that are to him odd. He’ll shake and he’ll become obsessive about it.  Smith: I imagine that skills like pikers are already being used by vets and perhaps doctors.  Goldin: Most of the work that’s done with these scenting dogs does have to do with the detection of disease. Some of the most famous of this work had to do with the detection of brakes in the oil pipeline. It’s a very famous example. When they built the oil pipeline under the tundra, they knew that it was leaking. They knew that they were getting different readings at one end in the other, but they couldn’t figure out where it was leaking. Whatever they tried to do, they couldn’t get readings of the leakage. Someone was hired to bring three dogs to train them on that particular odour. One of the dogs managed to find at least a hundred leaks, in fact, way more than a hundred leaks. This was leaks that were under the tundra. The ability of these dogs to scent and to scent things that we do not have mechanisms we don’t have machinery to do is pretty extraordinary.  Smith: Absolutely amazing. Also a beautiful link to your own childhood desire to be a detective and that you are uncovering information from the archives. Let’s start with your childhood. You grew up in New York wanting to be a detective or an archeologist or someone who found things out.  Goldin: Sort of. I can’t say that I grew up thinking that I was going to be anything. I grew up as a happy child in the Bronx and this was not a place of big playgrounds, trees, grass and flowers. This was the city. It was an okay place, but it wasn’t a beautiful green environment. I think that I, as a kid, unlike my students right now, did not have much of a sense of where I was going. I was just a happy kid. When I would go to museums in Manhattan, I would explore various parts of these museums. One of them happened to be the mummies. What child is not interested in beautiful gold cases that have things that are really unknown in them. So I was interested in how they found them and where they found them.  Smith: Do you think that there’s possibly too much pressure placed on young people now? Maybe there was then as well. To be something, to know what you want to be, to take a path when actually it’s better just to explore the world in general.  Goldin: Certainly the students I see have this notion that they have to have a plan. My guess is that they have felt this way for a very long time. It has been an incredible burden because when you have a plan, chances are you’ve been disappointed. Of course, the fact that they’re all at Harvard means that they haven’t been too disappointed.  Smith: Yes. But I suppose it’s possible that you will change as you grow up and that you will find that actually you’ve taken on a life and possibly even a personality that didn’t quite fit when you get to the end of all this study and work.  Goldin: I think in addition, there are many roads that one could take. I think if I hadn’t become an economist, I would’ve become maybe a field biologist or a bacteriologist. How would I be different right now? Maybe I wouldn’t be different at all. I’d be thinking in somewhat different ways about very different things, but the person wouldn’t be very different.  Smith: I read that one of the books you encountered as a young person was Paul Decree’s ‘Microbe Hunters’. That’s a book that so often comes up in conversations with Nobel Prize laureates, especially laureates who grew up at that particular time. What was it about that book that you found influential?  Goldin: It’s beautifully written. I think that’s part of it. It shows the importance of writing well and the stories are the stories of the great successes. In fact, one of the interesting things about what we were just speaking of is that it’s also important to write stories about people who took a path and it didn’t go anywhere. People whose experiments didn’t work out. Of course this book is not about that. This book is about the Pasteurs and the cos and the semi vices, the listers, the people who for much of the 20th century, we believe they saved our world. What’s interesting is that in my own work as an economic historian, up until three years ago, I would say to classes the germ theory of disease, finding that was extraordinarily important in making certain that the population supported public works that cleaned water and that separated sewage from clean water. That saved us, that these advances led to vaccines. They led to the acceptance of these vaccines by many. What it proceeded to do was (this is what I would’ve said three years ago) get rid of infectious disease as a great killer. What we’re left with is chronic disease. I would of course revise what I said then. In fact, what I would say now is that we dodged the bullet many times since 1918. We dodged it over and over again. The COVID pandemic was destiny.  Smith: There’s more coming down the line, no doubt at some point.  Goldin: Right? But we are far better prepared. We got that pandemic. We somehow were lucky to have that happen at a moment when we had the mRNA vaccine ability on the shelf. In fact, that’s what the other Nobel Prizes were for. At the same time, we had high speed internet, we had cloud storage, we had all of the things that allowed us to deal with the pandemic, stay at home, stay safe to the extent that we could stay safe at home and be productive.  Smith: Indeed, yes. Of utterly different world from 1918. I really love this idea of having well-written stories of failure alongside the well-written stories of success. So often people like you talk about the importance of learning to fail and students ask about it all the time. How do I get used to the failure as things start to go wrong as you get more into research? But we don’t at school really encounter stories of failure and we, we are not prepared for it. As you said earlier, that it’s about a succession of successes that you have to aim for and you feel terrible if it all goes wrong and you don’t meet your goals as a young person. How do you inculcate an acceptance of failure and a, a way of dealing with it?  Goldin: We do this in small ways. We don’t want to say to someone, and by the way, you’re embarking on something sort of like throwing someone out from a plane with a parachute and saying, there’s some chance that parachute isn’t going to open. We’re not going to do it in the big way. But what we do, I’ve been in charge of placement at Harvard for several years and I’ve done placement for a long time. What placement is every year we produce a certain number of PhD students and someone, me responsible for making certain that they get jobs. That doesn’t mean that I get them a job, but I make certain that when the school is looking for someone in a particular field, I talk up our students, I learn what they’re working on. What I do as well is I prepare our students for what it’s like to be on the job market. What we do is we discuss the small failure. We say, you’re going to get X number of interviews and some of them aren’t going to go very well and some of them are going to go very well, but you’re never going to know which one is going to invite you back. Let me just tell you that it’s generally a third of them will invite you back. Is it two thirds failure? No, it’s one third success. That’s one thing that we do. Another thing is that all of our students will be writing papers and submitting them to journals and getting referee reports back. Some of these reports are going to say, we don’t want your paper. Some of them are going to say, well we don’t want it unless you do the following. We help them with that.  They’re not failures. There are small types of moments when you didn’t get a yes, but you didn’t really get a no either. So you shouldn’t stop in your tracks and rethink your entire life. These are small moments when you know you didn’t get the yes. But these are moments that young people, someone who feels that their job is on the line, their personal sense of worth is on the line. As an advisor, I make certain that they realise that we all get these. So that said, it’s the same thing in love, of course.  Smith: Yeah. But it hurts more in love.  Goldin: In fact, I’ll tell them that whatever you feel now, it’s going to feel a lot worse if this was love. But in fact, as someone who teaches graduate students when they leave with their PhDs, we often say, and it’s really no joke, you have a lifetime warranty. We get students from 10 years ago sending us notes that say, I just got this referee report. What should I do?  Smith: How extraordinary that you take care of your flock in this way. That’s, I think probably pretty unusual that you have the energy and the resource and the desire to do it.  Goldin: We have excellent students and they are the next generation in our field.  Smith: One of your laureates from 2023, of course Katalin Karikó had failure in spades rejection by her university. She was downgraded in her job, eventually fired. She has this extraordinary way that she talks about that she dealt with it all, which was really to think that these are things I can’t control, so let it be, I’ll just concentrate on what I can fix. Which indicates a resilience that is beyond what most of us I think have.  Goldin: Right. She’s passed that resilience onto her daughter.  Smith: Exactly. The Olympic champion.  Goldin: I think we all have doubts about what we’re doing. I wouldn’t call that failure. It’s a sense that we question our own work. I always tell my students, be your own worst enemy or else someone else will be. That’s what I would think of as moments of not failure but moments of self-doubt. But they’re moments in which you think I made this statement. Can I really back it up?  Smith: Are you a very introspective person, do you think? Do you ask that question of yourself a lot?  Goldin: Never. I actually think that economists are not very introspective.  Smith: Why do you say that?  Goldin: Just because of the ones I know are sort of less introspective. They’re less filled with angst. There are some economists filled with angst, but I don’t see that many that are filled with angst. I think that we are more forward looking.  Smith: How very interesting. Is it too uncertain a field to be filled with angst? If you start worrying about the uncertainties, then you’re just going to get lost.  Goldin: I think that it is sort of like physics. It’s a field in which the world around you is governed by a set of forces. If something changes, you have some idea how the rest is going to change. The problem is that you don’t know with certainty what is going to change. For example, war. Did we know that Russia was going to invade Ukraine? Of course not. We know, for example, if there’s a group of children who are 1-year-old, we know how many are going to be there in 10 years with pretty good certainty. But do we know that in a country that is overrun, can we figure out what’s happening in Haiti now, for example. But I think that by and large, we understand how economies function. We do not have control over these more difficult events, obviously.  Smith: Extending that idea of the fundamental forces, do you think that economists have a good understanding of the fundamental forces that control the economy? Or is that search as in physics sort of still ongoing, that there might be more to be discovered?  Goldin: I would say that part of me says we know a lot and part of me says that I’m humbled and would say there are things that we do have to learn more about. Many of those have to do with how individuals make their own set of decisions. We see that in the US today. Many of us do not fully understand how it is that some large sections of America can support a candidate who, to us seems to be undermining the democratic process. How can they support a candidate who seems to not care about the votes of the majority? In that sense, we need to sometimes be more of an ethnographer and less of an economist. Sometimes I feel that I would learn more by embedding myself in a place that isn’t Cambridge mass to understand how the economy functions.  Smith: We discussed how you thought you might be out in the field as a biologist, but you found your way to economics and historical economics. What drove you in that direction?  Goldin: When I was an undergraduate, like many undergraduates, you realised that you don’t know a lot. I went to Bronx High School of Science. I came to Cornell University thinking that I knew a lot about a small field, which was bacteriology. When I got to Cornell, I realised that there were many fields that you’re not introduced to when you’re in high school. Political theory, for example, anthropology, philosophy, and economics. I never knew any economics when I was at Bronx High School of Science.  Smith: Just as an aside, one should mention that Bronx High School of Science has been a great generator of researchers of great importance for a very long time. What was it that was so magical about that particular place, apart from the intake? Because I know it’s very competitive to get into.  Goldin: Part of it was where I would’ve gone otherwise. I would’ve gone to a place called James Monroe High School then at that time was, I believe the largest high school in the United States. It had 10,000 kids, and it was in the South Bronx. That would’ve been a difficult place to go be and learn. What made science magical in part was the fact that we had a nice new building. In part because we had very nice, able and smart teachers. But what really made it special with the students. I was there with other students who had passed this test and who were just a group of wonderful, nerdy kids.  Smith: So the nerdy kid finds themselves at Cornell discovering all these other unknown.  Goldin: Right, discovering lots of other things. Somehow the way economics was taught to me by someone named Alfred Kahan, who called himself Fred Kahn, just attracted me immensely. Fred Kahn taught a portion of economics called industrial organisation and also regulations. When I was at Cornell, I specialised in industrial organisation and regulation. When I went to the University of Chicago, I went to Chicago to study that subject. I went there because there were phenomenal faculty who did work in industrial organisation and people who study industrial organisation are interested in product markets and how stuff is made, priced, delivered, the variety of goods, how we regulate utilities, why we regulate utilities. Of course that’s become of enormous interest recently with the internet. Those were the areas that I was interested in. In some sense. I just liked economics.  Smith: Then what turned you into a digger in the archives? A historical economist.  Goldin: When I was at Cornell, the other field that I did a lot of work in was history. In particular foreign policy with a great professor named Walter Lafe. Much of what I’m saying about why I took certain courses, why I like certain subjects points to people, people make these subjects special. I know that every day when I walk into a classroom that I am representing a field and that I am interpreting a field, I am diffusing the knowledge in the field. If I do it well, I will convince people. If I don’t do it well, they’re going to walk away. For me, Walter Leber, Fred Kahn convinced me of these two fields. When I got to graduate school, I also discovered that there was a connection between those two subjects. That was through someone named Robert Fogel, also a Nobel Prize laureate.  I took IO with George Stigler, a Nobel Prize laureate. Of course these Nobel Prizes were awarded to these people long after I took their courses. But Robert Fogel was so fascinating and so interested in issues in economic history and figuring out why things happened. What was the importance of the railroads? Was the US south poor all the time? When did it become poor? If it wasn’t poor all the time, what was the plight of black Americans? Questions that I always knew would be interesting suddenly became questions that I could explore and I could explore them in archives.  Smith: How exciting. Questioning the status quo. How did things come to be like this? Has it always been like this? Wonderful fundamental questions, which sometimes one ignores in the kind of maelstrom of everything else that’s going on,  Goldin: Right? But what Robert Fogel taught us was that you could confront these questions by taking the models of the economist. These are difficult questions. If you ask us, he did the railroads cause American economic growth? Or how much did they cause? How could you answer that big question? You can do it by expressing it in a somewhat different way. You can do it by expressing it in a counterfactual. If the railroads didn’t exist, what would the growth rate of the US economy have been? How can you do that? That takes us down a different path of how could you figure out how much longer it would’ve taken to ship goods if you didn’t have the railroad? Where would the goods have been produced?  Smith: Fascinating. So equipped with the right technique, you can dissect the question. The description you gave of yourself when you walk into a classroom was really beautiful. The idea that you are the standard bearer for diffusing this knowledge to your students. Of course it can apply to teachers of any subject at every level. It emphasises the enormous importance of teachers generally and the responsibilities and possibilities of being a teacher. What was it in particular about Fred Khan, for instance, that made him such an amazing teacher?  Goldin: I think that it was the fact that he would come into a class and know that he was the standard bearer for that. That he had something that he wanted to convey and he was going to convey it in any way he could. I remember clearly that he ran out of room on a blackboard and the floor in the auditorium was filled with dust and he just got down on his hands and knees and drew in the dust.  Smith: That’s amazing. I can imagine students crowding around to see what was on the floor.  Goldin: I think part of it is that if you really believe in what you’re saying, then you will try very hard or you should try very hard to diffuse the knowledge in a way that convinces everybody. If you are walking into a classroom and you’re doing elementary algebra, you don’t really believe that it’s important for your students, but you’re not going to have the same energy. If you do have that level of energy, I would be very pleased. But most people don’t have that level of energy to teach a subject unless they believe very strongly in it. Which is perhaps why preachers, televangelists in particular, are so good at what they do because they really believe it and they really believe that if other people believed it, their lives would be better off. In some sense, one has to walk into a classroom if you are telling your class something that you believe in and be your own televangelist.  Smith: That connects with something that I’ve come to realise about all the Nobel Prize laureates I’ve been speaking to over the years, which is that it seems blindingly obvious, but it actually I think is important, is that they really believe in what they’re doing. They are truly interested in the questions they’re asking because it’s quite easy to sort of fake an interest or even convince yourself that you are interested in something when you’re trying to get something done. That’s very different from having a genuine interest, which just keeps you going and keeps you awake and just drives you.  Goldin: That’s right. It’s endless curiosity, but it’s also curiosity about something in particular. I find that I’m curious about an extraordinarily wide range of subjects. Most of them have to do with economics or education or the labour force. But I’m curious about many things and you can’t follow all of those threads. You have to limit yourself somewhat.  Brilliant: Adam, Claudia Goldin was clearly interested in a lot of subjects, but what area did she focus on that ultimately led to her prize?  Smith: That was for her work on the engagement of married women in the US in the labour market. I suppose she could have asked many questions because she was interested in so many things. But she was looking for a question of importance where she could access data that nobody had seen before that would shed light on the answer to the fundamental question that she was asking, which is why are things as they are, why is it that now about 50% of women are employed globally?  Brilliant: She sort of looked at this from the perspective of both a historian and an economist. What did she find out?  Smith: She revealed that much to many people’s surprise, married women’s involvement in the labour market had not increased linearly with time. That in 1820 or so, about 50% of married women were actively involved in the labour market. Then by the beginning of the 20th century, that had dropped well below 20% and then it gradually increased and now it’s back to about where it was in 1820 or maybe a little better. There’s this very dramatic u-shape curve rather than the straight line that some people thought was going to come out.  Brilliant: Did she find an explanation for the dramatic u-shape?  Smith: Yes, broadly it’s you know, supply and demand in the labour market and all the other factors that come to play. This is where she balances her work as a historian, digging out the data from these sources where it’s not easy to get the information with the economic theory, looking at how much women were involved in child rearing labour law, the employment practices of the employers, and then also the advent of technology and what effect that had so many different factors coming together to influence this very complicated picture.  Brilliant: What were the implications of the work?  Smith: Claudia Goldin talks about this herself very interestingly. It’s worth noting that this was a great time to be publishing these results. In the seventies, there was a lot of interest in these issues. It was a poignant moment. Let’s listen to her talk about these implications.  Goldin: The 1970s in the US and in other parts of the world were periods of uprising, periods of democratisation periods in which in the US in the 1960s, then building up to the 1970s, the civil rights movement, also the women’s rights movement. The work that I was doing was also of incredible importance because of what was going on at that moment that people were questioning if labour force participation has gone up so much, why are we still only earning 59 cents on the dollar? Some of the questions that I was answering through history were questions that were incredibly relevant at that moment.  Smith: It’s trying to summarise everything in two briefer way, but what would you say the main implications are for current employment practices of all that work?  Goldin: I think that what’s important in my work is that we can look back and we can say there has been enormous progress. People are generally at some point in time, often frustrated by a lack of change in their own lives or what they see as changes in other people’s lives. They don’t realise how much progress there has been. Many people who read my work will write to me and say thank you. Because now I see how much progress there was and I was blind to that before. Yet we have to ask, even though there’s been so much progress, why is gender still an important division in the labour market? For that, there have been many answers over the course of history. For example, there was a time (if we take the US) when although women did better in high school and had higher graduation rates in high school, they went to college and graduated from college at much lower rates than men.  When we look at what they were doing in the labour force, it’s not surprising that they’re not occupying various professional positions. We can see over time that many of the differences have disappeared. Women do not have lower levels of education. In fact, they have higher levels of education. We have to now confront the question, why does gender still matter in the labour force? In a somewhat different way. In some sense, the clouds have parted the clouds that would’ve been these large differences that existed. The clouds have parted. We could see more clearly that many of the differences are differences about what women and men do in their own homes. Not only is it what goes on in their homes, but it’s reinforced by what goes on in the labour market. The simple notion is that if one earns an enormously large amount more by working more hours or by being on call and you have children to take care of then one member of the family will take a job that is more flexible and be on call at home and the other one will take the job that’s more greedy and be on call at the office.  In some sense, what we see now that the clouds have parted is that even though of course there’s still problems in the labour force, there’s still bad actors, for example, there’s still this word that we throw around discrimination. By and large, most of the differences are due to the fact that women step back and men step forward in their job. This means that men lose out in terms of family and women lose out a bit in terms of career. The point is that there’s both gender equality and couple equity. When we give up couple equity, we widen gender inequality. I think that that’s how many of us think about what’s going on in terms of gender in the labour market. Now there’s no question it’s more complicated.  Smith: In the context of the societal changes going on, the confusion that many people feel about the current state of democracy. Are you hopeful about what will happen to gender equality in the labour market?  Goldin: I can see that some countries that have support for public goods, like subsidized childcare that have good support for the elderly, that those countries do have lower levels of gender inequality. But in a diverse nation like the United States, we have to understand why it is that for a very long time there’s been a group of individuals who want reproductive rights. Until we listen to them and understand we are going to be a very divided nation.  Smith: I must let you go. But I wanted to finish by asking whether this extra attention that is focused on you with the award of the Nobel Prize. There’s already been so much attention focused on you as the first female professor at Harvard to get tenure in economics. For instance, whether this additional, I don’t know what to call it, burden or accolade is making life difficult or you can take it all in your stride.  Goldin: I have said from the day I received the Nobel, that it wasn’t just mine and that it’s been magnified thousands and thousands of times. For that, I will take whatever burdens there are, I receive many, many messages, thousands. I have no idea what the numbers are of thank you for what I do. Not just because it illuminates the past, but because it validates and vindicates and emboldens the individuals and their work. For that, I am very proud.  Smith: That is a beautiful and very important point to stop on. Thank you very much.  Goldin: Indeed. Thank you Adam. It’s always a pleasure to talk to you.  Brilliant: You just heard Nobel Prize Conversations. If you’d like to learn more about Claudia Goldin, 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 Karin Svensson. The editorial team also includes Andrew Hart, Olivia Lundqvist, and me, Claire Brilliant. Music by Epidemic sound. If you’d like to continue with the theme of women in economic sciences, then check out our episode with 2019 laureate Esther Duflo. You can find previous seasons and conversations on Acast or wherever you listen to podcasts. Thanks for listening. |
| Telephone  interview | 0803=CG  [Claudia Goldin] Hello.  [Adam Smith] Hello, am I speaking with Claudia Goldin?  [CG] Yes.  [AS] I’m calling from Nobelprize.org, my name is Adam Smith.  [CG] Yes, I was trying to get onto the press conference and I was somehow thrown off the phone.  [AS] It is most unfortunate, I’m so sorry that that happened.  [CG] That’s fine, but the person who took my place (Professor Randi Hjalmarsson of the University of Gothenburg) did an excellent, excellent job and I must congratulate her.  [AS] She was good, yes.  [CG] I think she deserves a portion of the Nobel.  [AS] How lovely, yes, yes. She was excellent under…  [CG] She was incredible.  [AS] Many congratulations on the award.  [CG] Thank you very, very much.  [AS] Tell me, how did the news reach you?  [CG] The news reached me by phone this morning when I received a call and was awakened by it, very pleasantly!  [AS] That is a nice way to be woken. I imagine you wake up pretty fast with that news.  [CG] Yes, and since I go to sleep very late, it wasn’t that much after I went to sleep.  [AS} What was the first thing you did on hearing about it?  [CG] The first thing I did upon hearing it was I told my husband, who obviously had some idea of what was going on. He smiled. He said, “That’s great. Just tell me what to do.” I told him to take the dog out and make some tea and that I had to prepare for a press conference, which I wasn’t part of.  [AS] I’m glad the dog made it into this call as well because of course Pika features on your website.  [CG] Yes, the dog is right here. He’s a very mature animal. He understands what to do.  [AS] In the context of somebody who studies historical trends in gender equality, what does the award of this prize mean to you? Only the third woman to have been awarded the economics prize, the first unshared economics prize to a woman.  [CG] Well, it certainly means a tremendous amount. It also means a lot because it’s an award for big ideas and for long-term change. The Nobel is often given for extraordinarily important findings and ideas, often theoretical, but there have been prizes awarded for what I call big ideas and long-term change. And several of them were given to my teachers and to their teachers. So I was a student of [Bob Fogel](https://www.nobelprize.org/prizes/economic-sciences/1993/fogel/facts/), who won a Nobel Prize in Economic History with [Doug North](https://www.nobelprize.org/prizes/economic-sciences/1993/north/facts/), and I was also a student of [Gary Becker’s](https://www.nobelprize.org/prizes/economic-sciences/1992/becker/facts/). I am a third generation Nobel since Bob Fogel was a student of [Simon Kuznets](https://www.nobelprize.org/prizes/economic-sciences/1971/kuznets/facts/).  [AS] There’s an emphasis on the detective work in your studies.  [CG] Yes.  [AS] That’s a lovely concept, the idea of the researcher as detective. Can you just tell us about that?  [CG] Yes. I have always thought of myself as a detective, and I wrote many years ago, over 20 years ago, I wrote a piece called *The Economist as Detective*. And I’ve always wanted to be a detective. I’ve been a detective since I was a little kid. I wanted long ago to be a bacteriologist and to do my detective work under a microscope. But instead, I do my detective work with archival documents, with large amounts of data. I mean, there was a time when we didn’t have this tremendous amount of data stored, and one had to pull it out from archival documents.  [AS] That’s physical work, that’s dirty work, isn’t it?  [CG] It’s dirty work. But the point is, being a detective means that you have a question. And the question is so important that you will go to any end to find it. In addition, a detective always believes that there is a way of finding the answer. And that’s the way I have always done research.  [AS] That’s wonderful. And it’s the passion for the question that drives it all, isn’t it?  [CG] Yes, I think that that’s what it is. Sometimes questions are so large and so important that no one’s going to tell you that you can’t answer them.  [AS] It takes you right back to being a childhood detective when children don’t understand that there are limits to what they can do, which is wonderful.  [CG] Precisely. When I was in high school and got interested in the field of bacteriology, I would read about the famous micro hunters.  [AS] That wonderful book by Paul de Kruif.  [CG] That’s exactly right! And it has influenced countless individuals.  [AS] That book has influenced generations of laureates before you. And how lovely that it continues to be an influence, yes.  [CG] Yes. So they’re now interviewing my doppelganger.  [AS] Oh, they’re interviewing Randi. Oh, yes.  [CG] Yes. I don’t know if I’ve ever met Randi, but I certainly need to meet Randi.  [AS] And you certainly will come December. Oh, that’s wonderful. And once again, many congratulations.  [CG] Certainly. Bye now.  [AS] Thank you. Bye bye. |
| Interview |  |
| Q1 | When did you know you wanted to pursue economic sciences? |
|  | I always knew that I wanted to be a scientist, but I didn’t know what economics was. I didn’t know what economics was until I got to college. There are many fields that you don’t know until you get to college. It’s hard to teach philosophy or religion or economics to high school students. We do a very good job teaching complicated sciences, but we don’t do a very good job teaching complicated subjects that concern interactions of societies and people and politics, and that’s what you learn when you’re in college. When I was a child, I knew I wanted to do something in science. I didn’t know a lot about science, but when I got to Bronx High School of Science, I discovered the wonders of what goes on under a microscope, and I decided that I wanted to study bacteria. When I went to college, I went to college to do bacteriology, but I didn’t, I wasn’t even informed sufficiently about what was happening in genetics and in the science of the cell. I don’t think I got the best information from people at the time, but it didn’t matter really in the long run. |
| Q2 | How did you decide to study the women’s labour market? |
|  | When I went to Cornell, I entered to do bacteriology, and then I transferred to the Faculty of Arts and Sciences to pursue just about anything that I wanted to pursue. I took courses in many, many different subjects. Then when I hit upon economics, it wasn’t simply economics, it was a person, it was Alfred Kahn, who was someone who was passionate about the field of economics and explained it to me in a way that I could understand and truly appreciate as being a subject in which there were a multiplicity of forces that also brought you eventually back to an equilibrium. It was a subject that was highly mathematical that appealed to me. His field happened to be industrial organisation and the theory of regulation. When I was in college, most of the economics that I studied was in fields that I do not pursue now, although I think of myself as a very generalist economist, because as an economic historian I have to be open to just about any type of economics that will work and fit and be relevant to the subject and the period that I’m studying. But when I was in college, in fact, I was working in the field of industrial organisation, which is the theory of product markets, and when I went to graduate school, I went to the University of Chicago, because that’s where some of the great minds in industrial organisation and in law and economics were. |
| Q3 | Did you have a mentor who influenced your career? |
|  | The role models and mentors that I have had, have all been individuals who have been passionate about their subject. Beginning, as I said, with Alfred Kahan, who is known as Fred Kahan, he was passionate about regulation. In fact, he served as the head of the Civil Aeronautics Board in America, and he deregulated the airlines, and that was effectively getting rid of the Civil Aeronautics board, so probably in the history of heads of agencies, he may be one of a very few number of people who actually dissolved the agency that he was the head of. That deregulated the airline prices and brought down airline prices in America, and led to the formation of many airlines that we no longer have, but that were very small airlines such as People’s airlines. When I went to Chicago I was fortunate enough to have a large number of extraordinary mentors and people who mentored because they were passionate about their subjects. [Gary Becker](https://www.nobelprize.org/prizes/economic-sciences/1992/becker/facts/), for example, was one of the individuals. [Robert Fogel](https://www.nobelprize.org/prizes/economic-sciences/1993/fogel/facts/) was another one of the individuals. These are the people who in some sense guided me more and more towards away from industrial organisation towards labour economics and towards economic history. Although I must say that they would not have been unhappy if I studied industrial organisation as well. |
| Q4 | How do you maintain your focus and motivation? |
|  | I don’t think that I could survive without such a curiosity. We just had a wonderful lunch in which we started talking about the history of cooking and the history of food, and why it is that certain countries have hard bread and certain countries have softer bread, and what is it about the mill of wheat and the use of water power versus wind power? That was just a lunch. I started thinking about other issues in culinary history. I don’t necessarily do culinary history, but in some sense, we all do culinary history. It’s simply a questioning about every single thing that we do, and we think about. The beauty of being an academic is that we can pursue any part of it. The beauty of having wonderful students, as I have, and I have had for all the years I’ve been teaching, is that I can mention these to my students and have conversations, and maybe they will pursue it as one of the essays that they will write, or maybe even a book one day. I do have a student right now who’s working on certain aspects of culinary history. |
| Q5 | Where does your curiosity come from? |
|  | I don’t want to say I was born with it, because that will be discouraging to certain people who will say, was I born with it. I think that it’s something that you feel as you are walking around our complicated world, a listening to news, thinking about people who don’t know and their travails or their happinesses that you want to know more about them. You want to know more about their history, you want to know more about their countries, you want to know more about the history of that, and this adds and becomes much, much larger. |
| Q6 | How did you celebrate the news of your prize? |
|  | It was celebrated by … I remember we went to a press conference at Harvard. My husband, Larry, one of our students, Dev, came over that morning and our dog Pika, and we all walked to 1 Brattle Street to the press conference. All of us went to the press conference. Then we walked from the press conference up to Littauer where our offices are, and we walked up the steps, and I was greeted by, oh, I don’t know, 150 students and faculty. It was a beautiful day. It was a gorgeous day. It was October 9th, and the sun was shining, and the sky was as blue as it could be over the gorgeous buildings at Harvard and the steeples. It was a lovely celebration of being with students and faculty. Of course, it was saddened by the war in which at the time wasn’t necessarily a war, but the capture of hostages of Israelis, that really saddened it terribly, so it was a bit of a mixture. I tried to feel a celebration, but I also felt a certain amount of pain. |
| Q7 | What was your first reaction to the news? |
|  | That’s going to be my little two minute. My first reaction was that I had to do something very quickly. We had an hour and a half to get up and get ready for the announcement, so it meant that we had to take the dog out and we had to make a little breakfast, and we had to take showers. I had to think hard about a long part of my life’s work and write something up, and think about what I would say and all the people I would want to thank. I sat down and we did all of that, then I sat down, and six o’clock came and I realised that something that I hadn’t thought would happen is that emails just started piling up in my only account. It’s not as if I had a private account and a public account. I knew that I had to work through these emails to find the ones that I needed, for example, from the Nobel Foundation, from close relatives, from close friends. Then I realised that it wasn’t a hundred emails, in a very, very short period it became almost a thousand. We went to the press conference and then came back and I started reading them thinking, how am I going to work through all of these? I needed something special, some superpowers. As I was reading them, I realised that many of them started out with the phrase “I cried”. It started out with, “I am so joyful”. These were statements of people, of course, I didn’t know, who felt that I had made their work validated, I had uplifted them. I gave them pride in who they were. Through these many joyful and jubilant emails, I found the strength that I needed to read all the rest. For me, it was an award that was clearly given to me, but I felt that it had been magnified thousands of times over by what I was hearing in these emails. |
| Q8 | How important is collaboration in science? |
|  | It’s a very interesting question and would have to be answered on many levels. On one level, there’s been collaboration in many types of science. We can hear our friends in chemistry and physics particularly in the biological sciences, or thinking about the Higgs boson, for example, which was a collaboration of hundreds and hundreds of people where we have science that is made up of so many multiple things that are needed. Economics had been different. It had been the way science was once done where a person, one person, would strike out and do something themselves. I think, for example Bob Fogel and, and Gary Becker that we were just talking about. Economics now is far more collaborative. It’s rare. It’s rare that someone will write an article by themselves or write a book by themselves. I have done a bit of that, but I also work collaboratively. When I first began working collaboratively, there were very few women to work with, so most of my early collaborations are with men, and one with someone who is brilliant and insightful and also my husband, and that’s Larry Katz. I love working with Larry Katz, but now I also work with women, Claudia Olivetti, who will be coming tomorrow or the next day. So I have finally been able to work with women. I wrote an important paper with Cecilia Rouse, for example. It’s wonderful working with smart, intelligent, collaborative people, but working with women is also adds another element. |
| Q9 | Tell us about the dogs in your life. |
|  | I will say, I have never brought a dog into an archive. Even though the dog looks like in the graphic, it’s in the archive, the dog has never been in the archive. When I grew up, I had no pets. I lived in the Bronx in New York, and I didn’t know anything about dogs. My first dog was when I was in graduate school, so that’s pretty late for most people. She was a phenomenal animal, her name was Kelso, but I didn’t have that much time to do very serious training with her. I’ve really only had three dogs. Thank goodness, two of them have lasted a very long time. Pika is 13 now, and I hope he will be with us for a couple more years. I started doing a competitive work with dogs, and Pika happens to be an incredible centre, meaning he … Whenever I say centre, it sounds like it’s a football player. I love training the dog. I love getting to know the dog, getting deep into the mind of something that we really do not understand, of course. But in addition, the dog is companionship, and finally it brings me to another group of people, my dog friends, the people I train with, the people I track with, the people I go scenting with, the people I train with in terms of obedience. Just like any sport, it brings you to a different wonderful group of people. |
| Q10 | What advice would you share with young scientists? |
|  | It’s very simple. Follow your passions. If you’ve gotten involved in this subject, there are questions that you have, and those are going to be your passions. Just keep on following them. Don’t think that you will easily find an answer, that sometimes going about and looking for description, understanding what happened, understanding its history, understanding how it impacted people. Even talking to people can help you a lot. Our students today are obsessed with finding the best causal identification. That’s sometimes difficult. It may be the gold standard, it may be exactly what we want, but it may not be always be attainable right then and there, and it may be that you’re asking the wrong question until you observe. But if you keep the passion, if you keep the passion to answer the question you’ll do very well. |
| Q11 | What’s the best way to overcome obstacles? |
|  | You just keep on going. I mean, some of the biggest failures I have had are funny stories, they become really funny. The biggest failures are when I thought I was after some important set of documents, and I found that I couldn’t get the documents, but sometimes it’s taken me to a place on Staten Island, which was supposed to have a set of documents from Citibank. It was one of these Ironman facilities that in which Citibank gives over its information every day to this place. They card it away, and they put it in boxes, and eventually they destroyed what’s in some of the boxes, but some of the boxes are supposed to be kept indefinitely. I was after those types of boxes, and not to get into the immense amount of difficulty I had, but I was coming with a letter from the CEO of Citibank to this Ironman facility, and I walk in with this letter and the person looks at me like, I have no idea what you’re here for. I’m looking up at the largest amount of boxes I have ever seen. It was something from some movie about hell on earth, just gigantic amounts of boxes. We checked through many of them. For this person to pull the boxes, the person actually had to be on a cherry picker. That’s how many boxes there were and the potential difficulties of getting them down. No, I never found what I was after and wasted several days, but that’s what happens. |
| Q12 | Did you feel alone as a pioneering woman in your field? |
|  | I don’t think I’ve ever felt alone because one always has other friends, other women friends. In part one feels alone because there are moments when you feel that there are clubs and that you’re not necessarily a part of the club. But I don’t think that I felt terribly much alone, and maybe that’s because going to Bronx High School of Science – science was as an institution at the time – was disproportionately male, so I didn’t feel very much alone. |
| Q12 | Do you have advice for young women in economic sciences? |
|  | Just like when I was a graduate student, and all of the fields were mainly male fields. Today, it is the case in economics that some of the sub-disciplines are more made up of more women than others. It would be very nice if more women became macro economists and econometricians, because we need you there. |
| Q14 | Why is diversity important in economic sciences? |
|  | There are two aspects of this. One is, why don’t we get diversity? and the other one, what is the role of diversity? I have dealt a lot with the first one, I can say a little bit about the second. Men and women often look at disciplines like economics slightly differently, and occasionally they’re both wrong. In my work on undergraduate women in economics and trying to figure out why economics isn’t doing as great a job as many other disciplines, despite the fact that women are just as good at math as men, and economics is a very math intensive subject, more than the biological sciences, for example, possibly more than chemistry is, not necessarily more than physics is. I thought about – this was several years ago – why it is, and realised that many individuals are making this decision before they even go to college. Many of the reasons why they’re making these decisions is what gets discussed at the breakfast table with their parents and what they read in the *New York Times*, the *Wall Street Journal*, the *Washington Post* or whatever, newspaper or social media they’re reading. Economics to many people looks like finance, but it’s not finance. Finance may be a part of economics, but economics is not finance.  Many of the young men, when they go to college and they’re asked before they go to college in the summer, what would you like to major in? Give me three subjects, and they’ll put economics down as one, and you’ll ask them why. They’ll say, because economics is finance and I want to go into finance. If you ask the women, why didn’t you put down economics? they’ll say, because economics is finance and I don’t want to go into finance. Both of them are wrong. They should both go into economics because of what economics is and not what economics is not. That’s part of the issue that diversity is important because men and women, or individuals of different ethnicities or races, may somehow believe that they want to get something out of a field. It’s very difficult to know what the field is until you take it, until you have appropriate exposure to it. Because some fields, as I said before, are not taught in high school or they’re not taught well enough in high school. Even the fields that are taught in high school, like history, may not be taught the way they’ll be taught in your college, but some fields simply aren’t taught. That’s one reason why we should want to have more diversity maybe related to why we don’t have enough diversity.  But the other reason is what do we get from diversity? This is much larger a subject. Within economics, it’s very clear that what we get from diversity is not simply having different views across the board, but the fact that women and men are tending to go into different parts of economics. If economics is 10 or 12 subfields, women are tending to go into one set of subfields and men are tending to go into another set of subfields. Therefore, all these years not having enough women means that those subfields were populated by a much smaller group of individuals, and one of them happens to be help. |
| Q15 | How do you try to encourage diversity in economic sciences? |
|  | Oh, absolutely. I devised with a group of people this undergraduate in economics, let’s call it a program. We came up with a large number of ways so that economics is seen as what it is. Economics is about people, and it’s about various aspects of people’s relationship to each other and to markets and to the world of economics. We devised it so that when freshmen or first year students too come in the door of universities and economics departments are able to give them information that demonstrate to them what economics is about and what economists do, that economists really do work with people. They work on inequality, they work on health, they work on economic development and so on. There are various ways of doing this so that it is not believed by first year students that within the social sciences, psychology deals with people and economics deals with just markets or money or stock markets and so on – that economics is about people. If undergraduates realise that economics … I mean saying economics is not about people to me as a joke, but that’s what many, many people think, that economics is not about people, and if they do believe this, it’s our failure not to tell them that that isn’t true. |
| Q16 | How would you describe a good teacher? |
|  | One of the problems that we face in economics is that we create our subject as a set of courses that begins with economic theory. Economic theory then is pushing students away from the notion that economics is about people, because economics then becomes about a set of abstract ideas. In the sciences, in physics, for example, it would be more accepted that physics is about abstract ideas, but economics really isn’t about abstract ideas. Yet we’re teaching them that economics is about these abstract markets. But a talented teacher should be able to blend the two. I would describe a good teacher as having an idea, a subject that you want to convey, and conveying it in a way that you could see in the eyes of a student is making them surprised and interested and inspired. If you can’t do that, you haven’t reached the individual. |
| Q17 | You named a recent article “Why women won”. Why did you choose that title? |
|  | First of all, the title “Why Women Won”. The article is a beginning exploration of mine. A gift that we academics have is that when we have a question and we have some time, we can sort of go about trying to answer it. My question was, I know where women are today and I know where they once were, how did they get to today? That’s sort of why women won. That does not mean that women have won exactly what they should win, or that along the way all women won what they should have gotten. But it is a statement that where we are today is very, very different from where we were in my own lifetime, not when I was born, not even when I was in college, but when I was in graduate school, and even when I was an assistant professor at Princeton. What women have today in terms of their legal rights is phenomenally greater. What their legal rights are in the workplace, what their legal rights are in the credit market, what their rights are with regards to their family, with regard to their name, with regard to their body, with regard to contraception – and the list goes on. What women’s rights are in America as well as in many parts in the world, are much, much greater. That does not mean that these rights are rights that they always have. That does not mean that these rights are the rights that they can easily begin to take advantage of, but that these are their rights. |
| ID | 0804 |
| Biographical | I was born December 13, 1953, in Augusta, Georgia, but spent my childhood and teenage years in Dillon, South Carolina. Dillon, a town of about 6,000 people, lies just west of the Little Pee Dee River, in the northeastern part of the state. When I lived there, the area was mostly dependent on agriculture – cotton and tobacco – although for a time there also was textile manufacturing. In the 1950s and 1960s, other than high school football, Dillon had little to offer in terms of services or entertainment. A visit to an optometrist or an evening at the movies required a trip to the larger city of Florence, about 30 miles away. Dillon’s main claim to fame when I was living there, and perhaps still today, was South of the Border, a sprawling Mexican-themed tourist attraction just south of the North Carolina state line. Many travelers on I-95 on their way to Florida would be entertained by the pun-filled billboards advertising South of the Border (“Try our honeymoon suite: It’s heir conditioned!”) that lined the highway. I was a serape-clad waiter at one of South of the Border’s four restaurants for several summers during my college years. Family Background My grandparents were all Jewish immigrants from eastern Europe. My mother’s parents, Herschel and Masia Friedman, emigrated from Lithuania around the outbreak of World War I. They lived in Maine and then Connecticut – where my mother, Edna Friedman, was born – before making a permanent home in Charlotte, North Carolina. Herschel worked as a kosher butcher and Hebrew teacher; Masia stayed at home with my mother and her brother Dan but was active in the family’s synagogue.  My father’s parents, Jonas and Pauline Bernanke, arrived in the United States a few years after the Friedmans. Jonas served as a corporal in the Austro-Hungarian army during World War I. When I was a young boy, he told me many romantic tales of his adventures. I know for sure that he was captured by Russian forces, escaped during the chaos of the Russian revolution, made his way somehow to Shanghai, and from there took a steamer to Marseilles. He married Pauline, known as Lina, and together they emigrated to the United States, via Ellis Island, in 1921. In New York, Jonas studied pharmacology, while Lina, who had earned a medical degree at the University of Vienna (one of the first women to do so), established a practice on the Lower East Side. My father, Philip Bernanke, was the second of three sons. Fred was older, Mortimer was younger.  Following several unsuccessful attempts at starting his own pharmacy in New York City during the Great Depression, Jonas brought the Bernanke family south after he saw an advertisement in a trade magazine about a pharmacy for sale in Dillon. He bought it, moved the family, and opened Jay Bee Drugs – the name based on his initials – in 1941. Jay Bee Drugs proved successful and sustained the family for many years, but the cultural shock of moving from New York to Dillon in 1941 (my father was a teenager) must have been tremendous. Lina was particularly opposed to the move, since South Carolina did not recognize her medical credentials. But Jonas insisted.  My father, Philip, and my mother, Edna, met when both were attending college in North Carolina. My father was a drama major, and he briefly tried to make a go of it as a director of local theater groups. He was often an artistic success but an economic failure. When I was born, adding to the family’s financial responsibilities, he changed careers. At the time, one could become an accredited pharmacist through experience and by passing a test, with no special schooling required. Philip passed the test and went to work for his father. Eventually he and his brother Mortimer bought Jay Bee Drugs from their father and were partners until they retired. My mother briefly taught elementary school and did back-office work in the store as well, but for the most part she stayed home with her children. My younger brother Seth was born in 1958 and my sister Sharon in 1960.  Theater had been my father’s first choice of occupation, but he made a success of the pharmacy. In a town with no hospital and typically only one or two practicing doctors, many people went to Doctor Phil and Doctor Mort, as my father and uncle were known, for advice on health and nutrition. Their business was built on close personal relationships with customers. Jay Bee Drugs gave credit to customers who could not pay, offered free deliveries of prescriptions, and opened for emergency prescriptions at any time, day or night. They treated White and Black customers with equal respect, unusual in a place and time of extreme segregation and racial discrimination. My father’s empathy with customers was so strong that, though born and bred in New York, he would unconsciously lapse into a heavy Southern accent and mannerisms when he spoke with them. Early Years Although I saw my paternal grandparents frequently – they lived in Dillon – I was closer to Herschel and Masia. From age four or five, I would visit them in Charlotte for several weeks every summer. On pleasant summer evenings I would sit with them on the front porch. I credit a conversation I had with my grandmother when I was very young with piquing, very subtly, what would later become my strong interest in the economics of the Depression. Masia told me about living in Connecticut during the Depression. She was proud that Herschel was able to earn enough that they could buy new shoes for their children for school each fall. Other children had to go to school in worn-out shoes or, according to my grandmother, sometimes even barefoot. When I asked her why their parents didn’t buy those children new shoes, she said their fathers had lost their jobs when the shoe factories closed. “Why did the factories close?” I asked. She replied, “Because nobody had any money to buy shoes.” Even at six years old I could see the paradox, and I would spend much of my professional career trying to resolve it.  I attended public schools in Dillon through high school. In the sixth grade I won the state spelling bee and went to the finals in Washington, my first airplane ride. (I was eliminated in Washington after I misspelled the word “edelweiss,” a Swiss flower.) I learned the saxophone and marched with the high school band at football games. As a senior I won a statewide academic award; the prize was a seventeen-day guided bus tour through a half dozen European countries – my first time abroad. The summer after my senior year I worked as an unskilled construction laborer. Together with my later restaurant work, it gave me an appreciation of how hard most people must work to support their families.  In South Carolina at the time, White and Black students attended separate schools. In Dillon, that did not change until I became a senior, when I attended a newly built, now integrated, high school. In a segregated society, most of my friends and acquaintances were White. A life-changing exception was Kenneth Manning, a Black friend, a few years older than me. We met because his family frequently shopped at Jay Bee Drugs. A brilliant student, Ken was a Harvard undergraduate when I was a high school senior. He believed that, instead of attending a nearby college, as my parents had planned and I had assumed, I should go to Harvard as well. The question was whether my parents would allow me to go. They were concerned about the cost and the long distance (both geographically and socially) from Dillon to Boston. Ken made several visits to our home to try to persuade my parents. Ultimately, he won them over. I was accepted by Harvard and entered in 1971. I promised my parents I would help relieve the burden of Harvard’s tuition, which led to my summers waiting on tables at South of the Border. Ken Manning went on to become a professor at MIT, where he specializes in the history of science. College and Graduate School I was excited to be at Harvard (and in Cambridge) but was woefully underprepared. My background was deficient, especially in math, and I didn’t know how to study, never having had to do so in high school. And I had no real idea of what I was good at or in what I should major. My first semester was especially difficult for me, and by Christmas break I considered dropping out. But with time to reflect at home, I resolved to do better. With time I began to acclimate to the coursework and college life and to enjoy the stimulating atmosphere of Cambridge.  My intellectual tastes were eclectic (or, less charitably, unfocused). I took math and science courses, including statistics; some history; and a variety of liberal arts courses, including art, literature, and philosophy. I liked everything and changed my mind about what I would major in multiple times. In my sophomore year, I took Economics 10, Harvard’s famous introductory course, *de rigueur* for practically all undergraduates. The well-known economist Martin Feldstein lectured in a big auditorium to probably a thousand students. More-personal instruction occurred in small breakout classes that met a couple of times a week. My section leader was Lee Jones, now at Boston University. Jones was interested in the economics of growth and development and helped me see that good economics could potentially improve the lives of millions of people. At a personal level, I saw that the field would allow me to use my skills and interest in math, statistics, history, and philosophy. I decided to make economics my major.  As a junior I had taken a course in econometrics from Dale Jorgenson, a prominent professor, and on an impulse one day I walked into his office and asked for a summer job. I don’t know to this day why he took a chance on someone with so limited a background. But he would end up hiring me for the next two summers. As part of Jorgenson’s team, I helped write FORTRAN programs to simulate econometric models Jorgenson had developed to study the economic effects of changes in energy prices and supplies. Our tools and methods were advanced for the time but today seem very primitive. We fed punch cards into a card reader, and it was not unusual for a program to take several hours to run. But it was my first taste of real economic research and whetted my interest for more.  Jorgenson, who died in 2022, was a wonderful mentor throughout my career. He advised my senior thesis, in which I studied the interaction of energy production with the larger economy. I integrated Jorgenson’s model of the economy with various sectoral models of energy production (coal, oil, electricity) developed by others. My thesis won Harvard’s top undergraduate prize in economics, and I graduated summa cum laude and Phi Beta Kappa, a big improvement over my slow start. With Jorgenson, I cut the thesis down to article length and in 1975 we published it as a jointly-authored article – my first professional publication. Putting my interests over institutional loyalty, Jorgenson advised me to do graduate work at the Massachusetts Institute of Technology, rather than Harvard. At the time, MIT, with luminaries like [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/) (a Nobel laureate), and future Nobelists [Robert Solow](https://www.nobelprize.org/prizes/economic-sciences/1987/solow/facts/) and [Franco Modigliani](https://www.nobelprize.org/prizes/economic-sciences/1985/modigliani/facts/), was considered the strongest economics graduate program in the world. I very much enjoyed my time there. I remained in contact with Jorgenson for the rest of my professional career, however, often turning to him for advice and encouragement.  After graduating from Harvard in 1975, I went directly to MIT. Once again, I struggled with uncertainty about what to study. I asked Stanley Fischer, my first-year macroeconomics professor, for advice. He gave me a copy of *A Monetary History of the United States, 1863–1960*, by [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/) and Anna Schwartz, a thick and rather dry tome that made the historical case for a strong link between the money supply and business cycles. “Read this,” he said. “It may bore you to death. But if it excites you, you might consider doing monetary economics.” I was fascinated. I was especially interested in the chapters on the Depression, which the authors attributed to monetary factors. If monetary economics could help explain the Depression, the most important economic event of the twentieth century, it seemed worth studying. The book, together with my economic history course with MIT’s Peter Temin, reawakened an interest in the Depression that I could trace back to the conversation with my grandmother many years earlier.  Stanley Fischer advised my dissertation, along with Rudiger Dornbusch and Solow. Fischer was an excellent advisor, widely read and skilled at helping students develop their research ideas. He also had a good sense of how academic economics could inform policy. He went on to become the governor of the central bank of Israel, and subsequently, the vice chair of the Federal Reserve under Chair Janet Yellen. Like Jorgenson, Fischer kept in close touch with me throughout my career and provided helpful advice and support, including during my own time as a policymaker. I was fortunate indeed to have two such accomplished and helpful mentors.  In 1977, when I was a third-year graduate student, I met my future wife, Anna Friedmann, a senior at Wellesley College near Boston. The daughter of Holocaust survivors, Anna grew up in Denver and had come to Wellesley on a full scholarship. She majored in chemistry to please her parents, but her real love was Spanish literature, in which she minored. Spanish was her fourth language, after English, Italian, and Serbo-Croatian. She went on to earn a master’s degree in Spanish literature at Stanford and later work as a high-school Spanish teacher.  My roommate and his girlfriend, also a Wellesley student, arranged for Anna and me to meet on a double date with them. We fell for each other immediately, and after a rapid courtship, we were married on Memorial Day weekend the next year – May 29, 1978. Anna graduated from Wellesley two days before the wedding, allowing her parents to make one trip for both events. We have been married now for more than 44 years. Our son Joel was born in 1982 and our daughter Alyssa in 1986. Joel is a child and adolescent psychiatrist, and Alyssa is in her residency at Northwestern’s medical school.  My MIT dissertation was three essays on macroeconomics (a common format for dissertations at the time). The lead paper (“Irreversibility, Uncertainty, and Cyclical Investment”), which would be published in the *Quarterly Journal of Economics* in 1983, showed that a firm’s ability to make an irreversible capital investment should be thought of as an option, in the financial sense of the word. In particular, it may be optimal for firms to delay irreversible investments to gain more information – that is, not to exercise their option to invest – when there is unusually high uncertainty, even if many of the possible outcomes are favorable. The paper concluded that, even when the fundamentals are strong, high uncertainty can generate an economic downturn. My dissertation did not include any reference to the Depression. But I continued to read and think about why the global economic collapse of the 1930s was so long and so deep. Stanford After graduation from MIT in 1979, I accepted an assistant professor position at Stanford’s Graduate School of Business. The possibility of working with faculty in both the business school and the Stanford economics department, both of which were among the world’s best, appealed to me. I learned a great deal from my colleagues during my six years at Stanford. Particularly important, as it turned out, the Stanford business school was a hotbed of work on the economics of imperfect and asymmetric information. The Stanford faculty included [Robert Wilson](https://www.nobelprize.org/prizes/economic-sciences/2020/wilson/facts/) and [Paul Milgrom](https://www.nobelprize.org/prizes/economic-sciences/2020/milgrom/facts/), later to be named Prize laureates, as well as David Kreps and John Roberts, all of whom helped develop the theory of how markets work when one party to a transaction has more, or different, information from the other. They and others showed that imperfect information can significantly affect market functioning and the incentives of market participants. In this they built on other exciting work, including a classic contribution (“The Market for ‘Lemons’: Quality Uncertainty and the Market Mechanism,” *Quarterly Journal of Economics*, 1970) by future Nobel laureate [George Akerlof](https://www.nobelprize.org/prizes/economic-sciences/2001/akerlof/facts/) of the University of California, Berkeley, which showed how asymmetric information between sellers and buyers could, under some circumstances, cause a market to collapse completely. Figures such as future Nobel laureate [Joseph Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/) applied the economics of imperfect information to better understand important markets, like the markets for loans, insurance, and health care.  My exposure to the new work on asymmetric information helped me clarify my thinking about the Great Depression, a subject that had continued to intrigue me. In my 1983 paper cited by the Committee (“Non-monetary Effects of the Financial Crisis in the Propagation of the Great Depression,” *American Economic Review*), I reviewed the extraordinary distress experienced by both lenders and borrowers during the Depression, including the bank runs that shut down thousands of U.S. banks and the widespread defaults by homeowners, farmers, and firms. Using insights drawn from the literature on imperfect and asymmetric information, I then argued that this extreme financial distress caused banking and credit markets to break down. The resulting damming of credit flows, I wrote, supplemented the monetary forces emphasized by Friedman and Schwartz as a principal source of the Depression. I noted that credit disruptions, especially those associated with banking crises, could help explain why a potential economic recovery in late 1930 and 1931 was aborted; why credit in the United States fell much faster than output in the 1930s; why price deflation was so damaging (it bankrupted debtors, like farmers, whose incomes fell with prices but whose financial obligations did not change); why, following the bank holiday and institution of deposit insurance in 1933–34, the economy began a strong recovery; and why the subsequent economic recovery was so slow (banks continued to be cautious and it took a long time to work out problems of borrower insolvency).  The 1983 paper included no formal model and only limited econometric analysis, but it was fruitful in that it led me to pursue two related lines of research. First, with Mark Gertler (and, later, with Gertler’s student Simon Gilchrist), I developed the implications of imperfect information in credit markets for the behavior of the broader economy. For example, we showed theoretically and empirically how variations in credit conditions could amplify economic fluctuations – an effect that became as the *financial accelerator* (see, for example, Bernanke, Gertler, and Gilchrist, “The Financial Accelerator and the Flight to Quality,” *Review of Economics and Statistics*, 1996).  A key insight from our work was that increases in the net worth (wealth) of borrowers better align the incentives of borrowers and lenders, reducing the cost to lenders of extending credit. For example, a bank will be more willing to lend to a small business owner who has a large equity stake, knowing that the substantial investment of the borrower in their business will incentivize the borrower to work hard and to avoid unnecessary risks. The financial accelerator effect arises when a weakening economy lowers the net worth of borrowers, which makes lending riskier and more costly for banks, which in turn worsens the downturn (the accelerator effect). The same dynamic in the opposite direction works to amplify economic booms. Gertler, Gilchrist, and I would later show how to incorporate the financial accelerator into an otherwise standard quantitative new Keynesian model of the economy (“The Financial Accelerator in a Quantitative Business Cycle Framework,” in *Handbook of Macroeconomics*, 1999).  Moreover, banks and other lenders are themselves borrowers, since they must raise funds from deposits or in capital markets in order to lend. Thus, lenders’ financial health (as reflected, for example, in the level of bank capital) also affects credit market outcomes. In particular, financially weak banks, who fear the loss of short-term funding (through a run, for example) will make fewer and less risky loans (the *flight to quality*). Importantly, this line of research provided a framework for thinking about large, endogenously determined financial crises (Bernanke and Gertler, “Banking and Macroeconomic Equilibrium,” in *New Approaches to Monetary Economics*, 1987), a framework that would be substantially elaborated in Gertler’s subsequent work with Nobuhiro Kiyotaki (“Banking, Liquidity, and Bank Runs in an Infinite Horizon Economy,” *American Economic Review*, 2015). Financial crises are of course an important practical concern for policymakers. In a 2018 paper (“The Real Effects of Disrupted Credit: Evidence from the Global Financial Crisis,” *Brookings Papers on Economic Activity*), I showed empirically that, in analogy to the Great Depression, increases in financial distress during the Global Financial Crisis were closely linked to subsequent declines in U.S. economic activity and employment.  Gertler and I used related models to study the so-called *credit channel of monetary policy* transmission. Several authors (a recent example is Gertler and Peter Karadi, “Monetary Policy Surprises, Credit Costs, and Economic Activity,” *American Economic Journal: Macroeconomics*, 2015) have shown empirically that the effects of monetary policy on the economy work largely through changes in credit conditions. Anticipating these results, in a 1995 paper (“Inside the Black Box: The Credit Channel of Monetary Transmission,” *Journal of Economic Perspectives*), Gertler and I argued that the logic of the financial accelerator applies to monetary policy shocks as well. For example, an unexpected easing of monetary policy, by strengthening the economic outlook and raising asset prices, improves the financial conditions of both borrowers and lenders. That improvement in turn stimulates credit extension and economic activity. We called this mechanism the credit channel of monetary policy.  The second broad line of research that followed from my 1983 paper was further investigation of the causes of the Depression, including cross-country comparisons. My articles on the Depression are collected in *Lessons from the Great Depression*. In this book, I contributed to the developing consensus that the breakdown of the interwar gold standard was a major cause of the global Depression, and I dug into the behavior of wages and employment during the 1930s. Providing empirical support for the 1983 paper, with Princeton historian Harold James (“The Gold Standard, Deflation, and Financial Crisis in the Great Depression: An International Comparison,” in *Financial Markets and Financial Crises*, 1991) I did an empirical cross-country study that found that the severity of the Depression in twenty-four countries depended primarily on two factors: (1) how long the country remained on the gold standard after the Depression began (staying on the gold standard led to more severe deflation and depression); and (2) the severity of the banking crises in the country, which in turn depended on structural factors, such as whether the country had many small banks, as in the United States. In several other papers, I would use the comparative approach to examine alternative explanations of the Depression and the persistence of high unemployment in the 1930s. Princeton Both Stanford and Princeton offered me full professorships in 1985. Both were attractive, but my wife and I chose Princeton (Anna saw its leafy environs as more conducive to family life). I remained on the Princeton faculty, jointly appointed in the economics department and the Woodrow Wilson School of Public Affairs, until I entered government service in 2002. I was chair of the economics department from 1995 to 2002. On three occasions I was a visiting faculty member at MIT, my doctoral alma mater. I also established strong connections with regional Federal Reserve banks, including those in Boston, New York, and Philadelphia.  At Princeton I began or continued several other lines of research. First, I made early contributions to what is now a very large and vibrant literature on the identification of the economic effects of unexpected changes in monetary policy. For example, in a 1992 paper (“The Federal Funds Rate and the Channels of Monetary Policy,” *American Economic Review*), my Princeton colleague Alan Blinder and I, building on work by future Nobel laureate [Christopher Sims](https://www.nobelprize.org/prizes/economic-sciences/2011/sims/facts/), estimated the macroeconomic effects of monetary policy shocks on the economy. One of our key assumptions was that monetary policy shocks can be equated with unexpected changes in the federal funds rate, the Fed’s main policy interest rate. With my student Ilian Mihov (“Measuring Monetary Policy,” *Quarterly Journal of Economics*, 1998), I developed alternatives to the federal funds rate as an indicator of monetary policy, which accounted for changes in the Federal Reserve’s operating procedures. In a precursor 1986 paper (“Alternative Explanations for the Money-Income Correlation,” *Carnegie-Rochester Conference Series on Public Policy*), I showed that vector autoregression (VAR) methods could accommodate additional assumptions about the structure of the economy. So-called structural VAR models are now widely used in macroeconomics.  Monetary policy, in practice, is made using large amounts of economic data, a fact which is not easily reconciled with VAR approaches that focus on the dynamics of only five or six variables. In work with Jean Boivin (“Monetary Policy in a Data-Rich Environment,” *Journal of Monetary Economics*, 2003) and with Boivin and Piotr Eliasz (“Measuring the Effects of Monetary Policy: A Factor-Augmented Vector Autoregressive (FAVAR) Approach,” *Quarterly Journal of Economics*, 2005), I showed how to incorporate information from a large set of variables into otherwise parsimonious vector autoregressions. The paper with Boivin and Eliasz introduced the use of factor-augmented vector autoregressions (FAVAR), in which the dynamic factor that best describes the common movements of a long list of data series is estimated along with the VAR. Overall, the current state of the art in measuring the effects of monetary policy has built on the papers noted above, along with other foundational research. However, in an important improvement, the most recent articles use unexpected changes in federal funds rate futures and other financial indicators in a short window of time (e.g., thirty minutes) around the Fed policy announcement to measure policy shocks more accurately. In a forthcoming paper in the *Journal of Economic Perspectives* (“Risk Appetite and the Risk-Taking Channel of Monetary Policy”), Michael Bauer, Eric Milstein, and I use this methodology (known as high-frequency identification) to study the effects of monetary policy shocks on investors’ risk appetite. We find that monetary easing tends to induce investors to take more risks in financial markets.  Another area of research to which I have contributed is the positive and normative study of central bank communication, particularly the pros and cons of setting an official inflation target as an instrument for communication and transparency. Much of my work in this area was with Frederic Mishkin (see for example our articles in the 1992 NBER *Macroeconomics Annual* and in the 1997 *Journal of Economic Perspectives*). In this line of research, my coauthors and I considered the experiences of central banks outside the United States with formal inflation targets. We argued that adoption of a target by the Federal Reserve, together with supporting communication such as economic forecasts, would increase the Fed’s public transparency and accountability.  Later in my career I also became interested in how monetary policy could retain its potency when the short-term policy rate reached its effective lower bound (typically zero). In my first year as a Fed governor, in 2002, I gave a speech which laid out some possible strategies. I did empirical work on the subject in a 2004 paper with Vincent Reinhart and Brian Sack (“Monetary Policy Alternatives at the Zero Bound: An Empirical Assessment,” *Brookings Papers on Economic Activity*), summarized in a short 2004 paper with Reinhart (“Conducting Monetary Policy at Very Low Short-term Interest Rates,” *American Economic Review Papers and Proceedings*). Broadly, I argued that monetary policy should retain considerable scope to stimulate the economy even when short-term rates were zero and could not be reduced further. I have continued to write about alternative monetary policy tools, such as quantitative easing and forward guidance, including in my American Economic Association presidential address (“The New Tools of Monetary Policy,” *American Economic Review*, 2020) and my recent (2022) book, *21st Century Monetary Policy: The Federal Reserve from the Great Inflation to COVID-19*. The Federal Reserve and Council of Economic Advisers In 2002 I was appointed by President George W. Bush to a seat on the Federal Reserve Board. In 2005 I moved over to the White House, becoming the chair of the president’s Council of Economic Advisers. Later that year, with the retirement of Alan Greenspan pending, Bush nominated me to be the chair of the Federal Reserve, effective in January 2006.  My time as chair was eventful, to say the least. It included the 2008–2009 global financial crisis, the subsequent European debt crisis, and most of the Great Recession and the associated recovery. I have provided detailed discussion of my experience at the Fed in my memoir (*The Courage to Act*, 2015) and elsewhere. Here I will just note that my academic research informed my policy decisions in important ways.  First, my work on the Depression and on the financial crises convinced me that a collapse of the financial system would have disastrous effects for the whole economy, not just Wall Street. Working with Treasury Secretary Henry Paulson, as well as with New York Fed President and later Treasury Secretary Timothy Geithner, in 2008-09 I worked to prevent that collapse. Many of these rescues were very unpopular. Bailing out failing financial firms and other efforts to contain the crisis were often seen as rewarding the malefactors who caused the problem. But I believed they were necessary for economic as well as financial stability. Except for the critical but unavoidable failure of Lehman Brothers in September 2008, our stabilization of the financial system largely succeeded, and the crisis was mostly contained by the spring of 2009. The economic recovery began soon after. Although our interventions in the financial system, together with unconventional monetary policies, ensured that I would remain a controversial figure, President [Obama](https://www.nobelprize.org/prizes/peace/2009/obama/facts/)’s decision to renominate me was an important mark of confidence. I was also named the *Time* magazine person of the year for 2009 and received other, more academic, recognitions, including appointment to the National Academy of Science.  A second influence of my academic research on policy was that, following Friedman and Schwartz, I believed that helping the economy recovery would require a very accommodative monetary policy. The federal funds rate hit zero in the fall of 2008. Thus, providing more stimulus required alternative tools, tools that I had studied before becoming chair. In 2009 my colleagues and I introduced the first of several rounds of large-scale asset purchases (quantitative easing), and we used forward guidance to persuade markets that monetary ease would continue for a while, which helped create more supportive financial conditions and strengthen the recovery.  In keeping with my work on inflation targeting and central bank communication, I was able to make the Federal Reserve much more transparent during my tenure. We instituted a formal inflation target, significantly expanded our public forecasts (including forecasts of the policy interest rate) and began conducting press conferences by the chair following policy meetings, among other innovations. These changes, taken still further by my successors, have changed the character of the Fed and its relationship with the public. The Brookings Institution Since leaving the Federal Reserve in 2014 at the end of my second four-year term as chair, I have been a senior distinguished fellow at the Brookings Institution, a think tank in Washington. My wife runs a small private school in Washington, called Chance Academy, which works mostly with underserved children. She and I have worked together to support the school. I have remained professionally active, writing two books and various articles, focused mostly on aspects of monetary policy. Work I did with John Roberts and Michael Kiley of the Federal Reserve Board on optimal policy at the zero bound (“Monetary Policy Strategies for a Low-Rate Environment,” *AEA Papers and Proceedings*, 2019) influenced the development of the Fed’s average inflation targeting policy framework, announced in August 2020.  In 2018, I was elected president of the American Economic Association for 2019. The principal objective of my tenure was to make the economics profession more welcoming to women and minorities. We appointed an ombudsman that AEA members could consult on issues of discrimination or harassment; and we created programs to encourage undergraduates to major in economics, to set best practices for economics departments, and to mentor young female and minority professionals. The extent to which these efforts will bear fruit remains to be seen. |
| Autobiographical |  |
| Podcast | “It was almost an accident that I ended up in economics” Meet economist Ben Bernanke in a podcast conversation. Bernanke tells us about his childhood interest in the origin of words, which ultimately led him to win spelling competitions as a child. He also speaks about economics and how that field unifies his interest in mathematics with social science and concerns about society.  The host of this podcast is nobelprize.org’s Adam Smith, joined by Clare Brilliant. This podcast was released on 1 June, 2023.  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.  Ben Bernanke clip: You have scholars going back and taking months and months and months to evaluate each decision. What you’re thinking is, well, obviously I couldn’t get all that information. I did the best I could with what I knew.  Adam Smith: Imagine being the most influential economist in the world. That was the position that Ben Bernanke found himself in when he was appointed chair of the Federal Reserve system in 2006, and then in 2008 the financial crisis hit and he had to deal with that.  In this conversation I wanted to explore what gave him the confidence to make policy as he needed to under such circumstances, given that he was, prior to his appointment, Dean of the economics faculty at Princeton University and, like many others, perhaps a rather introverted academic. Was it something about his upbringing, his education, something innate in himself?  Unsurprisingly, it’s a combination of all these things, as you’ll discover listening to this conversation with Ben Bernanke.  Clare Brilliant: This is Nobel Prize Conversations. Our guest is Ben Bernanke, the 2022 laureate in economic sciences. He was awarded the prize for his research on banks and financial crises. He shared the prize with Douglas Diamond and Philip Dybvig.  Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces.  Today Ben Bernanke is a distinguished senior fellow with the Economic Studies program at the Brookings Institute in Washington DC.  You’ll hear him speak about moving from academia to public life – and then back again, about his background as a spelling champion, and how he found his love for economics at the intersection of history and maths.  But first, Adam asks him whether his time in the public eye before being awarded the prize has informed his experience of becoming a Nobel Prize laureate.  Smith: In a few cases, for instance, [Barack Obama](https://www.nobelprize.org/prizes/peace/2009/obama/facts/) or [Bob Dylan](https://www.nobelprize.org/prizes/literature/2016/dylan/facts/) or yourself, you’ve been living in the glare of publicity for a long time. It’s a little bit different getting the call, I imagine. I wondered whether, since you were used to it already, it made it easier to enjoy Nobel week in December when you came.  Bernanke: Yes, I think it did. I think most economists, most academics, when this happens to them, their life changes in an important way. They do a lot more public speaking, they get asked for their opinions on things which they have never studied. That’s, I think, the typical experience. In my case, I was the chairman of the Federal Reserve for eight years. and also had other roles before that. I was used to speaking in public and hearing from policy makers and the like. I was more comfortable in that respect. Frankly, my speaking engagements and interviews have not increased, since I won the prize because I was already frequently speaking in public.  Smith: Yes, of course. The experience of being in Stockholm, I think many laureates find rather overwhelming. By the end of Nobel week they’re absolutely exhausted. Was that the case for you? Did you find yourself exhausted or were you fresh and ready for Christmas?  Bernanke: It was a very busy week. There were things going on every day, multiple interviews or ceremonies or dinners. I think my one disappointment was that my wife and my guests got to see more of Stockholm than I did. I would be going to various events and my family was able to go to a museum or take a cruise or otherwise see the city.  Smith: Let me just take you back there for a moment by playing a clip from the award ceremony.  *CLIP from the award ceremony*  Bernanke: It was very exciting, of course. You have to cross the stage to go accept the award. I was worrying that I might trip or do something else embarrassing. But of course, it was the culmination of the week. It was a quite impressive scene with the entire audience, all the men dressed in white tie and tails and the women in gowns. Such a nice formal occasion with the music and hearing my fellow laureates. It was obviously a very great moment. But, again, it was only part of that whole week of celebrations and interviews and the like.  Smith: What was emphasised in the presentation speech was the fact that you as an academic had found yourself in the position of being a practitioner and how you had been called upon in 2008 to act in a way that pretty much had to save the global economy, which is a very unusual position to find yourself in. I wanted to explore a little bit how, I suppose, an academic who is generally presumed to be a rather shy individual by society at large, suddenly finds the confidence to be able to act in that way.  Bernanke: Yes, I am by nature introverted. But necessity creates its own requirements. It was obviously a very difficult situation. My research helped prepare me to think about what was happening since my work was about the depression, about financial crises, about their effects on the economy. Obviously, here was an example in real time of a global financial crisis. It was difficult, but again, it was absolutely essential that we, I say we because I had great colleagues, both the Secretary of the Treasury, Hank Paulson, later Timothy Geithner, as well as many staff and colleagues at the Federal Reserve to help. I was rarely out there by myself. I was usually had some kind of support. Obviously it was incredibly important to voice my views and to let people know what we’re doing. I once said that central banking is 98 per cent talk and 2 percent action. The talk part was very important, both in policy and in politics.  Smith: It’s been commented of you that you’re somebody who likes to speak last, not first, which is perhaps not a usual trait in a leader, but perhaps one to be desired. Does that ring true?  Bernanke: Yes, I think that is true. For example, in the monetary policy meetings of the Federal Reserve, there are many policymakers around the table. I would, as a matter of routine, ask everyone else to speak first. I would then summarize what I had heard, and then make my own recommendation. That was indeed the practice that I followed. Prior to being a policy maker, I was an academic and I was for seven years the chair of the Princeton economics department. Once again, you had to deal with people with high intelligence and strong opinions. Coming to a consensus, I think was very important. From the outside, it often seems to the public that the chair of the Federal Reserve or the president, whatever leader we’re talking about, is sort of acting as an individual. That’s never true. It’s always important to build consensus within your organization or within your government and to listen to what other people have to say that will inform your decision and point out potential pitfalls, for example. I think the only drawback of that approach is that sometimes it takes a little bit longer to come to a decision than otherwise. On a few occasions when I felt that it was necessary, I took unilateral or very quick decisions. But whenever possible, it was both helpful and confidence building to try to get everybody’s input and to try to build a consensus.  Smith: Just from a personal perspective, it must be a little bit terrifying that if you, on a Monday, as you did in 2008, you announce a three quarter percent reduction in interest rates in order to stem the tide. Sure, you’ve made the decision based on everything you know, but it must be scary.  Bernanke: I think terrifying is too strong a word, but it was certainly anxiety producing. Again, one of the elements of a financial crisis is, first of all, it’s very hard to predict. We did not predict a crisis of the magnitude that happened months in advance. Almost by their nature, financial crises are very hard to predict because they do depend on confidence and on random events that may occur. You’re operating in what Timothy Geithner used to call the fog of war, that there’s always many things happening, you cannot possibly know everything you would like to know. What always happens of course is that 10 years later you have scholars going back and taking months and months and months to evaluate each decision and what you’re thinking is, well, obviously I didn’t have all that information. I couldn’t get all that information. I did the best I could with what I knew. Inevitably that creates a lot of anxiety, a lot of concern. But frankly, I was just so focused on what needed to be done and on trying to build a consensus with my colleagues that I didn’t have a lot of time to reflect on the emotional aspects of this. I had good support from my family and again from my colleagues and we just took one step at a time.  Smith: I suppose that in that position, it takes a lot out of all of you.  Bernanke: It does. Certainly, I had very irregular hours and very long days, weekends. I think the most important thing that my family could do would be to provide a refuge, just normalcy, where you could leave the chaos and have some place to go. At work you would make some $10 billion decision and then we’d come home and discuss the water bill with your wife or put out the garbage. It created sense of, first of all, of what you were working for, that is, for not just my own family but for families across the country, but also a sense of refuge and a sense of support that was unconditional and that I knew I had people on my side in that respect and that was always good. My wife had her own interests. She’s a long-time teacher and in 2008 of all time she founded a small private school in Washington which she’s still running today with innovative teaching techniques and she was doing that. She had her own things that she was concerned about, but she was always there. We always, when I was available, we would have dinner together and the like. That is very important for anybody in a high stress position, whether it’s public or private.  Smith: Actually, that brings me to a second clip I’d like to play. This is President Obama speaking about you. Let’s listen to President Obama.  *CLIP with President Obama*  Smith: First of all, it must be nice listening to that again.  Bernanke: Yes, and I would just like to say that one of the things that helped me considerably was that both President Bush, who appointed me originally, and President Obama, who reappointed me, so a Republican and a Democrat, were very supportive throughout the crisis. On the one hand, gave us what we needed, and on the other hand, respected the independence of the Federal Reserve to make decisions, especially monetary policy decisions. I was very lucky in that respect. I was gratified that President Obama saw fit to reappoint me because at the time that he reappointed me, which was in 2009, the crisis had been calmed, but the economy was still in very bad shape. A different president might have said that’s not a great record. I need somebody different, but he gave me the chance to continue to finish the job, so to speak. Again, I was very grateful to do that, even though part of me said, another four years of this is going to be a lot of stress. But I did have very good relationships with both presidents that I served under.  Smith: Very different administrations to work with. So again, it speaks to your ability, I guess, to talk to lots of different sorts of people and gain consensus.  Bernanke: I’m just not all that politically inclined. I’m more interested in the analytics of the economics, of the implications of economic decisions for the wellbeing of the broader public. I tried to avoid, when I was chair, I’d never voted, for example. I just felt that I needed to take a neutral political perspective. Again, this was the consensus building part that we’ve been talking about, which is, I frequently had to testify before Congress. I testified some 80 times before Congress. Of course, you had to deal with very different perspectives on the two sides of the aisle. I couldn’t always satisfy them, and sometimes they were very critical. But I did my best to try to answer in a straightforward way and to try to explain what we were doing, why we were doing it, and why it would be beneficial to follow that strategy.  Smith: That reappointment by Obama does speak volumes again about the confidence that he and others had in you, because he obviously had to project confidence himself. Reappointing you was quite a vote of support.  Bernanke: Yes, I interpreted it that way. There had been some tradition, going back to Alan Greenspan, who was reappointed, I think some four times by both Republicans and Democrats, I think there had been some tradition because the Fed, like other major central banks, is supposedly independent of the executive branch. Chairs who had done at least a reasonably good job were typically reappointed by the president, even if parties had changed in the meantime. In reappointing me, he was not setting a new precedent in a sense, but it was a vote of confidence, obviously given the circumstances of the time. I will say that he was very engaged, particularly, The Fed, besides monetary policy and financial crisis, financial stability issues, also has a regulatory role. In 2010, there were major regulatory reforms in the United States, which I, as the Fed chair, had considerable input to, and the Treasury Secretary and the like. The president would call us to the White House to talk about those things as well. He was engaged. I was, of course, gratified that he saw fit to reappoint me.  Smith: He mentioned your pharmacist teacher combination upbringing. You grew up in South Carolina, in Dillon, South Carolina. What was it like, the home life?  Bernanke: All my grandparents, all four of them were immigrants from Eastern Europe. They all came originally to New York and other large cities. My father’s father was a pharmacist also, and he moved to Dillon in order to buy a pharmacy that was for sale there, and he brought his family with him. Some cultural mismatch there. We were one of the very few Jewish families in that town. That being said, I made friends there and played in the high school band and participated in other activities. My father and his brother were pharmacists. They owned the store together. They bought it from my grandfather who had started it. They were important people in the town because I think in the entire town, we had one doctor, and so people would frequently come to the pharmacy and ask for simple medical advice or for nutritional advice etc. My father and uncle who were called Dr. Phil and Dr. Mort, had that kind of relationship. My mother was a teacher for a while, but when I was growing up, she was mostly a housewife. She stayed at home or she worked part-time in the drug store doing various jobs. My mother was there most of the time and it was a good home life. I had two younger siblings, a brother who is now a lawyer and a sister who is an administrator in a music college. It was a small-town childhood, and in that respect, similar to many other people. Having said that, there was quite a bit of culture shock when I left there and went to Cambridge, for example, to go to college.  Smith: Indeed, yes. From Dillon to Harvard, yes. Do you think actually it was a blessing to have been brought up in small town in South Carolina, rather than in New York, for instance, where it could have happened?  Bernanke: There were frankly pluses and minuses. I think on the one hand, I mentioned the cultural mismatch. I went to public schools in Dillon, which were probably not as good as the best schools in New York, for example. But on the other hand, living in a small town does have a lot of advantages. I worked various jobs. I worked construction. I worked waiting on tables. I worked in the drug store. I had friends, obviously, who had worked on farms and agricultural work. I think one thing that I got out of being in Dillon, besides just sort of a sense of what broader America is like, outside of the big metropolises, besides that, I think I got at least a sense of how hard it is for the ordinary person to put food on the table. Working as a construction worker, unskilled worker, as I did for one summer, is very hard, didn’t pay much. I said this is a hard way to make a living. Waiting on tables was also long hours. Sometimes you had customers or customers who didn’t tip. Again, it was a useful education for me when I was Fed Chair. Of course, I was always looking at the numbers. Here comes a number about how many jobs were created last month, for example. On the one hand, as an economist, I’m looking at that number and trying to think about how it fits into a broader economic picture. But on the other hand, having grown up in a small town, not a very rich town, economically very stressed place I could think about the real families, real people that those numbers represented. That was important to me.  Smith: You were also a stellar student. You seemed to excel easily. Or was it that you had a very good work ethic from a very early age?  Bernanke: I just liked to learn. I think I had the ability. I never had any problems in school. I was the state spelling champion in sixth grade. Later I won a state-wide award that allowed me to make my first trip, foreign trip, a short tour of Europe. when I was a senior in high school. Frankly, when I went to college, when I went to Harvard, I was fairly shocked because I found that I actually had to study, which I had never done before. I was faced with a much more extensive and competitive environment than I had been used to.  Smith: I’ve never participated in a spelling bee, but I’ve seen them televised and they look absolutely terrifying in fact.  Bernanke: Like everything else, they’ve become professionalised. When I was competing in the state, I actually won the state spelling bee even, and went to Washington to compete in the national spelling bee. I didn’t really study for it. Again, I didn’t sit and memorise long lists of words. I think now the competitors basically put their lives on hold for months in order to study long lists of difficult and obscure words. Like I said, it’s sort of become more professionalized than it was when I was, many years ago, when I was involved in that.  Smith: Does this ability to spell indicate a particular love of words? You write quite a lot of books. Are words very important to you?  Bernanke: Let me just first say that spelling is a sort of unusual talent, that not everybody has it. Some of the people who don’t have it include great authors and very smart people. I’m not saying it’s correlated with ability in any way, but I always have been interested in words. Even as a young child, I was interested in where words came from. I would listen to some expression, some idiom. It doesn’t really make sense. Where did that come from? My life is much better now because now I can look on my phone and it will tell me where the expression was originated or where the word came from. When I was growing up in Dillon, we didn’t have such easy references. But I’ve always been interested in the origins of words. I feel always very unwillingly kind of irritated when journalists and others misuse words or spell them wrong or the like. I know that’s not rational, but it’s just something that I have from my childhood.  Smith: Do you read a lot?  Bernanke: I read all the time. I read three or four books a week, probably.  Smith: Gosh.  Bernanke: Of all different types. The thing I would say is I don’t read much economics. I read mostly either fiction, some of it junky fiction, like detective novels, but other kinds of fiction as well. Then I’ve always tried to be broad in my interest, readings about science or mathematics or biology or astronomy, whatever other fields are interesting. Right now, we have a lot of interesting things happening in artificial intelligence, for example computing. While I’m obviously not an expert or a specialist in any of these fields outside of economics, I do like to keep up. I like to read good popular writers who can explain in reasonably clear terms what’s happening in these different fields. I do have very eclectic tastes and I think I could have been something else other than an economist. It was almost an accident that I ended up in economics because I liked some courses that I took at Harvard. I do have broad tastes and I do like to read and it fits with my introverted personality that rather than going out to a big party, I would rather stay home with the book if at all possible.  Smith: Would you identify one book that has particularly influenced you?  Bernanke: I don’t think so. I could give you many books that I found fascinating. It’s a very esoteric example, but there was a computer scientist and philosopher named [Douglas Hofstadter](https://www.nobelprize.org/prizes/physics/1961/hofstadter/facts/) who wrote a book called *Gödel, Escher, and Bach*, which was about self-referential thinking. Essentially, it sort of tried to get into the meaning of intelligence and consciousness from a philosophical point of view and a mathematical point of view.  *CLIP reading from* Gödel, Escher and Bach*:*  “If words were nuts and bolts, people could make any bolt fit into any nut: they’d just squish the one into the other, as in some surrealistic painting where everything goes soft. Language, in human hands, becomes almost like a fluid, despite the coarse grain of its components.”  Bernanke: A very obscure book. Most people don’t find it very interesting. I just found it very stimulating. I read it many years ago. But I’ve read many great books since then. I’d have to sit down and try to make a list. I feel anyone I told you would be offending or leaving out things that were important.  Brilliant: Adam, Ben Bernanke’s research has focused on the Great Depression. Could you tell us a little bit about that period of history?  Smith: Yes. The Great Depression was the longest and deepest downturn the modern economy has ever seen. It began in 1929 in the US, at the end of a decade of relative affluence in the US, the Roaring Twenties, and then suddenly this downturn began. Then it went on right up and into the Second World War, only stopping in 1941. It lasted over a decade, and although it started in the US, it spread around the world, causing misery for millions and having profound consequences that extend, I suppose, up till this day. It was a period in which there were massive rises in unemployment and massive decreases in production. I suppose for most of us, it’s some of the iconic images from that time that stick with us. Pictures of very long lines of people trying to get a job, people just desperate to find work of any sort. Many films that capture the misery of that time from [John Steinbeck](https://www.nobelprize.org/prizes/literature/1962/steinbeck/facts/) to for instance, *It’s a Wonderful Life*.  Brilliant: Do you know, I’m actually embarrassed to admit I haven’t seen that film, Adam.  Smith: What do you do at Christmas?  Brilliant: We’re watching *Elf* at Christmas.  Smith: Nevertheless, I’m sure you’re familiar with the iconic scene from it, in which people are massing outside the doors of a bank trying to get their money back. One of the things that happened during the Depression was that many banks failed. In the end, in the US, it led to a complete collapse of the banking system in 1933. That collapse of the banking system is something that Ben Bernanke, in particular, has identified as being important in the Great Depression.  Brilliant: What have been the consequences of Ben Bernanke identifying the importance of that?  Smith: It’s changed, I suppose, the way that people look at bank failure. What he saw was that the closing of banks, which before his work had perhaps been seen more as just a consequence of the Depression, actually had a causative influence, that the fact that there were no longer banks around who could provide loans to people meant that people couldn’t get credit to build their way back out of the recession. That just prolonged the thing. Now people realise that even in the midst of crises, it’s very important to preserve the credit system. Central bankers around the world these days try very hard to preserve the integrity of banks. Central banks tend to act as a lender of last resort, so that even if a bank is about to collapse, there’s somebody who will guarantee that the depositors can get their money back. That’s playing out around us in real time, unfortunately, at the moment. We’ve recently seen three commercial banks in the US fail, which wasn’t expected. The role of policy makers in reacting to that sort of situation is very much in the news at the moment. It’s very interesting to listen to Ben Bernanke talk about what got him interested in this huge topic of the Great Depression. Let’s take a listen to that.  Bernanke: The Great Depression was this global catastrophe where you had all these workers and factories, unemployed and not producing when, in some sense they could have been producing. It’s just a big puzzle, because the underlying paradigm of economics going back to Adam Smith in 1776, which has also been very influential in modern economics, is that market economies will make good use of resources. Prices will direct resources into the most productive uses and give people incentives to find the most useful occupations and the like. That world of market clearing where prices are signals, etc, doesn’t fit very well with the idea that, for more than a decade, we had unemployment ranging from, depending on how you measure it, ranging from 15 to 25 percent in the United States. At a time, by the way, when we didn’t have unemployment insurance and other kinds of things to help the unemployed make do. I always found it to be just a fascinating puzzle. By the time I got to grad school, there were obviously a lot of theories out there, and none of them were completely satisfactory. Let me tell you a quick story. My mother’s parents, my grandparents, who lived in Charlotte, North Carolina, I used to visit them as a young child. I used to visit them in the summer. I’d sit on the front porch, particularly with my grandmother, and just she’d tell me about her life. She told me once about when she lived in Connecticut in a town that had shoe factories. The shoe factory shut down during the Depression because there wasn’t enough demand. She told me that many children in that town went to school with ragged shoes or maybe even no shoes at all. I said, well, why would a six-year-old child? I said, why would they do that? and she said, well, because their fathers had lost their jobs when the shoe factories closed. I said, wait a minute, why don’t they just open up the shoe factories and make shoes for the children? She said, no, it doesn’t work that way. I just thought that was just so incomprehensible and why resources are sitting there and not being used. I read *Grapes of Wrath* and other depression related stuff even through high school. When I got to graduate school and began to think about what fields I was interested in, I became convinced that macroeconomics and monetary economics was really worth studying, because that would help you understand big events like the Depression, which not only created an enormous amount of economic hardship, but arguably led to the ascent of Hitler and World War II and all that followed from that. These are very important issues. I couldn’t see how you wouldn’t at least be interested in those questions.  Smith: The interest in the depression was there, and then the tools were provided by economics to address what happened. Were you surprised by what you found? Which was that banks, the role of banks, the stability of banks was much more important than had previously been assumed. It was that they really had a deciding factor.  Bernanke: The prevailing story when I was in graduate school was that the depression was caused by a collapse of the money supply, which in turn led to falling prices and to other problems. I think there’s actually some truth to that because the gold standard was prevailing at that time and it collapsed after World War I brought down money supplies and prices and was a powerful depressing force. People like [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/) and Anna Schwartz documented the relevance of the collapse of the money supply to the economy. If you just read about the depression, if you read diaries or memoirs of the depression, one of the other aspects of it is just simply the financial distress. Not just falling prices, but the fact that businesses were failing, banks were failing, individuals were obviously in huge financial distress. They couldn’t pay the mortgages. There was the rate of failure of delinquency on home mortgages in the depression was probably two or three times what it was during the global financial crisis. That was a horrible period. It seemed to me, in some sense, obvious that the breakdown of the credit system, which at that time most credit was provided by banks, and about a third of all the banks in the United States, thousands of banks failed during the depression, that the breakdown of the credit system would have to be a negative for the economy. I don’t dispute the fact that monetary issues were important. In fact, when I was Fed chair, obviously, I tried to ensure that monetary policy was supporting economic recovery. It really did seem to me that there had to be some influence of the financial distress from bank failures and debtor bankruptcies and firm failures and the like. I think there are not very many good aspects of the global financial crisis, but one thing I think there weren’t very, you can’t explain that crisis of 2008 by monetary forces. That was clearly a crisis that was brought about by a collapsing financial system and widespread default and widespread financial distress. Ironically, the experience of 2008 kind of reinforces the idea that these issues must have been important in the 1930s as well. What I hope I did was to try to add a dimension to our understanding of depression. I feel now that I understand the I feel that I understand why it was as bad as it was and why some policies helped to improve it and others did not. I at least have been able to answer my six-year-old question to my own satisfaction at this point.  Smith: Indeed. Do you think, given recent events with Silicon Valley Bank and Credit Suisse for instance, do you think we’re always teetering on the brink of these things?  Bernanke: Let me say yes and no. Yes, in the sense that confidence is always an ephemeral thing. If there’s a sufficiently widespread loss of confidence or other unexpected events then you can have a lot of financial distress. One thing I’ve learned is to never say never. It’s always possible. But many of the warning signs that we saw in the depression and at the early stages of the global financial crisis, like large quantities of bad debt, like subprime mortgages and a credit boom, weakness in a variety of financial institutions. We’re not seeing that at least so far in in the current system. Since the global financial crisis, there have been a lot of changes in, for example, capital requirements and the like that have made banks stronger. Now, again, I think you should never say never. In the United States, unlike Europe, banks actually provide less than half the credit that people and firms use. A lot of credit is provided by other kinds of institutions, which are collectively known as shadow banks, which are not official banks, and which are much more difficult to monitor, and which are much less regulated. I’ve been concerned about those ever since the financial crisis. I don’t think that the regulatory strengthening that was done for banks has been quite as effective and comprehensive in the case of shadow banks. There probably could well be things happening in the shadow banking system that the regulators and policymakers are not aware of. I think vigilance is always important. You should always assume that financial instability is a possibility. You should always be on the alert. You should be always trying to find ways to strengthen the financial system. At the moment, I think we’ll see. But again, never say never. It looks like the response of the Fed and the Treasury and FDIC has stopped what seemed at the time to be some risk of contagion to other banks and at least so far we haven’t seen that kind of contagion.  Smith: Yes, and again it must be nice to see your own approach to policy vindicated.  Bernanke: I think that if you say my approach to policy means that financial stability is very important for the economy, which is a one-sentence summary of my research, I think that has clearly been vindicated. I certainly have plenty of critics even today about exactly how we approach that and why we didn’t identify the crisis earlier and what other steps we might have taken. I myself am not completely satisfied with all the regulatory changes that have been made. There’s plenty of room for debate and discussion. But the idea that financial stability is important for the economy, which surprisingly enough, when I was first working on these issues back in the 80s, was not a mainstream idea, I think it has to be well accepted by this point.  Smith: In a way, your career describes as kind of an arc that you’re an academic, you become Dean of the Economics Department at Princeton, and then you’re thrust into this position of having to be doing as well as thinking, and now you’re back to being an academic again. What’s it like to see those different sides of life as an academic and then return to academia?  Bernanke: The experience of being a policymaker, and it’s only a minority of economists who get that opportunity, certainly influences the way you think about economics and affects the decisions about what research you’d like to do and the like. I’m glad to be back in a research mode. I think that I got a very difficult but also informative experience as a policymaker. I think I’m ready now to reflect and to write and to try to distill the lessons that I learned in that experience. Particularly at this stage in my life, when I’m no longer a young professor scrambling to publish or perish and I can write what I want to write, I think it’s actually a very good life because you can work on what you want to work on. If something doesn’t work out, it’s not a disaster. But I’ve actually done some work since my policy time, which I’m proud of. I think it’s been good and it’s contributed to policy debates and the like. I hope to continue to do that, both in articles and in books.  Smith: I mean, obviously curiosity came naturally to you and it seems to continue unabated. It’s a marvellous gift to have.  Bernanke: My curiosity is intellectual curiosity. I like concepts and ideas. I’m probably not so curious about, I don’t know, mechanical things, for example. I’m not adept in fixing things around the house, for example. In that respect, I’m probably quite different from my physics laureate colleagues, and… But I just enjoy ideas and enjoy conceptual thinking. I thought in high school that I might become a mathematician, but it was a little bit too withdrawn from social life for me. I found economics to be an area that combined quantitative mathematical and abstract thinking with social science, concerns about society, concerns about the public. It was a good compromise for me in terms of my interests in general. But I’ve always been just very stimulated by ideas and I continue to be like to follow ideas. That’s why I think we mentioned earlier artificial intelligence. I think some of the directions that science and technology are going today are extremely interesting. I don’t pretend to be a contributor or even fully understand these developments. I like to follow them closely.  Smith: But it is what you just said is so common to hear among economists and economics laureates that they’d thought of doing maths and then they realised, if you like, that you can study and understand and perhaps even tweak social issues through maths and that by becoming an economist.  Bernanke: It also interacted with my interest in, I’ve always been interested in history as well. I think that understanding the economics of how things work really sheds a new dimension on all of these important periods. What was the economics behind World War II? I read a very dense but very interesting book by Adam Tooze about the war economy in Nazi Germany. How the economy worked in Germany and how the ability of the Nazis to develop weapons and get oil and develop ammunition supplies, etc. supported or constrained their ability to wage war. Ultimately, economics is a very important component of warfare, social conflict and opportunities that ordinary people have. A knowledge of economics really throws a new light on history that if you don’t know economics, you think it’s all about kings and queens and battles. There’s a lot more to history than that. Economics captures how ordinary people live and why they live the way they did, which is not often left out of history books.  Smith: Absolutely, yes. It becomes a lens with which to analyse everything.  Bernanke: That’s right, yes.  Smith This has been very informative and a huge pleasure. Thank you very much indeed.  Bernanke: Thank you.  You just heard Nobel Prize Conversations. If you’d like to learn more about Ben Bernanke, 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 Karin Svensson. The editorial team also includes Andrew Hart, Olivia Lundqvist, and me, Clare Brilliant. Music by Epidemic Sound.  Serving as head of the Federal Reserve certainly put Ben Bernanke in the spotlight. We’ve spotlighted over a dozen brilliant economists in earlier episodes. Find them on Acast, or wherever you listen to podcasts.  Thanks for listening. |
| Telephone  interview | 0804=BB  Adam Smith: May I speak with Ben Bernanke, please?  Ben Bernanke: Speaking.  AS: Oh, hello, my name’s Adam Smith.  BB: I was expecting your call.  AS: Good. First of all, congratulations on the award.  BB: Thank you very much.  AS: I understand that you didn’t actually receive the news directly.  BB: No, we were not expecting this to happen. We turned off our cell phones as usual at bedtime. And we got a call from our daughter in Chicago first thing in the morning letting us know that she’d heard a news report.  AS: Have you actually spoken to the committee yet?  BB: I have not. I received the letter in an email and I replied that it was received. And I apologized for not being available, again. I did not, I was not, you know, considering this possibility, so I didn’t, you know, prepare for it. But I acknowledged the letter and thanked the committee for the honour. And of course, I will do whatever I can to, you know, participate effectively and to enjoy the whole experience.  AS: I think Barack Obama famously described you as ‘the epitome of calm’. Does that capture the situation when she told you the news?  BB: Yes. I’ve been pretty calm about it, but I think I’ve been trying to process this remarkable development. So I’ll see how I feel in a couple of days, but I’m still trying to take it all in.  AS: I suppose for many laureates, it thrusts them into the limelight. You are obviously there already, so I suppose that changes things a bit, that puts a different dimension on it.  BB: Well, I spent most of my career as an academic doing research. And then of course I got into government work. But I’m glad to be thinking again about the work that I did. And now that I’m no longer in the government, I’m doing academic research again. So that was an important part of my life and I’m glad that that was recognised by the committee.  AS: Yes, indeed. The change from being an academic and your work, your 1983 paper for instance on analysing the role of bank-runs in the Great Depression, or in exacerbating the Great Depression, is mentioned as part of the citation. It’s very rare for somebody who does that sort of work then to find themselves in the position of being a practitioner, isn’t it? And a practitioner on such a grand scale?  BB: It is rare. Perhaps it should be less rare, I don’t know. But I would say that what I did as a researcher was on the one hand very helpful in understanding the situation and responding to it. On the other hand, the real world is very complicated in issues like politics and communications and dealing with a committee of people to make decisions. All those things are complexities in a real world situation that an abstract economic model doesn’t capture.  AS: It’s calmness, again. I guess you need a very calm head to decide what to do in the face of all that uncertainty.  BB: Well, I tried to keep my focus simultaneously on the very near term, what I had to do next. The next speech, the next testimony, the next decision, while at the same time periodically thinking about the big picture. And I found that that worked for me. And the same thing is true, you know, in research, that you have a program, you have a plan, but on any given day, you have a set of specific problems you have to solve. And keeping your mind focused on those problems helps you keep things in balance.  AS: Fascinating. The award really underlines the importance of banks in society, as if that importance wasn’t obvious already. I think back to the peace prize, to [Muhammad Yunus](https://www.nobelprize.org/prizes/peace/2006/yunus/facts/) in 2006, and the emphasis that placed on the power of banks to get money into the hands of even the poorest people. Is that how you see it, as underlining the importance of the bank?  BB: Yes, and I think banks is not quite the right word, actually, because what we’re talking about here is credit. And Yunus was right that you know, the kinds of institutions that provide credit for very poor people in developing economies might be very different from a formal bank. You know, like we have in the US. It might be a small collaborative with community cooperation and enforcement. It might be in the United States, it might not be a bank formally. It might be a hedge fund, or it might be a venture capital company or a private equity company or many other forms of credit provision. So, it isn’t so much banks per se, but rather the idea that if something destroys or seriously hampers the ability of people to borrow or get liquidity when they need it, they will become very conservative, very cautious, and that will cause the economy to slow considerably. And I think that’s the real insight, that credit can help provide growth. But if the credit mechanism is badly disrupted, it can also be a very adverse development for the economy.  AS: And I realize this is too general to question, but given that people the world over are now very worried about what’s happening economically, do you think that we’re in a safer place now than we were when the crisis hit in 2008?  BB: Well, it’s inherently very hard to know, but I think that this is a different situation in that the recession and the consequences of the recession were not due to financial problems, per se. Of course, the recession was caused by an external event, the pandemic. And we came into the recession generally speaking with pretty strong financial institutions, strong banks, and the like. We did, of course, have a disruption in March of 2020 in the treasury market, which the Fed responded to quite quickly using some of the same tools we did in 2008. But again, this whole experience, recent experience, was not caused by financial weakness. But as I mentioned the other day that if things continue, if we have a bad recession, which I’m not predicting, but if that were to happen, that could weaken financial conditions and financial institutions, and that in turn could make the downturn more persistent. So it’s certainly something that the Fed and other central banks pay close attention to. They want to be sure that even as the economy slows that financial institutions and credit supply remain healthy.  AS: One of the things that the award celebrates is the working relationship between you and your co-laureates. And Douglas Diamond, when we spoke to him, emphasized that, how did he put it? That I can’t think of two people I’d rather be discussing things with than my co-laureates. Sometimes in research you just happen to find colleagues with whom it’s good to discuss things, and it’s the right time. And 1983 seems to have been the right time for the publications that the three of you produced.  BB: I’ve known Doug Diamond in particular for a very long time. And I knew the Diamond and Dybvig paper, it was about, as you say, it was published about the same time as my paper on the Depression, an extraordinarily interesting paper, which I always taught whenever I was teaching these things in graduate school. I think the reason that this was all happening in the eighties was a combination of the fact that on the one hand people were paying more attention to financial aspects of the economy rather than thinking of financial markets and financial institutions, that being sort of a background institution. They began to start thinking more about financial crises, for example, as propagators or even causes of downturns. I’m thinking of people who were looking at emerging markets, for example, which suffered periodic financial crises. And then we had crises in some advanced economies, including a little bit later, of course, in Sweden. But at the same time as people were paying more attention to the role of financial institutions, there were developments on the theoretical side of economics looking at problems of imperfect or asymmetric information and how those might be solved, and how they’re related to the resources of both the lender and the borrower, for example. So, the insight I think that is in much of the work that I’ve done, and a lot of it with Mark Gertler, is that when financial conditions weaken generally so that people have less net worth, less collateral, it becomes more difficult and more costly for lenders to make sound loans. They tend to pull back, and that creates more stress in the economy. So that theoretical approach that helps you think about lending and borrowing as a problem of imperfect information subject to the various ramifications that the theory provided, that was happening at the same time. And the combination of those two things, I think generated a lot of very interesting work in banking, in corporate finance and in macroeconomics.  AS: Isn’t it interesting how ideas have their time? All that time has to pass from the Great Depression in order for your work to come to fruition?  BB: Well, many, many people worked on the Great Depression. Of course, there was the famous work by Friedman and Schwartz, for example, which was being debated when I was in graduate school. And then later there was a lot of very important work on the gold standard and other causes of the Depression. So it was something that people have been working on for many years. And I hope that this, you know, my work contributed to the understanding of the Depression, but the directions we took it were more general than that. We wanted to think, we, referring to my various co-authors, and I wanted to think about how fluctuations in financial conditions can affect not only the economy in a deep depression, but also how they play a role in more ordinary recessions or fluctuations in the economy.  AS: Lastly, the Swedish Academy of Sciences clearly have a vision of what they mean to say by awarding this prize. What message do you hope that the prize sends?  BB: I think from a real world perspective, I hope it does underscore the importance of a stable and healthy financial system, both for long term growth and also for short term stability. And I think that we’ve made a lot of progress there. Bank regulation is much better than it was in when I wrote the 1983 paper. But I think there’s also lot of work to do, and I think there’s still parts of the financial system that could use a stronger oversight, particularly with respect to their solvency, this safety and soundness. And I hope that as a practical matter that regulators and policy-makers will continue to think about the financial system as being a critical part of the broader economy and not just a a side show. It’s really one of the things that makes the economy successful, makes it work. And by the same token, if the financial system is breaking down, then the economy will feel the effects of that.  AS: Thank you very much indeed. So, we very much look forward to welcoming you to Stockholm in December. I assume you’ll be coming.  BB: Yes, of course. And my wife as well, and perhaps my daughter who phones us with the news. But I will say that I’ve been to Stockholm a couple of times and I think is a lovely city, but I’ve never been there in December. So, we’ll see what that is like. But, I think it’s a very nice city and I’m looking forward to coming back there.  AS: Thank you very much indeed for taking the time to talk to me, and we look forward to seeing you in December.  BB: My pleasure.  AS: Thank you.  BB: See you then. Bye-bye |
| Interview |  |
| Q1 | Where does your passion for economics come from? |
|  |  |
| Q18 | What are the key implications of your research? |
|  |  |
| Q5 | How do you maintain your curiosity? |
|  |  |
| Q3 | Was there a particular person that influenced you? |
|  |  |
| Q11 | How do you cope with failure? |
|  |  |
| Q19 | How do you move past failures and disappointments in your work? |
|  |  |
| Q13 | What advice would you give to a student or young researcher? |
|  |  |
| Q20 | Can you tell me a bit about your childhood and teenage years and how this influenced your interest in economics? |
|  |  |
| Q21 | Do you have advice for young people who had a similar background and who might want to follow a similar path to you? |
|  |  |
| Q22 | What skills do you need to succeed as a researcher and policy maker? How can young students develop these skills? |
|  |  |
| Q23 | What skills are important for researchers or students to develop? |
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| Q24 | What made you think that there was more to uncover with bank runs? Do you think it’s important to revisit or re-analyse subjects that people may have studied a lot already? |
|  |  |
| Q25 | In recent years, a lot more organizations have begun investing in the Environmental, Social, and Governance (ESG) movement. Is it important to invest in climate conscious actions? Do you think we’ll see more of these in the future? |
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| Q26 | Can you tell us about the object that you are donating to the Nobel Prize Museum? |
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| Q27 | How does it feel going back to academic research after so long as a policymaker? |
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| Q28 | Do you think it’s important to have hobbies outside of your research? |
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| Q29 | If you could give someone one piece of advice, what would that advice be? |
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| Q30 | People often look for mentors when developing their skills. Do you think there’s a certain way to identify and find a mentor? |
|  |  |
| Q31 | When you’re approaching a problem that you know is going to be difficult, how do tackle that problem? |
|  |  |
| Q32 | How did you react when you heard you’d received the prize? |
|  |  |
| Q33 | How does it feel to know that your research has had this lasting impact? |
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| ID | 0805 |
| Biographical | I was born in Chicago in 1953. My maternal grandparents were Catholic. My grandmother, Ethel Elizabeth Houlehan Gunkel, was of Irish descent. She was a strong woman who lived to 100 and was lots of fun. My grandfather, Frederick Peter Gunkel, was of Austrian and German descent. He was the executive hog buyer for Oscar Mayer in Madison, Wisconsin, which insulated the Gunkel family from the Great Depression of the 1930s. In that period, the Gunkel family was able to help feed their neighbors with food from Oscar Mayer. My grandfather died in his 50s, and my grandmother became the matriarch of the Gunkel clan. My grandfather had told her to never sell her Oscar Mayer stock, almost her entire net worth, which violated all of investment portfolio theory. She never sold, but she did well when General Foods acquired Oscar Mayer in 1981.  My grandparents on my father’s side were Jewish. My grandfather, Harry Diamond, emigrated from Austria at age 14. He owned and managed a family rabbinical wine company in New York City, started by the family of my grandmother, Hattie Wirklich Diamond. The wine company went out of business during the Great Depression, and he never recovered emotionally. He died in 1944 at age 58. Hattie Diamond lived until age 84. She never forgave my father for not marrying a Jewish woman. I only met her a few times in my life.  My mother, Margaret Irene Gunkel Seehafer, was born in Madison, Wisconsin, one of four children. She married her college sweetheart, William Bleckwenn Jr., soon after she graduated from the University of Wisconsin, majoring in economics and psychology. He died of a brain aneurism in 1947, less than two years later. My mother subsequently earned a Master of Social Work degree from the University of Minnesota. She met my father, Leon Diamond, in Minneapolis, where they were married in 1952. They moved to Chicago. Their marriage ended in divorce before I was two years old.  My mother joined the PhD program in Social Service Administration at the University of Chicago while she was raising me as the only child of a single mother. She was very devoted and supportive. She always encouraged me even though I was a terrible student until high school. She told family members (but not me) that she would be happy if I improved sufficiently to graduate from junior college. We lived in the Hyde Park neighborhood near the University of Chicago until I was in eighth grade. My mom was close to her three siblings, and we spent holidays with them. I loved visiting my cousins, who made me feel like a sibling of theirs when we came.  My mother was a Democrat and involved in local and national politics, hosting events for campaigns to elect candidates such as John F. Kennedy. I heard more about politics and government policy than most seven-year-olds did. She later became quite interested in investing in mutual funds, and discussions with her before I was in high school may have helped develop my interest in economics (which only became my primary interest years later).  My mother did not finish her doctoral dissertation at the University of Chicago. She had a successful career as a social worker, eventually becoming the deputy director of the Illinois Department of Mental Health. In 1973, she moved back to Madison, Wisconsin and began teaching social work at the University of Wisconsin. She married an old friend from college, Gene F. Seehafer, in 1976. He passed away in 1996. My mom passed away in 2017.  My father grew up in the Bensonhurst neighborhood in Brooklyn, New York. He went to college in 1941 at the University of Louisville. He joined the navy in 1943 and became part of the V-12 Navy College Training Program to quickly get an MD and serve in the Navy during World War II. He was discharged in 1945, at the end of World War II. He completed his MD in 1946. After an internship at Kings County hospital in Brooklyn and serving as a general practitioner in Red Jacket, West Virginia, he became a resident in Obstetrics and Gynecology at the University of Minnesota in 1951. He (and my mother) moved to Chicago in 1952, where he became a resident in psychiatry at the University of Illinois Chicago, switching specialties in medicine. He had a successful career as a psychiatrist, both in private practice and academic medicine. He was on the faculty of Northwestern University, eventually as the head of residents’ training and inpatient programs at Northwestern University’s hospital. He retired in 1992 and passed away in 2022. My father lived in Chicago almost my entire life, and I saw him regularly as a child and as an adult. He remarried in 1962 and I have two brothers, Mark and Jason, from that marriage. **My Education** In high school, I became a better student, especially in science and mathematics courses. I attended a private school starting in eighth grade, The Latin School of Chicago. The Latin School changed dramatically while I was there, from a traditional and highly structured school to a progressive and innovative one. As a not very disciplined student in eighth grade, the traditional and structured school suited me well. Latin hired new young leaders from California, starting in my junior year. They brought in new courses and attitudes. By then, the less structured approach also suited me well, as I had become a more serious student. A course on US constitutional law expanded my interest in social science. This was followed by a seminar on capitalism. A main reading for the course was *Capitalism and Freedom*, by [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/). The following year, I helped teach the course by reading chapters from [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/)’s classic introductory textbook, *Economics*. The other students and I would read a chapter, and I would lead discussion. High school provided an introduction to very accessible books by two of the greatest living economists. Nonetheless, by the time I graduated, I wanted to become a molecular biologist. This was in part due to the influence of my biology teacher who arranged a one-student advanced second course in biology for me. **Brown University** I applied early decision to Brown University to study biological science. I enrolled in a biology course that I immediately found uninteresting. I decided to drop the course two weeks into my first semester. I found an intermediate microeconomics course that fit my schedule and had no prerequisites. Due to my background in high school, I felt that skipping introduction to economics was appropriate. The course was quite advanced, but not very mathematical. A more mathematical microeconomics course followed the next term, and I decided to switch my concentration to economics.  A summer job in Chicago at A. G. Becker, after my first year in college, turned out to be related to my subsequent research interests. A. G. Becker was an investment bank that was developing a database for a product, the Commercial Paper Program Service. The project measured the full borrowing costs to firms for whom they had issued commercial paper. This was to allow Becker to show the borrowers how these costs compared to borrowing via bank loans on the same dates. My job was to collect the data (from paper confirmation slips) and find errors in the data. I also collected data on the costs of bank loans with similar maturities and issue dates. It did not use economics, but it did introduce me to the idea of the choice between bank loans and directly placed debt.  A budding interest in health economics led me to take a job during another summer with the state of Rhode Island House of Representatives Commission to Study Medical Malpractice Rates. I learned about the political economy of regulation, with competing interest groups and regulatory capture. I also became more aware of other non-economic issues involved in setting health policy. This is one reason that I subsequently switched to finance; it is an area that has fewer objectives that compete with attempts to resolve moral hazard and conflicts of interest.  In my senior year, I took a small undergraduate course taught by Jerome Stein on Milton Friedman and Anna Schwartz’s *Monetary History of the United States*. This taught me to think about the effects of monetary policy, while also learning many facts about inflation and economic fluctuations in the United States. Almost half of the course was devoted to the chapter on the great contraction of the 1930s. Friedman and Schwartz describe the bank failures of the period and ascribe much of it to the Federal Reserve’s unexpected unwillingness to serve as a lender of last resort. Bank failures and tight monetary policy led to a reduced supply of money and this led to a falling price level. They argued that this was the way that bank failures damaged the economy. It seemed to me at the time that bank failures would have other adverse effects on the real economy. In my final exam for the course, I argued that the bank failures would cause an inward shift in the [Hicks](https://www.nobelprize.org/prizes/economic-sciences/1972/hicks/facts/)-Hansen IS curve; there would be less real activity for a given rate of interest. I did not provide a mechanism for the shift. I only remembered this recently after looking at my old exam. My exam book from the course is the artifact that I gave to the Nobel Prize Museum.  Jerome Stein was a monetarist like Friedman, but he was a student of [James Tobin](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/facts/), who was a Keynesian. He had the highest respect for both Friedman and Tobin. He encouraged me to go to Yale and try to work with Tobin. I also benefited from great graduate macroeconomics courses at Brown from Herschel Grossman and William Poole. **Yale University** I joined the PhD program in Economics at Yale in 1975. The ideas from the emerging field of finance showed up in the first-year macroeconomics courses. James Tobin emphasized financial markets and institutions in his research on monetary and fiscal policy. I soon met Martin Shubik, who was working on a theory of money and financial institutions. He pointed out many questions that competitive market general equilibrium theory could not address or would not provide a satisfactory prediction. Nothing could be more useful to someone looking for a topic to study. Shubik’s research used noncooperative game theory to model trade with money and price formation as a playable game. Banks were important in payments and the price formation mechanism. Although I never succeeded in adopting his methodology, I learned that one could use game theory to reexamine important applied issues in economics. I hoped to use game theory to model money and banking together.  James Tobin helped me get a job offer for the summer of 1976 from the Financial Institutions and Nation’s Economy (FINE) study. It was commissioned by the U.S. House of Representatives Banking Committee to propose reforms to regulation of banks. By the spring of that year, before I could begin work, the FINE commission was disbanded, supposedly due to pressure from lobbyists for the large banks. Professor Donald Hester, of the FINE commission and the University of Wisconsin, encouraged me to apply for a summer job at the Financial Studies section of the Federal Reserve Board of Governors. I learned both practical and conceptual aspects of banking at the Fed. Donald P. Tucker (formally on the faculty at the University of Chicago Economics department) hired me to fix a simulation model built to see what would happen to savings and loan associations if interest rates greatly increased. I tried to fix the old model, but I could not. Instead, I built a new one from scratch. Not too surprisingly, the savings and loans became market value insolvent from high interest rates, but it took a very long time for their book accounting capital to become negative. This was an accurate prediction of the future because they borrowed short-term to finance 30 year fixed-rate mortgages. I was then hired the next two summers to maintain the model, and also to examine the relative merits of introducing floating rate mortgages and long-term deposits to avoid the risk of insolvency. The Fed was an amazing place to learn from one’s colleagues. Sandy Grossman was on staff, while [George Akerlof](https://www.nobelprize.org/prizes/economic-sciences/2001/akerlof/facts/) and John Boyd were visitors.  Peter Lloyd-Davies of the Financial Studies Section suggested that I take a course in Financial Economics, as that field addressed some of the questions I was working on. I returned to New Haven and learned that Yale did not have any such course. I found out that Harvard and Yale had an exchange program, and I took a great finance course from John Lintner at the Harvard Business School. I also read the reading lists of several other finance courses and the detailed class notes from [Robert Merton](https://www.nobelprize.org/prizes/economic-sciences/1997/merton/facts/)’s course at MIT. I became a convert to financial economics. I decided to take my oral exams in the two fields of Finance and Monetary Economics. The director of graduate studies (William Parker, an economic historian) told me that I could not do both, because these were the same field. An intervention by James Tobin convinced him that these fields were distinct.  In my oral exams, one of my examiners in finance was Steve Ross, who had just been hired away from the University of Pennsylvania and would join the Yale faculty the following autumn. I had never met Steve, but I passed. I took a course from him that autumn and developed a much better understanding of finance. Steve brought the great students, Chester Spatt and Phil Dybvig, with him from Penn. I learned from them both.  My interests moved toward the effects of private information on financial markets and financial institutions. My first attempts at models were unsuccessful and complicated. Discussions with Steve helped me figure out what I was trying to do. Steve taught me how to build simpler applied models. For my first paper, he told me to take everything out of the model until it was not a model. Then, he suggested that I return one of the removed elements to the model and see what I got. I did that, and the model became simple to solve and interpret. I presented it to Steve, and he was happy. He told me not to put any of the elements that were removed back in the model. He told me that this simpler model was the first chapter of my dissertation. I was more than surprised. I feel that I would still be in graduate school if I had not had Steve Ross as an advisor. He encouraged me to do what I was good at and mattered to me, not what others seemed to value. He continued to be a friend and mentor until he passed away in 2017.  I became a recruiter for Steve, telling my friends to take his course and try to work with him. I convinced Greg Connor, Mark Grinblatt and Paul Pfleiderer to go take Steve’s course. Each ended up with Steve as their advisor. They have all thanked me several times. All were also very helpful with my research, especially Greg Connor.  I rushed to finish my dissertation in my fourth year, which was the normal time of completion in those days. I got my job market paper to Steve around Thanksgiving, which was a bit late. It contained two chapters of what would be my dissertation, including an early version of *Financial Intermediation and Delegated Monitoring*. I did reasonably well in getting interviews at the Allied Social Science Associations (ASSA) meetings in Chicago from business schools and departments of economics, but the University of Chicago did not interview me. I did interview with Professor Nancy Jacob from the University of Washington, who worked on banking. She told Professor Robert Hamada from the University of Chicago Graduate School of Business about my work. After the ASSA meetings, I received a letter in the US mail from Robert Hamada inviting me to send my materials to him. Soon, the GSB invited me to visit campus and present my paper. By this time, I had a job offer from the Wharton School of the University of Pennsylvania, with a pending deadline for acceptance. I visited Wharton on the day of their deadline. I was sitting in Sandy Grossman’s office when a call came (to Sandy’s phone) from the deputy dean at the Chicago GSB, with a job offer. Sandy Grossman encouraged me to come to Wharton but made it clear that he had a high opinion of the University of Chicago. Despite Sandy’s encouragement, I accepted Chicago’s offer. Sandy eventually became a highly valued colleague when he joined Chicago’s economics department two years later. **The University of Chicago** The University of Chicago Graduate School of Business (GSB) was especially vibrant when I joined in late 1979. The senior finance faculty members in finance were [Merton Miller](https://www.nobelprize.org/prizes/economic-sciences/1990/miller/facts/), [Gene Fama](https://www.nobelprize.org/prizes/economic-sciences/2013/fama/facts/), [Myron Scholes](https://www.nobelprize.org/prizes/economic-sciences/1997/scholes/facts/), Robert Hamada and James Lorie. The first three were future Economics Prize Laureates and were always around to give feedback. The junior faculty members were George Constantinides and John Ingersoll. I was put in a suite with Fama, Scholes, Lorie and Ingersoll. All provided helpful feedback, especially Gene Fama, who was also doing research on banking at the time. I would feel like a very hard worker when I went to the office on a Sunday morning until I found that Gene had been there already working for several hours. The finance group was very small, so I started near the top. Myron Scholes read my paper on Delegated Monitoring and advised me to improve the exposition before I submitted it. This took quite a while, and I waited until 1982 to submit it.  From my first days at Chicago, I also had good interaction with members of the Department of Economics. I attended the Money and Banking workshop and got comments and advice from [Bob Lucas](https://www.nobelprize.org/prizes/economic-sciences/1995/lucas/facts/) and José Scheinkman in particular. Charlie Kahn joined the department soon after and also became a great colleague. Sandy Grossman joined two years later.  At Chicago, I met Robert (Ro) Verrecchia, a new Assistant Professor of Accounting hired the year that I joined. Ro was a Stanford GSB student of [Robert Wilson](https://www.nobelprize.org/prizes/economic-sciences/2020/wilson/facts/). He was developing theories of information in market prices and the link of prices to information from accounting. I was focusing on rational expectations models of asset prices. We decided to work together, and I converted Ro to rational expectations models, which were very similar mathematically to the models he had been building. Ro taught me how to write papers more clearly and to overcome excessive perfectionism. My first two publications were with Ro. We wrote a series of papers and became good friends. Ro left Chicago for Wharton in 1983, which slowed our collaboration but did not end it. **Personal life** After a year in Chicago, I met my future wife, Elee (Elizabeth B. Cammack). We quickly spent lots of time together, although we were both working very hard. We were married less than two years later. Elee is also an economist in empirical finance. She is brilliant, charming, fun and much more organized than I am. We have two wonderful children. Our daughter, Rebecca, was born in Chicago in 1984. Our son, William, was born in Chicago in 1989. We are very proud of them, and both are economists. Our family does not have a well-diversified portfolio of human capital, but we do enjoy sharing economics jokes at the dinner table. **Working with Phil Dybvig** In 1981, Phil Dybvig and I began a project on game theoretic models of finance. We decided to try to understand bank runs as multiple [Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/) equilibria. Bank runs and financial crises are an important part of economic history, and I had read the details of the US runs when I studied Friedman and Schwartz. We made some progress while we were at the Western Finance Association meetings in Grand Teton National Park. We had similar training but different modeling styles, so we debated the structure of the model before we entered into model solving. We originally had the idea that we might explain in the model why bank loans are illiquid. Our idea was that banks had private information about the ex-post prospects of loans made previously. They might have incentives to roll over bad loans and declare their payments to be current, rather than to foreclose and reveal bad news to depositors. Selling a loan that might be bad would then have an asymmetric information lemons problem. After thought and discussion, we agreed that the reason that loans were illiquid was less important than the illiquidity itself. In addition, if we just assumed that the loans were illiquid, we could set up a model where loans held to maturity were free of risk, with a clean logical separation between insolvent and illiquid banks. We hoped that the ideas in the model could influence policy makers, so we worked hard on both simplicity and intuitive exposition.  It was a great pleasure to work with Phil. He understands things quickly, has a great intuition, and an amazing ability to use the right type and amount of mathematics. The paper writing process went quickly, and we sent it to the *Journal of Political Economy*. We got a very useful and very positive referee report, which recommended acceptance with minor changes. The editor, Michael Mussa, wrote us a long letter suggesting quite a few changes. We made the changes, and the paper was accepted. This is the fastest editorial process I have yet experienced at an economics journal.  Phil Dybvig and I wrote only one other paper together, in 1986. We applied the ideas in from our model of bank runs and my model of delegated monitoring to practical issues in the regulation of financial intermediaries, both bank and non-bank. We remain friends and still bounce new ideas off each other. **A Year at Yale SOM** In 1987, Elee and I joined the faculty of the Yale School of Management. Unfortunately for all, the president of the university had removed the dean and was reevaluating the future of the management school after we decided to join and just before we arrived. Its future looked precarious and its present was unpleasant. After several months, I called Bob Hamada, who was deputy dean at Chicago GSB, and asked if I could still return to Chicago. He said that he had just finished processing my resignation but would talk to the senior faculty in finance and get back to me. He soon told me that I could come back, and we returned to Chicago in 1988.  The year at Yale was difficult, but I did enjoy having great colleagues in the management school, including Steve Ross, Phil Dybvig, [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/) and Jon Ingersoll. After that year at Yale, I never again complained about anything at the University of Chicago. Thankfully, Yale SOM subsequently recovered from that period. **A Career at The University of Chicago** After Ro Verrecchia left Chicago, we wrote two more papers together (our most cited), but for several years, I mainly wrote papers by myself. I relied on my colleagues in the profession and at Chicago for feedback on my work. My wife Elee helped me through this period by giving feedback after listening to me talk about projects, and she greatly helped me with the mathematical proofs in my 1989 paper on reputation acquisition.  In the 1980s and 1990s, Chicago hired many great young faculty members in finance, and many were working on finance theory. I learned a lot from them, especially Gur Hubermann (also a student of Steve Ross), Charlie Jacklin, Rob Visnhy, Andrei Shleifer, Ken French, Anil Kashyap (a co-author), Kent Daniel, Raghu Rajan (a co-author), John Cochrane, Luigi Zingales, Per Strömberg and Ulf Axelson. We did not frequently hire outside tenured faculty, but I was lucky that we hired the great Milt Harris in 1987. Chicago Booth (then GSB) is and was an exciting place to do research. We take each other’s research seriously, and all attempt to help improve what we all do. Some find our workshop comments intense, but in the end they improve the quality of papers.  In 1991, Chicago hired Raghu Rajan, who worked on corporate finance and banking. In addition to being a great researcher, Raghu has an unusually broad perspective on the world and its problems. He has been a great colleague and friend from the first day he arrived. Raghu got tenure in four years, and a bit after that, we began our joint work. We continue to work together.  Chicago hired Zhiguo He in 2008, and he has been a very close and insightful colleague and friend since then. We have written a paper together and continue to talk regularly about research.  I have learned from and greatly enjoyed advising many great PhD students at Chicago, 62 at last count. I pass along the philosophy that I learned from Steve Ross: I listen to what each is trying to do and give each my take on how to explain or improve it. My students work on many diverse fields of finance, and most of their research is reasonably far from mine. I am proud of them all. Rather than thanking them individually, I thank them all and especially those who organized a great conference for my students around my 65th birthday. For this, I thank Effi Benmelech, Philip Bond, Andrea Eisfeldt and David Musto. I am also very grateful to Yunzhi Hu for putting together a set of videos and written messages that he presented to me in Stockholm the day before I received the Economics Prize in honor of Alfred Nobel. **The Richmond Fed** I have been a regular visiting scholar at the Federal Reserve Bank of Richmond starting in 1990 and every year before 2020, when Covid ended my regular visits. I was invited after they hired Jeff Lacker and John Weinberg, who were working on issues close to those that I study. I talked to them frequently, and to Marvin Goodfriend (who I knew from Brown) and Mike Dotsey. In later years, Ned Prescott and Huberto Ennis were my closest colleagues there. Those discussions and those over lunch with members of the research department and the bank presidents kept me in touch with macroeconomics and central banking. **Public Policy** My research deals with issues that have implications for public policy regarding the financial system. Usually, I present my ideas via scholarly papers, communicating to scholars, policy makers and a few practitioners. During the 2007–2009 period, I spent time working directly on policy proposals and entering into discussions with policy makers. Ken French recruited me to join a group of economists to propose some changes to financial regulation, and the result was the *Squam Lake Report*. I became an informal advisor to the US Treasury and made policy presentations to the Federal Reserve Board. Together with Chicago Booth colleagues, I wrote op-ed pieces and blog posts for the public. I was pleased to be able to participate in the crisis response, but I was happy when the crisis faded and I could get back to doing research with a longer time horizon. **After the Crisis** Life settled down a bit by 2010. I returned to my research program and worked on two papers with Raghu Rajan, applying our previous models to issues in financial stability. We used some insights from the crisis to frame the questions. In 2012, Raghu went to India, first to advise the finance ministry and then to become the Governor of the Reserve Bank of India until 2016. We continued to work on the research projects that we started, which are not exclusively about financial intermediation, but at a slower pace. A few years later, we began to work together with Yunzhi Hu of the University of North Carolina, and we continue to do so. Yunzhi adds a new perspective to our work, both empirical and theoretical. Yunzhi is a former PhD student of mine and the only student that I have ever had as a co-author. He, too, is a friend from whom I continue to learn.  I have not been someone who worked from home (at least not before the Covid pandemic). Chicago Booth is a place where most of us are in the office almost five days a week. My colleagues have always been helpful to my research, but my set of co-authors has always been small. In the period after Raghu went to India, I greatly enjoyed working on a paper with Zhiguo He on debt overhang, showing that short-term debt leads to more debt overhang than long-term debt, in some circumstances. I collaborated with Anil Kashyap, producing a paper on the optimal regulation of liquidity holdings by banks. This is my first paper, other than those with Phil Dybvig, to use a version of the Diamond-Dybvig model. Anil and I generalized it to partial runs, which has surprising implications for liquidity regulation.  I visited the MIT Sloan School in the 2015–16 academic year. Apart from my year at Yale SOM, this was my only year away from Chicago since I joined in 1979. I enjoyed visiting their department of finance. I would have enjoyed it more, but my mother’s health deteriorated, and I flew back to Chicago many times to help her. I got to know all of the Sloan finance junior faculty, who joined me for lunch every day, and I reconnected with the senior faculty. Steve Ross was on leave, regrettably. I also spent many fascinating hours talking to Bengt Holmström from MIT’s department of economics. A great bonus of the visit to Cambridge was the ability for me and Elee to spend some very pleasant time with our son Will, who was working on his PhD in economics at Harvard.  After our return to Chicago, scholarly life was as usual, but personal life became very busy. My mom’s health continued to deteriorate, and she passed away in 2017. On a happier note, our first granddaughter (Rebecca’s daughter) was born that year, and we spent very happy times visiting her. Rebecca’s son was born in 2020, just before the extent of the pandemic became clear. In 2021, my father’s health deteriorated, and he passed away in the middle of 2022.  Life began to return to normal. Raghu, Yunzhi and I continued working on a project that is quite different from our previous research. On a flight back from a conference, I made some progress on it, and we had plans to discuss the project that week. Very early the next morning, October 10, 2022, I received a phone call from Stockholm. |
| Autobiographical |  |
| Podcast | **“I think economics is getting closer and closer to being a respectable science”** “I think economics is getting closer and closer to being a respectable science. Even when we were not the most respectable science, we still needed to keep pushing forward because the topic actually matters to the planet, to the humans on the planet, and to the animals on the planet.” Douglas Diamond is a strong advocate for economics as a scientific field. His passion for economics was sparked at a young age when he accidentally took an undergraduate course in the topic.  In this conversation, conducted in February 2023, Diamond reflects on the working environment at University of Chicago – a work place that has become his home after working there 30 years – and how he sees more and more women enter the field of economics, something he thinks is a very positive development. He also tells us about the “No” bell that he received from [Richard Thaler](https://www.nobelprize.org/prizes/economic-sciences/2017/thaler/facts/) – a tool to helps him say no as a newly awarded laureate.  The host of this podcast is nobelprize.org’s Adam Smith, joined by Clare Brilliant. This podcast was released on 4 May, 2023.  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.  Douglas Diamond: “Because our business model is hiring young people and turning them into famous scholars, or hope they become famous scholars as they get older, it’s a good place to do research. But I think I got unusually lucky. I got that environment when it was tiny. The field was tiny, and most of the questions were unanswered. Or if we had answers, they were completely incorrect answers.”  Adam Smith: The environment Douglas Diamond is describing it the University of Chicago, his home for over 30 years, where he tries to find theoretical explanations for real world phenomena. His passion for economics, as you’ll hear, developed early – sparked by an undergraduate course he took almost by chance. And that passion is still abundantly evident.  So welcome to this conversation in which Douglas Diamond talks about the young science of financial economics. And yes, in his mind, it is definitely a science. I hope you enjoy it.  Clare Brilliant: This is Nobel Prize Conversations. Our guest is Douglas Diamond, who received the 2022 economic sciences prize for developing theoretical models about the role of banks in financial crises – models that form the foundation of modern bank regulation. He shared the prize with Philip Dybvig and Ben Bernanke.  Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces.  Douglas Diamond is the Merton H Miller Distinguished Service Professor of Finance at Chicago’s Booth School of Business. He talks to Adam about the joys of working among researchoholics and learning to say no with the help of a rather rude electronic assistant.  *CLIP: No-bell*  But first, he looks back on a memorable encounter during Nobel Week 2022.  Smith: Let me start by asking, what was your best moment in Stockholm?  Diamond: My favourite one was on the second to last morning when I spoke to, it was called the high school. I guess it’s like a junior college, the people who are like one year past our high school in the US. Speaking to them, they asked interesting questions. They seemed truly excited by the topics. It was Ben and myself speaking there. That was my favourite thing. I was relaxed. They were very appreciative. They were a bunch of interesting kids. That was my favourite.  Smith: That’s lovely.  Diamond: That was, I think, the busiest week of my life. I don’t remember the week when I was born, but it’s certainly up there of those two. Several of my friends and colleagues who’d won in the past warned me about pacing and making sure you used every free minute to put one’s feet up and relax.  Smith: That must be one of the blessings of being at the University of Chicago, that you’re surrounded by people who’ve been through this before so they can give you tips.  Diamond: Yes, I have three colleagues currently who are non-emeritus on the faculty who won in the last decade. They gave me a lot of advice and warned me of potential pitfalls in this process.  Smith: Maybe you can’t reveal all the advice, but is there one piece that springs to mind?  Diamond: Don’t overbook yourself. Both of them told me, just don’t put anything you don’t have to in your schedule for the next six months and wait until you get to Stockholm and you’ve returned from Stockholm, until you even think about it. I’ve still followed that advice. I’m still putting the finishing touches on the paper version of my prize lecture and my autobiography. When people ask me to do something, say get in touch with me in the month of March and I’ll think about it.  Smith: Is it an enjoyable process writing your autobiography?  Diamond: It is. I hadn’t thought through all of these things. There are certain things I hadn’t tied together in my life until I sat down to put them in some kind of an order that made sense. I enjoyed that. I would have rather done it on a more leisurely basis. My mean time for writing a paper of any sort, start to finish, is around 15 to 18 months. I wrote two of them in a month and a half now. It’s a little off my normal pace.  Smith: That’s a very interesting point about paper writing, because one would have thought that there was always the worry about being scooped and getting the information out there as fast as possible. To be so comparatively leisurely about it.  Diamond: I put a strong premium on getting things more or less right or as right as I can get them. Being the first person to get it 80 per cent right is not as good as being the second person to get it at least 80 per cent and getting to per cent right. For empirical work, if you just present the data, if you don’t analyse it quite right, people can still learn something. If you present a theory that actually doesn’t follow from the assumptions you say it does, you’re actually destroying knowledge rather than creating it.  Smith: Yes, that’s a great responsibility. I did hear that Richard Taylor gave you a special bell.  Diamond: Yes, it’s called the “No” Bell. It’s still in my office. Whenever I think about saying yes to something, I take a look at it. I don’t have to push it as often as I did in the first few days. It basically says ”no” more than 100 different ways. ”No, no, no, no”, and things like that. ”What are you thinking about”. ”Forget about it”. It’s a little toy that he passed along.  Smith: I think we’d dearly like to hear it.  Diamond: Let me just look. Oh, I see it. Hold on.  Smith: It says ”no” with a big exclamation mark.  Diamond Yes. Then you push it. ”No.” You can push it again. ”N-O.” Lots of different ways of saying no.  Smith: Was that Richard Taylor’s voice? Has he recorded it for you?  Diamond: No, no. This is something that someone gave to him. I think Danny Kahneman maybe gave it to him.  Smith: Fantastic.  Diamond: He said he’s in California in the winter right now. I tried to return. He says, no, no. You’ll still need it a little more. Hold on to it.  Smith: At least until the next round of announcement.  Diamond: Yes.  Smith: Yes. The art of declining firmly but politely.  Diamond: Yes.  Smith: The University of Chicago has 33 laureates who are associated with the university in economics, out of just 92 people who’ve been awarded. That’s about a third of awarded laureates in economic sciences have associations with the university. One has to ask, what is it about that place?  Diamond: The interesting thing is they’re sort of divided pretty equally between the Booth School of Business and the Department of Economics. Our cultures are a little different in the two areas. But the thing that both of us have is everybody takes the other faculty members’ research very seriously. There’s no notion that publishing papers is some kind of a game or accounting exercise. We’re all trying to do something that has an impact on the world in very different styles. We read the papers. We all go to workshops, our names for seminars, and listen carefully to the other people’s work and try to get it as clear and perfect as possible. Compared to other business schools, our senior faculty stay more or less as productive as our junior faculty. Often in many places, people slow down and get other interests besides doing scholarly research. We all are sort of researchaholics here and work very hard on it and get a lot of pleasure out of research when it goes well. I think that’s probably the most important thing that explains why so many people here have done research that had lasting impact. I don’t have a great model on how the prize committee for the economics prize chooses things, but it seems like lasting impact and other people either in the economics profession or in the real policy world using one’s work, that seems to be a common trend of the people who they’ve picked. That’s sort of very consistent with what we all try to do here. I’ve been here, this is my first job out of graduate school. I’ve been here since 1979. Of the people who got Nobel Prize since I was in economics, the only one who I haven’t had as a colleague was Milton Friedman. He just left when I came. When I started as an assistant professor in the business school, the people in the business school were very nice to me and gave me lots of help. But the economics department, particularly Bob Lucas, who won the economics prize, and Jose Shankman, who is a potential future winner, were very helpful to me. It was a broad community of economists.  Smith: It’s such a special thing to create an environment where people feel secure and collaborative and are really taking each other seriously. Is it a conscious effort to do that, or is it just self-sustaining? Once you have it, all you have to do is not lose it. It just magically renews itself.  Diamond: It does magically renew itself, as long as you realise how unusual and special it is. Within the Graduate School of Business, which was the name of this place before David Booth gave us some money and we renamed it, the finance group was basically run by Merton Miller and Gene Fama. When I came, there were three other senior faculty besides them and two junior faculty. It was a very small group when I joined. Both Fama and Miller were always in the office, did almost no outside consulting, just basically tried to train students, teach MBAs, and do their scholarly research. That was a very good culture, which I sort of picked up and valued a lot. I’ve been here a long time. I try to make sure that that culture persists. The other part of our culture is we occasionally, every decade or so, we’ll make an outside offer to someone who already has tenure elsewhere. Most of our faculty here were hired when they were assistant professors, either right out of graduate school or a year or two later. The treatment effect of our culture is pretty big on most of us. It’s a fun place to work.  Smith: It sounds it, and a productive place, too.  Brilliant: Douglas Diamond was born in 1953 and was raised in Chicago. He describes himself as an over-confident high school student, who took a course on capitalism and enjoyed it, but dismissed economics as a career and decided to study biology at Brown university.  Diamond: Economics seemed like it was pretty interesting, but seemed sort of easy. I thought maybe you could do more important things in the human genome and that kind of stuff. I got to college and took a molecular biology course and I didn’t like the course. Two weeks into the course, I realised this was not a great course. I hadn’t changed majors yet, but I decided to drop that course and then look for a course that met at more or less the same hour. I found an intermediate microeconomics course. I knew some economics. Since I’d had an introductory, I could take intermediate. I took that course. It was a truly amazing course from a Professor Brown, who had no relation to the University Brown, but Professor John Brown. It wasn’t mathematical, but it was super high level. There’s this part of, a very advanced part of economics that’s called Arrow-Debreu theory. It’s general equilibrium, where you think about uncertainty as a type of composite commodity of a lot of different goods that pay off in different states of the world. It’s one of the most advanced things. This was in this freshman course. I said, ”this economic stuff, it’s still pretty easy for me, but it seems a little less trivial than just like supply and demand”, which sort of seemed a bit obvious after you’ve been around the market a little bit. I took that course. The second half of the course was a more mathematical course that used a lot of calculus and things and did some interesting applications of economics, like where different companies would locate in different parts of the world, location theory that was sort of linear programming almost. I said, OK, this economic stuff has more depth than I would have guessed. It seemed comparatively easy to me. I was comparatively good at it. Once I thought about it as me being comparatively good rather than it being comparatively simple, I decided it was a good major. Sometime in my sophomore year, I decided to switch the major.  Smith: What difference a good course makes and a good teacher.  Diamond: Yes, that was an amazing course.  Smith: This brings me to a clip I’d like to play you of Ben Bernanke speaking at the banquet for the Nobel Prize Award.  *CLIP with Ben Bernanke speaking:* A perennial question is whether economics is really a science. It’s true, for example, that we economists can’t do large-scale experiments, although neither can evolutionary biologists or seismologists. However, one thing we surely have in common with physics, chemistry, and the rest is that ignorance or misapplication of basic principles can result in enormous damage. In economics, that damage takes the form of financial crises and economic depressions.  Smith: Given what you just said about your courses at Brown, I guess there’s no question for you, economics is definitely a science.  Diamond: It is, and particularly in the dimension that the quote from Ben just referred to, that one reason economics is important as a science is because humans actually use data from the economy to make policy, government policy, business policy, personal decision policy. One needs some kind of a model to figure out what data should tell you about the world. One type of economics, economic theory, especially applied economic theory, which is the kind that I do, is to give people of all sorts, including government policy makers, a little paradigm when they see data to think about what might be happening. Sometimes they need multiple things that could be going on, so they can look at the comparative merits of each explanation in a given bit of data. Since people make decisions, we need some theories and models. They need to be, for things like policymakers in monetary policy or financial stabilization, the kind of stuff that I focus on. They need to be something you can sort of integrate quickly into your head without running something through the computer. If you’re going to see a crisis like we saw in 2008, you need to say, what is this telling me? That’s important. The other thing that they talked about, how like seismologists, we can’t just have a bunch of earthquakes to see what would happen. We don’t really want to have a big depression to find out how you’d fight the next one. The other thing that economics needs to do and has been very successful in the last 30 years is figuring out, how do you use data to figure out what’s causing something else? Just think of a causal inference, the people who won one year ago. All three of them had a huge impact on coming up with methods for economists and non-economists to look at data about the world and see, can we really say that this other feature that happened in the world is the cause of what happened? Or can we just say, no they just happened together for some kind of common reason. That’s, I think, why economics is potentially a science, is that we need to have a way to get views of the world that are distilled enough so that the human brain can use them in real time. I think economics is getting closer and closer to being a respectable science. Even when we were not the most respectable science, we still needed to keep pushing forward because the topic actually matters to the planet, to the humans on the planet, and to the animals on the planet.  Brilliant: One of the things that turned Douglas Diamond onto economics was the book *The monetary history of the United States* by Milton Friedman and Anna J Schwartz. Written in 1963, it argued that sound monetary policy is necessary for economic stability. The authors pointed to the consequences of the American Federal Reserve’s actions – and inaction – during the Great Depression as an example of why it’s important to have effective monetary policies in place.  Diamond: The wonderful thing about that book is it was written in the style of economic history, where they described what actually happened. They showed the actual data. They had transcripts of the meetings of members of the Federal Reserve when they were thinking about monetary policy. They described what actually happened. They showed the data, and then they gave their interpretation. No statistics in that book, no statistical analysis, just the data, and then their words, and their descriptions of what happened. It was very transparent. You could form your own view of what it meant, and you could see how their argument really seemed to fit the data. The beautiful thing is because they showed you the unanalysed data, you could put your thumb on the scale wherever you wished. That’s so rare. There’s not too much policy analysis that’s that descriptive of what happened and what the people were talking about when they made the decisions, what data they saw. Just a lot about human decision making and how a narrative that could be completely incorrect but sort of looked right could make people make bad decisions, how they could use it for persuading people who shouldn’t be persuaded because the persuader was incorrect. It had the impression on me as a kid in college that policymaking was important, understanding these models, like what was causing what in the world was important, and that people disagreed to a huge, surprising extent about what was causal and what wasn’t, even in the 1970s when I was reading the book.  Smith: When you were a graduate student at Yale, you and Phil Dybvig were both students of Stephen A. Ross.  Diamond: Yes.  Smith: You told me in the telephone interview how you met in his waiting room, because he had this strange policy of not making appointments with his students. You had to just sit outside his office waiting for him to do you the favour of opening the door and let you in.  Diamond: Yes. He was, in addition to being an amazing scholar and advisor, he also ran some business and consulting things. Some people would just leave town and go on the road and not be around for the students. Steve would try to do his business work in his office. He would always be around in case someone needed him in an emergency basis, or he was willing to talk. Steve would be in the office pretty much every day. Some days he wouldn’t have time to talk. Other days he’d have two or three minutes. Occasionally, if I or Phil or other students would have something interesting and urgent, you might talk to Steve for an hour. Steve would rarely read anything. You’d just have to go talk to him and write it on his blackboard and have him think about it in real time. Thankfully, he was the quickest person in the economics profession, or certainly in the top two or three. I think I still might be in graduate school if it wasn’t for Steve, because I was sort of barking up the wrong tree in the method. I was trying to make things so general that they were unable to be solved by someone like me in the model. The math model was just too hard. Even if I’d solved it, it was too complicated for people to understand. He convinced me to simplify things until I could understand exactly what was going on. The math was like totally transparent.  Smith: What did he see in you that made him take you on?  Diamond: I’m not exactly sure. I had an agenda. I’d had some incomplete tries at trying to get models that had the ideas that I had. I think that was unusual to already have an agenda as a third year PhD student. My longest suit is thinking quickly and talking about it. I’m better in talking than I am in writing. He was good at both, but he was also quick and a talker. We could talk about ideas of what I was trying to do and what I thought the economic model might be before I wrote any equations down. He was quick enough that we could both picture the equations that would be there without actually writing them down. He was good at listening to what I wanted to try to do and then linking it to what was already out there. Actually, in the period after, I was the teaching assistant for the PhD course in macroeconomics that James Tobin taught. I managed to convince the top three or four students from that class to go become students of Steve Ross. They all did, and they’ve all become fairly famous economists and thanked me many times.  Smith: I want to ask a little bit about the relationship with Phil Dybvig. Together in 1983, 40 years ago, you published this seminal paper on bank runs, which has been so influential. Something about your partnership leading to the Diamond-Dybvig model was very special. Let’s hear Phil Dybvig talking about you for a second.  *CLIP with Phil Dybvig:* Doug is an amazing guy. He’s a great co-author. We worked so hard to make the paper simple. But during the time we were writing it, it could be somewhat intense. It was never unpleasant. But one of us would say, well, we should assume this. The other one would say, no, that’ll be too complicated. We can never solve that. The other one would say, well, how about if we try that? And then I’d say, no, no, that’s going to throw away all the economics, and back and forth. I’m hoping that as a result for economists that they’ll find that to be a simple paper.  Smith: The paper did indeed get a very good reception. But what made it so productive to be working together?  Diamond: We had similar training. We’re both students of Steve’s. We had very different approaches to doing research. Phil’s main research was in the prizing of financial assets and how money management would work. Mine was thinking about banks and private information and markets. We came with a different background but similar training and different skills. Like Steve, Phil is also very quick and very verbal. We talked about pretty much the whole model before we wrote any equations down. That’s not the way most people work. Most people write some equations down, stare at them for a while, try to figure out what it means. I’ve done that many times. But this issue of thinking, so we decided we’re going to write a paper about how the finance can be viewed differently when you do it via the lens of game theory than just competitive supply and demand economics. That’s how we started the project. We quickly decided that thinking about bank runs was the place to begin.  Brilliant: Adam, what is a bank run?  Smith: A bank run is when lots of people who have money deposited in a bank decide they want their money back at the same time and the bank doesn’t actually have the money available to give them. Happily enough most people only encounter bank runs in fiction. For instance in the film that tends to get shown at Christmas, *It’s a Wonderful Life*.  *CLIP from It’s a wonderful life.*  Smith: Suddenly if the bank can’t meet its obligations it’s in crisis. One of the implications of the work of Douglas Diamond and Phil Dybvig in their model is that it’s very important to prevent bank runs happening because if they happen it creates a very damaging financial instability across the whole financial system.  Brilliant: Does the Diamond-Dibvig model make any recommendations?  Smith: I suppose the answer to that is no, the model doesn’t make implications. What it does is helps you understand what is happening. I suppose that’s the fundamental point of their work. It allows policy makers to get a better grasp of what’s going on in the banking system. Therefore, be in a better position to avoid having bank runs happen in the future. That is the idea at least. It’s been put into practice because the Diamond-Dybvig model has been central to the thinking of central bankers who in the main think that it’s very important to avoid bank runs. The counter argument is that it’s better to let banks fail rather than prop them up.  Brilliant: There’s still quite a lot of debate about this right?  Smith: Most central bankers seem to think that the path is clear and that it’s very important to keep banks afloat when you can. But certainly there are people who disagree. Let’s listen to what Douglas Diamond has to say about his critics.  Diamond: Some people who don’t like the government ever doing anything said this was, we are the devil because we convinced them to not let all the banks in the world fail, which would have been good for the economy. That’s a slightly mean characterization of what other people think. There are people who said the Federal Reserve and Ben Bernanke did the wrong thing. They shouldn’t have intervened into AIG. They should have just let them fail. Maybe if Goldman Sachs had gone down too, that would have been a good thing. I don’t agree with any of that. Just like we don’t want to have earthquakes to find out how earthquakes work, we don’t want to have financial crises that are bigger than they need to be to find out whether we really need to stop them, nip them in the bud.  Smith: Let me ask you about the age you were when you did this work published 40 years ago. You mentioned already that at the University of Chicago, both young and old contribute, that you don’t stop producing work as you get older. Is there something very special about being a young person in this case, the field of finance, that allows you to do things differently, think of new approaches in a special way? Or is it really just that the time can be right any time? You could be young or old.  Diamond: I think the two things that were special about that period – financial economics was a very young field. The oldest important paper at that point was Modigliani and Miller, which was 1958. Most of the stuff on pricing of financial assets was the mid-1960s. It was a young field. I think people had realised that we needed to expand. There were lots of questions that we knew we didn’t know the answer to. The University of Chicago was right in the center of the finance revolution. My senior colleagues, particularly Gene Fama and Mert Miller, were very open to thinking about new, more nuanced ways of doing this. The fact the group was so small meant that I got lots of attention from the senior faculty. Because I was the only assistant professor. There were two associate professors, John Ingersoll and George Constantinides. I had a lot of attention. We’re a much bigger profession and a much bigger group today. I think the University of Chicago, because we’re very open, we don’t hire senior faculty, except very rarely. Because our business model is hiring young people and turn them into famous scholars, or hope they become famous scholars as they get older, it’s a good place to do research. But I think I got unusually lucky. I got that environment when it was tiny. The field was tiny, and most of the questions were unanswered. Or if we had answers, they were completely incorrect answers. Like Fisher Black wrote a paper on banking and interest rates in a world without money. He basically said, well, now that we have efficient financial markets, the traditional things that banks do are no longer needed. Because we can learn all of the information by looking at the stock price etc. We don’t need to do financial accounting anymore. We don’t need to monitor. That was a paper he wrote in 75. I read that paper, and then I read Friedman and Schwartz, and I said, no, no, that’s the wrong way to think about it. Banks are still important, even though financial markets are remarkably efficient. Most of the ideas we have around in finance right now are either pretty correct, or the other ones, we all realise what’s wrong with them and why we can’t do a little better. We don’t have the tools or the ideas to fix the things that are just not correct in our profession.  Smith: The advice to the young is seek out new fields if you can.  Diamond: Students ask me, what should I work on? Should I look in the literature and see what’s the next advance we need to move on? I say, no, look for the stuff we don’t understand at all. You might be better off, instead of reading the Journal of Finance, you might be better reading the Financial Times or the Wall Street Journal. Stuff in the world that we don’t understand. Then ask, in the paradigm of economics, what’s our explanation?  Smith: Yes.  Diamond: What’s wrong with this picture? What’s wrong with this explanation? I think that’s a much more promising area to get a great idea. Now, the trouble is you often, particularly in a mature field, if we don’t know the answer, it’s probably because the right answer is quite difficult, rather than because we’re not looking in the right place. That’s one reason it was easier for me, because we didn’t even know how to ask the question of, how do you optimally design a financial system? Because that requires a stuff called mechanism design that won a bunch of economics prizes. That was a new tool that no one had ever put onto thinking about finance before. Both my dissertation paper, *Financial Intermediation Delegated Monitoring*, and the paper with Phil, *Bank Runs Deposit Insurance Liquidity*, both of those are mechanism design papers at some level. I was the first person to try that approach on this question. Without intending to, it produced the two main contracts that people use in finance. If I tried that 10 years later, somebody else would have done it first. It wasn’t obvious, but the answer turned out to be sort of obvious once I figured out what it was.  Smith: What do you do for relaxation?  Diamond: Spend time with my family, my wife in particular, because my kids are grown and live on the opposite coasts of the United States. Our main thing, which we’ve still managed to do a bit of in this post-October period where my time became a bit scarce, we go for nice walks along the lakefront in the city of Chicago where we live. We like to go on hiking trips and vacations. We actually took one week of vacation, which was already been planned in January. Went to Mexico for a week and enjoyed things down there. Didn’t do any work on my speech in that period, which means I’m still finishing it today. That’s what we like to do. I like to go see my children, my grandchildren.  Smith: Music, I guess, is also important, because I heard on the grapevine that you were a DJ in college.  Diamond: Yes. WBRU in Providence. That was the Brown University station, which was the commercial station and the number one radio station from 18 to 34 ratings at the time. Radio is less of a thing for music and young people these days. They’re now an internet-only station. But I spent a lot of my time in college listening to music, playing music on the radio, helping to take care of the station and its music. I still listen to a lot of music. In fact, I listen to some music from the 1960s and 70s that I used to listen to. I’d spend more time listening to classical music, particularly it’s a bit more relaxing than rock and roll or German space rock or things like that.  *CLIP with Freak’n’roll by Ash Ra Tempel*  Smith: You mentioned your children living far away, but your daughter is an economics professor.  Diamond: She’s an economics professor at the Stanford Business School. She’s an amazing economist. She won the Elaine Bennett Prize for the best female economics under a certain age just this last year. She won a week after your people called and told me that I won the economics prize. We had a good week that week in our family.  Smith: What a Diamond family week.  Diamond: My son, who is a bit younger, he’s an assistant professor of finance at the Wharton School of the University of Pennsylvania. He’s doing incredibly well as well. My wife, Elizabeth Lee, is also an economist. There’s some memo on diversification of human capital that we never received. But it’s worked out OK for us, nonetheless.  Smith: On the question of women in economics, the track record of the economics prize, the economics sciences prize to women has been very poor, just two female laureates out of all that have been awarded. What do you have to say on the subject of women in economics?  Diamond: There historically have been very few women in economics. Given it’s a social science, it’s a bit surprising. Among the social sciences, it’s sort of the least social. It’s more about money and business to a certain extent. But then, my daughter is a labour and urban economist. There are plenty of things that are very important to the well-being of humankind that are studied in economics, particularly after my late colleague Gary Becker sort of extended the breadth of what economics does. It’s a very competitive, tough profession. We’re a little too tough in seminars and things. There are plenty of women out there who are overqualified to be economists. They just don’t tend to go into our field very much because they either don’t understand the interesting things that we do, or they understand the obnoxious things that we do as colleagues. If we can get rid of the obnoxious things that we do as colleagues about being too aggressive in seminars and not understanding that men and women may have different weights on how much they put into child care when their children are young, and that doesn’t affect their long-run productivity, which I think was a major problem in the profession, which is not completely gone, but it’s much better. I think those are the issues, I see just in our applicant pool, the PhD program, there are many more women, particularly super well-qualified women, than there were 20 years ago. I think it’s getting better. But I think even today, economics is toward the bottom of all technical professions in the fraction of women in our profession.  Smith: You obviously love your field so deeply. You’re a good advocate for it already. I suppose the platform of the prize in economic sciences gives you even more visibility to go out there and pull people in from putting women, putting people from diverse backgrounds.  Diamond: No, I completely agree with that. I’m thinking I’d like to get more people who are not considering science in general. In economics, I’m particularly a fan of getting people who think about things in a different and broader way. It’s particularly important in a policy-oriented science like economics is. The fact that there are very few African-Americans in this profession, and there’s all kinds of policy that affects everybody, including African-Americans, means that there’s a whole set of policy biases that we don’t see. We have our biases, but we don’t see them in the profession. Other people have their biases. If we could hear the views of those people, we could all understand everybody’s implicit assumptions and how they’re thinking about the world in a much better way. Getting more really good people to go in the profession who are different from the ones today is really important in economics.  Smith: The way you describe the collaborative working environment at the University of Chicago, who would not want to be part of that? It just sounds gorgeous.  Diamond: It’s a fun place to work. One of the things that I try to do to keep our culture alive, is make sure that our business model continues to be hire young people and turn them into successful older people. Some economics departments hire young people for low cost teaching of undergraduates, don’t mentor them much, and then very few of them get tenure and they often drop to not as good schools as they could have got jobs from when they came out of graduate school. When you’re into that setup, then you have to hire senior people and you hire them after they’ve done their work. Many of them retire at that point. We don’t want to get into that. We’re far from that.  Smith: It’s been an enormous pleasure speaking to you. Thank you.  Diamond: My pleasure.  Brilliant: You just heard Nobel Prize Conversations. If you’d like to learn more about Douglas Diamond, 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 Karin Svensson. The editorial team also includes Andrew Hart, Olivia Lundqvist, and me, Clare Brilliant.  If you’re in the mood for more listening from laureates with a connection to the University of Chicago, check out our earlier episodes with Richard Thaler or Paul Romer… Or Roger Myerson or David Card or Andrea Ghez or Frank Wilczek! You can find previous seasons and conversations on Acast, or wherever you listen to podcasts.  Thanks for listening. |
| Telephone  interview | 0805=DD  Adam Smith: Hello, am I speaking with Douglas Diamond?  Douglas Diamond: Speaking.  AS: Oh, hello. My name is Adam Smith, calling from nobelprize.org, the website of the Nobel Prize.  DD: Thank you for calling.  AS: Congratulations on the award of the prize in economic sciences.  DD: Thanks very much.  AS: I believe so far you’re the only one of the three laureates that the committee has reached?  DD: Yes.  AS: How did the news reach you?  DD: So, I was sound asleep. And my cell phone was right next to me. And the phone rang and I was wondering what it was. And then I heard a Swedish voice on the line. I was hoping I wasn’t being pranked by one of my friends and it turned out I was not. And then a couple members, three members of the actual committee spoke to me on the phone and I realized this was indeed the real thing.  AS: You have been tipped many years in the past, have you prepared for this in your thoughts?  DD: I have for a couple of reasons. One, Washington University had a conference several years ago on the paper with Phil Dybvig, so we had to think a little bit about, you know, the broader context in which this stuff, which our work and my work fit and then, you know, just from teaching both masters and PhD students about it, I’ve thought many times about, you know, why did we do things and what were we thinking when we were building certain parts of the model, so that gave me a chance to think a little more broadly rather than about the exact research project I was working on that particular day.  AS: And that Diamond-Dybvig model has been so influential. What drove you to explore the fragility of banks?  DD: So, there were two things in the background. One just from my own background, before I started speaking with Phil Dybvig about it is I had been very influenced by an amazing book by Milton Friedman and Anna Schwartz called *A Monetary History of the United States* and it had a very interesting chapter about the 1929 to 1933 great depression and the role of bank runs in it. And it always struck me that the verbal description they gave there of how the process worked was only part of the problem. So I felt that was very interesting but an incomplete story. But I didn’t really have a good way to think about what was the better explanation. And then Phil Dybvig, who was a classmate of mine, we were both students of Stephen A. Ross, the late Steve Ross, so we thought we need to think about the idea of how we can use some parts of game theory to understand financial crises. And sort of the simple idea is that bank runs are sort of self-fulfilling prophesies, to use the term of Robert K. Merton, the sociologist. And then the point of our model was, why does the financial system, banks as a good example, write a contract where a bank run is a self-fulfilling prophecy and not having a bank run is also a self-fulfilling prophecy. That’s sort of the natural instability we’re trying to get at. The point that we came up with was that it was not that the banks were somehow responsible for falls in the price level and deflation. Not that the banks liabilities were a means of payment like money. But just that bank liabilities, bank deposits were short term and a source of liquidity in the portfolios of their holders. So if I’m a depositor I have a short term deposit, and I think hey I can take this out whenever I want. And then that’s very valuable to me. But if everybody takes it out then we’re in trouble. So that’s how we started thinking about self-fulfilling prophesies, that’s how, and then it’s not about banks, it could be many types of other financial institutions that are sometimes, in modern times called shadow banks. It’s about contract form, it’s not about money. Clearly related to money but we had no notion of money in the way we modeled it.  AS: Interesting. And are you broadly happy with the way that policy-makers have interpreted your work, especially in the way that they’ve insured deposits in banks?  DD: Yes, so we think deposit insurance, particularly very high levels of deposit insurance, not say ten thousand euros worth of deposit insurance. High levels of deposit insurance definitely makes the financial sector as a whole, not just banks, much more stable. You know, after the 1930s, the financial sector of, you know, the developed world was quite stable basically until the 2008 crisis. And then during the 2008 crisis the policy makers around the world who were you know informed by the economics literature were thinking about ‘fear of fear itself’, self-fulfilling prophesies was something that they had to be strongly cognizant of in any of the policies they came up with. One reason the paper with Phil Dybvig is so simple, it’s not simple basically to a laymen but to an economist it’s relatively simple and not particularly detailed, it’s because Phil Dybvig said to me we’ve got to write this paper a little more carefully and simply than the average paper because we’ve got to make sure that bank regulators who are not necessarily economic theorists can read it and understand the main point. And it seems to me like they did.  AS: Yes, it’s certainly been taken up the world over.  DD: I can also say that the world was incredibly lucky to have Ben Bernanke sitting in the Federal Reserve during the crisis. He’d been thinking about this just exactly as long as Phil Dybvig and I had. You know, our papers basically appeared in the same year. The world was very lucky to have someone who had thought very carefully about it and done the best empirical work on it in the center of the policy-making world.  AS: It must be very pleasing to you to have the three of you united in this prize.  DD: Without question. There are very few people in the world I’d rather be discussing these issues with and be sitting next to when discussing these issues.  AS: Well how nice that the three of you will be brought together in Stockholm. I need to finish because I know you will have the world at your door, but I did want to just ask what was so special about the working relationship between you and Phil Dybvig that allowed you to produce this work?  DD: I mentioned that we were both students of Steve Ross at Yale Economics and Steve was an amazing advisor and stupendous mentor to both of us. But Steve used to have this policy of not making appointments but you would have to sit outside his door, waiting to see when he had some time to talk. So Phil Dybyig and I sat outside his door, you know, waiting for time to talk to him, and while out there we talked to each other quite a bit. And while we were in graduate school we sort of came up with the idea we wanted to try to work together on something related to these issues. But we didn’t actually start working closely on it until we both had essentially finished our PhDs. And then we basically spent all of our thinking hours in the period we were actually formulating this model together and trying to understand it, and simplify it, we had a more complicated idea in the beginning and we kept simplifying it. And Phil is among the clearest thinkers of anyone I know in social science. So the fact that our model is quite clear and simple would not be possible without him as a co-author.  AS: That’s absolutely fascinating. I imagine people listening to this the world over now will adopt that approach of having students wait outside the door. And talking to each other, it was obviously tremendously successful.  DD: Yes, and he had an assistant who was very nice and gave us cookies and things like that while we sat out there too. So that was also very helpful. No one starved outside of Steve Ross’s door.  AS: Combination of cookies and patience. Worked wonders. Gosh, it’s been an enormous pleasure speaking with you.  DD: And you ask very insightful questions, thanks for the good questions.  AS: Well I look forward to speaking further. We’ll have a chance to do a longer interview when things quieten down a bit. But for now I’ll let you get on with the busy and exciting day ahead.  DD: Thank you very much.  AS: Thank you so much.  DD: Bye  AS: Bye |
| Interview |  |
| Q1 | Where does your passion for economics come from? |
|  |  |
| Q13 | What advice would you give to a student or young researcher? |
|  |  |
| Q34 | Do you ever feel pressure knowing so many people have read, cited, or built upon your work? |
|  |  |
| Q23 | What skills do you think are important to for researchers or students to develop? |
|  |  |
| Q22 | Why is it important for students and researchers to approach complicated subjects? |
|  |  |
| Q3 | Was there a person who influenced you? |
|  |  |
| Q11 | How do you cope with failure? |
|  |  |
| Q18 | What are the key implications of your research? |
|  |  |
| Q24 | What made you think that there was more to uncover with bank runs? Do you think it’s important to revisit or re-analyse subjects that people may have studied a lot already? |
|  |  |
| Q35 | Do you like to work on “popular” topics? How do you come across subjects that people aren’t thinking about? |
|  |  |
| Q26 | Can you tell us about the object that you are donating to the Nobel Prize Museum? |
|  |  |
| ID | 0806 |
| Biographical | This biography is intended to talk about how my early life contributed to my development as a scholar. This is necessarily speculative, since I have no evidence of what would have happened if my early life had been different. However, my impression is that my development as a scholar built on using my brain on various passions when I was young. I have always been curious about many things, and for me the breadth of interests provided a lot of ideas that could be combined in doing research. I read most of the mystery and science fiction books and many biographies in the local public library, and I read a lot of math and science outside the relatively small amount required for school. I did a lot of experiments with my chemistry set, which unlike chemistry sets today that have been regulated down to a few experiments with dyes, had a lot of interesting chemicals including a lot of chemicals I bought that were not in the original set. I also spent a lot of time solving puzzles. I was also interested in astronomy, and I participated in a regional astronomy club where I attended meetings and built my own telescope, grinding and polishing the mirror myself. In this essay, I will focus on two other interests that seemed to have a profound impact on my development, playing music and playing games.  My great grandparents on my father’s side came to the US in the late 1800s, and my grandfather and grandmother were born soon afterwards, in 1890 and 1897. The family settled in South Dakota, colder than Stord Island, Norway, where my father’s father’s father came from, although it is much further south. They thought the cold clear air was healthy, but actually many of them died of pneumonia, tuberculosis, smallpox, and polio. Under the Homestead Act, land could be obtained by new immigrants who were willing to help the country to expand westward by building and inhabiting houses on the empty lands. My grandfather went to law school in his 40s and was a successful patent lawyer in Dayton, Ohio. On my mom’s side, the family has been in the US for longer, and records do not seem so good. She said our ancestry on her side is Scottish, Irish, and some French, but she also told my father’s mother she is part Danish to curry favor (not knowing about the traditional rivalry between Norway and Denmark). She was never one to let cold facts get in the way of a good story, so I am not very certain about her account about my ancestry on her side. Her father was a professor at a theological seminar in Dayton and also worked a small farm.  I was born on May 22, 1955, in Gainesville, Florida. My father was finishing an undergraduate degree in engineering, and my mom, who had already finished her undergraduate degree in music education, was working as a telephone operator. When I was six months old, we moved to Alexandria, Virginia, near Washington, D.C., where my father worked in the Patent Oﬀice days while going to law school nights. When I was six years old, we moved to Kettering, Ohio, a suburb of Dayton, and my father joined my grandfather’s patent law firm with my uncle. I lived in Kettering through high school. Around the time I went to college, my father hoped I would join the family law firm. Patent law is interesting because you get to learn enough about new technologies to describe them and evaluate claims of priority of patents. However, I knew my father spent a lot of time writing in his job, and that ruled it out for me. I didn’t realize then that almost all professionals spend a lot of time writing, and indeed I spend a lot of time writing in my work. Fortunately, I have learned to enjoy writing a lot more than when I was in high school.  My introduction to music began at the age of 4, when my mom started giving me piano lessons. I started with a regular teacher, Mrs. Berges, at the age of six, and I studied with her through high school. She gave me a firm foundation in classical music. I also worked on my own at playing popular music. I taught myself to “play by ear,” which is the ability to play the musical notes of a song you hear (or hear virtually in your head). I also worked on improvisation, which is making up music on the fly. Occasionally, I started to get “perfect pitch,” the ability to identify notes that I hear without any reference to other notes, but I had an idiosyncratic theory that perfect pitch would interfere with my ability to transpose songs into different keys. To keep from getting perfect pitch, I would pick up an instrument in my house, put it out of tune, and play it for a while. I am very good at transposing, but this is not a test of my somewhat dubious theory, since I do not know how good my transposing would have been if I had perfect pitch.  By the time I was a senior in high school, I was playing piano five or six hours a day, at school with the orchestra and chorus and during study hall, and at home an additional two or three hours. Over time, my interest turned more from classical music towards popular music, and Mrs. Berges was annoyed (but patient) if I forgot a section in a classical piece and improvised a substitute. I played piano very little for about 10 years during my PhD studies and the subsequent start of my academic career. When I started up again, it was initially painful because my skill level was so much lower than before, but within about six months I was happy again with my playing. I have been working a little playing piano on and off since then. My biggest gig was performing with the Marsha Evans Coalition, billed as “jazz, blues, and Motown.” I was working with them for nine months in the 1990s, four nights a week, 9 PM until 1 AM. When the job ended, I was disappointed but also relieved because it was really too much work on top of my full-time job at the university. These days, I still try to get out to sit in with other musicians about once a week. Playing music keeps me grounded, and my initial training helped me a lot with discipline. Also, the patterns in music helped me to develop both intuition and analytical skills for mathematics. I was happy to have several opportunities to play music with other people during Nobel Week.  My exposure to playing games started when I was very young. I don’t think it was literally true that I knew how to play bridge before I could talk, but that is the right idea, and the first thing we would do at family reunions was to set up bridge tables. In the middle of the year when I was in the third grade, my family moved within Kettering to a new house about 2/3 mile (1 km) away from where my cousin Kip (Kevin F. Dybvig) lived. Kip was my best friend growing up, and he was one of my oﬀicial guests during Nobel Week. Kip and I played games nonstop. Kip got a PhD in biophysics at the University of Rochester and ended up doing research in microbiology at the top medical school at the University of Alabama, Birmingham, until his retirement a couple of years ago. He says that his success in research was due to playing games as a kid, and I suspect he is correct. In high school, he was more serious about chess than I was – for some reason chess didn’t capture my imagination when I was young – and I was more serious about bridge. When I was in high school, I thought seriously about becoming a bridge professional. I thought about the decision as “bridge versus the rest of my life.” I decided that focusing on bridge would be too narrow and I chose the rest of my life. I didn’t play many sports, but there were some athletic games I played. I played tennis a lot; I liked doubles because of the strategic aspects. I also shot baskets by myself after school for a half hour or forty-five minutes each day. My most serious sport was sailboat racing, and I did a lot of intercollegiate sailing in college. It combined nicely some athletics, physics and other science, and strategic thinking. For example, when sailing on a small lake, you think about how the wind direction and speed will be affected by hills around the lake, and how to take advantage of the differences in wind direction and speed, all the while requiring coordination, strength, and physical understanding (of the wind direction and laminar flow around the sail) to control the boat and make it move quickly. Like in other games, the rules affect strategic competition. In particular, the right-of-way rules in sailing are complex and have rich implications for decision-making.  Playing games gave me a good intuitive understanding of and faith in probabilities. I read a book on extra-sensory perception, which claimed it is possible to predict and influence the outcome of random draws. I sat down with a toy roulette wheel (playing a game against “the house”) and first tried to forecast the outcome of the wheel, and then tried to influence the outcome of the wheel. In either case, when I bet on my predicted outcome or the outcome I tried to induce, my pile of chips slowly dwindled down to zero again and again (due to the house advantage of having 0 and 00 on the wheel). This made it plausible to me when I heard that it is hard to beat the stock market consistently, a notion of eﬀicient markets pioneered by Cowles (1933). I also used ideas of strategic decision-making and conditional probabilities to think about my daily life. For example, sometimes my father came to pick me up after middle school, and he was invariably late. If I needed to use the restroom, I could go to use the restroom, but it risked my father’s fury if he arrived and I was not waiting outside. As a result, I would think about the conditional probability that he would arrive in the next two minutes given that he was already 45 minutes late. Like inference and decision-making in bridge and other games, this was good training for thinking about strategic issues in economic models.  An even earlier example of my strategic thinking regarded walking to and from elementary school, a walk of about a mile and a third (about two kilometers). The roads were not arranged in squares, and it was not obvious what would be the best route to school. I noticed that wandering along (optimizing locally!), I took a different route to school than going home. I thought about whether this was because the optimal route was different, or just because I put different emphasis on what I was experiencing rather than what was further on. I discovered that the answer was a combination of both. When I was walking along a busy road I would have to cross sooner or later, it made sense to cross when I reached the first intersection with light traﬀic. As a result, it made sense to cross the road earlier on average than the middle of the stretch, whichever direction I was going. This meant that the typical route was optimally different going to school than returning home. Thinking about this was not intended to serve any long-term goal; rather it was pretty automatic given my playing games all the time and was just to satisfy my curiosity (and prevent boredom) in the moment.  I was fortunate to grow up in Kettering, which had great public schools, especially for music, but there were also great teachers in other subjects. I remember some particular favorites: Mr. Detrick (orchestra), Mrs. Elicker (French), Mr. McKelvey (math and health), Mr. Neff (math), Mr. Tackett (chemistry), Mr. Tarzinski (english), and Mrs. Williams (fifth grade). I was also fortunate to grow up in a reasonably stable family, not without warts, in an upper-middle-class neighborhood, which gave me the resources and freedom to thrive. When I was in school, there was not so much homework, which left me a lot of time to pursue outside interests and develop creativity. Good classes are important, but often you can get a deeper understanding if you teach yourself.  I suspect it was important that I had intellectual activities that were fun. When I was in the fifth grade, I made some primitive weather instruments to measure wind speed, wind direction, and some other basic weather variables. I brought them to school, and the teacher said in front of the class that I would get extra credit for making them. Another student in the class said it wasn’t fair to get extra credit for something fun. I thought he missed the whole point that learning is supposed to be fun. It is the job of students to find what is fun in each subject, and to overcome whatever the teacher or textbook (or the student’s bad attitude) is doing to make the subject uninteresting. Fortunately, I was always interested in many things. Right before she died, my mom remarked to me that when I was young, I never once complained “Mom, I’m bored. I don’t know what to do.” I always had so many interesting things to learn about and not enough time.  In the eighth grade, I did let a teacher ruin my appreciation of history, something I still regret today. When I went to the first day of history class, my teacher, Mrs. Bleck, yelled at me “You have to get your hair out of your eyes!” I was a good student, well-behaved, and a little timid, and at first I did not imagine that she could be talking to me. She yelled again, “You! I’m talking to you!” I realized she was talking to me. My bangs were touching my eyebrows, perhaps a technical violation that of the dress code at that time. I brushed my bangs aside so they no longer touched my eyebrows, but she was not satisfied, and she bellowed “That’s not good enough. You are going to have to BUY a comb. I can tell you are a troublemaker. Sit here in the front where I can keep an eye on you.” I was devastated, and to this date I like knowing history, but I have trouble studying it, and I have trouble remembering names and dates. It’s a shame I was not tougher, and I broke my (subsequent) rule that I should not let a bad teacher spoil a subject for me. Perhaps I did not know how good I had it; at that age a “bad boy” was probably the hero of a lot of the girls in the class. The episode is also a good reminder that teachers should be nice to students, and that unkind words could have a long-term impact. On a related point, teachers often play favorites to students and are very nice to some students or nasty to others. However, this is not being a good teacher. It is fun to interact with a good all-around student who is very likeable. However, you are more likely to make a difference in the life of a student who is not so likeable and is performing below capacity. This is someone who needs a friend.  When I finished high school, I went to Indiana University for my undergraduate training. Indiana had the top-rated music school in the country and a beautiful campus. And it was close enough to return home comfortably for vacations but far enough away to deter my parents from dropping in unannounced. I planned to major in math and music, but when I arrived on campus, I found out the music school was “too good” and did not accept double majors. “If you have any interest in anything else, do it; making a living with music is too diﬀicult.” I did take music lessons as a nonmajor. I also played a lot of music as a nonmajor, including accompanying a violin recital and making pocket money accompanying opera singers. There were great musical performances – Bill Evans, Vladimir Horowitz, and the student opera were all amazing. Given that a music double major was unavailable, I started out by majoring in math, but I soon decided I preferred to have a major that applies math, either physics or economics. (Music was ruled out partially because I had some stage fright, but in fact I have gotten over that over time and it is a shame it entered my decision.) I went to the physics department, and asked whether I could get into a top PhD program in physics if I majored in math and economics. The advisor laughed and said that seriously, there is a body of knowledge I would be missing. Then I went to the economics department and asked whether I could get into a top PhD program in economics if I majored in math and physics. The advisor replied that would be great, that undergraduate and graduate economics were not so similar, and that physics would teach the problem-solving approach without the emotional burden of economic questions. At that time, I did not know about the concept of dominant strategy, but I did understand the value of putting off a decision, and I chose to major in math and physics.  At Indiana, I competed in sailing against other schools. Sailing is a sport that was a student activity, not a varsity sport, at Indiana, but we took it seriously. Except in the coldest months of the year, we drove to other universities to compete every week. In the fall of my third year at Indiana, I grew weary of competitive sailing and decided it was time to think about graduating from Indiana and moving on. I had a lot of advanced placement credits when I came to Indiana and I had a lot of course credits, so it was relatively easy to arrange to graduate at the end of my third year. But what to do next? It was time to decide whether to continue in physics or economics. I knew more about physics, and I was especially interested in elementary particle physics. Unfortunately, this was a crowded area, and it was my impression that most PhDs in physics since I was born were also in this area. I heard a story about a new experimental result in this area reported in the Physical Review Letters, followed within 6 months by publication of three possible theoretical explanations. At the end of the six months, the people who published the experimental result published a retraction, saying that their result remained when they removed the sample from the particle beam, indicating a problem with the equipment. I imagined my role as providing a fourth theoretical explanation for the experimental result, which didn’t seem like much of a contribution to society. Therefore, I decided to apply to graduate school in economics.  When I decided to focus on economics, I got acquainted with Michael Magill, an economics professor, and I took his course on mathematical economics. I was missing many prerequisites for the course, including the first-year economics PhD courses and the first semester of his sequence, but the first problem set used Hamiltonian dynamics to solve a physics problem I understood because of my training in physics, so I figured (correctly) that all would be okay. Michael helped me to choose graduate schools where I should apply, and I also started attending a seminar series he ran. I was admitted with fellowship money to Rochester and Penn, and I met Lionel MacKenzie from Rochester and Steve Ross from Penn when they came to speak in Michael Magill’s seminar series. I met with Steve after his seminar on April 14, the day before I had to choose which graduate school to accept. I asked Steve why I should come to Penn over Rochester, and at the end of the conversation he agreed to be my advisor. He was the best advisor ever, and a year after I went to Penn he moved to Yale. He arranged for me to come with him to Yale, because he had promised to be my advisor while I was still an undergraduate.  I want to digress and talk about why a student going to a research seminar should always ask a question. This has many advantages: it will improve your understanding, it will keep you engaged, it will make you look good, and it is good practice speaking about research. I think a lot of students avoid asking questions because they are worried they will look bad, but they don’t understand. If a student asks questions, the professors will think the student is engaged and will assume the student is smart. If the question is not so sophisticated, no problem, have you listened to the professors’ questions? And occasionally the student will ask a smart question (if only by accident) and then the student looks very smart. (It is risky for a student to ask a lot of questions in one seminar, even if they are good questions, because some faculty will have a misplaced sense of hierarchy and get upset. However, asking one question per seminar should be safe.) When Steve Ross visited Indiana, I had a habit of asking one question per seminar, a smart strategy but for a little silly reason: it helped me stay awake during the seminar. (This reason was not completely frivolous, because being more engaged is a good reason to ask a question.) I looked at Steve’s paper, the working paper version of his incentive-signaling paper Ross (1977). The paper has a model in which good firms take on a lot of debt to show how tough they are, which signals they are good firms. I did not have a good question, and reading some of Steve’s papers he referenced didn’t help either. So, I asked a friend who was an accounting major what it means when a firm has a high debt-equity ratio. She said that maybe the firm has cash flow problems, and will have trouble making debt payments, and will fight to stay in business. I figured it would be safe to ask how the model differs from the standard argument, and in the seminar I asked Steve how he knows his firms are taking on more debt to show how good they are, and that they are not in trouble and borrowing a lot to try to cover costs and stay in business. Well, this is a much better question than I had a right to ask (only by accident), and it is why we think of that model as a parable and don’t take it too literally. That is probably at least part of why Steve agreed to be advisor to the awkward skinny longhaired kid.  I had mostly great first-year courses at Penn, especially from Dave Cass, Bob Pollak, and Karl Shell. When I was a student at Yale, I had more personal interactions with faculty members, including my co-author Gerry Jaynes, Bill Brainard, Don Brown, Al Klevorick, Sharon Oster, and of course my advisor, Steve Ross. Besides my advisor, Bill Brainard probably had the largest influence. I think he didn’t like writing papers and fighting through the review process, but he was the source of the main idea of a large fraction of the papers produced in the very successful Yale Economics Department. He and Steve Ross were old-school in the sense that they worked on research on a wide variety of topics, something that seems rare in my vintage and in younger economists. I followed these advisors in adopting a wide breadth of research. Working on many topics suits my restlessness, a sort of intellectual short attention span. My co-author Doug Diamond has focused on banking in his whole career, but I have only written three banking papers: the Prize paper (Diamond and Dybvig (1983)) and two policy-oriented pieces (Diamond and Dybvig (1986) and Dybvig (1993)). My remaining papers cover a lot of topics.  My advisor, Steve Ross, was a great teacher, and he placed many PhD students at top universities. He seemed to have a second sense about just how much help a student needed. While I was taking his PhD-level finance course, I stopped by his oﬀice. He said some people don’t know how to take a hint. He threw out good research ideas in class, but nobody ever took him up on them. He pointed to one idea from the most recent class, an open question about whether the market portfolio is eﬀicient, which is (in principle) a testable restriction that can help us to distinguish alternative theories. He pointed out that this is a finite-dimensional problem that should be tractable, and the answer would be interesting either way it came out. This became the paper published as Dybvig and Ross (1982). The experience of writing with Steve taught me what I needed to jumpstart my research career. After that, Steve gave me help in my research, but not too much. The amount and style of help Steve gave seemed to vary a lot across students. He just knew what each of us needed.  My PhD thesis at Yale included papers (published as Dybvig (1982,1983)) that looked at the question of recovering preferences from behavior. The results in these papers can be interpreted as versions of the uniqueness results of revealed preference theory specialized to risk preferences. In some form, these results can be used in practice for eliciting a client’s risk preferences from stated choices. By this time, most of Steve’s work was in finance and he was viewed as a financial economist. I asked him whether my thesis was in finance, and he responded, “Finance is the set of things people interested in finance study,” which I took to mean “No, but it’s okay.” I liked finance a lot at that point, but I suspected it was only because Steve was so charming and made everything magical. After a year and a half in the Economics Department at Princeton, I figured out that I really do like finance, and now most of my work is in finance.  In graduate school, I wrote one finance paper, three papers in economic theory, two papers in labor economics, and one paper in industrial organization. Later, most of my papers have been in finance: endowment management, asset-liability management, risk management, fixed-income, option pricing, capital structure, asset pricing, performance measurement, lifetime consumption and investment, incentives for managers, decisions by corporate boards, banking (including the Prize paper with Doug Diamond), risk management, performance measurement, and liquidity. I have also written papers on diverse topics in other areas, industrial organization, law and finance, incentives in hierarchies, economic theory, and the central bank decision problem. My general rule is to work on what is most interesting to me at the time, since it is hard work to spend three hours on something I don’t care about but not a challenge to spend 60 hours on something I am interested in. There are many reasons for being interested in a problem, for example, curiosity, large benefit to society, or dissatisfaction with what other people are saying. Dissatisfaction with what other people are saying does not sound very nice or important, but it is honorable because sorting out confusions in the literature can often make a big contribution. My best contributions have probably come from curiosity. I think that curiosity points us naturally at where understanding is most lacking, and this is more likely to benefit society than the direct approach of setting out to benefit society from the outset. Another thing that is important is not to be too rigid. Although there are types of experiments that require a lot of advanced planning, it is better to be flexible and understand we have an option at every point in time to switch to more promising project, in case we learn that the current project is less interesting than we expected. One exception to working on what is most interesting is that we need to finish good papers that are nearly done and push them through the publication process. It is our job to communicate good research, and without communication it is just a personal diversion that does not benefit anyone else.  To summarize, I think that passions about a number of activities in my early life primed my brain for research and kept my curiosity alive. I have focused on music and games as two activities that helped my development, but I think it was important to have passions that used my brain, and perhaps the particular passions did not matter so much. I would suggest that parents should help kids to find passions that keep them thinking, but I would also warn against dictating what those passions should be.  References  Cowles, Alfred, 1933, “Can Stock Market Forecasters Forecast?” *Econometrica* 1, 309–325.  Diamond, Douglas W., and Philip H. Dybvig, 1983, “Bank Runs, Deposit Insurance, and Liquidity,” *Journal of Political Economy* 91, 401–19. 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| Autobiographical |  |
| Podcast | **“I was just curious about everything”** Meet economist Philip Dybvig in a podcast episode. He tells us about his endless curiosity and how his parents encouraged his interest in the world from an early age. In his childhood home music played an important role and Dybvig shares how he and the rest of his family played musical instruments. The host of this podcast is nobelprize.org’s Adam Smith, joined by Clare Brilliant. This podcast was released on 18 May, 2023.  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. **Transcript** Philip Dybvig: I was just curious about everything. Everything was interesting and I always felt like I didn’t have enough time to learn all the interesting things. I think that was a big strength that I had.  Adam Smith: Being curious about everything is truly a great gift. One of the things I liked about this conversation with Phil Dibvig is listening to how he embraces the many opportunities out there for research. He talks about his ideas pretty freely and sometimes people steal ideas. But that doesn’t worry him overly because there are so many ideas to have, it doesn’t matter if something gets stolen, he can just work on the next idea. That’s nice because it shows you that there aren’t really so much missed opportunities as just lots of opportunities that one can pursue. That’s a very encouraging thought, I think that the world is more full of possibilities than perhaps one imagines.  Clare Brilliant: This is Nobel Prize Conversations. Our guest this time is Philip Dybvig, 2022 laureate in economic sciences. He was awarded the prize for developing theoretical models about the role of banks in financial crises – forming the foundation of modern bank regulation. He shared the prize with Douglas Diamond and Ben Bernanke.  Your host is Adam Smith, Chief Scientific Officer at Nobel Prize Outreach. This podcast was produced in cooperation with Fundación Ramón Areces.  Philip Dybvig is the Boatmen’s Bancshares Professor of banking and finance at the Olin Business School of Washington University in St Louis. He is also a musician, and very nearly chose that professional path instead of going into economic research. We’ll hear him talk about this fork in the road, as well as his boundless curiosity and his love of weightlifting.  But we start – with music.  Smith: This, of course, is Mozart’s March in D Major, K. 249, which is the music that always plays to accompany the new laureates as they walk on stage at the award ceremony in the concert hall in Stockholm. Do you remember that moment with special pleasure, and can you recollect what you were thinking as you walked on stage?  Dybvig: Actually as I walked on stage, I was happy to see Ingrid Werner, who’s an old friend who walked up alongside me for eight meters or whatever, and she’d been a little under the weather the previous day, and I hadn’t been able to see her, so it was nice to give her a nod. That was what I was thinking.  Smith: Once you took your chair and you were seated there with everyone around you, did anything in particular occur to you as you were on stage?  Dybvig: Mostly I just wanted to make sure I didn’t screw up, that I didn’t make any big mistakes. It was a nice moment. Everything during Nobel Week was so well orchestrated. It was exhausting. If I had gotten the prize 20 years ago, I would have signed up for all the optional events and I would have had the energy to do them all. But I think that it’s just very overwhelming the whole week and a big schedule and you start out jet-lagged. I had more and better champagne than I’ve ever had before in my life. It was a spectacle the whole week.  Smith: Yes, the champagne haze. Was there one favourite moment?  Dybvig: I think that for all the wonderful things in the week, I think it was really nice. We booked a room for a private dinner with our official guests and a few other people. That was of course nice. We don’t get to see our guests all that much during the week because we’re booked up so much. That was something that was special. I had arranged for a piano to be there and I have a picture of me playing the piano and my daughter singing and a good friend of mine, you can actually see in the mirror in the room, playing the flute. That was a nice moment. Music’s always been important for me so that’s something that actually came up several times. **About music** Smith: That is indeed why I thought it would be nice to start this conversation with music. What were you and your daughter playing and singing at that occasion?  Dybvig: I don’t remember. It was whatever she requested.  Smith: That’s clever that you can just provide whatever she wants.  Dybvig: Not always but I’m pretty good at requests.  Smith: You play the piano. What else do you play?  Dybvig: That’s my main instrument. The interesting thing is that in several of the productions I’m playing a baritone ukulele which is an instrument that I only started, I don’t know, in September. I’ve worked professionally playing the piano but never the baritone uke. But somehow that seems to work its way into everything. I have a friend who 70 years ago at the age of 12 was a radio and television personality in St. Louis and he has trouble playing now because of some physical problems with his fingers, so he’s been giving away some of his instruments. He gave me this very lovely baritone uke.  Smith: I guess it’s more portable than a piano.  Dybvig: Yes, and when the Associated Press interviewed me in Boston, I didn’t have a piano with me because I was on the road, but I had brought the baritone uke and they used that in their initial news announcement.  *CLIP: Dybvig playing the ukulele to the Associated Press*  Smith: Did you ever think of becoming a musician professionally?  Dybvig: Yes, that was something I thought about. One of the reasons I decided not to was actually because I knew that would have to be 100% of my life and I wasn’t quite ready for that. But also at the age when I was deciding I had stage fright which was quite bad. I realise now that that would have and has gone away over time. When you’ve had experience talking in front of audiences, for me in classes and in conferences, and also playing music in front of people after a while, it becomes normal and for me at least the stage fright went away. There are famous musicians, I think Vladimir Horowitz was one who supposedly had terrible stage fright their whole life.  Smith: Yes, it’s common. I’m told the actor David Niven, who seems the epitome of confidence on screen, also suffered incredible stage fright before going on screen.  Dybvig: That’s interesting. Yes, I find it helps to pretend that I’m confident. And then there’s actually psychological research that says if you pretend that you’re happy then you actually get happier. If you pretend that you’re confident you actually get more confident, so it’s curious.  Smith: It’s nice that confidence in music and confidence in as you say teaching economics and finance reinforce each other and build.  Dybvig: Yes.  Smith: Do you play a full classical repertoire?  Dybvig: Maybe past tense. I am classically trained and by the end of high school I was playing five or six hours a day. Various things in school like playing with the orchestra and the chorus and playing during study hall and also playing after school. My sister’s a concert pianist. She claims that I could have been. I’m not sure because I’m not sure I really had the single-minded devotion that’s really required.  Smith: What was it about your home environment that produced two potential concert pianists, one actual concert pianist but two such talented musicians?  Dybvig: I think that we had a culture of music. My mother was a music education major in college and my father is also a drummer and he had made spending money in college playing in a band. So there was that culture and they were into music and we had music playing a fair amount in the house. We had a lot of musical instruments sort of sitting around that you could pick up and play if you wanted to.  Smith: It must be a very valuable lesson going through life to know that you can dedicate five or six hours a day to doing something because it’s not so usual that young people do that.  Dybvig: I think there are a couple of things. One is that I’m sure that the music helped a lot with my self-discipline but it’s also a question of finding the things that you like. When I talk to my students about research and picking a research topic I tell them that they need to work on something that they care about. There could be a lot of different reasons why they care about but if it’s something where you’re really curious you gotta know or you’re really angry because you think that people are saying the wrong thing and they got it backwards or you just think it’s so important for the humanity to get this right and you’ve got a passion that you need to do it, then you can work on it for 20 hours a week or 80 hours a week and it’s something you want to do. If it’s something that you’re doing because you think this is something that you can get published and it’ll help your career but you don’t really like the project, you don’t care about it, then it’s a big chore to work on it for three hours a week. I think that if somebody would look at me and say, oh that means that my kids have to play music, I would say no. It means that you have to find something for your kids which is a hook for them. And for different people it’ll be a different thing. Maybe art is what they want to spend five hours a day on or dance or some sports, whatever it is. I think it’s good to have something that you care about and that you’re focused on. For what I do, something that uses your brain too and gets you thinking.  Smith: It’s very good advice. Do you think it matters if young people spend their time doing something that seemingly doesn’t seem to be terribly productive like six hours playing computer games a day?  Dybvig: Yes, at least with computer games there’s some, it’s not passive. I think I would be happier to see my kid playing computer games than just watching television. I’ve watched plenty of television. I think you need time too to just play and to let your mind be free and be creative. What I really worry about is all these things that are good uses of time but the kids are just completely booked up with the amount of homework they have plus the dancing lessons, plus the fencing lessons, plus the soccer game and all of these structured activities. They don’t have time for unstructured play that was good for creativity. **Childhood** Smith: Your childhood was different. You had time to be yourself.  Dybvig: Yes, I had time to be myself. Before my mom passed actually she taught me something about myself. She said, Phil you never once when you were a kid said, ”Mom I’m bored I don’t know what to do”. Because I don’t know I was just curious about everything. Everything was interesting and I always felt like I didn’t have enough time to do all the interesting things and to learn all the interesting things. I think that was a big strength that I had.  Smith: That’s fabulous and very lucky to have a mind that is so hungry I guess.  Dybvig: I’m not sure how much of that is heredity or how much of that is environment but I think it’s very easy to kill that off in kids. My gut tells me but I have no evidence of this that little kids are just naturally curious, naturally creative. If you can keep that alive I think that serves people really well.  Smith: In your own case you went on to study maths and physics as your major at university. You obviously were very good with numbers. Were numbers something that always came easily to you?  Dybvig: I think so. I think it was an advantage being in a family where my parents told me math was easy. I can imagine being in a family where your dad says, math was so hard for me. If you can get a C that’s really great. I think that would really make you not so good at math. But if your parents are saying or other people in your family are saying math is easy then of course you don’t want to be bad at something that’s easy. You look really bad. But I think it helped in terms of the math. I think music both the discipline part and also the mathematical side of the music helped. Also playing games was so important. We joke about how I knew how to play bridge before I knew how to talk. That’s not literally true but it’s the right idea. I think that you get into the bridge game. You want to try to remember the cards play. You want to count the cards. You want to try to infer what suits your opponents have left when you’re playing a hand. There’s all sorts of things which are sort of mathematical exercises even if they’re not posed that way.  Smith: Yes, I think that an interest in bridge at a very early age is pretty unusual.  Dybvig: It was just normal in my family. At family gatherings the first thing is to get out the bridge tables.  Brilliant: Philip Dybvig grew up in Dayton, Ohio, before he left to study mathematics and physics at Indiana University. Eventually switching to economics, he received his doctorate at Yale where he met his co-laureate Douglas Diamond. For 12 years, until 2021, professor Dybvig led a summer research program at Chengdu’s Southwest University of Finance and Economics, in the Chinese province of Sichuan. **About the time in China** Dybvig: I like saying I was a dean because dean and director are the same word in Chinese and I think it’s funnier to think of myself as a dean.  Smith: Why is it funny to think of yourself as a dean?  Dybvig: When I think of a dean I think of some kind of authority figure that I don’t feel like I am. My main role there was to help the local people, especially the local faculty in their research. I would bring in speakers and set up lecture series and advise them on the research and do joint work with them.  Smith: Is there something in particular about the Chinese environment that makes it a pleasure to work there and to nurture an institute in that place?  Dybvig: I think I started going at a time in my life when I needed a change. It was really fun the first time I went to China and I didn’t speak any Chinese I just know how to say hello and no problem. That would get me by. I would go into a restaurant and I would look at the menu and in Beijing most of the menus have pictures on them so I would point to the pictures. Then they would ask me something and I didn’t know what they said. Do you want pepper sauce or garlic sauce? And I would just say no problem. And then it’s like you’re the pro, bring me something good. That was fun. It was a little bit I imagine like a vow of silence in some religious orders because it was impossible for me to have a conversation. It was very peaceful but it was also a little exciting and not too dangerous because I had a cell phone and friends who could help me out if I needed it. That was interesting and in Sichuan especially, Chengdu where I was working, they have the spicy food which is really great. It’s called Ma La Cai. Ma means numbing and la is hot and spicy. The food there has both Sichuan peppercorns which have this numbing effect and the hot peppers.  Smith: Over the years you’ve learnt Chinese, I gather?  Dybvig: Just a little. I learn enough to, I know some food names and I know a few funny things. I know how to say I can speak absolutely no Chinese in Chinese.  Smith: Do you want to say that for me?  Dybvig: Huo gan ben bu hui suo zhong wan. That has a little bit of a Sichuan accent flavour. There are some countries you can be very proficient in their language and speak with them for ten minutes and they’ll say at the end, yyou can’t speak French, can you? But they’re actually very grateful for your knowing a little bit and for making the effort. It’s a way of signaling that you respect their culture and you’re interested in it. It’s really rewarding, you just know a little bit. It’s nice if I’m going to go to some kind of a dinner with officials like they have 12 people sitting around a round table with a lazy Susan in the middle with the food on it, to be able to name some of the foods and just give pleasantries, hello, how are you, etc. in Chinese. Maybe I don’t understand much what they’re saying the rest of the dinner, but at least it’s established that I respect them and I’m part of it. I’m not just an outsider, 100%. **Economics and finance** Smith: Let’s go back to you as a maths and physics major. What turned you to economics and finance? What switched you?  Dybvig: I’ve always had a lot of interest. That means that you don’t quite know which direction you really want to go in. I went to Indiana because it was at that time the top music school and also it was kind of the right distance away from home. It was close enough so that I could come home for holidays conveniently and far enough that my parents were unlikely to visit me without calling first. I was planning to be a double major in math and music, but I found out the music school was too good. They say we don’t do double majors. If you have any interest in anything else, just do it. Music is too hard. So I said, okay, well, I guess I’m a math major. I took some courses in the music school. I actually accompanied opera singers to make pocket money, play for their lessons and their practice. I was in a number of student music things and went to lots of performances or rich set of performances, so I benefited a lot from the good music school there, even though I wasn’t a music major. Now after I’d been there for like a year or so, I realised I really wanted to do something that applied math, not pure mathematics. At that point, physics and economics were the main candidates. When I went to the physics department, I say, what’s my chance of getting into a top PhD program in physics if I’m majoring in math and economics as an undergraduate? The advisor laughed and said, well, seriously, there’s a whole body of knowledge that you need to know that you wouldn’t have. That’s a problem. I went to the economics department and I asked the parallel question. I said, what are my chances of getting into a top graduate school in economics if I may have an undergraduate major in math and physics? The advisor said, oh, that’d be so great because you learn the skills you need. You get the problem solving approach from the physics and the math background from the math and undergraduate economics, at least at that time, he said, was not very close to graduate economics.  Smith: It’s such an insightful question on your part. And what a nice answer on his part. Yeah, they’re open to outside influence. It’s fantastic.  Dybvig: I didn’t know the notion of dominant strategy, which I would call it now to major in math and physics, but I knew putting off a decision. I could put off a decision on what I wanted to do by majoring in math and physics. When it was time to graduate, I had to make a decision. That’s when I went to economics. I actually made another transition, which is within economics, I ended up focusing mostly on finance.  Brilliant: So Adam, what was the work that led to Philip Dibvig being awarded the prize in economic sciences?  Smith: Together with his co-laureate Douglas Diamond, he produced the Diamond-Dibvig model, which describes how liquidity works in financial systems in banks. Basically, banks receive deposits from investors and then they give money out as loans to people who want to build things like buy houses or develop businesses, etc. The model describes the interplay between those two things. One rather short-term deposits, they come in and people might want their money back very quickly. One very long-term loans where you might not get paid back for many years. That interplay turns out to be very key in understanding how liquidity is created in financial systems, how people get credit.  Brilliant: Why is the Diamond-Dybvig model so important?  Smith: Taken together with the work of the other laureate in this economic sciences prize, Ben Bernanke, former chairman of the Federal Reserve Bank, who had shown how important banks are in preserving the financial system and in the provision of credit, the work helps people understand just how important it is to make sure that banks are there and functioning and aren’t foreclosing. Therefore, that has influenced the way that central bankers around the world think about banks when times are hard. During, for instance, the financial crisis of 2008, people realised in a way that they hadn’t during the Great Depression of the 1930s, that it was really important that the banks didn’t all close or that lots of banks didn’t close, because then that would have deprived the financial system of credit. And without credit, nobody can build up again. It has actually had quite a profound influence on the way that policy makers view the role of banks. This is quite a change in perspective. And it’s interesting to listen to Phil Dybvig talk about what the situation was like back in the 1970s when he started to work on financial systems.  Dybvig: It’s strange that at that time, people did not think of banking as being in finance, but I think people in finance departments now would say that banking is in finance. Curiously, they still don’t think insurance is in finance. I don’t understand that. Actually I find it a little annoying when people need to put me or put different parts of disciplines in boxes. I think that creates artificial restrictions on the way people think. In my experience, things work out better if they happen organically. For example, a few times I said about saying I want to do something that’s important for policy and it was never anywhere near the magnitude of importance of the things like what Doug and I did in the prize paper that I did because of curiosity.  Smith: Sticking to your own rule of doing what you care about, truly care about.  Dybvig: I cared about the policy things too because I thought they were important, but somehow it didn’t seem to be as important as what I did when I was really curious. I just wanted to figure out what’s going on.  Smith: That leads me to ask how you pick problems. Basically your work always involves applying a theoretical analysis to some kind of practical problem. But there are so many problems. How do you pick where to go?  Dybvig: I don’t have any formula. It’s what I’m interested in, what I’m curious about.  Smith: Is there any problem that you can’t apply a theoretical analysis to?  Dybvig: Of course. You may be able to apply to some aspects, a lot of things in life I do think about using these things. That started when I was a kid. It probably grew out of doing games and puzzles. When I was walking to and from school I noticed that when I walked to school, I took a little different route than I did coming home. Our neighbourhood was not arranged in squares, it was curved streets. It wasn’t obvious, what’s the shortest way or what’s the fastest way. So I asked myself, is there a good reason for that? Or is it just that if I’m just looking at what’s in front of me and not paying attention to the fact that there’s something up further, which is going to be worse if I go that way. When I thought about it, actually, there was some of both. So if I’m walking along, and I’m going to walk along a busy street for a while, and when I get to the street, it’s coming from one side, and I leave the street, it’s going to be from the other side, then what I want to do is to, each time I come to an intersection, I see how busy the traffic is. There are no cars and I cross. That way I can minimise the time that I wait for the cars. I could just cross at the first intersection I get to, but if there are a lot of cars coming, I’m going to waste time with that. If I get to the intersection and don’t cross, so I go to the next one. It turns out that’s a reason for it to be asymmetric, because I’m going to tend to cross earlier, say farther north, in one direction, and earlier in the other direction as well, which would be farther south. So there’s an asymmetry in the path that I take, which comes from actually doing the right thing. I also figured out there are some things I was doing which were just not optimal, that I was just looking at when I’m coming from this direction, then this one thing seems more important. When I’m coming from the other direction, another thing comes more important.  Smith: Did you change your route to optimise it after that?  Dybvig: Yes, of course. I also randomised some just for interest. I think partly that was just curiosity and also a way of relieving the boredom of walking through the same route every day.  Smith: It is indeed revealing about how your mind works. It shows you grew up perhaps in a gentler place than I did. I grew up in London, and I think my route to school was mainly dictated by which way was less likely to encounter bullies.  Dybvig: Yes, and I think being in this upper middle class, very peaceful suburb of Dayton with very good public schools, gave me kind of the breathing room to develop. As you say, I didn’t have to deal with gangs or bullies. I did encounter, like everybody does, a few bullies, but they were kind of low-level bullies. They were kind of easy to deal with.  Smith: I think ours were low-level too. But yes, the breathing space again, so important.  *MUSIC*  Smith: I wanted to just talk about the Diamond-Dybvig paper. The thing about that paper is that as so often when two bright people get together and do something special, the whole is more than the sum of the parts, if you like, that somehow by working together, you create something truly special. It’s interesting because you have to, I suppose, to a certain extent, kind of put your own ego aside to share knowledge with somebody else. Especially in academia, that’s not perhaps so normal. People are trying to plow your own furrow and make your own career. But obviously it was a very successful relationship. I just wondered what you thought about that.  Dybvig: Okay, I’ll mention a couple of things. Once we got into the paper, we were just full equal partners and we did everything. We came in more different than we exited, I think. Doug knew a lot about banking institutions. He has talked about a professor he had at Brown as an undergraduate that had a fabulous course and that was a piece of background he had. I had a stronger background in theory and I’d done papers with multiple equilibrium, which I talked about in my Nobel lecture, that primed me to be thinking about at least part of this in a way that worked for us. I think that one thing that helped me a lot in my early career is that I was a young kid. I got my PhD at the age of, around my 24th birthday. When I got the job at Princeton, it’s like, wow, people are just going to pay me for a few years to do something fun. I had no career concern. I knew I was smart and I was proud I was smart, probably. But I didn’t have a huge ego and I didn’t feel like I had anything to prove. I was just having fun. That meant if I had a project that I’d been working on for quite a while and it wasn’t working, I wouldn’t say, oh, I’m not going to throw away those eight months. Instead, I was thinking, this isn’t fun. I put it aside and worked on something else. I think that helped a lot. I don’t know, Doug and I never, I don’t think we ever had a moment of anger between us.  Smith: It’s nice to hear, but perhaps also surprising to hear that when you’re a young mind so full of ideas, you don’t have any worries about sharing those ideas. You don’t think if I share this with somebody else, they might run away with it and publish it themselves. I mean, you obviously didn’t suffer from that. I think a lot of young people going into academia do worry about what they should share and what they can’t share.  Dybvig: Things do get stolen. I’m not going to go into any stories about that. To the extent I worried about it, maybe it influenced me a little bit when I moved that I would be away from a colleague that I felt I couldn’t talk to. But mostly, I just talk too much. I have sort of academic tendency to share everything. Some people seem to have trouble coming up with ideas and projects to work on. I’ve never had this problem. It’s kind of like when I was a kid, there are too many things that are interesting out there. I look around and there are just so many interesting problems to work on. If somebody hears my idea that I haven’t started working on yet and wants to work on it, I say, yeah, go ahead. I’ll have another idea. I don’t know why that’s easier for me than for some people. It’s a little bit like, if you’re good at algebra, it’s hard to conceive of what it feels like to be weak at algebra. Because, you learn this when you’re so young, you don’t remember the time that you didn’t know it.  Smith: Having an abundance of ideas, how lovely. Do they just pop into your head? Do you work at it?  Dybvig: Yes, I mean, there can be different things. For creative work, it’s good to have kind of a playtime when you’re relaxed and when you’re not working too hard. I think if you try to force it, then it’s harder, but it can be possible. For me, I really hate the idea of working under deadline, and I work very hard not to commit, say, to present a paper that it’s not ready yet, now, so that I have to push it through and come up with it. Sometimes the trigger is something, you’ll see something in the news, and you look at it and you say, that’s really an interesting setting, and I wonder how that works. You start writing down a little model, and you say, no, no, that’s not what’s going on. You try another one, and after a while, you have something that kind of fits that setting. At the end, you may say, yeah, that was kind of curious, and go on to something else. Or you may say, you know, I think that other people would be interested in seeing this, and there’s more development to do there, and you can write a paper. Sometimes you see something in the literature. You see a paper presented at a meeting or something in a journal, and you think the problem is interesting, but you think they have completely the wrong answer. And you say, why don’t I believe this answer? And you say, well, there’s a strange assumption here. Well, what happens if you switch the assumption? That’s something where there’s a more explicit trigger there. But a lot of them just come from the ether. It’s like it comes from your subconscious somewhere. It’s been thinking about things and mulling over some stuff, and then it sends you this idea. It’s like it came from outer space. Or maybe there is somebody in outer space who beamed it in. Who knows? That’s possible.  Smith: Yes. Phil Dybvig’s programmer keeps sending these ideas down. But yes, it’s very interesting to hear that account of an inquisitive mind at work. Very nice. **About weightlifting** Smith: How did you get into weightlifting?  Dybvig: When I was about 45, I weighed about 100 pounds more than I do now, about 45 kilos more. I’m sure I had high blood pressure for a long time, but that’s when I learned about it. My ankles were swollen, I felt bad, and I just had the definite impression that unless I changed something, I was going to die soon. I went out and tried all sorts of different types of exercise. I said, I’m going to have to have some kind of exercise that I’m going to be happy doing all the time. It’s really the same thing as doing research on a problem that you care about, because otherwise you won’t keep doing it. Before that, I would have thought that weightlifting was boring and stupid, but when I tried it, I really liked it. I think there are combinations of things, it’s very visceral, there are good chemicals in the brain that come, and you can measure your progress. When you start out, your progress is good, because a lot of that is coordinating your nerves and your muscles and not just building strength. You can see that you were able to do three more lifts than last time, or five more pounds of weight, and that’s really good for keeping motivated. It doesn’t take so long. If you’re going to walk 10 miles, that takes a long time. Weightlifting doesn’t take that long. I ended up with weightlifting and tai chi. They’re good compliments, because it’s possible to do the weightlifting and you end up like these Saturday Night Live parodies of Arnold Schwarzenegger.  *CLIP: Hans & Franz on SNL*  Dybvig: You’re just muscle-bound, you can hardly move, and then the tai chi, you can still relax.  Smith: It’s interesting to see that the same method is applied, whether it’s finding a problem or finding an exercise routine. It’s obviously a very disciplined approach.  Dybvig: I don’t feel disciplined at all. One thing that’s interesting is that, after I had like five or ten papers, I went back and looked at my papers. I don’t know if I had to write up a report on my research. They were on a lot of different topics. It struck me how similar they are. I thought that they were completely different. They were if you just look at the topic or the title, but the methodology and the way I thought about things, had a lot of patterns in it that I didn’t even realise at the time.  Smith: Interesting to analyse yourself in that way. Fascinating. It’s been a huge pleasure speaking to you. Thank you very much indeed. I hope you’ve enjoyed the conversation because I very much have.  Dybvig: Yes, very nice. Thank you so much. |
| Telephone  interview | 0806=PD  Adam Smith: This is Adam Smith.  Philip Dybvig: Hello.  AS: Hi, so am I speaking with Phil Dybvig?  PD: Ah, yes you are.  AS: Oh, how lovely. Thank you. Well, first of all, how did you hear the news?  PD: So, basically I woke up this morning and my phone had been on ‘do not disturb’. And when I woke up I had what seemed like thousands of messages and things. So I thought something is up. And I figured out pretty soon what it was.  AS: When did you know for sure that it was true?  PD: I knew that this was the day, and actually a couple people had suggested I might get it. I said, no, that won’t happen. And, but anyway, I went to nobelprize.org, your website and there it was with Ben and Doug and me. So…  AS: Very nice, very nice.  PD: I was half asleep and my initial response was probably stress. What’s this gonna do to my life? But now that it’s settled in some, I’m quite happy.  AS: Yeah. So you’ve had a couple of hours to get used to it. What’s it done to your life so far?  PD: Well, I just, I have more phone calls and emails and messages than I can possibly respond to… quickly.  AS: Yes. I suppose it’s gonna be playing catch up for quite a few days. Douglas Diamond, when we spoke to him, was very eloquent in speaking about your relationship and how much he valued your insight into social sciences and to the clarity of your thought. It was obviously a very special relationship that led to you being able to develop this model.  PD: Yeah, Doug is an amazing guy and he’s a great co-author. And we worked so hard to make the paper simple, but during the time we were writing it, it could be somewhat intense. It was never unpleasant. But, you know, one of us would say, “Well, we should assume this.” And then the other one would say, “No, that’ll be too complicated, we can never solve that.” And the other one would say, “Well, how about if we try that?” And then we say, “No, no, that’s gonna throw away all the economics,” and back and forth. And I’m hoping that as a result, you know, for economists that, they’ll find that to be a simple paper to read. And I think it paid off some because the model’s pretty simple. It’s easy to extend, with you know, we’ve left some room where people can add some things and still solve the model.  AS: Well, it’s become so important and is so very widely used. Do you feel that it’s being applied correctly?  PD: Oh, one never knows. I do think, you know, on a few occasions I’ve talked to practitioners who are, especially regulatory practitioners, who do understand this. And of course Ben, he understands our model and he’s made his own separate contributions and I assure you he understands their model and to the extent that it was useful for what he was doing, he made good use of it.  AS: From a theoretician’s point of view, do you think that the world is better set now than it was when you were working on this model to avoid crises? I mean, obviously, although this may not be a, I don’t know whether this counts, what we’re in now, as a full-blown crisis. An awful lot of people are very worried about what’s happening. Are we in a better place?  PD: That’s always hard to assess. And as the theorist, I know more about the models than I do about the details of what’s going on in the world. Personally, if I look at the economy, I think we’re in a little bit of a tough time. But I’m worried more about inflation and government debt and big spending than I am about an actual financial crisis where the banking sector has problems. One of the takeaways that you could have from our model is by nature financial crises are not predictable because if people could predict it, then they would not enter the contracts they’re in that are gonna be subject to run.  AS: Yes, well, we will have a chance to speak about this at greater length once some time has passed and things are a little bit calmer.  AS: Well, it’s been a pleasure speaking to you, and many congratulations.  PD: Yeah, thanks so much.  AS: Look forward to speaking again soon. Thank you.  PD: Yeah. Bye-Bye. |
| Interview |  |
| Q1 | Where does your passion for economics come from? |
|  |  |
| Q24 | What made you think that there was more to uncover with bank runs? Do you think it’s important to revisit or re-analyse subjects that people may have studied a lot already? |
|  |  |
| Q35 | You’ve said that you don’t like to work on the hot topics, you like to work on things that people aren’t think about – why is this? How do you come across things that people aren’t thinking about? |
|  |  |
| Q11 | How do you cope with failure? |
|  |  |
| Q34 | Do you ever feel pressure knowing so many people have read, cited, or built upon your work? |
|  |  |
| Q22 | Why do you think it’s important for students and researchers to approach complicated subjects to explain them? Why do you think it’s important for students and researchers to approach and explain complicated subjects? |
| Q21 | For students who are trying to approach complicated subjects, what advice do you have for them? |
|  |  |
| Q3 | Was there a person who influenced you? |
|  |  |
| Q4 | How do you maintain your curiosity? |
|  |  |
| Q26 | Can you tell us about the object that you are donating to the Nobel Prize Museum? |
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| Q28 | Do you think it’s important to have hobbies outside of your research? |
|  |  |
| Q23 | What skills do you think are important to for researchers or students to develop? What skills are important for researchers or students to develop? |
|  |  |
| ID | 0807 |
| Biographical |  |
| Autobiographical |  |
| Podcast | **“I spent all my time with my dad, learnt how to take things apart”** Hear economist David Card speak about his experience of growing up at a farm. In this podcast episode, conducted in May 2022, Card tells us about how his upbringing has shaped his life and how his dad taught him to drive tractors at the age of 10.  Card was awarded the 2021 prize in economic sciences for his empirical contributions to labour economics. Besides his work on labour, he has also done extensive research on educational systems. Here he tells us about the positive progress of more gender balanced admission at universities but highlights the increasing problem with underrepresented minorities in economic sciences.  Card also tells us about how he spends the very little time off he has wood working. He describes it as “very lone work. It’s you and the wood.” We also get to hear about another Nobel Prize laureate that he finds fascinating and that he would have loved to converse with, 1998 literature laureate [Jose Saramago](https://www.nobelprize.org/prizes/literature/1998/saramago/facts/).  The host of this podcast is nobelprize.org’s Adam Smith |
| Telephone  interview | 0807=DC  Adam Smith: May I speak with David Card please?  David Card: Speaking, yes.  AS: Hello, my name is Adam Smith calling from the website of the Nobel Prize.  DC: [Laughs] Yes. I heard that that was… I heard that Adam Smith was going to call me. That’s kind of ironic.  AS: Yes, on this… on this strange morning it must make you feel even more disorientated.  DC: Yes, anyway, pleased to meet you.  AS: Lovely to meet you.  DC: Sorry. [Laughing]  AS: Not at all. Congratulations on the news.  DC: Oh, thank you.  AS: So how did the news reach you?  DC: Somebody called my home phone number and we actually have a kind of weekend house somewhere, and we’re actually there right now and, but the call forwarded to my wife’s cell phone.  AS: Were you both asleep when it came?  DC: No I just got, actually I just woke… I actually literally had flown back from a memorial for my grandmother who passed away recently.  AS: I’m sorry.  DC: I just, just arrived about 20 minutes ago from the airport.  AS: Gosh, so you must be exhausted already. And now the prospect of a sleepless night ahead I suppose as people bombard you with phone calls like this.  DC: Yes, I don’t really know the drill – I was kind of hoping I might go to sleep! [Laughs]  AS: I think you should! I think that’s absolutely right. I’m delighted to catch you just before you turn in and turn off your phone maybe. But it sounds like it’s really caught you very much by surprise?  DC: Yes, I must admit, you know, I don’t think I would have been a very high probability.  AS: That’s a modest thing to say. You use real world events to find the evidence for causal relationships. It must be fascinating just waiting for events to take a turn that allows you to interrogate the data.  DC: I do sometimes tell graduate students that crazy political regimes have a lot of disadvantages, but one advantage is that they do create very good conditions for trying to do a causal analysis – that’s true.  AS: As I suppose does all this, all the awful disruption caused by the pandemic, presumably there’s tonnes of potential in that for analysis.  DC: Yes, this pandemic is very difficult because there’s so many things that are disrupted at once, and I think… yeah, I bet you that 20, 30 years from now we’ll still be trying to sort that out, what exactly happened. You know, if you look at the increase in unemployment or the drop in GNP, it’s you know like one of the worst, or the worst recession since the great depression, or maybe worse than the great depression. And certainly in it’s… the speed of onset it’s worse. But then almost miraculous recovery after, so it’s going to be something that I think people are going to struggle with interpreting for many, many decades.  AS: Just to highlight one bit of data from your work, I mean, people are talking a great deal about immigration these days, and very worried about the effects of it on jobs but your work shows that there doesn’t seem to be an effect of immigration on the labour market, which is fascinating.  DC: Actually that’s not quite true. The one paper that I think probably you’re referring to is a paper I wrote about the Mariel boatlift and it didn’t show much of an effect but it… you know, a cautionary tale is that there could have been small effects because them the ability to precisely measure the effects when it only affects one city is somewhat limited. Another… other work I did… I did some find, you know, very small effects of immigration on native wages. But people don’t remember that, but that’s clearly written in the paper.  AS: I suppose I’ve been guilty of just what must be the big problem of your work, which is that the implications of these data sets are enormous for public policy and people tend to want to make a decision – go one way or the other. And it must be, I suppose, frustrating to have people simplify things that way.  DC: I’ll tell you… for instance on the minimum wage I remember Al Kreuger and I did some research jointly and independently of each other in separate papers, and then we wrote a book in the early 1990s, and after that the minimum wage was frozen in place for a very long time. And so we always assumed that our work had completely stopped all progress for the minimum wage! So I think, similarly, to tell you the truth, I’m not sure that the kind of research that I’ve done has had much impact on, on immigration because at the end of the day on the immigration issue… I don’t think… I think people talk about these impacts on native workers but I think that’s really what the big concern is. The big concern is about changing the composition of the country and making it more non-white or more different religion, more, different language, different racial composition. I wrote a paper on that too, but no one remembers that one!  AS: What was your conclusion?  DC:That basically, well it was based on data from the European Social Survey, and the very first European Social Survey they did in I think like 23 countries and I was working with Christian Dustmann at UCL and Ian Preston at UCL and we submitted a proposal for questions and it was accepted, and so the very first ESS – European Social Survey – had this battery of questions. And we, we tried to get at what’s the main reason that people are against immigration. Is it because of economics or because of the sort of concerns about cultural issues? And we concluded that it was mostly the cultural issues.  AS: They’re very deep rooted, and it will take a lot of changing.  DC: Pardon me? My wife is here making… I’m standing here in my pyjamas – my wife’s just taken a picture of me, kind of making fun of me.  AS: You know what, she should… rather than making fun of you, what I’d like her to do is to send me that picture!  DC: [Laughs] Okay! This guy wants the picture.  AS: I most definitely do. You know we have this… we have a tradition now of not only interviewing you but trying to get you to send us a picture capturing this moment, which we post on our channels for Nobelprize.org.  DC: Oh okay.  AS: People have been doing it all last week. You know, that would just, that would just be a dream to have that. Thank you, people will adore it and…  DC: Okay. [Laughs] Sure! I’m speaking on the phone in my… yeah okay. I think it should be okay.  AS: I’m sure you look fantastic. Anyway, it’s a happy moment, it’s worth remembering, right? Yeah.  DC: Yes, absolutely.  AS: It’s been a huge pleasure speaking to you, thank you very much indeed.  DC: Yes, thank you. Your name really is Adam Smith?  AS: I’m afraid it really is, yes. It’s a peculiarity, and anyway, if it gets a laugh it’s worth having a good name. Thank you very much indeed, and I hope…  DC: YSure, Adam, and I’ll upload the jpeg of the picture.  AS: Thank you very much, and good luck with getting back to bed.  DC: Okay, yes.  AS: Bye.  DC: Thanks. |
| Interview |  |
| Q36 | You received the 2021 prize in economic sciences for your “empirical contributions to labour economics.” What was your first real job? |
|  | If you grow up on a farm, you’re working when you’re eight or nine years old. You help out a little bit and then as you get older and learn to drive the tractors and have a little more independence. I had many of those kind of jobs. The first of the farm jobs I had, I would’ve been working for other farmers. Then eventually there was a local university near where I grew up and they had a huge research farm where they grew crops to try out and things like that. So I had a job there for a few summers. |
| Q1 | Was there a single defining moment when you decided to pursue economic sciences? |
|  | Maybe in my senior year, my final year of college. At that time, at the university that I went to, economics used to be a much smaller subject than it is now. So even though there was a fairly large number of faculty, there were relatively few students. We had very ideal conditions. We had lots of faculty, and there was a small course where we learned how to write papers. I did my senior thesis on strike activity, at that time – in the 1970s – there were a lot of strikes in Canada and the United States, and I got interested in that topic. I found it interesting and we were using computers, which was somewhat more cumbersome than it is now. You had to type your programs on cardboard cards and then they would be read by the computer. There were a lot of other students working at the same time, some of whom I’m still friends with. There’s a certain amount of comradery doing that research. Then I decided maybe I would go to graduate school. |
| Q14 | How would you say that we can encourage more diversity in the field of economic sciences? |
|  | Somehow, I would say in the last 10 years, at least in the United States, things have really changed. You can see that really all over the place. I’ve done some research on what kind of distinguished faculty receive special awards and the fraction of the awards that are going to women in economics and in other fields has really shot up. Having female scholars being recognised and shown as role models might have a positive influence.  A very long time ago in 1960s and 70s, there was definitely discrimination against female scholars. You can see examples of people who were passed over for these kind things. Things have really changed very much in the graduate education, so right now I think most of the top programs are virtually 50/50, so they are really reaching out and pushing hard to make sure that they stay close to 50/50, whereas even five years ago, that just wasn’t as much of a concern. So that has really come to the fore for reasons I’m not entirely sure of.  On the issue of minorities, that is an even worse problem. Graduating with a PhD in economics every year in the United States, there’s probably only four or five African Americans, a really low number. That’s caused by a number of factors, including real shortfall of people graduating from high quality universities in economics that are African American. So we’re not doing a very good job of keeping kids, especially highly talented minority kids, in economics and getting them through the bachelor’s program and then going onto a PhD. Now, on the other hand, it’s not entirely clear that going to do a PhD is the right thing if you’re that person, there’s a lot of other opportunities. For instance, going to law school might be pretty important for really talented minorities. We really need some of those people in the law profession. In the United States, everything is really run by lawyers. If you don’t have representation in the bar, you’re really in trouble and it’s going to change the way things go and we see that happening now, to some extent. So I think that that might be actually more of an emergency there than in economics. |
| Q22 | What kind of qualities do you need as a person, if you want to be a successful scientist or researcher within the field of economic sciences? |
|  | Our profession, economics in particular, is very front loaded. It’s a little different than the hard sciences. So when people graduate with a PhD, they will have done a very original piece of research at that point. It’s not like they will have been engaged in a lab and helped their lab director and PhD advisor. They will have had to come up with their own independent research. There’s a huge amount of pressure on students to do that, which I’m not entirely sure that’s the right system, but that’s the system we have. There’s just an enormous amount of pressure and that pressure maintains until someone gets tenure. So that’s about eight, nine years of intense pressure coming at the end of the age 25 to 35. I think that basically people who can deal with a lot of pressure and put in enormously long hours, unfortunately that’s what’s required at that exact time. You can see right away, that’s a disadvantage for females because it means it’s going to cut right into time when they might reasonably think about starting a family. |
| Q23 | Would you say that creativity, persistence and coping with failures are important traits for becoming successful in science? |
|  | All of those things, for sure. The hardest one of those is coping with failure, that’s difficult because most of these people with flawless resumes haven’t really had any failures yet. So that’s a little difficult. They’re going to probably be 25 to 28 years old by the time they have their first failure, which is when they don’t get a really good job from the PhD program. That can be a really difficult problem that some of the students are not equipped to deal with. Because they haven’t had any of that kind of failure before. Persistence – that kind of goes without saying, I think. It’s a high commitment field. Most people who are doing economics who are going to be very successful are pretty nerdy and working 12 to 15 hours a day, seven days a week with a few breaks here and there. |
| Q11 | How have you yourself coped with failures in your life? |
|  | Well actually I probably haven’t had any huge monumental failures. A lot of times the most standard problem that you have is that you work on a project you think that it’s really interesting and that the results are important and then other people don’t really care for it. I think the best antidote for that kind of problem is that, you know, there’s a new idea. So people are always looking for the new idea and thinking about, this is a really exciting project. And hopefully this one will really get people excited. |
| Q28 | I heard that you like bikes and you also like woodwork. Can you tell me a bit more about what you like to do in your spare time? |
|  | I have a woodworking job and I make furniture once in a while, but the last couple of years, I haven’t really had much time. To tell you the truth, the Nobel Prize thing is kind of time consuming so I’ve kind of let that slide a little bit. But yeah, that’s something I enjoy doing, biking. I ride my bicycle to school, but that’s because there isn’t any parking on campus. Now I supposedly have a parking place from the university, but even then it’s faster to ride your bike than to drive your car. |
| Q8 | Your co-laureate Guido Imbens, you have known for a long time, haven’t you? |
|  | I can tell a pretty good story. I’d come to Berkeley and I was sitting in my office and he gave me a call. I mean, I knew him reasonably well. He gave me a call and said, “David, I’m going to get married to Susan and she’s got a job at Stanford. I need a job at Berkeley.” Cause he was at UCLA at the time. So I said, “Okay, I’ll see what we can do.” It turned out to be really good because he was a great addition to the faculty at Berkeley and really made a huge positive impact. Unfortunately, he and Susan got a job offer to go to Harvard and they went but then they came back to Stanford. |
| Q7 | How did you react when you heard the news about your prize? You came home very early in the morning and you just landed from a flight? |
|  | I had just flown back from Canada visiting my mother. I drove from the airport to our house in Sonoma County, which is a little bit outside of San Francisco area. I got there about one o’clock, our time in the morning and was having a shower. My wife said, “Well, I just got a signal on my cell phone that there’s a message on her home answering machine.” And then she played the thing and it said, well, you know, they’re calling from Sweden. Then we thought that it was actually my friend from Canada playing a joke. Then we figured out that it was actually a real Swedish phone number. |
| ID | 0808 |
| Biographical | I’m fortunate to be the son and grandson of enterprising and resilient people who aspired to build strong families and meaningful lives even when uprooted. My academic success is a tribute to my parents’ and grandparents’ hard work and readiness to try new things, to the value they placed on learning, and to their desire to engage with and improve the world. **North America** My mother, Shirley Sarah Bloomstone Angrist, was born in 1933 and raised in a Yiddish-speaking household in Montreal. Her parents and their siblings had immigrated to Canada from small towns in Lithuania in the mid-1920s. My maternal grandfather Velvl (William) and grandmother Pessl (Pauline) Bloomstone settled on Montreal’s Saint Urbain Street, an incubator of working class Jewish culture immortalized in the novels of Mordecai Richler. Pessl’s parents and other family members who stayed in Lithuania were murdered by the Nazis in 1943.  In the 1930s, Velvl’s six siblings left Canada for Israel to join the Shomer Hatzair kibbutz movement, which sat on the far left side of intrinsically left-leaning kibbutz ideology. Velvl and Pessl were Communist Party sympathizers, members of the pro-Soviet United Jewish People’s Order. Hoping for a new and better world that never materialized, they elected to stay in Montreal to run a struggling grocery shop. Their daughter, Sarah, was a strong student who became “Shirley” when a grade-school teacher pushed a name change, implying that “Sarah” sounded too Jewish. My mother earned her BA and a master’s degree at McGill University. Her spare essays evoke her family’s pre-war journey to the new worlds of Canada and Israel, and the rising fear and desperation among those who didn’t escape the Holocaust.  My father, Stanley Angrist, also born in 1933, was raised in Dallas, Texas in a family that originated in Ukraine and Poland. My father’s mother was Freda Beck, known to our family as “Nana”. Fleeing pogroms in Odessa after 1905, Nana’s family landed in Galveston, Texas, aiming to reunite with family already settled in Dallas. Her husband, Isadore Angrist, suffered a fatal heart attack in 1953 when Stan was a college student. I never knew Isadore, but I met his father, William “Papa” Angrist, who had immigrated to New York from Poland in 1888.  Stan attended Texas A&M University, which was then a military school. He enlisted in the Reserve Officers’ Training Corps (ROTC) to become part of A&M’s Corps of Cadets. Service in the Corps was challenging, and Stan’s time there was more challenging than most. In addition to losing his father, my father suffered a bout of polio, missing the 1955 Salk vaccine by only a few years. As an experimental subject participating in the 2020 Phase 3 trial of Moderna’s mRNA-1273 vaccine, I thought often of my father’s youthful encounters with disease and loss.  My parents met as college students at a Hillel-run Jewish summer camp in the Catskills and married not long after. Although endogamous in a religious sense, their 1955 marriage was unusual for its time – their parents hoped and likely expected that they would marry and settle close to home. Instead, they moved to Ohio, 800 miles from Montreal and a thousand miles from Dallas. On family road trips north and south, I learned the measure of these distances, both physical and cultural.  After graduating from A&M in 1955, Stan was stationed at Wright Patterson Air Force Base near Dayton, Ohio. The newlyweds lived in base housing while my father worked at the Air Technical Intelligence Center (ATIC). A soldier in the Cold War, Stan deciphered technical documents and clandestine pictures of Soviet aircraft and pilots. The more-or-less-known dimensions of a pilot’s head, he explained to me once, could be used to gauge the size of other pieces of equipment. My father had studied Russian in graduate school to meet a language requirement. Not coincidentally, I studied Russian in high school. My father and I now agree that Russian is a terribly hard foreign language and that it’s a wonder anybody ever learns it.  I was born in 1960 in Columbus, Ohio, where my parents were enrolled as graduate students at Ohio State University after my father completed his service. Shirley did her Ph.D. in sociology; Stan studied mechanical engineering. **Pittsburgh, Pennsylvania** In 1962, Shirley began teaching at the University of Pittsburgh, later joining Stan at Carnegie-Mellon University. CMU was then the Carnegie Institute of Technology – Carnegie Tech – an engineering school something like MIT. In addition to their strong engineering bent, both schools sit atop an extensive warren of steam tunnels (the CMU tunnels get a cameo in the book *A Beautiful Mind*, which recounts the story of Nobel Laureate [John Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/)). My younger brothers, Misha and Ezra, were born in Pittsburgh in 1964 and 1970, respectively.  Restless academics, both of my parents left CMU to pursue second careers. Stan became a columnist for Forbes Magazine and then a reporter for the Wall Street Journal; Shirley worked in government affairs at PPG Industries. In the late 1970s and early 1980s, my father also owned and managed a bookstore and a jewelry store in downtown Pittsburgh. Misha and I sometimes worked at the bookstore, not always enthusiastically. Our mother was a trail blazer, first as one of CMU’s few female faculty, then at PPG, where she confronted sex discrimination in the business world.  My parents left CMU but they never left Pittsburgh, where they live happily today. Their love of the city was tested in October 2018, when a gunman killed 11 people at the Tree of Life synagogue complex, including a member of their congregation, Dor Hadash. The city and community’s generous and caring response to this tragedy reinforced my family’s appreciation of our adopted hometown.  I remember little of my early schooling beyond many hours of TV after school and occasional adventures in *Frick* and *Schenley Park*. The cloud factory described in Michael Chabon’s *Mysteries of Pittsburgh* is an image from the time. Most days, my friends and I retired to someone’s house to watch *Kung Fu* and the original *Star Trek*, screened back-to-back on weekday afternoons. Star Trek now seems cloying and preachy (a personal connection notwithstanding: my mother attended high school and college with William Shatner, another Montreal Jew). As readers of *Mastering ‘Metrics* (my econometrics text with Steve Pischke) will know, Kung Fu made a lasting impression. The Kung Fu Panda franchise is funnier than the original series – our avatars, Master Joshway and Master SteveFu, come from Panda. But the notion of paths taken and paths forsaken lives larger in the original 1972 series; that’s where the econometrics lies.  At Pittsburgh’s Taylor Allderdice High School in Squirrel Hill, I was an indifferent student. For one thing, it was a two-mile walk to school. Pittsburgh is hilly, the San Francisco of the Midwest, so it was uphill both ways. At school, I most enjoyed print shop, where I used the facilities to silkscreen record album covers onto t-shirts (the cover of Black Sabbath Volume 4 was the apogee of this work). In middle school in the 1970s, my brothers and I participated in Pittsburgh’s experiment with busing for racial balance – in 8th grade, I was bused one day a week from majority-white Allderdice to Arsenal middle school in the Lawrenceville section of the city. My brothers were bused daily to the fortress-like Reizenstein middle school in the mostly-Black East Liberty neighborhood. In contrast with my relaxed time at Arsenal (where I took another shop class), Reizenstein was a tough experience: Misha recalls frequent altercations between students and staff; Ezra was assaulted. As a young teen, I wouldn’t have guessed that I’d spend decades researching and writing about the schooling (and busing) experiences of children in the US and around the world.  My high school years were filled with non-school activities: acting in a theater group based at the Jewish Community Center, studying photography, and working in my own basement darkroom. Starting at age 13 and through college, I held part-time jobs so as to have money for darkroom supplies, upkeep and insurance on the high-maintenance cars I owned from age 16, cigarettes, and dope. My first job was at the Gaslight Club in Shadyside, where the young busboy crew worked for a dollar an hour plus a percentage of waiters’ tips. Since then, I’ve been curious about the world of work – it’s no surprise that I became a labor economist. Home from college one summer, I drove a jitney ferrying passengers into town from distant Greater Pittsburgh Airport. The money was good, but the activity illegal – it was sometimes necessary to outrun PA PUC officials to keep driving. I enjoyed a brief return to this sort of work as an Uber driver in the summer of 2016, doing field work for a study of the rideshare business.  Exploiting Pennsylvania’s then-modest graduation requirements, I left school at the end of 11th grade in 1977, my diploma earned by taking two each of the remaining required classes in health, gym, and English. Because I had enjoyed working at summer camps for the mentally handicapped, I took a series of disability-related full-time positions. The first was a brief stint in a group home for the mentally handicapped, followed by a longer spell in a private residential facility for severely disabled children, and, finally, work in a state psychiatric institution. I was fired from the first and last of these jobs, a maturing experience. Forty or so years on, our son Noam had similarly rocky teen employment experiences, from which he says he also benefitted. **Oberlin, Ohio** In the fall of 1978, following a year and some months of full-time work, I enrolled at Oberlin College, three hours west of Pittsburgh by car. With poor high school grades, I was lucky to be admitted from the waitlist on the basis of my SAT scores, a couple short stories I’d written, and a campus visit. My engagement with economics – to become a lifelong love affair – began in freshman year with Robert Piron’s Econ 101 class (where I first encountered cold-calling, then on the receiving end). I was not initially a standout performer and needed tutoring in math. Working my way up academically, I was eventually invited to write an economics honors thesis. As a thesis-writing senior, I benefited greatly from the advice and guidance of Oberlin faculty David Cleeton, Hirschel Kasper, and, especially, econometrician Luis Fernandez. With no advanced econometrics course at the College, Luis enrolled me in a class of one, meeting weekly in his office.  I spent my junior year as a General Course student at the London School of Economics (LSE), my first time overseas. LSE’s year-long courses were built around reading lists featuring journal articles, with no interim graded assessments given before the final. Accustomed as we were to textbooks and frequent low-stakes exams and problem sets, American LSE students had to grow up fast. My attentive and generous LSE tutor, the distinguished Sir Partha Dasgupta, helped me make this transition.  My college years set me on two paths, one intellectual and one personal. On the intellectual side, work on my honors thesis, titled “Sample Selection Bias and the Nature of Unemployment,” was a revelation: I loved doing applied econometrics. My undergraduate thesis didn’t solve the econometric problem I set out to tackle, which was how to estimate the effects of variables like schooling on offered wages when offered wages are unobserved for those who don’t work. I tried to apply the work of Nobel laureate [James Heckman](https://www.nobelprize.org/prizes/economic-sciences/2000/heckman/facts/) and my future thesis advisor, Orley Ashenfelter. Successful or not, I found the effort satisfying. Summer work as a research assistant for CMU economists Allan Meltzer and Scott Richard also engaged me. My RA experience at CMU is described in Mostly Harmless Econometrics, my first econometrics book with Steve Pischke. Although I was but a greenhorn RA, I sometimes argued with Allan and Scott about the substance of their work. They treated me like a colleague, reflecting the lack of hierarchy that I’ve come to see as characteristic of academic economics.  As a thesis-writing Oberlin senior, I was all-in. Lucky for me, Oberlin’s honors program features an outside examiner (a role I have since played). The honors class of 1982 was treated to a few days with legendary Princeton labor economist Orley Ashenfelter, who read our drafts and gave a graduate-style oral examination. Fatefully, Orley followed up with an invitation, offering me a school voucher good for one Princeton Ph.D. **From London to Jerusalem** My personal path forked in London, while visiting the LSE as a college junior. London in 1980-81 was boiling over. Punk rock was an energizing force: as at Oberlin, I played raucous guitar in a band. But the culture of anarchy in the UK was clearly not meant for the likes of me. Protests sparked by Thatcherite reforms and racial tension spilled over onto LSE’s urban campus. At the same time, antipathy to Israel among British students sparked my interest in Zionism. Presaging similar efforts today, the British National Union of Students had “no-platformed” pro-Israel speech. I spent my spring break from LSE vacationing in Israel, connecting with my maternal grandfather’s kibbutz siblings and their brood, as well as with a high school friend, Mike Drescher. Israel was energizing, but, unlike London, also welcoming. Although Israel too was rocked by ethnic and political strife in the early 80s, I glimpsed the chance to participate there in a nation-building adventure. Mike was similarly inspired, and so we both vowed to return soon.  After graduating from college in 1982, Mike and I made good on our plan. I enrolled at the Hebrew University of Jerusalem in a master’s program in economics, while Mike worked as counselor at the University’s School for Overseas Students. To my dismay, I struggled to keep up with Hebrew University’s tough curriculum, delivered in fast-paced Hebrew (an econometrics course taught by renowned Canadian-born economist Ariel Pakes was delivered in slow-paced Hebrew, but no less challenging for that). Still, my spirits remained high. In 1982, I met my future wife, Hebrew University student Miriam Poznanski, a native of Acco (Acre) in Northern Israel. Her parents, Holocaust survivors Esther and Yakov, had met in a displaced persons camp in Italy and immigrated to Israel in 1948, where they raised Mira and her two older sisters in a tiny apartment.  Entranced by my new girlfriend and thrilled as ever to be in Jerusalem, I left Hebrew University but doubled down on Israel, becoming a citizen in March 1983. Eschewing the possibility of military deferment, Mike and I were drafted in the summer of 1983 for a two-year tour of duty in the Israel Defense Forces (IDF). While I was on active duty, Mira and I lived together in her small dorm room, an arrangement facilitated by my extended soldierly absences. As an added bonus, my brother Misha spent his junior year of college at Hebrew University in 1983–84. Misha and his friends offered me a second home-away-from home on brief weekend leaves.  Following a path then typical for Israeli immigrant soldiers, Mike and I enlisted in the IDF’s Nahal Brigade, an infantry unit. We aimed to serve in the Nahal’s 50th battalion, a well-known elite airborne (paratrooper) unit. For me, this required an initial foray into battle with army bureaucracy: I had been deemed unqualified for elite combat service by virtue of excessive myopia. This problem I fixed by convincing the Argentinian-born Army medic who’d been tasked with re-evaluating my fitness to look the other way while I studied his eye chart.  Paperwork sorted, I served more or less as planned: basic training, further training in field communications, armored personnel carrier driver’s ed, jump school. Training was followed by active duty deployments in Lebanon, where I served mostly in the Bekaa Valley and spent a winter on scenic, frozen Jabal el Barouk. Lucky for me, by the time of my deployment, Israel’s ill-considered Lebanon War was winding down (a few friends were not so lucky). My battalion was one of the last to withdraw behind a new southern security line in the winter of 1985. The Lebanon War and its attendant catastrophes fractured Israeli society like nothing before or since. Although Mira and I participated in the occasional antiwar protest, I was – ironically, though not, I think, exceptionally – isolated from civil strife by virtue of my service.  My army service concluded with six months of farmwork on a kibbutz, an essential part of the Nahal experience. On Kibbutz Netiv HaLamed Heh near Jerusalem, I was welcomed by my Aunt Pessi Bloomstone’s daughter, Netta and her husband, Holocaust survivor Nachum Bogner. Kibbutz life was idyllic. But I found farmwork exhausting and relentless, farm animals smelly and unforgiving, and farm machinery troublesome and dangerous. I looked forward to time with Mira in Jerusalem.  In the Spring of 1985, Mira and I were at a crossroads. She had finished her B.A. in social work; I was ready to return to Hebrew University. Visiting my folks for Passover, my father suggested I check back with Orley Ashenfelter to see if his offer of a Princeton Ph.D. was still good. On a quick visit to Princeton, he invited me to enroll that fall. Mira and I married in August 1985 on Kibbutz Ein HaShofet (home to Pessi Bloomstone and other Bloomstone offshoots) and headed to Princeton shortly thereafter. **Princeton, NJ** Princeton was paradise. A couple of mystifying macroeconomics classes notwithstanding, I felt I was where I was meant to be. I was particularly energized by the camaraderie and intellectual ferment of the Princeton Industrial Relations Section, where I found my second thesis advisor, future co-laureate David Card (also an Ashenfelter student). The Section at that time was a cramped warren of offices in the basement of Princeton’s Firestone Library, a spaced shared by faculty and graduate students. I shared an office with Brian McCall, and overlapped with Dwayne Benjamin, Janet Currie, John DiNardo, Tom Lemieux, and Steve Pischke. I soon met an additional Ph.D. advisor in the classroom, assistant professor Whitney Newey, later to become a colleague and friend at MIT.  At Princeton, my debt to Orley quickly accumulated: not only had he given me a golden ticket to a Princeton Ph.D., it was Orley’s idea to use the draft lottery to gauge the long-run consequences of Vietnam-era military service. The draft lottery project became my Ph.D. thesis. Other aspects of my work at Princeton are detailed in my [Nobel Lecture](https://www.nobelprize.org/prizes/economic-sciences/2021/angrist/lecture/), while [Card’s Lecture](https://www.nobelprize.org/prizes/economic-sciences/2021/card/lecture/) paints a picture of the Section from his slightly older vantage point.  Mira and I left Princeton with a precious gift: our infant daughter, Adie, born in March 1989 as I was giving an academic job talk. Adie was born at St. Francis Medical Center in Trenton, NJ, where Mira had been working as a psychiatric social worker. Only 25 minutes by car, gritty Trenton was a world away from leafy Princeton. The St. Francis team gave Mira and newborn Adie the royal treatment.  Alan Krueger came to the Industrial Relations Section as an assistant professor in my third year of graduate school (again thanks to Ashenfelter, who recruited him). In 1988, Alan and I embarked on the project that led to our 1991 compulsory schooling paper. “AK91” (Angrist and Krueger 1991) uses quarter of birth (QOB) to construct instrumental variables (IVs) for schooling in a wage equation. This project began as a discussion of empirical strategies that might reveal the causal effects of World War II military service on veterans’ earnings. The WWII study was my first working paper with Alan, released in 1989, though published only in 1994. Alan and I had discovered that WWII veterans were drafted in birthday order, and so an IV identification strategy was born. But it remained to discover exactly which endogenous variable this newborn birthday instrument was to be an instrument for.  The QOB project led to the terrifying weak instruments problem, first demonstrated for the AK91 research design in a 1995 paper by Alan’s Harvard classmate John Bound and Bound’s students David Jaeger and Regina Baker. It had long been known that two-stage least squares (2SLS) estimators (used to combine many instrumental variables) are biased. So what? The BJB paper shows that some of the 2SLS estimators in AK91 are biased towards the corresponding ordinary least squares (OLS) estimates. Our findings, which indeed showed OLS and 2SLS estimates to be close, might therefore be seen as spurious.  The many-weak IV story broke in 1993 when I was back in Israel, doing a stint of military reserve duty in the Judean desert. Tired and anxious in an isolated military outpost, I couldn’t have been more miserable. In June 1993, Alan and I withdrew an accepted paper that used draft lottery dummies to instrument schooling because, in light of BJB, we understood that the 2SLS estimates in this paper were badly biased. But BJB soon became an inspiration for creative solutions to weak-IV problems. Some of my work on this topic was done with my co-laureate Guido Imbens, as well as with Alan. Happily, when these new solutions were tried, the AK91 findings held up.  That period of trepidation, excitement, and understanding was a high point in my life and, I believe, in Alan’s life as well. I remember the joy in our work, the exhilarating sense that we were on to econometric applications that were novel, interesting, and useful. Had he lived, Alan (who collaborated extensively with our co-laureate David Card) would surely have won a Nobel too. **Cambridge, MA** Before returning to Israel in the summer of 1991, I had taken my first academic job in the Harvard Economics Department in the fall of 1989. I apprenticed there to the masterful econometrician and teacher, Gary Chamberlain. Gary was a towering figure in our field and a patient and thoughtful sounding board for me. He schooled me in the art of econometric instruction when we taught Harvard’s Applied Econometrics course together.  At Harvard, I intercepted my co-laureate, friend, and collaborator Guido Imbens upon his arrival in 1990. Guido and I overlapped at Harvard for just one year, but we have been together one way or another ever since. The Angrist family lived in Harvard’s Botanic Gardens housing complex, about a mile up Garden Street west of campus, while Guido and his first wife, Alison, lived down the street. My Nobel Lecture details my partnership with Guido, the gestation of the LATE Theorem, and how we came to team up with eminent statistician Don Rubin. In 2002, I was thrilled to serve as best man when Guido married Susan Athey. **Jerusalem, Brookline; Jerusalem, Brookline** Two years into my Harvard appointment, Mira and I made good on our commitment to return to Israel. But not before our son Noam was born in Boston in July 1991. With 6-week-old Noam and toddler Adie in tow, we took up residence in Hebrew University’s French Hill faculty apartment building, walking distance from the East Jerusalem Mount Scopus campus. We enjoyed life in Israel, connecting with friends and family. In 1993, we bought a lovely apartment in the Jerusalem suburb of Mevaseret Zion, with a spectacular view of the city.  My time in the Hebrew University Economics Department was exciting and productive, due in large part to a wonderfully fruitful collaboration with my Hebrew University colleague Victor Lavy. Among other rewarding endeavors, Victor and I discovered and deployed the Maimonides Rule research design, exploiting the fact that Israel caps elementary school class size at 40 to estimate the causal effects of class size reductions on student achievement. Our Maimonides paper was one of the first in economics to use a regression discontinuity research design.  In addition to Maimonides’ Rule, I’m especially proud of two studies of the Palestinian labor market. After long negotiation, and in the wake of the 1993 Oslo Accords between Israel and the Palestine Liberation Organization, Israel’s Central Bureau of Statistics allowed me to be the first scholar to work with micro data from the Territories Labor Force Survey (TLFS). The TLFS was a large household survey, something like the Israeli Labor Force Survey, carried out by local enumerators in the occupied West Bank and Gaza Strip. My first TLFS study examined the impact of an influx of Palestinian college graduates on the economic returns to schooling in the Territories. The second used the Palestinian Intifada as a natural experiment that shifted the supply of Palestinian migrant labor, thereby identifying Israeli employers’ elasticity of demand for their Palestinian workers.  My time at Hebrew University was fruitful and fun, but the pay was low and the cost of living high. I also came to resent IDF reserve duty, which pulled me away from research and my young family. My Israeli friends suggested I should have thought of that when I finagled my way into a combat unit a decade earlier.  In 1994, I was invited by then-department-chair (and future governor of the Bank of Israel) Stanley Fischer to visit MIT’s storied Economics Department. Mira, the kids, and I decamped from Jerusalem to Brookline, Mass. On that visit, I entertained a stream of senior faculty asking probing questions. This felt to me like something between a job interview and psychological screening. I recall the late Rudi Dornbusch stopping by to observe that I seemed “reasonable enough.” Not long after, newly-installed chair Paul Joskow offered me a tenured appointment, which I was thrilled to accept in 1996, one year after we had returned to Jerusalem. MIT and Brookline have been our academic and family homes ever since.  I quickly came to appreciate the broken concrete of MIT’s east bloc campus (since spruced up), which proved fertile ground for my kind of applied econometrics. The turn of the century found me collaborating with MIT colleagues David Autor and [Daron Acemoglu](https://www.nobelprize.org/prizes/economic-sciences/2024/acemoglu/facts/), steering the world’s top graduate program in labor economics.  A little over a decade after I joined the department, MIT got even better. In 2008, Parag Pathak arrived, and an enduring friendship and partnership was born with our first paper in 2009 (published in 2011). This work was the fruit of a delicate negotiation between Parag, Atila Abdulkadiroglu, and me on one side, and Tom Kane and Sue Dynarski on the other. Sue, my former Ph.D. student, closed the deal, bringing our competing teams together to share data agreements and empirical strategies. This collaboration led to the first study to exploit centralized school assignment algorithms for the identification of causal effects. The paper is also of major substantive interest, the first to document the remarkable success of Boston’s charter schools.  In 2011, Parag and I founded the School Effectiveness and Inequality Initiative (SEII), with the help of then-department-chair Ricardo Caballero. SEII has since evolved to be Blueprint Labs, with ever-widening horizons. This expansion includes a new healthcare initiative led by Nikhil Agarwal. I’m also proud to work with co-lab-director David Autor and our former student Mandy Pallais on a standard-setting randomized trial examining the effects of financial aid.  Our lab’s work has produced not only scholarship and policy impact, but also intellectual property for which there is a real market. The COVID pandemic notwithstanding, in 2020, Parag and I launched a startup that aims to help school districts and other organizations solve high-stakes matching problems and make the most of the resulting data. Avela Education is led by our gifted CEO, Greg Bybee. I’m gratified beyond description to see our academic research deployed to improve education and training in the real world.  As well as having been blessed with a remarkable family, I’m fortunate to have found so many talented and generous advisors, friends, and colleagues. At every juncture, I’ve benefited from more than my fair share of wise guidance, warm companionship, and selfless collaboration. I try to remember to count my blessings, but there are, happily, too many to count. |
| Autobiographical |  |
| Podcast | **“I never stop thinking about my work”** Joshua Angrist believes that to be a good labour economist, you should have had some real life job experience. In this podcast episode, conducted in May 2022, Angrist tells us about his disinterest in school and how as a teenager he was more interested in earning money and maintaining his car. His later surprising and instant connection with economics led him to dedicate his life to his research: “I never stop thinking about my work.”  He also speaks about being awarded the 2021 prize in economic sciences and speaks about how the award has affected his life; “It was wonderful to win the Nobel Prize but I am the same guy I was on October 10th.”  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0808=JA  Angrist: Hello?  Adam Smith: Oh hello, am I speaking with Joshua Angrist?  JA: Yes, speaking.  AS: Hello, this is Adam Smith from Nobelprize.org  JA: Hey Adam.  AS: Hi.  JA: Great name, great name.  AS: [Laughs] Many, many congratulations.  JA: I’m sure you’ve heard that before. Yes.  AS: Yes, I hear it particularly on this day of the Nobels.  JA: [Laughs]  AS: Many, many congratulations.  JA: Thank you, thank you, I’m honoured, I’m thrilled.  AS: How did you actually discover that you had been awarded the prize in economic sciences?  JA: I woke up early in the morning for no particular good reason and I saw that my phone was flooded with text messages, and I had had it off because I was not expecting anything special this morning, so I had to look at the phone. And then I tried to find the phone number that I should call to figure out if it was true.  AS: So you came through to Stockholm, to the Nobel Foundation offices.  JA: Actually I called someone at MIT who had called me.  AS: You spoke to someone who confirmed it?  JA: Yes, our press office.  AS: Right, great. Given that you’re in the business of collecting evidence for causal relationships, yeah, you wanted to be certain.  JA: Yes, I wanted to be sure … I didn’t really expect it, I suppose everybody says that, and you’re supposed to say that. But I think in my case my lack of preparation is evidence that I didn’t expect it.  AS: How does it feel to have received it?  JA: It’s wonderful, it’s overwhelming. It’s you know, it’s the beginning of the day here in the United States, and I’m trying to absorb it. I am especially happy to be grouped with Dave Card and Guido Imbens, who are certainly worthy, and, you know, I’m lucky to have been able to work with them and learn from them.  AS: Because you were David Card’s student for a while weren’t you?  JA: Yes, I was one of his graduate students. I had three wonderful advisors, but he surely had a huge influence on me and my development as an empiricist. Then Guido Imbens was one of my first collaborators, and we embarked on a path of methodological work that proved to be very rewarding to us and, and influential.  AS: What was it about that relationship in the early 90s that kind of spawned those papers. What was special about the combination of the two of you?  JA: We enjoyed working together, and we found that we were interested in the same kind of problems. We were both assistant professors at Harvard. I had been there for one year. Guido came in the second year, and then I left after that actually. I moved back to Israel where I taught for five years, and at Hebrew University. But Guido and I kept collaborating and we, we had started on something that we thought was interesting and important. I guess we didn’t really know quite how important, and we set out to kind of figure out what instrumental variables, which is the statistical technique that Guido and I have studied together most closely. What we really learnt from that is, it was based in part of my applications, and it developed with his theoretical insights, and we kind of went back and forth between, you know, applied questions and methodological questions, and came up with a whole framework. People today call it the LATE framework. In the beginning it was one paper, and then that paper didn’t get published, and then it was the second paper that did get published, and then it developed further.  AS: Your work is all about finding the basis for people’s beliefs, if you like. So can you give me an example of, for instance, in education, something that is a popularly-held belief that actually is not supported by evidence, that you’ve found?  JA: That’s an interesting question Adam. I don’t know if my work is about people’s beliefs, but it’s about, you know, people can believe what they want to believe. But it is about discovering what is true in the sense of ‘is there a causal effect of something?’ In other words, if you did a particular thing, maybe related to education, what would happen to you. [Phone beeps] My phone is going off here. A great question of that is something that many people believe, for example, that highly-selective public high schools, or highly-selective universities are the key to a successful career, especially in my world where we work in such institutions on the university end. And what I’ve been able to show using some of the techniques developed in work with Guido and in, in the training that I had with Dave Card, is that that’s often illusory. It reflects a phenomenon we call selection bias, that people who go to those schools tend to do well in life, but they were going to do well in life anyway, that’s how they got into the schools. So that’s a great example of selection bias. And the techniques that all three of us have worked on are about reducing or mitigating selection bias in empirical estimates, and using mostly observed data to get at the kind of evidence that we would like to have, say, from a randomised trial.  AS: I would like to talk much more about this, but your phone buzzing is going to be the story of the day really, isn’t it?  JA: [Laughs] Yes.  AS: What would be very nice is if we could have a longer conversation when things quiet down a bit for you, although I think it will be quite a little time before things do quiet down.  JA: Yes, probably.  AS: I don’t know, I don’t know, you sound pretty, you sound pretty together and calm at the moment.  JA: I like to talk about my work, so that always calms me down.  AS: I think I hear a coffee cup rattling there as well, maybe.  JA: Yes, I have a little coffee here.  AS: That’s an important constituent of the day I think. I shall let you get on with your day. Good luck with it – it was a pleasure to speak to you, congratulations.  JA: Thank you so much Adam. Great speaking to you. |
| Interview |  |
| Q37 | Can you tell us a little bit about your childhood? |
|  | I was born in Columbus, Ohio and I grew up in Pittsburgh. We moved to Pittsburgh when I was three. My parents were academics. They got their PhDs from Ohio State and my dad had also been stationed in the air force and an air force base near Columbus, Ohio, where I was born. My parents got their first jobs in Pittsburgh, so we moved to there and eventually they both left academia, but they never left Pittsburgh. That’s where I grew up, went to elementary school, middle school, high school. |
| Q38 | We understand you weren’t particularly academic as a youth? |
|  | Yeah, I wasn’t a very good student and actually my parents didn’t like being professors very much so they both quit that. So maybe it’s a little ironic that I ended up being a professor. I got in some trouble for various reasons in school and I didn’t really get engaged with high school at all. After maybe ninth grade, I didn’t take any of the accelerated college courses and I just wanted to finish as quickly as possible. So I managed to finish high school in 11th grade by meeting all the state requirements for diploma. I took two English classes, two health classes – that was sex education, badly done – and, two gym classes, physical education classes. And I was able to get my diploma and leave high school. I worked for a little bit more than a year and then I decided probably I should go to college. |
| Q39 | Why were you so keen to leave school and start working? |
|  | Well, I hadn’t found anything that interested me in school. There were things that interested me. I had a lot of hobbies. I had a photographic dark room – this was before digital photography – in my basement where I developed my own film. I enjoyed print shop in high school and we made t-shirts out of rock albums covers and that sort of thing. I had various other hobbies but nothing in school really engaged me. I liked working partly because I thought working was fun, but partly also I wanted to have money. Once I was 16 I bought my neighbour’s car. So I had this awesome powerful car, but it was high maintenance and had terrible gas mileage. It got six miles to the gallon, so I always needed money to pay for gas, repairs, and insurance. I had started working anyway – I had my first job when I was 13, I think I worked in a restaurant as a bus boy. Then later I got interested in working with people with mental disabilities and I worked at camps. So that’s what I went to do. |
| Q3 | So where does your passion for economic sciences come from? Was there a particular person that influenced you? |
|  | I think, like many people, I got excited about my field through an inspired and inspiring teacher. When I arrived at Oberlin College in Ohio, I didn’t know what I wanted to study. I thought maybe I would continue in the special education field so I took psychology. But already in my household we talked about economics, my father in particular was interested in economics, even though he was an engineer. I had some sort of dinner table background so I took Econ 101 and I really liked that a lot. I had a wonderful teacher named Bob Piron and I thought his class was so much fun. I liked the material. I also liked the way he taught. It was sort of a high-pressure classroom. Later I adopted that style. I think that’s going out of style now, but it was lots of cold calling and a very challenging dynamic classroom atmosphere. It wasn’t relaxing, but he was also a very funny guy. I enjoyed it a lot and I connected with the material I saw. I had some affinity for it. It wasn’t that it was easy, but I was willing to spend the time on it. So I took a lot of economics and I liked almost all of it. |
|  | You yourself say that you weren’t a good student but a lot of your research is around education. Can you tell us a little about that? |
|  | It is sort of inconsistent with my personal history that a lot of my scholarship shows that education is very important. It’s an important determinant of people’s earnings. I didn’t think so, but then I was a teenager. That’s one thing. And anyway, I’m only one data point. One thing I teach my students is try not to learn from your personal experience. Your personal experience might be idiosyncratic and not really informative. Everybody, for example, has a view about schools and what schools are good. I try not to pass judgment on that until I see the data. Another thing to keep in mind is that, even though I wasn’t a great student and I grew up in a fairly modest household, it was a very educated household. So I had a lot of advantages and I had the opportunity to goof off in high school and learn nothing and yet recover. I think a lot of people, particularly people whose parents aren’t very educated, they’re not going to get that second chance. |
| Q41 | Your [prize in economic sciences lecture](https://www.nobelprize.org/prizes/economic-sciences/2021/angrist/lecture/) was very engaging. How important is it to communicate about your work? |
|  | It’s very important. I like doing research, but I also love teaching and a good teacher is an evangelist for their field. Particularly when you’re teaching undergraduates, you have the opportunity to change somebody’s direction by convincing them or showing them that economics is really fascinating and rewarding. I think about my own experience in this case, even though I don’t want to learn too much from it, as I said, but I had charismatic teachers and they affected my direction and I think maybe I can have that effect on my students. That’s what I aspire to. |
| Q22 | What qualities do you think you need to become a successful research scientist or economist? |
|  | If you want to be a successful research scientist, you have to really love what you’re doing. You have to find it interesting. You can’t just be going through the motions. When people come to talk to me about graduate school, I caution them that it shouldn’t be a momentum play – meaning that you’ve been a good student all your life, you’re bright, you did well in college and so you figure you should be go to graduate school. I think that’s not a very good scenario for success because graduate school is really not more of the same. There’s a different quality to it. You have to be very self-motivated and you have to be able to set your own goals and push yourself to meet them. Nobody’s waking you up in the morning and saying this is what the team has to do today. There are moments like that, but it’s mostly not like that. And so if you don’t love it, you’re not going to get it done.  That would be the first thing. And then you have to have some affinity for it. You have to have the right skills, the right mindset within economics. I suspect within other fields there’s more theoretical and more empirical type of people. Even the prize I share, there’s two parts. One is for David Card and his empirical work. He was actually one of my thesis advisors. I share [the other half] with Guido Imbens for methodological work, but I’m an empiricist at heart. |
| Q8 | You are good friends with your co-laureate Guido Imbens, and were even best man at his wedding. Can you tell us a little about your friendship? |
|  | We’ve been friends since our academic childhood. I got my PhD in 1989, and I went to Harvard for my first job. I was only there for two years, but luckily Guido came in my second year and we were also neighbours. And, you know, I wasn’t very excited when we hired him. I thought his thesis was kind of boring. It was very dry and technical, but we soon hit it off and became friends and we saw a lot of each other. We started talking about these kind of applied problems that I was interested in, like using the draft lottery to estimate the effects of military service, and then some subsequent work I had done with Alan Krueger using season of birth to estimate the economic returns of schooling and Guido and I started saying, well, what could we say formally about what you’re actually learning when you do that? |
| Q42 | Do you think friendship’s important in work and collaboration? |
|  | It can be, but it’s not essential. I think it’s more fun to work with your friends. I tell my students that you want to pick your collaborators as carefully and thoughtfully as you pick your spouse. If it goes badly it can be very painful and hard to unwind. Certainly, when you’re friends with the people you work with, you worry less about that. Academia is very competitive and the stakes are seen as very high for the participants. Society maybe doesn’t care too much who wrote which paper or who gets credit for which idea, but we care. That can be a source of great tension. So you need to trust your collaborators and you trust your friends. Not everybody I’ve worked with is a close friend. I don’t think that’s essential, but it’s definitely more fun. |
| Q10 | Talking about advice to students, is there one piece of advice that you would give to a young person interested in research? |
|  | I talk to a lot of people who want to have careers in research, and I try to give them a clear picture of the life of a scholar. Some of it’s fun, a lot of it’s a drag – every job has its good parts and bad parts. We serve on committees and sometimes we disagree within the department and those can be unpleasant situations, a little bit like a family quarrel that can be very tiring. So you have to be very motivated. You really have to believe that if you want to have a career in research. I always say, who’s your role model? What’s the scholar you want to be, what sort of work you want to do, tell me some paper you wish you’d written. I try to get them to think about it very concretely, not as an abstract. And if they’re excited about that career, I think that bodes well. |
| Q28 | It sounds like you are very passionate about your research – what do you enjoy doing outside of work? |
|  | I like my work a lot and I’m sort of obsessed by it, so I never stop thinking about it, but I do have some hobbies still. I’m a cyclist. I ride every weekend. Sometimes I go mountain biking or road biking. That’s my main hobby. I ski a little in the winter and now I have a new hobby, which is even more rewarding – that’s grandchildren. My family is very important to me and the highlight of the week is definitely seeing the grandchildren. |
| ID | 0809 |
| Biographical | **Introduction** In this biographical sketch I will discuss part of my personal and academic journey, how I originally got interested in econometrics, and how I continued in this area as it changed from a field where causal inference was almost non-existent to one where causality is now explicitly a major part. During the same time that causal inference became a major part of econometrics, it also flourished in other disciplines with statisticians, political scientists, psychologists, epidemiologists, and computer scientists all bringing new questions and methodological perspectives to the table. The applications range widely from biomedical to social science ones, generating interest among researchers and policy makers in academics, government as well as private sector organizations. I see this prize therefore partly as a recognition of the importance of this general interdisciplinary enterprise and hope it serves to further invigorate the field. **My Early Years** I grew up in the Netherlands. I was born in Geldrop, a small town outside Eindhoven, on September 3rd, 1963. I have one brother who is a year and a half older than me, and a sister who is three years younger.  I do not remember much from the six years I spent in Geldrop. When I started primary school, we moved to the nearby town of Eindhoven. My father worked at the Philips electronics company whose headquarters were in Eindhoven and which at that time was the biggest employer in the Netherlands. Eindhoven was essentially a company town with the local professional football club, as well as most of the cultural institutions and parks funded and often named after the founders of the Philips company and some of their relatives. My father had studied physics in college for one year before taking a position at Philips for financial reasons. He spent his entire career at Philips, initially on the engineering side, and later in the marketing and sales of oscilloscopes and other equipment. My mother had also worked at Philips before they got married. Both my parents later went back to college, my mother when my siblings and I were in high school, and my father after he retired from Philips. I went to a Catholic primary school in Eindhoven. Both my siblings and I excelled in school. It all came relatively easy to us, and we enjoyed having our father give us mathematics problems in the evening. Typically, these involved long sequential calculations that − close to the end − included multiplication by zero to make them easier to check for him. At times we were bored in school with the curriculum, and I recall a year when my brother and I would work through a mathematics book early in the morning before school, simply because we enjoyed doing so. There was little pressure from our parents to study hard, or any sense that we were unusually good academically.  When we were living in Eindhoven my mother also got involved in local politics. Initially she focused on persuading the municipal authorities to provide more playgrounds for the children in our neighborhood. She organized and successfully led a group of parents to lobby the authorities. She was a remarkable and strong-willed person with firm principles, and not easily dissuaded by peer pressure or fear of being different. Once a year the housing corporation would come and paint all the front doors of the rental houses, all owned by Philips, mustard yellow. Every year the following day my mother would paint our front door black, much to the embarrassment of my siblings and myself at the time. Later she became involved in groups agitating for change in the Catholic Church. At the time the custom in the weekly religious services in the church was to collect money from the parishioners twice. Once for charity, and once for operating expenses for the local parish and the diocese. Out of unhappiness with church policies she would donate liberally for the charities but refused to donate any amount for the parish and diocese, often leading to a silent standoff with the person attempting to collect the money. Again, at the time this was a source of great embarrassment for us, but now I see that many of the character traits that made me successful in my scholarly endeavors, including stubbornness, came from her. My father was more easy-going. His outside interests include gardening. He had a vegetable plot, in Eindhoven, and later in Deurne, that he rented from a local organization, and would grow a lot of our vegetables there, long before organic gardening became fashionable.  After six years in Eindhoven we moved to Deurne, a small town about 20 miles east of Eindhoven. My father continued working for Philips in Eindhoven and would commute by train. In Deurne I started high school at Peelland College Deurne. Within the Dutch educational system this was an academic-track high school. It was led by a visionary principal, Theo Hoogbergen, who expanded the school substantially and made it a great fit for me with many committed teachers and extracurricular programs. The choice had been between Peelland College and the more old-fashioned Gymnasium next door, which was run by a religious order. The decision was an easy one when we visited Peelland College, and Hoogbergen himself helped us fix the tailpipe on our car which broke down when we were about to leave.  Early on in high school my favorite subjects were mathematics and Latin. I enjoyed translating poems by Marcellus and reading parts of Caesar’s *De Bello Gallico*. In the 4th (out of 6) year of high school I started taking an economics class. I was not particularly taken with the subject. Moreover, I had a dispute with the economics teacher that led to me being banished from the class for about three weeks. The dispute involved the solutions manual for the textbook we were using, and which out of principle I refused to turn in on the grounds that I had paid for it. The dispute got resolved when the principal caught me wandering the hallways, and I went back to class. During my high school years, we had school trips to Prague (still behind the Iron Curtain at the time) and Rome, both making a great impression on me, and making me more curious about exploring the wider world.  Towards the end of my high school years in the Netherlands I had to decide what college major to apply for. In the educational system in the Netherlands, students apply for a specific major, rather than to a particular university. Once at the university it is possible, but not completely straightforward, to change majors. This does lead to some inefficiencies in the educational system, because potential students often have little sense what a particular major will actually involve. At that time, I enjoyed mathematics, and both my siblings ended up studying mathematics in college, but I wanted to do something more applied, with more direct relevance for society. My economics teacher, with whom I was at that time on better terms after the initial banishment from class, introduced me to a small book by [Jan Tinbergen](https://www.nobelprize.org/prizes/economic-sciences/1969/tinbergen/facts/), the co-winner, with [Ragnar Frisch](https://www.nobelprize.org/prizes/economic-sciences/1969/frisch/facts/), of the first Sveriges Riksbank Prize in Economic Sciences in memory of Alfred Nobel in 1969 [Tinbergen, 1941]. Tinbergen was an inspiring figure in the Netherlands. Beyond his scholarly achievements, he had been a tireless institution builder. He founded the Bureau for Economic Policy Analysis, which became an authoritative non-partisan voice in Dutch policy circles, and later worked for the League of Nations and the [United Nations](https://www.nobelprize.org/prizes/peace/2001/un/facts/). Curiously he not only won the Prize in Economics, but was also nominated for the Nobel Peace Prize. In addition, his brother [Niko Tinbergen](https://www.nobelprize.org/prizes/medicine/1973/tinbergen/facts/) was awarded the Nobel Prize for Physiology or Medicine. Tinbergen also established econometrics as an academic discipline in the Netherlands, separate from economics. This was partly out of unhappiness with the economics program at the time. This was a bigger accomplishment than it may seem, because in the Netherlands establishing a new academic discipline requires formal government approval. In this program the term “econometrics” was used in a broader sense than it is used nowadays, and more in the spirit of the way it was interpreted by the founders of the Econometric Society in 1930: it included mathematical economics and operations research. The book I read, and more generally Tinbergen’s story [Dekker, 2021], inspired me to enroll at age seventeen in the econometrics program at Erasmus University in Rotterdam, where Tinbergen had spent most of his academic career. I have continued to study econometrics ever since. **My Undergraduate Years** The econometrics program in Rotterdam was a fabulous program. Beyond Tinbergen the program had been heavily influenced by Henri Theil, who led the program for many years and who made it into an internationally renowned research environment [Kloek, 2001]. The program was very small. There were about sixty first year students in the program the year I started (compared to over 1,000 in the regular economics program), and by the end of the year there were about twenty-five left. That was the result of the program being quite challenging. It had that reputation in general, and so all the students who entered the program were quite strong, but still, many found it hard to keep up with the workload and switched to the general economics program, which was much easier. Because the program was so small, we had a lot of direct interaction with the faculty. The faculty took great pride in the program and its illustrious history.  The faculty did make us work hard. We started in the first year with the microeconomics course that was also intended for second year students in the economics program. I particularly recall an operations research course by Alexander Rinnooy Kan. He had just spent a year in the US at MIT and Berkeley, and exuded brilliance. It was the first time that I actually started questioning whether I was smart enough to stick with the program, and it forced me to work harder than I had ever done before. The main drawback of the program was that it was largely focused on technical material. This made it extremely good preparation for a PhD program in the United States, but it left us somewhat unprepared for discussions of real-world economic problems. I remember preparing for an exam in macroeconomics after we had studied theoretical models for economies that included markets for goods, money and bonds. At the end of the semester, when studying for the exam I realized I did not actually know what “bonds” were, even though we had discussed models with bonds for much of the semester. To my surprise none of my classmates had any idea what bonds were either, suggesting that while we might have been comfortable with the technical part of the material, we were not so clear on the substance!  By the end of my second year, I was more comfortable with the demands of the program. I registered for a double major (the second being social history) and started working as a research assistant in the international economics group for Jean-Marie Viaene and Caspar de Vries. Both had done their PhD in the US, and made that sound like an exciting possibility, even if a somewhat remote one.  During my third year I also made another small, but consequential decision. Marcus Berliant visited from the University of Rochester and offered a course in mathematical economics. Specifically, the course was on general equilibrium with infinitely many goods. We had not even done general equilibrium with a finite number of goods in the [Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/)–[Debreu](https://www.nobelprize.org/prizes/economic-sciences/1983/debreu/facts/) sense, so it was not clear why this was a good course to take, but I was curious and registered for the course. Marcus taught the course incredibly well, with problem sets every class that really forced me to engage deeply with the material. The mathematical level of the course put most of the students off, though, and after the third class I was the only student left, with a couple of faculty attending, presumably out of politeness more than anything else. This caught Marcus’ attention, and he encouraged me to think about doing a PhD in the United States. This was not just cheap talk on his part: when I applied later for a PhD at Rochester he managed to get me an offer of a full scholarship. **England** Towards the end of my third year, I had applied on a lark for an exchange program to spend a year at the University of Hull in Kingston upon Hull in the north of England. Together with a fellow econometrics student, Wilbert van der Klaauw, now at the Federal Reserve Bank in New York, I went to Hull, initially for the academic year 1984-1985. At the University of Hull, we both enrolled in the master’s program in economics. The head of the department, and senior econometrician, Anthony (Tony) Lancaster, was very surprised and pleased to suddenly have two well-prepared econometrics students in his class. Tony was working on duration models at that time. He had finished an influential paper on that a few years earlier [Lancaster, 1979], and during my time in Hull he was finishing a monograph on duration models [Lancaster, 1990]. He taught us from drafts of various chapters and had us do empirical exercises in this area. Tony’s teaching style involved a lot of silences. A student would ask him a question, and rather than answering, he would let the question hang in the air, while he pondered it. These silences were even more emphatic because he would get out his snuff box and snort some, while he and we were pondering the question. Sometimes he would respond with a different question, often one that at first did not appear to be connected to the question that had prompted it. But eventually we would figure out the connection and get to the answer. It was a very effective way of forcing us to engage deeply with the material.  By the end of that year, I had obtained a master’s degree. There was now little point in going back to finish an undergraduate degree in Rotterdam. Moreover, Lancaster was negotiating a move to Brown University and he asked Wilbert and myself if we were interested in staying another year in Hull as research assistants and then doing a PhD at Brown. We both gratefully accepted and moved to Providence, Rhode Island, in the United States. Both of us ended up staying in the United States, with Wilbert van der Klaauw moving from New York University to the University of North Carolina and eventually to the New York Federal Reserve Bank. **Graduate School at Brown University** The PhD program at Brown University suited me well. It exposed me to much more economics as opposed to econometrics than I had seen in Rotterdam and Hull. In particular the macroeconomics courses by Peter Garber and Herschel Grossman made much more sense to me than the earlier courses I had taken. Because I had taken most of the econometrics classes Brown had to offer, I took some classes in the applied mathematics department on Bayesian statistics. The workload was much higher than it had been in Hull, and during my second year I started questioning whether I really wanted to continue in the program. I even applied for some jobs outside of academics. When I read about a position at a bank in New York that required a master’s degree in economics, as well as fluency in Dutch, I was sure I would be a shoo-in for the job. However, I did not even make it to the interview stage, and ultimately decided to buckle down and continue in the program.  I also enrolled through an exchange program in some courses at Harvard University, including one with Gary Chamberlain that made a big impression on me. In my fourth year of the PhD program Brown, I moved back to Holland to take a position at Tilburg University, not quite sure whether I wanted to spend the next phase of my career in the US or Holland. Arie Kapteyn, dean at the University there, had met me at a conference, and always looking to expand his econometrics group, invited me back. I did go on the US job market though. With considerable good fortune I got an offer from Harvard University and that settled the question of Holland or the US. Harvard had not actually interviewed me at the annual meetings, but after other candidates had not impressed them, and faced with the prospect of not having enough faculty to teach some of the required econometrics courses, Gary Chamberlain interviewed me over the telephone and then invited me for a formal job talk. Although Josh Angrist, who later became a very good friend, apparently was not impressed, Gary was, and I got the offer. **Life as Junior Faculty** I moved into Harvard faculty housing on Walker Street in Cambridge, walking distance to the university, and in fact next door to [Eric Maskin](https://www.nobelprize.org/prizes/economic-sciences/2007/maskin/facts/) (winner of the Prize in Economics in 2007). The Harvard economics department was an exciting but also an intimidating place as a junior faculty at the time. The econometrics seminar run jointly with the economics department at MIT had a remarkably large number of senior faculty attending. This included Zvi Griliches and Dale Jorgenson from Harvard and [Dan McFadden](https://www.nobelprize.org/prizes/economic-sciences/2000/mcfadden/facts/) and Jerry Hausman from MIT, all winners of the Clark Medal, given biannually for the most influential economist under 40, and traditionally a strong predictor of the Prize in Economics. It also included the more recently tenured Gary Chamberlain and Whitney Newey as well as some junior faculty, including Jeffrey Wooldridge and Danny Quah on the MIT side and Josh Angrist and myself on the Harvard side. The four senior people, Zvi Griliches, Dale Jorgenson, Dan McFadden, and Jerry Hausman, had been at the forefront of econometrics for many years and had strong views on what was good research in econometrics, forcing speakers at the seminar to defend their ideas and hone their arguments.  Josh Angrist, although originally not a big fan of hiring me, was a great junior colleague. We had many discussions over coffee in the nearby Science Center, as well as long sessions in the local laundromat where we would do our laundry on Saturday mornings. He introduced me to many of the debates in labor economics he had been exposed to at Princeton in the Industrial Relations Section with his advisors Orley Ashenfelter and David Card, and his collaborator Alan Krueger, and which had not made it to Brown University yet when I was a graduate student there. These discussions had a great impact on my thinking, as did conversations with Gary Chamberlain which focused more on the theoretical econometrics side. It was the combination of the two perspectives that would prove to be the lasting influence of my junior faculty years at Harvard.  Causality was not a term that was widely used in econometrics in the 1980s when I studied in Rotterdam, Hull, or Brown University. There was [Granger](https://www.nobelprize.org/prizes/economic-sciences/2003/granger/facts/)–[Sims](https://www.nobelprize.org/prizes/economic-sciences/2011/sims/facts/) causality, a concept used in time-series analysis. This measured, in various forms, whether one time series could or could not predict a second time series, conditional on the past. In micro-econometrics or empirical micro, however, it was not a term that was regularly used in papers or seminar presentations. This figure, based on similar figures in a paper by Janet Currie, Henrik Kleven and Esmée Zwiers (Currie et al. [2020]) illustrates this. It shows that among empirical micro papers in the NBER working paper series, about 5% used the term causal or causality at that time. This was not just in empirical papers. Looking back at the syllabus for a graduate econometrics course I taught at Harvard in 1991, there was no mention of causality either. It was not even that we taught the students the common adage that “correlation is not the same as causality.” The term was simply not used. In the econometrics courses I taught in my early years at Harvard it would not come up, and even experimental design was not part of the curriculum. This avoidance of the term had a long history. Although Tinbergen used the term explicitly in his work including [Tinbergen, 1940], later there was explicit advice against the term. As late as 2001, McFadden wrote in his 2001 Prize lecture that “detection of true causal structures is beyond the reach of statistics” and recommends that “For these reasons, it is best to avoid the language of causality” (p. 369, McFadden [2001]), despite the fact that the questions he analyzes regarding the demand for public transportation under different regimes are clearly causal.  In the 1990s that began to change, although slowly. Papers started to use the terms explicitly. Josh and I did not actually use the terms causal or causality in our 1994 paper [Imbens and Angrist, 1994], but two years later in the 1996 paper with Don Rubin [Angrist et al., 1996] there are already 123 instances of the “causal” or its derivatives. In 1995 when Don Rubin started what was, as far as I know, the first graduate course entirely devoted to causal inference, the registrar’s office was not used to the term. So, every time we wrote the word “causal” in the course description, and this was at least ten to fifteen times, the registrar’s spellchecker patiently “corrected” this to “casual.” Of course, even today this happens. On October 11th, 2021, National Public Radio in the US announced, perhaps in an attempt to drum up more interest in the story, that Angrist and I won our share of the Prize for our “analysis of casual relationships.” Now in 2021, however, the term “causal” is well established in empirical economics: according to the same Currie-Kleven-Zwiers figure approximately 50% of current NBER working papers (a major working paper series in economics) use the term explicitly, and there are many statistics and econometrics books devoted to it (Imbens and Rubin [2015], Rosenbaum [2002], Cunningham [2020]). While this change signifies a substantial change in the focus of empirical work in economics over this time, what Josh Angrist and Steve Pischke later labeled the “credibility revolution,” it does of course not mean that suddenly the economics profession discovered an interest in causal effects. Economists have always been interested in causal effects; they just did not always call them that. Sometimes they did, and Herman Wold took the position that “The concept of causality is indispensable and fundamental to all science” (abstract, Wold [1954]), but the terminology did not catch on at the time. Language matters, though, and the new terminology signifies a change in emphasis, a change in methods, and a chance for understanding. Why did the mid-1980s provided such a fertile ground for the new econometric methods for the estimation of causal effects? There were multiple reasons. One is that there was a sense of unease with the state of empirical work [Leamer, 1983, LaLonde, 1986]. Second, the physical proximity at Harvard of Josh Angrist, Don Rubin and myself facilitated connections between econometrics and statistics that allowed us to benefit from the insights of both fields.  The course on causal inference with Don Rubin was a big success with the students, despite the initial confusion in the course description. It was not that large, probably about twenty to thirty students, but those who attended, as well as those who taught it, found it exhilarating. A number of influential papers originated directly in this course. We taught the paper by Robert LaLonde [LaLonde, 1986], leading our students Rajeev Dehejia and Sadek Wahba to write [Dehejia and Wahba, 1999]. Comments by Bruce Sacerdote in the class led to our paper on the effect of unearned income on labor supply [Imbens et al., 2001]. **California, Part I** In 1997 I got turned down for tenure at Harvard University. This did not come as a huge surprise, tenure rates were very low at Harvard in general, and in the economics department few people had received tenure from the inside in recent years. There was clear appreciation for some of the work both in the department and the general profession, including the local average treatment effect paper, but not sufficient to change the norm. I did receive a number of tenured offers, including from the University of Texas in Austin and the University of California at Los Angeles. I had never spent much time in California before moving there, and I fell in love with the state. Living in Santa Monica, about twenty minutes’ walk from the beach, and a bike ride away from the UCLA campus was amazing! I got many more visitors from the Netherlands than in Boston, further reflecting the magic image California in general, and Los Angeles in particular, has in parts of Europe.  Academically UCLA also turned out to be a great fit for me. I ended up working closely with my colleague Joe Hotz, who joined from Chicago at the same time. His more structural bent was very helpful in further developing my views on causality and the direction of econometrics. I had some great students there, including Julie Mortimer, who got her first job after graduate school at Harvard University.  I also got to know Ed Leamer, who was in the business school at UCLA. Ed’s views about econometrics had not changed much since his famous “let’s take the con out of econometrics” paper [Leamer, 1983], but hearing some of the arguments first-hand, and discussing some of the subsequent developments in econometrics was enlightening. We ended up teaching a course together where we picked some influential papers in econometrics, some we liked, and some we did not like. Each week one of us would present and defend the paper, and the other would criticize it. This made for a great course from the student’s perspective, even though it was a stressful experience on my part! During my UCLA years I also spent some time at the RAND Corporation in Santa Monica, right next to the beach. Many years ago, [John Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/), the 1994 winner of the Prize in Economics had been a consultant there.  In 2001 I moved to a different campus in the University of California system, Berkeley. This was partly for personal reasons, with my then partner, now wife, Susan Athey working in the economics department at nearby Stanford University. Berkeley was a fabulous place for me academically. It had a great econometrics group, with Dan McFadden (who had move there from MIT in the early 1990s and who had just shared the 2000 Prize in Economics with [Jim Heckman](https://www.nobelprize.org/prizes/economic-sciences/2000/heckman/facts/)), Jim Powell, Tom Rothenberg, Paul Ruud, and Michael Jansson. They were broad-minded in their interests and had formed a cohesive group that regularly had wide-ranging discussions about new developments as well as foundational issues in econometrics. In addition, and this has always been very important in my career, it had a great empirical microeconomics group. David Card had moved there a couple of years earlier, and quickly he had built a very impressive group, including at various times David Lee, Ken Chay, Emmanuel Saez, Enrico Moretti, and Raj Chetty. Part of this group was centered around the Labor Economics Center that David founded, and that attracted many doctoral students as well as a steady stream of visitors from other universities. It made for a perfect setting for me, with many colleagues to discuss theoretical econometrics who were always happy to go out for coffee to the famous local coffee shops Brewed Awakening, Strada, or the Free Speech Cafe, and at the same time many empirically oriented colleagues to get inspired by in terms of questions and problems. Berkeley was also a very collegial department in general. Over the years, the economics profession has had a poor reputation in terms of culture, but Berkeley stood out with a large number of great departmental and professional citizens. I recall a department meeting early in my days there, where I presented a tenure case, and, somewhat unusually, one of my colleagues was critical about a particular reference letter, despite that having been addressed clearly in the report. Afterwards one of the other faculty came to my office and told me I had handled the situation well. It made Berkeley feel like a great community and one of my favorite places.  My Berkeley years were very productive, despite a long commute from Stanford where Susan and I had bought a house on campus, coincidentally on the same street as [Paul Milgrom](https://www.nobelprize.org/prizes/economic-sciences/2020/milgrom/facts/) and [Bob Wilson](https://www.nobelprize.org/prizes/economic-sciences/2020/wilson/facts/), who later shared the 2020 Prize in Economics. I started a long collaboration with Alberto Abadie on matching-type and causal inference methods. We had co-authored a paper before together with Alberto’s advisor Josh Angrist, but now we started a series of papers that substantially deepened our understanding of matching and other unconfoundedness-based methods. **Back at Harvard** In 2004 my wife Susan Athey, who at the time was working at Stanford, got an offer from the economics department at Harvard University. By this time, we had an infant, so my commute to Berkeley was becoming more of a strain and we reevaluated our joint options, which ended up including Berkeley and Stanford in addition to Harvard. We decided to visit Harvard for the Fall of 2005, staying at the house of our friend [Esther Duflo](https://www.nobelprize.org/prizes/economic-sciences/2019/duflo/facts/), who later won the Economics Prize in 2019. Many of my former junior colleagues, including David Cutler and Ed Glaeser, were now tenured and the department felt quite different from what it had been when I was there as a junior faculty member. We decided to accept the offer there, and in 2006 moved into a Victorian house just off Brattle Street. The previous owner, former Labor Secretary in the Clinton administration Robert Reich, made the cross-continent move in the opposite direction to take up a position at Berkeley.  Coming back to Harvard was a great experience. The students at Harvard were by now very interested in causal inference. The research I had worked on ten years earlier had become part of the standard first year curriculum, and the second-year courses I taught in this area became popular with participants from all over the university. Moreover, there were many colleagues and collaborators in Cambridge very interested in this area. This included Gary Chamberlain, Don Rubin, Josh Angrist (who was now at MIT), his former student Alberto Abadie at the Kennedy School, and Whitney Newey. In addition, being back at Harvard allowed me to reinvigorate my collaboration with Rubin on our book, which eventually came out in 2015 after almost ten years of on-and-off work. It also led me to find new collaborators including Nicholas Christakis.  One of the highlights of those years came from a conversation with Marty Feldstein. He had observed that other disciplines had regularly courses for faculty at their annual conferences to keep them up to date. He thought economics should do something similar. At one of the faculty lunches at Harvard he asked me if I was interested in teaching such a course, with the general theme “New Methods in Econometrics” at the National Bureau of Economic Research, where he was CEO. I was up for this, and together with Jeffrey Wooldridge I developed a three-day, 21 lecture course that gave an overview of modern microeconometrics. As the dates for the course in the summer of 2007 approached we had some trepidation because the enrollment was over 400 researchers, all faculty at universities all over the US and some from overseas. I had never had, and never again will have, such a high-quality audience. The course was a big success, ending with a standing ovation at the end of the third day, and sparked many follow-up courses. The National Bureau of Economic Research has been running courses like this, typically one day rather than three, every year, on different topics. The American Economic Association started organizing yearly courses along the same lines. **California, Part II** In early 2012 a family ski-trip to the West Coast sparked some conversations about moving back to California. Without an expansive search we quickly decided that moving back to Stanford made sense and, in the summer of 2012, we packed our belongings, and, with now three kids in tow, moved back, about a mile from where we had lived previously. Stanford was a much more challenging place for me. Unlike Harvard, where there was a lot of interest in the type of econometrics I was working on and I had many collaborators, Stanford had a very different tradition, and I had not collaborated previously with anyone. There was considerable skepticism in the economics community about the causal inference literature which was viewed as Cambridge-style econometrics, as well as in the statistics department where there was little interest in it. Moreover, the business school where we ended up had never had any econometricians on the faculty, with their primary focus on economic theory.  What made it exciting though was the location in Silicon Valley with the tech companies around. Susan had gotten interested in the economics of the tech world, which led to a stint as Microsoft chief economist. This in turn got her to start a new research area in the intersection of computer science, machine learning and econometrics, which subsequently got me interested in the questions and methods used in the new data-driven private sector organizations. These companies now routinely advertise for positions requiring skills in causal inference, including expertise in instrumental variables, regression discontinuity, causal forests, and matching, many of which were not part of the standard curriculum in econometrics ten years earlier. When I arrived at Stanford, Jas Sekhon from Berkeley asked me to do a presentation at a new interdisciplinary seminar he was setting up on general data science issues. I convinced him to make it a joint Berkeley-Stanford seminar. When he agreed we ran the seminar for a couple of years, inviting academics from various departments as well as researchers at the local tech companies. Although the seminar itself was a big success, doing it jointly was difficult with the long commute, so we ended up doing it at our separate institutions. These data, society and inference (dsi) seminars were very popular with the students as well as the faculty, opening up a window into the tech world. The move to the Bay Area later let to some of my collaborations with these companies, including a summer I spent working with researchers at Facebook and consulting arrangements with Amazon.  Now, ten years later Stanford has a strong presence in causal inference, with faculty in the business school, the statistics department, the political science department, the school for management science and engineering, the medical school, and the law school.  In early 2020 the pandemic shut down in-person teaching and seminars. Together with collaborators, I started two online seminars to fill the gap. The first was the Chamberlain seminar, named after my former mentor Gary Chamberlain, who had passed away in early 2020, focusing on econometrics. The second was the Online Causal Inference Seminar, focused on causal inference broadly defined, and including various disciplines. Both have continued even as in-person seminars have come back. They have contributed to the integration of the causal inference community across disciplines and geographies. **Epilogue** The call from the Prize Committee early in the night of October 11th in 2021 came as a big surprise. Receiving it at that time meant all three of my kids were still living at home (the oldest would leave for college 8 months later), making the celebration with my family that morning all the more special. Sharing it with David Card and Josh Angrist was another highlight. We managed to find an hour to catch up on Zoom later that day, even though it would take till the next summer for the three of us to be in the same place in person at a conference in Berkeley. Both Josh and David have been great colleagues and friends over the years, and the only disappointment was that Alan Krueger, who passed away in 2019, was not there to share it with us. Note that Alan was the only economist who had co-authored papers with all three of us.  The Prize marks a new stage in my career. While I had not felt that I was in any sense finished with my research program, it clearly creates incentives to refocus my attention on broader themes and find new ways of contributing to the development of econometric methods for economists. **References** Joshua D Angrist, Guido W Imbens, and Donald B Rubin. Identification of causal effects using instrumental variables. *Journal of the American statistical Association*, 91(434):444–455, 1996.  Scott Cunningham. 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| Autobiographical |  |
| Podcast | **“It is very good at developing good work habits”** In an increasingly chaotic world, how can you learn to concentrate deeply on a single problem? Guido Imbens found his powers of concentration while getting lost in chess games as a child. ”For four or five hours you would just shut out the rest of the world, you would be focused on one task.”  In this conversation, conducted in June 2022, Imbens also talks about the beauty of chess, the pitfalls in talking publicly about uncertain data and the challenge of keeping an open mind in research. He also speaks about the morning he received the news about his prize in economic sciences in October 2021 and how his kids and wife celebrated the award with him early in the morning.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0809=GI  Adam Smith: Am I speaking with Guido Imbens?  Guido Imbens: Yes, this is he.  AS: Hi, this is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize.  GI: Hi.  AS: Hi, sorry to keep…  GI: How are you doing?  AS: Very well, thank you. Sorry to call so very early in the morning and…  GI: [Laughs] Well, things have been hectic here anyway. I’m not getting much sleep.  AS: No, the day is well underway.  GI: Yes, exactly.  AS: Were you actually asleep when the news came?  GI: Yes, I’d managed to go to sleep. I actually had a busy day yesterday – we went, I went biking, taking the kids on some orienteering trip, so I was mountain biking myself, so I was pretty tired by the end of the day.  AS: Well, so somehow your body has to cope with a day where I guess it’s going to be hard to get back to sleep.  GI: Yes, I’m sure the adrenalin will get me through.  AS: Indeed, and you’ll be carried along by all the people wanting to talk to you.  GI: Exactly.  AS: And I think I hear the clanking of coffee cups around you, so maybe that’s…  GI: Yes, I’m going to get some coffee here.  AS: That’s good. Very wise. Your work is all about asking where is the evidence, and developing tools to extract that evidence from the data. I suppose a good place to start is to ask whether these natural experiments that you work on are hard to find, or whether there’s just an abundance of data out there, and it’s a case of just picking which bit appeals the most at the moment.  GI: I think part of the, the contribution, and in some senses that’s sort of more Josh and David’s contribution, is kind of showing actually there’s many of these, many places where these natural experiments occur. And I think my part of the contribution is then showing, if these things are there then these tools are going to be very helpful and that actually helps you see that many of these natural experiments are there. One of the techniques I’ve worked on is regression discontinuity designs. That’s something that came up… that was actually around in the psychology literature in the 60s but it wasn’t really used in economics. And then when, in the early 2000s, people started using it and the methodology got improved, and suddenly people realised that there were lots of places where you could use these methods. The same with the Local Average Treatment Effect stuff. Once the methodology was clear it became easy to recognise when… where these natural experiments were… when they might occur, and how you could exploit them.  AS: So it’s really a very symbiotic relationship between the gathering of the data…  GI: Yes, and you know, the… what, where I was incredibly fortunate was very early in my career, kind of meeting up with Josh and kind of seeing how he was working, how he was thinking about questions, and that helped shape my research agenda in terms of the methodological questions, kind of figuring out how to do econometrics in a way that was actually useful for empirical researchers. Kind of later I had David Card as a colleague, I had David Card as a colleague, I had [unclear] as a colleague, at Berkley and Harvard and Stanford. So I’ve been very fortunate to be around these incredible empirical researchers, who can help ask great questions for the type of methodological work I was doing.  AS: It’s a lovely illustration of the social side of research, that it’s partly a case of sitting in your office and thinking, and partly a case of talking to people.  GI: Well, again when I was a first year assistant professor, my senior colleague there Garry Chamberlin, kind of said you know you need to go to the labour seminars, you need to talk to Josh Angrist, you need to listen to these people doing empirical work to get… because that’s how you do good econometrics. That’s exactly right, being around these leading researchers was incredibly helpful for me in developing my own research agenda.  AS: What keeps you focussed? What is it about the work that just keeps you going?  GI: At some level it’s sort of like doing puzzles. When I was a kid I was into chess, and once I get hooked on a question I think, you know, at some level econometrics is kind of like… it’s just like applied maths. The challenge is really coming up with good questions, but then once I have a good question I just get very deep into them and I keep… I like keep mulling over questions. And I get a lot of enjoyment out of that. My former colleague at Harvard, Gary Chamberlain, told me this story where, when he learned that you could actually make a living doing… being an econometrician or being an economic economist and get to work on the question you’re interested in, he said that was just a great eye-opening experience. He just… and I can relate to that. I mean I am incredibly fortunate, I get up in the morning and I like going to work, I like working with the students, working with young, smart people. There’s nothing better than that.  AS: Beautifully said. And some days you end up getting up in the morning incredibly early.  GI: Well, that’s, you know, that’s, it’s… I’m incredibly excited. It’s really just icing on the cake. Last week I loved getting up in the morning and going to work, and if this hadn’t happened I would still love getting up in the morning and going to work, and working with the students. There’s a lot of projects I’m working on that I’m excited about, and the only sad thing is there’s not more hours in the day. I mean I do some professional service, I’m the editor of one of the main journals, and so I like reading the literature, and kind of talking to people, going to seminars, so it… a sad thing at the moment is there’s no conferences to go to, to actually have the live interactions with people. But I set up this online econometrics seminar a year and a half ago now, that’s been going on every two weeks, and it’s been a real great thing for the profession to build community in these challenging times.  AS: It does sound as if you are indeed very fortunate. It’s a… it’s a lovely thing you describe. I have to let you go, but I have one last question – I hear people around you, is it possible that somebody could take a photograph of you and send it to me?  GI: Yes, yes, sure.  AS: …for Nobelprize.org because we want to capture this precise moment. We already have a photograph of David Card in his pyjamas, so this would be great.  GI: I’m not in my… could somebody take a picture of me for the Swedes? Is it OK to have my kids iin it?  AS: It’s better to have your kids in it, that would be just fantastic, please.  GI: Just give me one second to…  AS: Thank you so very much.  GI: We’re trying to get one of the kids.  AS: We’ll talk again another time.  GI: Sorry, I’m, I’m just losing a little bit, thanks so much.  AS: Thank you very much indeed, and congratulations.  GI: Bye.  AS: Bye. |
| Interview |  |
| Q1 | Can you tell us about your childhood and how you became interested in economic sciences? |
|  | I was born in a small town in the south of the Netherlands, Geldrop. The nearest big town was Eindhoven, which in those days was a company town. The Phillips electronics company had their headquarters there and employed a large fraction of the population there. So both my parents worked there for a while. My father worked there his entire career. When I was about six we moved to Eindhoven and then later to another small town about 20 miles east of there called Deurne, which is where I went to high school.  Neither of my parents had completed a university degree. My father had spent one year at university, but then he started working. This was in the fifties when Holland was still rebuilding after the war and studying wasn’t quite as common as it became later. But they were both very intellectually oriented and they stimulated that in us. So both my siblings and I ended up going to university. In fact, my brother got a PhD in mathematics. That was something that was very much stimulated, not in the sense of putting a lot of pressure on us, but they were very interested in us going to university and pursuing intellectual endeavours.  In my teens, I played a lot of chess which in some sense, was actually very useful in learning how to focus on problems for a long time. I still find that a very enjoyable thing, sitting in my office and not doing email, not talking on the telephone, just struggling with theoretical problems and trying to figure them out, trying to think very deeply about the problem. It’s hard to find the time to do that for long stretches of time, but that’s something I enjoy very much and I still try to find time to do that. From my perspective, that really goes back to those days. |
| Q43 | Do you see a lot of parallels between chess and what you’re working with now in your research? |
|  | Yes. At some level, I see a lot of what I do as trying to solve puzzles, trying to come up with theoretical questions and trying to solve them. And the ability to sink deep into a problem is really the key thing, and where chess I think is very helpful. |
| Q44 | Do you still play chess now? |
|  | Not very often. One of my kids got interested in it during the pandemic. So for a while we would play a lot. Actually we started playing this 24 game match, which we were almost done with, but then we postponed for a while because he was doing other things. But at the beginning, clearly I was still better, but by the end he’s clearly a better chess player than me. |
| Q28 | We hear you enjoy biking too. How important is free time to you? |
|  | I think for everybody these days it’s very hard to get away from all the electronics and all the constant stream of information with social media, with email, with lots of things. So biking is just a very good way to get away from that because you’re not doing email, you’re not doing any social media, you’re not working on the computer. It’s a great time to think and to unwind. So certainly one of the things I do while biking or running or walking is thinking through problems I’m working on, but it forces me to do it at a more abstract level because obviously I don’t have a computer, I don’t have pen and paper, but I can just mull over problems and keep turning them over and think about fruitful directions.  It’s a very relaxing activity for me. I do both road biking and mountain biking, but I don’t do very complicated stuff. So I don’t need to really focus on the biking itself. It’s much more of a relaxing thing where I can think about questions. Having the physical activity and the freedom to think at the same time, I just find very relaxing and productive. |
| Q45 | Can you tell us what you like most about your work? |
|  | It’s probably two things. It’s talking with the students and seeing them grow as researchers, and being able to influence them and seeing them kind of develop their own ideas. And just sitting in my office, working on problems and feeling I’m making progress. The thrill of feeling I’ve learned something that I didn’t know before – more than the actual process of getting things published, but the point where I feel like, yes, I know something now that I didn’t know before and that I think is useful. |
| Q10 | What advice would you give to a student or young researcher? |
|  | Looking more narrowly at my students who are working in econometrics, I try to encourage them to really spend a lot of time talking to people doing empirical work and people doing more applied economics rather than getting their inspiration from reading journals. I really try to get them to focus on problems that are more directly useful for people actually analysing data and collecting the data. Over my career, I’ve really benefited a lot from collaborating closely with people like that. I see econometrics at some level, as a service field, we’re trying to help economists do better data analysis, do better empirical work and so to do that, you need to talk to the people who are actually doing the empirical work. |
| Q8 | We understand that you are close friends with your collaborator and co-laureate [Joshua Angrist](https://www.nobelprize.org/prizes/economic-sciences/2021/angrist/facts/) – and that he was even the best man at your wedding. Do you think it’s important to become friends with your collaborators? |
|  | Yes, totally. I would find it very hard to write papers with people that I’m not friends with, that I don’t like to spend time with. I can’t be very close friends with all of them, but writing a paper is a vulnerable process. You share ideas, some of them are not going to work out, some of them are not good ideas, you make mistakes, so you need to have a fair amount of trust and a level of comfort with people to work with them.  Nowadays we can do a lot over the internet, but I would still find it very hard to start working with people if all I’d done is meet them over the internet. I do want to spend some time with them. I want to have coffee with them. I want to talk to them. I want to understand how they think about problems, how they work because it helps me interpret what they do in the course of these collaborations, how they like to write papers, how they think about having speculative ideas, whether they’re defensive about making mistakes, how they’ll respond if I make mistakes. Research is very difficult and when you’re doing this together, you need to have a fair amount of trust to be able to do good work. |
| Q42 | It must have been really special to be awarded the prize with your best friend. |
|  | Yes, that was very special. That was my first question when they called me on October 11th – who I was going to be sharing it with. It was just very exciting to hear that I was getting it getting it with Josh. [David Card](https://www.nobelprize.org/prizes/economic-sciences/2021/card/facts/)‘s a very good friend of mine as well, I was his colleague at Berkeley, but Josh has really been my best friend in the profession since very early on. So that was really very special. |
| Q7 | How have you found the time since your prize was awarded? |
|  | It’s been fun. It’s been a little strange with almost all of it being over zoom, but it’s been a lot of fun. It’s been particularly interesting to see the reaction in the Netherlands where they were very excited about it. One of the things that I find very nice is, unlike the Olympics where you always see these rankings, like the US has ten medals, Sweden has five, England has seven, here it seems much less exclusionary. So my high school is very excited. They claim me as one of them. The university I went to in the Netherlands is excited, the graduate school I went to in the US is very excited, UCLA where I was on the faculty was very excited – everybody shares it rather than it being very exclusionary. |
| Q37 | We saw a sweet video of you and your children after the news was announced – where they were explaining your research in their own words. How was it to share that moment with them? |
|  | Obviously from my perspective that was just one of the absolute highlights. The kids were obviously very excited, but that was just incredibly nice to see, incredibly rewarding. In that sense it was so nice to get the prize this year, despite the pandemic, but having all three of the kids there. The oldest is a high school senior so he’s going to go off to college next year, but having all three of them there was just really special. |
| ID | 0810 |
| Biographical | I was born in 1948 in Detroit, Michigan to Abraham Isaac Milgrom and Anne Lillian Milgrom nee Finkelstein. Abraham Milgrom was born in Canada to Polish-Jewish immigrants, and Anne Finkelstein in Detroit, Michigan, to Ukrainian-Jewish immigrants. I am the second of the Milgroms’ four sons; Stuart is my older brother and Barry and Steven are my younger twin brothers. We grew up in Oak Park – a suburb of Detroit – where I attended the John Dewey School followed by Oak Park High School, from which I graduated in 1966.  From a young age, I had a strong interest in math, encouraged by my middle and high school math teacher Mr. Habermas. He also encouraged me to apply to the Ross summer math camp at Ohio State University in 1965, where I was #1 in my class. In high school, I took joy and interest in solving math puzzles, playing chess, and participating in various youth groups in the local Jewish community.  After high school, I attended the University of Michigan in Ann Arbor but was bored by my non-major classes and earned unimpressive grades. Initially choosing between a math and a physics major, I dropped my first physics class because of its 8 A.M. time-slot far across campus during a freezing cold Michigan winter. I remember climbing out of bed, opening the door to the sub-zero temperatures, and then turning around and going back to bed. I graduated from the University of Michigan in 1970 with an AB in mathematics and then followed a girl to Berkeley, California, landing my first job as an actuarial trainee at Metropolitan Life Insurance Company in San Francisco. After about a year, I quit San Francisco and returned to Ann Arbor to stay with friends and study for the actuarial exams, before moving to Columbus, Ohio to join the Nelson and Warren actuarial consultancy. In Columbus, I first discovered my inclination to do theoretical research on practical problems. I published two prize-winning theory papers in the *Transactions of the Society of Actuaries.* It was also in Columbus at a friend’s house party that I met Jan Thurston, whom I later married. We lived in the German Village section of Columbus for about a year before moving to Stanford University in 1975, where I planned to pursue an MBA and broaden my understanding of business.  In the MBA program, I took an advanced methods class from Evan Porteus, who wondered about the rate at which some approximation would converge to the true value. When I explained that it depended on the subdominant eigenvalue of the Markov matrix, Porteus encouraged me to quit the MBA program and pursue a Ph.D. instead. It was excellent advice, and I took it.  Unsure about what to do next, I asked an advanced graduate student, [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/), who told me: “The secret is to get Bob Wilson to be your dissertation adviser.” So, I signed up for Wilson’s class in “Multi-person Decision Theory,” which involved reading new research in economic theory. The readings included one paper of Wilson’s, about auctions. Hoping to get Wilson’s attention, I wrote a term paper extending Wilson’s work. He was excited by the results, telling me that the paper could be the main chapter of my doctoral dissertation.  Holmström became a lifelong friend, colleague, and co-author, as well as a Laureate in economic sciences in 2012. Wilson, now a friend and neighbor, became my co-Laureate in 2020.  Finishing my dissertation in 1978, I stayed at Stanford so that my first child, Joshua, would be born there in December. My Ph.D. degree was awarded in 1979, celebrating with Joshua on my shoulders. I moved my family to our new home in Evanston, Illinois when Joshua was just a few days old. Joshua would go on to have a son of his own, Shepherd Prem Thurston-Milgrom. Of being a grandfather, I can only offer the oft-repeated quip: “if I’d known how much fun a grandchild could be, I would have had him first!”  I took my first academic job at the Kellogg School of Management at Northwestern University, where I met yet another future Laureate, Roger Myerson. I also recruited my old classmate Bengt Holmstrom to join me there. The three of us challenged one another every day, inspiring each other to do better, deeper work. We lunched and socialized together, becoming life-long colleagues and friends. I also met my two most frequent coauthors there: Robert Weber and John Roberts.  The group at Northwestern was intent on bringing game theory with incomplete information into the center of economics. That was a controversial endeavor. When John Roberts (1982a) and I wrote a paper to explain limit pricing – the practice of setting low prices to discourage entry by a competing firm – our explanation was that the incumbent firm was trying to convince any potential entrant to expect fierce competition. Critics raised a howl of resistance. Scientific modeling of industrial competition, they insisted, must explain outcomes based on the measurable conditions of supply and demand and barriers to entry, and not on factors like the parties’ beliefs or how those could be influenced! We were undeterred. Milgrom and Roberts (1982b) and Kreps and Wilson (1982) similarly explained predatory pricing as discouraging *future* entrants by influencing their beliefs, making them anticipate that entry would lead to sharp price reductions.  Maybe its worth adding the reference to this paper – it is also missing from the reference list at the end – in which players play against one another perhaps twenty or thirty times – in a similar way. According to the traditional Nash equilibrium analysis, players in that game will never cooperate, but experimental evidence rejects that prediction. Human players nearly always manage to cooperate until near the end of the game. Our theory worked differently. If cooperation and reciprocal behavior can encourage others to expect more of the same, then there is nothing puzzling about cooperation. Criticism of our approach eventually gave way to praise. The National Academy of Sciences highlighted these papers in announcing the Carty Prize for Advancement of Science in 2018. Our work had created powerful new tools allowing game theory to analyze economic institutions and human behavior.  However, it was my research in auction theory and my discovery of new auction designs that the Prize Committee named in the prize citation. My auction-related research evolved through three distinct eras. In the first, which began with my Stanford dissertation under Wilson’s mentorship and continued for about five years, I extended Wilson’s research program, mostly in joint work with Robert Weber. Our best-known paper (Milgrom and Weber, 1982) characterized the equilibrium strategies of auction games and studied the extent to which bidders’ private information became reflected in prices in auctions and securities markets and how a seller’s expected revenue depended on the auction rules. The second era, beginning around 1993, was launched when Congress authorized the FCC to sell radio spectrum rights using an auction. The FCC order establishing and justifying the auction rules cited me by name more than 100 times and adopted virtually all of Wilson’s and my proposals.  Despite the praise the auction had received, I worried that the rules were not well-enough grounded in economics and game theory. Like others at the time, I had based my analyses on the assumptions that the items for sale were substitutes and optimization was tractable. The third era, which began around 2001, relaxes those assumptions. In this era, the teams I led aimed to solve practical problems, which sometimes required solving research problems in computer science, economics, and game theory. **The first era** When I began my economics research career under the tutelage of co-laureate Robert Wilson, game theory had just started to penetrate economics. General equilibrium theory, which was then dominant, assumed that market-clearing prices guided allocations. It had little to say about the price discovery process. Wilson suggested that auctions, with their explicit rules and long history in practice, could be studied by game-theoretic methods to understand how prices and allocations emerge together.  Much of the early research in the Wilson tradition focused on the information content of prices. How much of the bidders’ information comes to be reflected in the prices that are paid? To what extent can bidders profit by using their private information? Do some auction rules lead to systematically higher expected prices than others?  That information problem took on a special urgency following the publication of an influential paper by Grossman and [Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/) (1980), in which investors with private information about values traded in a market. To incorporate effects like the winner’s curse, Grossman and Stiglitz adopted a general equilibrium model. Their twist was that traders’ demands would be based on inferences from the market-clearing price about others’ information. But how could that information become embedded in prices even before anyone had committed to a trade? In their simplest model, asset prices make it possible to infer *all* information available to all traders in the market. Consequently, even the best-informed traders cannot profit from trading, so nobody has any incentive to acquire information. But if nobody acquires information, then presumably prices must be uninformative, so any trader who acquires private information should be able to earn a profit in equilibrium. This was the Grossman-Stiglitz information paradox. It confirms that one cannot adequately study the information content of prices without looking directly at the trading mechanism’s detailed rules.  Even before Grossman and Stiglitz, Wilson (1977) had introduced such a model. It was a “common values” auction model with a single asset for sale with equal value to all bidders. There is no information paradox in that model. Bidders invest in information because better-informed bidders earn more profits in the auction. Prices reflect private information because they depend on the bids that informed investors make. Although Wilson’s analysis also highlighted the “winner’s curse,” which is the tendency of a bidder to win more often when his estimate is too high than when it is too low, Wilson’s main point was different. The paper first derived the Nash equilibrium of the bidding game. It then showed that when a particular condition is satisfied, the equilibrium auction price converges in probability to the common value as the number of bidders grows large. This convergence means that the price “aggregates” information because it holds even when no individual bidder has a tight estimate of the value when it submits its bid.  My first published economics paper (Milgrom, 1979) was my term paper for Wilson’s class. It extended the just-described analysis to identify a necessary and sufficient condition for that convergence-in-probability to occur − the mathematical analysis that impressed Wilson. I used a statistical argument to show that my condition is necessary for convergence, regardless of the bidding strategies, and a game-theoretic argument to prove that the same condition is sufficient when bidders use their equilibrium strategies.  The year 1982 was an especially productive one for me. With various coauthors, I wrote papers about auction theory, game theory, industrial organization, and the famous “No Trade Theorem.” The Milgrom-Stokey No-Trade Theorem model extended my earlier auction models to focus on security trading when *both* buyers and sellers can suffer a winner’s curse. The agents were risk averse, with strictly concave utility functions, and could have private information about the security values and their pretrade security holdings. Each agent trades only if that increases its expected utility conditional on its information. The information includes the trader’s own information and the fact that the market clears, that is, the remaining traders are willing to take the other side of the trade. We concluded that: “Risk averse traders never make (non-zero) trades based solely on differences in information.” Thus, for trade to occur among rational traders, there must be at least the possibility that, if there were no differences in information among traders, some Pareto-improving trade might exist. In other words, it must be possible that someone has a transactional motive for trading and not just a speculative motive. Typical examples of a transactional motive in real life include an agent wanting to buy securities because she has just added monthly savings to her retirement account or sell securities to pay this year’s tuition bill at some school or college.  In 1981, just two-and-a-half years after receiving my doctorate, I accepted an offer from Yale University to occupy the Williams Brothers chair, with tenure. It was a joint appointment between the new Yale School of Management and the Department of Economics. I moved my family to New Haven in 1982. In that same year, Jan and I had our second child, Elana Suzanne Thurston-Milgrom, delivered at Yale-New Haven Hospital.  Having a daughter changed my life! Growing up a shy boy with only brothers, Elana taught me about girls and later young women. Later, watching her sexist treatment during her graduate studies at the University of Florida, I was inspired to focus on training women economists at Stanford.  Arriving at Yale, I again recruited Bengt Holmstrom to join me. We wrote a series of papers together. The best-known one was our multitask principal-agent theory, which highlights the danger of explicit incentives. Providing incentives for a valuable task that is easily measured may lead an agent to substitute valuable effort away from other, unmeasured tasks. For example, rewarding teachers for improving student test scores might backfire by encouraging the teacher to shift effort away from social development and creative thinking. That is why culture, professionalism, and loyalty are so essential to motivate workers in many jobs.  During my years at Yale, I continued my work situating incomplete-information game theory in the center of economic theory. In one of my bestknown papers, which Larry Glosten and I (1985) began during my Northwestern years, we continued the Milgrom-Stokey study of trading with private information on both sides and the Wilson theme of how much information would be reflected in the prices. In our model, there is a single financial security to be traded. Each trader may have any mix of transaction and speculative motives for trading. A group of risk-neutral “specialists” is present in the market, competing against one another to buy from or sell to each arriving customer. The specialists set bid prices, at which they offer to buy, and ask prices, at which they offer to sell. The customer sees all the bids and asks and takes the best deal if any are acceptable or walks away without trading. Price competition among the specialists for each transaction drives their expected profits to zero on each.  One conclusion of this model is the so-called “weak form” of [Fama](https://www.nobelprize.org/prizes/economic-sciences/2013/fama/facts/)’s (1970) efficient markets hypothesis: transaction prices are equal to the expected value of the security to the specialists at the time of any trade, conditional on the available public information. If the trader buys, then the market price will be the expected value conditional on the trader choosing to buy. Similarly, if the trader chooses to sell, the market price will be the expected value conditional on the trader choosing to sell. In this way, the transaction price includes some of the trader’s information, but not all of it. It resolves the Grossman-Stiglitz information paradox because a trader who acquires more information makes better decisions and earns higher profits. This paper, along with a contemporaneous one by Kyle (1985), launched the modern literature on financial market microstructure, which examines the detailed rules of trading and how those affect various measures of market performance.  Another set of questions that had attracted economists’ attention in the first era of auction research concerned how bidders’ payoffs and the seller’s expected revenue depended on the auction rules. When I began my work, there were two main game-theoretic models for studying auctions: the [Vickrey](https://www.nobelprize.org/prizes/economic-sciences/1996/vickrey/facts/) model and the Wilson model. Working at Northwestern with a frequent collaborator, Robert Weber (Milgrom and Weber, 1982) I introduced a new auction model that subsumed the Vickrey and Wilson models. We also developed new methods to study bidder profits in an auction and provided answers to the payoff and revenue questions. We extended the earlier models by weakening the assumption of how types were distributed and allowing each bidder’s value to depend more generally on everyone’s information and some unknown parameters. In my 2004 book, I summarized the literature including the Milgrom-Weber analysis by deriving all of the revenue and payoff results using the envelope theorem. For the Milgrom-Weber analysis, this approach generalized older “revenue equivalence” results and also led to a new “linkage principle,” which establishes certain inequalities relating to bidder payoffs and seller revenues in different auction designs.  After five years at Yale and a half-year visit to UC Berkeley in 1986, I received an offer to teach at my alma mater. I relocated my family to Stanford in 1987.  From the mid-1980s to the early 1990s, I moved away from working on auctions. I focused on game theory, robust methods for comparative statics, principal-agent theory, and economic organization theory with co-authors Roberts, Weber, Holmstrom, and Drew Fudenberg. Roberts and I wrote our book, *Economics, Organization and Management, w*hich helped define the scope of the field. Holmstrom and I wrote the first continuous-time principal-agent model and used it to create our multi-task analysis and our theory of firms as a system of incentives. With [Douglas North](https://www.nobelprize.org/prizes/economic-sciences/1993/north/facts/) and Barry Weingast (1990) and with Avner Greif and Barry Weingast (1994), I pioneered the use of game-theoretic models to illuminate the role of trading institutions in economic history. **The second era** In 1993, when Congress authorized the FCC to auction the radio spectrum licenses, I grew interested in auction theory again. The first licenses to be auctioned would be for paging and mobile telephones. According to the law, the FCC’s goals were, first, to promote the efficient and intensive use of radio spectrum and, second, to capture a portion of the spectrum value for the US Treasury.  Many years earlier, [Ronald Coase](https://www.nobelprize.org/prizes/economic-sciences/1991/coase/facts/) (1959) had advocated that the FCC should use an auction for assigning television broadcast licenses but had nothing to say about what the auction rules should be. Perhaps that seemed unimportant to him for the applications of the time. Licenses for TV broadcasting mostly support independent business opportunities, which could be reasonably sold one at a time using almost any standard auction method. If businesses bid against one another for license rights, then to a first approximation, the licenses would tend to be assigned to the bidder with the most valuable business plan, which (in the absence of externalities) could promote efficiency.  The situation in 1993, however, was quite different. Companies like AT&T, MCI and Sprint, which were then providers of long-distance telephone services supported by nationwide networks of fiber-optic cables, developed business plans based on nationwide wireless networks. They were not interested in buying isolated license rights in a collection of unconnected areas and lobbied to have the FCC offer nationwide licenses for mobile services with enough bandwidth to support a brand-new service. Local companies like Cincinnati Bell wanted to acquire just a local license with narrower bandwidth to supplement its existing cellular services and advocated that the FCC offer the corresponding licenses. Regional companies like Pacific Telesis and Bell Atlantic had existing regional cellular services and lobbied accordingly. Trying to be all things to all bidders, the FCC decided to offer 99 different licenses covering different areas with different amounts of bandwidth.  The FCC had never run an auction before, and nobody anywhere had attempted a sale of this complexity. From the perspective of 1993, Wilson and I identified the biggest challenge as one of “price discovery.” If bidders could learn the approximate prices before making their final bids, that would promote an approximately efficient market outcome. Most of the leading proposals, however, did not respect that challenge. For example, one proposal was to employ the same rules used by a traditional auction house like Sotheby’s, with each auction lot sold one-at-a-time in a  predetermined sequence. That approach would provide bidders with no useful information about the prices for licenses that will come up later. For example, if the first licenses offered covered Chicago and Los Angeles and if the bidder’s business plan required those plus licenses covering New York, Boston, and San Francisco, then how much should the bidder be willing to pay? The bidder faces several risks. One is the “exposure risk” that the bidder might succeed in buying the early licenses only to learn that the late licenses cost too much. Another is the “overpayment risk” that the buyer of the first Chicago license subsequently finds that a second Chicago license, offered later in the auction, is sold for a much lower price. These auction rules would require bidders to make many guesses, some of which would likely be mistaken. Those mistakes could undermine the main statutory goal of efficiency of the outcome.  With the exposure risk for the long-distance operators in mind, another proposal was to have licenses covering smaller areas but to allow bidders to offer a single price for specific packages of licenses, such as a package covering the whole country. There were howls of objections to that procedure as being too heavily biased in favor of nationwide bidders. It would force bidders for smaller areas to somehow coordinate their bids to compete against the highest national bidder without offering any mechanism to promote such coordination. Neither of these alternative proposals conceives of the issue as one of price discovery, as Wilson and I (and, separately, Preston McAfee) had done.  Questions about how to address the auction design challenge led both the FCC and several telephone companies to seek the advice of academic economists. Wilson and I were approached by Pacific Bell while the FCC hired Professor John McMillan. As I pondered the novel challenges of the spectrum allocation problem, I was reminded of my experience bidding in “silent” auctions at charity events. In a typical such event, people donate things to be sold in an auction. For example, one person might donate cooking lessons; a second person might donate an evening with a celebrity; a third, a weekend at a privately owned ski chalet; another, a bottle of wine, and so on. Items or their descriptions are placed on tables in a large room, and everything is for sale at once. There is a bidding form and a pencil in front of each item. Bidders would write their name or ID number and a price on the form, subject to the restriction that the bid must exceed the price on the preceding line by some minimum increment. A bidding deadline is set just before dinner or before another fund-raising activity, such as a live auction. The simultaneous ascending design allows a bidder who is unsure which item to bid on to begin with her most preferred items and then switch to others if the preferred items become too expensive. This process eases the bidder’s task of deciding which item to bid for and how much to bid.  I assessed that the silent auction design had worked well for the charities, but “snipers” often gamed the rules. Sniping is a strategy of waiting until the last moment to bid, hoping to keep the price low. By bidding only in the final seconds, a sniper can sometimes win by denying other bidders an opportunity to compete.  To retain the silent auction’s advantages while also eliminating sniping, Wilson and I proposed a new design: the simultaneous multiple round (SMR) auction. It differed in three ways from the silent auction.  First, it would be run in a *series of discrete rounds.* The FCC would set the minimum price for each item by adding a minimum increment to the best previous bid. Only after a round closes would information about bids from that round be shared among the bidders. Then, the next round would begin.  Second, the auction would not end until a round with no new bids for any license. This new termination rule would frustrate sniping, for if a sniper were to bid in a late round, there would always be another round allowing a willing competitor to raise the bid.  Third, since the auction had no fixed ending time and since any one bid could keep the auction alive, there was a risk that an auction of many licenses could take an untenably long time to finish. FCC staffer Evan Kwerel worried about that and challenged me to respond. I invented a new category of rule – the *activity rule* – which prevents any bidder from bidding for a larger quantity of licenses in any round than in any previous round. Activity rules similar to this one shorten auctions and are still standard features in modern auction design worldwide.  Critics alleged that the SMR was too novel to be reliable and too complicated for bidders to use. I countered those allegations by presenting Kwerel with a working SMR coded using Excel spreadsheets. Each bidder entered its bids on one spreadsheet, which included code to check that its bids satisfy the activity and eligibility rules and to inform the bidder if it violated either rule. The auctioneer had another spreadsheet, which imported bids for each round from the bidders’ spreadsheets, rechecked that the bids were legal, processed the bids, and output the round results. That proof was enough, leading Kwerel to recommend that the FCC adopt the Milgrom-Wilson design.  The initial auctions ran with no significant glitches. The design was welcomed by bidders, celebrated in the popular press, copied by several other countries, and declared a success by politicians and regulators. While the basic principles have remained largely unchanged, the rules continue to evolve from auction to auction. As I write these words, the “C band” auction has just finished, raising more than $80 billion for the US Treasury. Other auctions based on the SMR design raised hundreds of billions more in the US and other countries.  As successful as 1994 was for me professionally, 1995 was a personal disaster. My wife, Jan Thurston, suffered a brain injury that left her seriously disabled and with permanent personality changes. I spent the next year trying to salvage our marriage, but our relationship had changed too drastically. Instead of being my partner in caring for our two children as they struggled with the same family catastrophe, Jan had become an extra and difficult dependent. She will always be part of my family, but we could not remain married. Today, 25 years after her brain injury, we continue to have dinners together every Tuesday night, and I still manage her welfare and finances.  About a year after Jan’s injury, personal and professional changes began to happen. William Vickrey, the founder of modern auction theory, was selected to become a 1996 Laureate, but he died suddenly just after the prize announcement. Jean-Jacques Laffont and I were tapped to lecture in Stockholm to celebrate Vickrey and his life. At the Nobel Prize dinner in Stockholm, I was seated next to my future wife, Eva Meyersson, who charmed me from the start with her wit and laugh and tolerance for my awkward dancing. But there was a problem: she lived in Stockholm while I lived in California.  How does love overcome such a great distance? Home in California, thinking about Eva, I considered my next move. Any woman in Stockholm, I imagined, would be skeptical about starting a relationship with a California man and would likely be discouraged by her confidants, too, but a sufficiently grand gesture would be hard to dismiss. So, I wrote to Eva, “since you live in Stockholm and I live in Palo Alto, I’ll send you an airline ticket and meet you anywhere in the world.” That offer left some confidants speechless, but when Eva spoke to her grandmother, she got some sage advice. “Come back to Stockholm,” was Eva’s reply. So, I did.  Eva and I were married on September 17, 2000, on Eva’s 48th birthday. Eva and I traveled and had fun and visited Sweden often. In 2006, to celebrate our tenth meeting anniversary, the Nobel Foundation invited us back to Nobel celebrations and arranged a press conference for us, where I was to deliver the best single line of my life. On live TV, with Eva’s family and friends watching, I was asked how I felt that I had not won the Prize that year. I answered: “Oh, but I’m the one who went home with the biggest prize!”  My struggles in the mid-1990s led my research program to atrophy. As I emerged from my crisis, I restarted my research and teaching by narrowing my focus to study the challenges in designing spectrum auctions. Visiting Harvard and MIT in successive years, 2000 and 2001, I collaborated with [Alvin Roth](https://www.nobelprize.org/prizes/economic-sciences/2012/roth/facts/) to introduce the first graduate class in Market Design. Al had recently helped redesign the residency matching program for doctors and our class sought to connect our experiences and launch a new subfield in economics.  I wrote a book and several research papers to mine the same rich vein. Milgrom (2000) proved the efficiency properties of the SMR auction when bidders bid straightforwardly, showing how the development of prices could promote an efficient outcome when the licenses are substitutes. My 2004 book explored bidders’ incentives to bid more strategically; John Hatfield and Milgrom (2005) connected auction algorithms like the SMR to the matching algorithms like the one doctors use for their residency match; Milgrom (2009) introduced a bidding language that can be used in a sealed-bid auction to replicate some of the advantages of the SMR auction; and Nick Arnosti, Marissa Beck and Milgrom (2016) derived a new auction format for internet display advertising that mitigates the winner’s curse. I also consulted to help companies and governments design auctions for radio spectrum licenses, electrical power, internet advertising, agricultural commodities, and more. **The third era** Despite the SMR design’s celebrated performance, I worried that its supporting theory relied on the strong and sometimes badly unrealistic assumption that licenses were substitutes. My doubts grew in 1995, when Australian regulators proposed auctioning licenses to thin slices of spectrum covering tiny geographic areas (“postage stamps”). They had reasoned that phone companies could assemble combinations of postage-stamp licenses to fit any business plan. If the SMR could be counted on to reveal market-clearing prices before bidders made their final decisions, that would promote an efficient outcome. In actual auctions, however, it had become clear that the SMR was just a partial solution to the price discovery problem. There were two reasons. First, as I proved in my 2004 book, there might not be any market clearing prices if licenses are not substitutes. Second, in actual SMR auctions, bidding for more valuable licenses typically ends earlier than for less valuable ones. Even in an SMR, a bidder could still wind up acquiring too small a set of licenses to operate a viable business. Because the postage stamp licenses in the Australian proposal were far too small individually to support a viable business plan, they could sometimes be complements – the opposite of substitutes – creating an exposure problem.  This concern gave new urgency to re-evaluating the idea from the first FCC auction that bidders could submit bids on any package of licenses that make sense, with each bid winning either the entire package or nothing. Package auctions, however, raise difficult new problems. One is that there are too many packages for bidders to evaluate without the guiding information of gradually revealed prices. For example, in FCC auction #66, with 1,400 items offered, the number of packages is 21400, which is more than 10421 – far too many for any bidder to list. Computing winning package bids is NP-hard, which means it can be too challenging even for a fast computer in such a large-scale auction. Vickrey pricing rules could provide incentives to bid truthfully in a package auction, but Lawrence Ausubel and Milgrom (2005a) identified a long list of other problems with that auction design.  My work to improve combinatorial design included sealed bid designs in Ausubel and Milgrom (2002a, 2002b, 2005b) and Robert Day and Milgrom (2008, 2013) and the dynamic “combinatorial clock auction” of Ausubel, Peter Cramton and Milgrom (2005), which has become the next most common spectrum auction family after the SMR family of designs. John Kagel, Yuanchuan Lien and I (2010, 2014) report lab experiments to evaluate how certain complex combinatorial designs perform in practice, at least when the bidders are students of the kind who are subjects in these experiments. Jeremy Bulow, Jonathan Levin, and I (2017) report on the exposure problem in the SMR from the perspective of a bidder that we advised.  My greatest auction design challenge, however, was yet to come. In 2012, as part of the US stimulus legislation, the FCC was required to conduct an auction to buy television broadcast rights, relocate the remaining broadcasters into a smaller set of channels, and sell mobile broadband rights to willing buyers. The idea had been proposed by Kwerel and John Williams and the auction problem it created was the most complicated one in history. The FCC asked me to assemble a team through my company, Auctionomics, to design and create software for the auction. Two new auction problems were the most challenging ones.  First, given a number of channels to clear, how would the procurement auction work to buy enough broadcast rights to make that possible? The question of whether it is possible to pack a given set of stations on a given set of channels is in a computer science complexity class called “NP-complete.” For such questions, using even the fastest known algorithms, the time to solve a problem cannot be guaranteed to be less than an exponential function of the problem size. Our problems had more than a million constraints and the auction to select a winning combination needs to solve thousands of such problems, so the auction would have to work well even when the packing problems are sometimes unsolved. I hired Kevin Leyton-Brown to develop customized algorithms that worked for most of the problems we encountered and another collaborator, Ilya Segal, to help devise auction rules that would work well even when the algorithm failed to solve some packing problems.  Second was the question of how many channels to clear. In a standard market with buyers and sellers, that is solved by finding a price at which the numbers of goods demanded and supplied are equal. But television broadcasters and broadband providers cannot just be counted on to find the clearing price. Each TV station operates from a distinct location and power that distinguishes it from every other station. Different broadband licenses similarly serve different areas and populations. To accommodate this heterogeneity, the auction would need thousands of different prices.  The technical solutions to these auction design problems were worked out between 2011 and 2015. The details of those were reported in Milgrom and Ilya Segal (2017, 2020) and Leyton-Brown, Milgrom and Segal (2017).  In 2020, in recognition of “improvements to auction theory and creation of new auction designs,” Bob Wilson and I were awarded the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel.  I have continued to publish new research but now dedicate more of my time and effort to training students. Reflecting on the historic accomplishments of my friends and collaborators and the exceptionally fine scholars who surround me now, I am optimistic about the future. As Tennyson wrote in his poem Ulysses: “Some work of noble note may yet be done, not unbecoming men that strove with Gods.” **Bibliography** Arnosti, N., Beck, M., and Milgrom, P. (2016). Adverse selection and auction design for internet display advertising. *American Economic Review*, 106(10), 2852–66.  Ausubel, L. M., and Milgrom, P. (2006). The lovely but lonely Vickrey auction. *Combinatorial auctions*, 17, 22–26.  Ausubel, L. M., Cramton, P., and Milgrom, P. (2006). The clock-proxy auction: A practical combinatorial auction design. *Handbook of Spectrum Auction Design*, 120–140.  Bichler, M., Milgrom, P., and Schwarz, G. (2020). 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| Autobiographical |  |
| Podcast | **“I try to lead a conversation rather than telling students what to think”** ”Sometimes I make a mistake during the lecture, I’ll make an argument that’s not quite right and not even notice it, and a student will catch me. I just love it when students do that.” Economic sciences laureate Paul Milgrom loves being a teacher. He also loves when his students correct him and ask him hard questions that he doesn’t know the answer to. In this podcast episode, conducted in December 2021, economist Milgrom talks about his own school years and what type of teacher he aspires to be.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0810=PM  Adam Smith: Hello, am I speaking with Paul Milgrom?  PM: Yes, you are.  AS: This is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize in Stockholm. Well, congratulations on the award of the Prize in Economic Sciences, which has just been announced in Stockholm.  PM: Well, thank you. Thank you very much.  AS: May I ask how you received the news?  PM: My doorbell rang, and Bob Wilson, who was the co-winner I understand, was knocking at my front door, because nobody was able to reach me. So that woke me up, 20 minutes ago or so.  AS: I think that might be a first, that one laureate comes knocking at the other laureate’s door. Are you … Do you live very close to each other then?  PM: Pretty much right across the street from each other.  AS: What a lovely way to receive the news.  PM: Yes, he and his wife just walked over and rang the doorbell, and I have a video doorbell, so I answered and it was him.  AS: Yeah, quite a way to be woken up.  PM: Yes, yeah, and now there’s lots and lots of stuff coming in.  AS: So, you’ve already introduced the fact that it’s … you’re amazingly close to Robert Wilson. You were his student. It must be extraordinary to win with him.  PM: Yes, it really is quite amazing. He was my PhD dissertation advisor, the… I picked the topic mainly to, you know, working on auctions for my PhD, mostly to get him interested in being my advisor. He was the advisor everybody recommended you should have. So I said okay I’ll try working on a problem of interest to him and he got excited about it, and here we are. And lots has happened in between.  AS: And what is it about the two of you that makes you work so well together? What’s the magic?  PM: It’s a really good question. I mean, I think we’re both a little bit … we’re both nerds in, you know, in a certain way. We … I don’t really know what to say about the magic between us. We haven’t done that many joint projects. I think probably … you know I haven’t seen what the Foundation has cited, but I’m guessing …  AS: So, the citation is “improvements to auction theory and inventions of new auction formats.”  PM: You know, I was not working on practical things at all in 1993 when Bob and I were both mostly interested in economic theory, and I remember the … there was a need for an auction for radio-spectrum in the US, there are now auctions for radio-spectrum all over the world, and Congress had just authorised it, and the … and nobody knew how to do it, because of the complexities involved and that particular design, and when people called me, I thought ‘well, I don’t know how to do it’, that’s you know, there’s all kind of practical details I don’t know anything about. And they asked me to look at the proposal that had been made, and I looked at it and said ‘well, I may not know the best way to do it, but I can do better than that’ and I started talking to Bob. People had no clue of how it … of how to even approach it, and so we put together a practical design which, you know, has subsequently been used round the world.  AS: And for so many other things more than just radio frequencies. People are terribly worried right at the moment about the economic effects of the pandemic. How does the work you’ve done together on auctions help policy makers improve individuals’ economic stability these days?  PM: Yeah, I mean, the special challenges now are about, you know, how to solve hard resource-allocation problems, and how to create markets that allocate goods efficiently. I mean the biggest challenges today are not those. The biggest challenges today are of course the medical challenges, the vaccine development, the way we will address the shortages that exist and taking care of displaced people that has resulted from the pandemic. So I wouldn’t say that it is directly relevant to solving the biggest … those biggest challenges. But then other big challenges today, like environmental resources, that … allocating those well, where efficient method design can be valuable.  AS: You are, of course, intimately connected with the Nobel Prize already because didn’t you meet your wife at the Nobel Prize banquet?  PM: Yes, that’s correct. In 1996, I did.  AS: I guess soon enough, although not this year unfortunately, you’ll have the chance to be there again together. It’s been lovely to speak to you, and many, many congratulations.  PM: Thank you.  AS: Bye bye.  PM: Bye. |
| Interview |  |
| Q7 | How did you find out you’d been awarded the Prize in Economic Sciences? |
|  | Even though I knew it was the night of the prize announcement, even though people had been talking to me about it, I said, ‘I’m just going to do what I always do every night.’ I turned off my phone because that’s who I am, that’s who I’m going to be. Sure enough, this was the year that they decided to call me and I don’t have a landline anymore. I only have my cell phone. So Bob [Milgrom’s co-laureate and PhD supervisor [Robert Wilson](https://www.nobelprize.org/prizes/economics/2020/wilson/facts/)] comes by and Bob made this really hard because not only did he wake me up, he rang the doorbell and said, ‘Paul, they’re trying to call you from Stockholm. You have won the Nobel Prize.’ He doesn’t say we have won the prize. He says, you have won a Nobel Prize. I wake up and I’m a little groggy, I’m still not fully awake and I’m thinking, ‘There’s something that’s wrong with this picture.’ A call from Stockholm that went to Bob at two in the morning and he’s come to my house to tell me that I won the Nobel Prize? I didn’t know what I was supposed to think. I’m trying to figure out what’s actually happening because I don’t quite believe what he’s saying and haven’t quite put it all together yet. So that’s what happened. |
| Q6 | How has your life changed since receiving the award? |
|  | I’ve won lots of awards but nothing has the public impact like this prize. This is just completely different. But it’s also this combination of the Nobel Prize and the pandemic year. On my side, the pandemic year, it was already an unusually busy year. We’re asking the faculty just this one year to teach a little more and add some things. So I had added actually quite a lot to my schedule. I had more class teaching, teaching more hours, recording lectures. Then on top of that, once the prize came in there, thousands of extra emails and requests to give speeches and saying no, it’s really hard. There are perfectly reasonable requests, but there are too many of them. So that was one of the reasons I hired my daughter. She says no for me. And [Alvin Roth](https://www.nobelprize.org/prizes/economic-sciences/2012/roth/facts/) [Prize in Economic Sciences 2012] gave me this ‘NO’ bell that is a great gift. |
| Q8 | How did you meet your co-laureate Robert Wilson? |
|  | I got into graduate school at Stanford and then I wasn’t sure what to do to be a successful graduate student. I talked to a graduate student who was a year ahead of me, [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/). He said, ‘The important thing is to get Bob Wilson to be your advisor. He’s the best advisor around here’.  So I decided I would take Bob’s class about current research and economics. Every year, he teaches a class where he goes over papers that are recently written and he went over one paper of his own and it was about auctions. I wanted to get his attention so I decided I would write my term paper to see if I could extend his work. Little did I know Bob had worked very hard on this problem. He’d been stumped and he had worked with another faculty member. I wrote a term paper, in which I solved the problem. He said, ‘This is a PhD thesis. This is it, you’re practically done.’ He was really excited and said, ‘Well, okay, it’s not quite a PhD thesis. You need to write an introduction. You need to write a literature review. Here’s all the papers you need to read better, but then you’ll be done.’ I finished very fast, two and a half years in graduate school. |
| Q7 | You received the prize for your work on auctions – have you ever won anything on an auction? |
|  | I don’t know if you’re into sports, but one of the most famous plays in a US sports history is something called ‘the catch’, which took place on 10 January 1982. I acquired that ball at auction with auction strategy. I saw that it was for sale at auction and the auction was badly designed, but I looked at it and said, ‘That’s not a well-designed auction. I’ll bet I can get it relatively cheap.’ And I did. |
| Q3 | Do you have a role model, mentor or teacher that has influenced you? |
|  | Bob Wilson, but there’s another man who’s another Nobel Laureate actually – [Kenneth Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), who’s always been my intellectual hero and who was my colleague here at Stanford for many years. He had multiple laureates among his students; [Eric Maskin](https://www.nobelprize.org/prizes/economic-sciences/2007/maskin/facts/), [Roger Myerson](https://www.nobelprize.org/prizes/economic-sciences/2007/myerson/facts/), [Leonid Hurwicz](https://www.nobelprize.org/prizes/economic-sciences/2007/hurwicz/facts/). He devoted time to educating students. I spend a lot of time working with students as well. |
| Q14 | Is diversity important in the field of economic sciences? What is your perspective on that? |
|  | My perspective mainly is that I want people to achieve what they’re capable of. I have some really talented people that are here and it’s a disappointment to me that only some of them are soaring. That must be that I’m failing the quieter students and the women in particular. It’s not so much that I’m aiming for diversity as I’m aiming for success and training for all of my students. It turns out what I’m doing now is better for the quieter men too. It’s even better for the noisier men. It just works better. |
| Q10 | What advice do you usually give to younger researchers? |
|  | Don’t waste your time with things that aren’t going to matter. Make sure you have a couple of things going at a time, some stuff that’s trackable that you can make progress on and some stuff that you’d really like to make progress on that you’re thinking about.  Failure’s part of it. I also tell them stories about the greatest researchers and the failures that they’ve had. I’m not going to try to repeat them. They’re more or less confidential, but the most famous people in our profession have all had times when they bang their heads against something and failed. I tell them those stories and say don’t give up. |
| Q11 | What advice do you have on dealing with failure and unexpected problems? |
|  | One of the things is that I want these guys to have portfolios and not be working on a single thing so that if they get stuck on something they could shift attention to something else. I think that it’s also dangerous. It can isolate you. That’s part of the reason for the social part of what I do. I think that people can become isolated and depressed and then they can go hide. I have to prevent that from happening. I’d like people to have in mind that there are lots of things they could be working on and they’re working on something right now, if it fails, there’s something else they can move to and get support and collaborate.  I was an assistant professor at Northwestern University. We had a group of people that included [Roger Myerson](https://www.nobelprize.org/prizes/economic-sciences/2007/myerson/facts/), [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/), [Dale Mortensen](https://www.nobelprize.org/prizes/economic-sciences/2010/mortensen/facts/) who all are laureates. A group of young people, all of whom just thrived. We were not people who were getting offers from all the best places so how did this happen? I think that it’s partly social. It was a corridor we all worked in and we had lunch together every day in a coffee room. You could go in and see one guy explaining work to somebody else. We made comments on one another’s work, which were helpful. And yet we also competed. I would go out there and see Myerson and Holmström doing the work and say, ‘Holy cow, that’s awfully good. I’ve got to make my work better’. You’re comparing yourself to the others in a group but you also provide support. I’m trying to create a similar structure for my graduate students. One that support them in the way that I think I was supported in. |
| Q28 | How do you like to spend your free time? |
|  | There’s something called free time? I don’t have a lot of free time, but I love spending time with family. When I was healthy (I had a knee replacement last year) I used to ski and I’m looking forward to skiing here again. I used to play tennis. We go hiking, we go for bike rides. I hang out with family, nice dinners.  I read some fiction, some nonfiction. I haven’t had much free time in the last months actually. So I haven’t done a lot of reading, but hope to get back to that. I have some books that I loved reading but then I get onto the next one. It’s like in my research to write the thing that I’m most excited about is usually something recent. That’s how I am, I move on. |
| Q46 | You’ve been to four Nobel Prize banquets and I heard that you met your wife at one of them. Can you tell us about that? |
|  | I was there because [William Vickrey](https://www.nobelprize.org/prizes/economic-sciences/1996/vickrey/facts/), who won the prize along with [James Mirrlees](https://www.nobelprize.org/prizes/economic-sciences/1996/mirrlees/facts/) in 96, had a heart attack and died and they needed somebody to give the prize lecture. I was invited to give a portion of that lecture. They seated Eva next to me during the dinner. We had a really good time during the dinner. I really liked her. I thought she liked me. We danced and we had a good time. I could tell you more stories about the dinner, but I got home and I thought, ‘Well, am I going to do anything about this? She lives in Stockholm. I live in California. I’m going to reach out to her and she’s going to talk to her girlfriends and they’re all going to say he lives in California. That can’t work.’ I am a game theorist so I decided I needed to make a move that would counter that. So I sent her a really nice letter. I told her how much I enjoyed our evening and that I really wanted to see her again. Given that she lived in Stockholm and I lived in California, I would send her a plane ticket to meet her anywhere in the world. We got married around 20 years ago.  Ten years later, we were invited back by the Nobel Foundation to celebrate our 10th meeting anniversary and to give a press interview. I have to tell you this story too, because I delivered the best one-liner of my life in this interview. During an interview they asked me, ‘How do you feel about the fact that it wasn’t you who won the Nobel Prize?’ I replied, ‘Oh, I’m the one who went home with the big prize back here.’ It’s the best line of my life because people were talking about that line for the whole next year. I wish I could come up with a line that good again, but it just came right out of me. |
| ID | 0811 |
| Biographical | **1.Overview at age 83** After rocky early years, I had a happy youth in a small town, and then stumbled through eight years at Harvard, emerging with little sense of what to do next, until I moved to Stanford where my research thrived. A minor project on adverse selection in auctions led me to join in the nascent reconstruction of economic theory using game-theoretic models, and then later, foundational topics in game theory, all focused on the role of agents’ information and their effect on incentives. I’ve enjoyed working with PhD students and been fortunate to have superb co-authors with better skills. **2. Youth: 1937–1955** I was born in Geneva Nebraska on 16 May 1937 to Robert Butler Wilson Sr. and Naoma Luella Norgren, the first of three sons, with my brother Charles following 13 months later in 1938, and then Timothy 4 years later.  My father Robert was born 5 May 1917 in Moore Montana, the youngest of three children of Clarence Wilson and Elizabeth Butler. Clarence was an unschooled mule trader from Missouri, descended from one of two brothers from Virginia who crossed the Appalachian Mountains circa 1820 to start farms in a stretch of fertile land along the Ohio River across from Tennessee, later called Wilson’s Bottoms. Elizabeth, the daughter of a prominent landowner in the small town of Geneva Nebraska, graduated *summa cum laude* from the University of Nebraska in 1902. They began their mismatched marriage by emigrating to Montana to homestead near Moore, starting from a sod hut to build a house and farm using her inheritance. They were ill-equipped to till rocky soil, and the farm was barely sustainable. When funds ran out, they moved back to Geneva circa 1929 to run a paint mill that Elizabeth inherited, but Clarence died a few days after I was born, and amid the great depression the paint mill went bankrupt in 1939.  My mother Naoma was born 25 December 1916 in York Nebraska, twenty miles from Geneva, the second of three daughters of Leonard Norgren and Grace May Muse. Grace descended from English stock with little trace of their origins. Leonard was the son of John Stahl Norgren and his wife Katherine, who married the same day in 1880 that he left the army as a medic and they emigrated from Kristianstad in Skåne County to settle in the Swedish-speaking small town of Ong Nebraska, ten miles from Geneva. They were successful farmers and all their ten children survived into adulthood. Later, my beloved ‘grandpa’ Leonard could still recite the Lord’s Prayer in Swedish. Leonard and Grace lived in York in near poverty through the great depression, sustained by a poultry supply business. Grace died early, cared for my mother Naoma.  Robert and Naoma married 23 August 1935, and settled in Geneva, where I was born two years later. My father clerked at the paint mill until it closed, and then we moved to the Montana farm, sharing the farmhouse for a year with his sister’s family, and then two years in Billings Montana, where my father was a salesclerk until the war started in 1941. With most men away at war, he got a good job in Lincoln Nebraska. We were mildly comfortable until he was drafted on Christmas day 1943. To cope with renewed penury, we moved to the farmhouse of Naoma’s younger sister near Ogallala Nebraska. I attended first grade in a one-room schoolhouse, and one day gleefully rode there on a horse. To ease our predicament, grandpa Leonard bought for us in 1944 a tiny house in York, where we lived for nine years until I was 16 and then moved to Lincoln in 1953. After his release from the Navy in 1946, my father was a candy salesman and then an insurance salesman, and later adamant that I become an insurance actuary rather than ‘waste’ my life in a university.  My childhood in York was wonderfully happy, usually playing with my brother Chuck, who was so close in age that we were like twins. We loved exploring the creeks, fields, and farms around York. Our mother was very loving, but she didn’t know how to handle boys and let us run wild. Chuck and I rode our bikes into the countryside to hunt, fish for bullheads and catfish, trap furry animals, and camp along Beaver Creek and the Blue River, all with minimal supervision. In the summers, we swam every day at York’s pool. In the evenings after the clouds broke on hot humid nights, we caught nightcrawler worms that we nurtured in dirt-filled boxes in the garage and sold as bait. My grandmother Elizabeth Wilson lived with us in the late 1940s and instilled a love of reading by often taking us to the library. We went to three double-bill movies each week, read hundreds of comic books, and played poker with pals. Chuck and I had been sneaking out with Dad’s rifle since I was 9 to hunt varmints like pigeons, crows, rabbits and squirrels, but by 12 I had bought my own rifle and shotgun and moved up to doves, pheasants, ducks, and geese. In a neighbor’s unused garage, we kept birds and animals we trapped for wildlife study. We mail-ordered pigeons (white kings, Russian trumpeters, pygmy pouters) that we raised with pride, and won prizes at the York County Fair. I raised angora rabbits to shear for wool, a ferret that eventually bit our mother, and families of orphaned opossums and owlets; and once I brought home a skunk that I mistakenly trapped while trying to catch mink, but alas it sprayed me thoroughly. We lived a carefree existence, rather like Tom Sawyer and Huck Finn.  I always had a job. From age 7, I delivered newspapers (even in blizzards), mowed grass, shoveled snow, bagged groceries, swept businesses’ walkways, and for two summers toiled in a bottling plant. One summer I managed the golfers’ clubhouse for my grandpa Leonard who had the concession. I excelled in football, basketball, and track until my right leg broke in a football game, but I continued basketball and golf to win a letter. The local schools were good, and I was always at the top of my class.  We were innocent of the economic struggles of our peers, except to know that some had less than we, and a few had more. An exception was my deep concern as a young boy for a kindly old lady with a broken hip who, before Medicare, suffered with little help until her death. After a severe concussion at age 14 in a playful melee I had vision and mood problems, retreated from social interactions, and became the school nerd with no steady girlfriend. Later I began anti-convulsive medication that cleared it away and I regained my old self.  In 1953 my father got a better job in Lincoln Nebraska and bought a nicer house. For two years I attended Lincoln High School, which was ten times the size of York’s, with better teachers and diverse courses, and was much more interesting and challenging. I was an exchange student in Giessen Germany for the summer of 1954, which was deeply stimulating after a childhood in insular small towns. In 1955 I graduated as salutatorian, and senior class president. **3. Education: 1955–1964** Urged by my mother, I applied for and won a prestigious General Motors National Scholarship to attend Harvard, but the stipend (set by Harvard, but GM would approve twice that amount) was so meager that at considerable risk I hitchhiked to and from Nebraska, and for four years I worked as library page, sold milk and donuts in the residence halls, and entered data at the computer center; and in summers, I sold ice cream and then insurance before real jobs as engineer on the Regulus missile and then actuarial trainee. At the end of my third year in 1958 I married my girlfriend Barbara Ferne Saylors, born 29 October 1937 in Hobart Oklahoma and then a student at Radcliff College from Albuquerque New Mexico, whose scholarship was so paltry that she had a board-and-room job, and then had to drop out after her second year. With her job at a local insurance company, we were more comfortable, but Harvard threatened discipline for marrying and living off-campus. We were in one of the first cohorts in Harvard and Radcliffe’s programs of ‘national distribution’ to diversify their students, but often the privilege of being there was outweighed by snobbery towards naive midwesterners who had not attended elite preparatory schools, and Barbara hated the sexism. I tried various majors (engineering, math, philosophy) but they were all unsatisfying, and some of the teaching was abysmal. Other than basic skills, little of it served me later.  Still, I thought I was lucky to be at the ‘the center of the universe’ (in the smug view that prevailed) and I had met Howard Raiffa, then in the statistics department but moving to the business school, so I applied to the doctoral program there, knowing that I had to meet their bizarre prerequisite of first completing the two-year MBA program. I borrowed funds, diligently pursued the program, got a citation for my high-grade average, and worked one summer as financial officer of a small insurance company in St. Paul Minnesota. But other than elective courses with John Lintner and Howard Raiffa it was a waste because the case method of teaching and insistence on an ‘administrative point of view’ were accompanied by abhorrence of theory and science generally. I was dismayed that most professors were ignorant and dismissive of the technical literatures of their fields. I came to view case studies as useful inputs to what should continue with deeper consideration of their implications and the construction of explanatory theory.  Undaunted, I entered the doctoral program in 1961 with a Ford Foundation Fellowship. I made the required field trips to companies, wrote the requisite number of cases, and eked through the mandatory ‘administrative point of view’ exam by faking it. Studies of decision theory with Howard Raiffa were superb. I took some courses in the economics department and studied applied math on my own to produce a dissertation about a new algorithm for constrained optimization. It would not have been finished without Howard’s intensive efforts to help me complete an acceptable version. At the end, I had a pregnant wife and two-year-old daughter Jennifer, born 14 May 1961, but no money, no car, and losing our apartment, so Howard gave me $1000 dollars to get us to UCLA for my first teaching job in June 1963. I always liked and admired Howard and remained deeply grateful that he mentored me and set me onto the path of my career.  At UCLA, Jacob Marshak was kind and supportive, and he loaned me another $1000 to tide me over the transition, but when, contrary to the letter of appointment, UCLA docked my pay for what they had previously paid to reimburse my moving expenses (eventually reversed by the president of the university), I was suddenly destitute, with Barbara about to deliver our second daughter Holly, born 10 December 1963, and the doctor requiring prepayment. So, I began consulting on reliability analysis at the RAND Corporation. It was wonderfully interesting due to discussions with Albert Madansky, and especially with Lloyd Shapley about game theory. I stayed at UCLA only one year because I learned that Howard Raiffa was visiting next year at Stanford’s business school, so I pleaded for and got a visiting appointment beginning July 1964 that was then continued with a regular appointment. **4. Early Career: 1964–1980** We lived in Palo Alto for 6 years and then moved to a house on campus that we built on a parcel leased from the university. I have happily stayed at Stanford for 56 years, always encouraged in my research. The travails to get to Stanford were worth it because Stanford is a perfect place. Palo Alto and the faculty housing on campus are pleasant communities with excellent schools for Jen and Holly, and dear friends. I liked biking, later walking, still later running to and from the university. There was endless joy in hiking in nearby parks, plus frequent backpacking and cross-country skiing in the Sierras with Barbara, Jen, and Holly. I had stimulating colleagues at the business school and elsewhere in the university, began lifelong friendships with Michael Harrison and David Kreps whom I esteemed, and had a nice lunch routine of Ping-Pong with students or chess with Mike. Barbara and Jennifer finished college at Stanford and loved it, including the absence of sexism. Tenure came effortlessly after only 4 years because it was awarded to counter an offer from Harvard initiated by Howard Raiffa. I was set for my life’s work as a scholar, which I relished, though I did serve 5 years as administrative director of the PhD program.  I was a passable teacher in basic MBA courses, but students’ evaluations often had 10% saying I was terrible and 10% saying I was excellent. Essentially, I taught to the segment having what I deemed intellectual curiosity, and this rankled some others. I had no regrets when I retired in 2004 from MBA teaching after 40 years, precipitated by a class where my style was disastrous. The elective courses I offered were more successful, and in later years a few former students, presumably in the 10%, said they were the best they took. Those courses (Multiperson Decision Theory, Competitive Strategies, and later one on Market Design) were my way of introducing strategic analysis into the MBA curriculum.  I loved PhD teaching. I regularly taught a first-year PhD course, and the advanced course Multiperson Decision Theory in which I presented current research in game theory and its applications; I’ve now taught it for 52 years. I liked working with talented young scholars, and we were blessed with many. Moreover, a series of fine students came to me from engineering and the economics department because I was doing research on what their departments were ignoring. My life feels fulfilled by their accomplishments and I am very proud that I had a role encouraging their early work. ‘Encouraging’ is the right word because my style was simply to help them pursue their interests. A great delight is that [Alvin Roth](https://www.nobelprize.org/prizes/economic-sciences/2012/roth/facts/), [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/), and Paul Milgrom were awarded Nobel Prizes, but I am also elated by the success of three dozen others, including Robert Rosenthal, Jean-Pierre Ponssard, Claude d’Aspremont, Peter Cramton, Christopher Avery, Marciano Siniscalchi, Muhamet Yildiz, David Ahn, David McAdams, Jozsef Sakovics, Qingmin Liu, Yuval Salant, Ron Siegel, Yuliy Sannikov, …  At UCLA I had wasted the year working on sequential sampling and developing and distributing computer code for my optimization algorithm, which came to be called the quasi-Newton method or the Chen-Wilson algorithm, neither of which I published. At Stanford I made a slow start on a vague agenda to develop multiperson decision theory. My first work was about efficient risk sharing, and the second was about vote trading in legislatures, which led me to work on social choice theory in the style of [Kenneth Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), with whom I often had stimulating conversations. My main contribution characterized social choice without one of Arrow’s assumptions. But I moved on because social choice theory in that style seemed to lead nowhere. I was much more interested in using the more promising tools of game theory, and indeed I had sustained an interest since my conversations with [Lloyd Shapley](https://www.nobelprize.org/prizes/economic-sciences/2012/shapley/facts/) at RAND and UCLA. A first contribution (never published but cited by Shapley and reproduced by von Stengel and others) was a simple example of a two-player game for which a pair of [Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/) equilibria cannot be found by the Lemke-Howson algorithm unless it starts with one of them, and then a second was a generalization of that algorithm to many-player games, published in the same issue of the SIAM journal as an alternative generalization by another author. In the meantime, I was also studying auctions, motivated by a case at HBS about bidding for offshore exploration leases. First, I took seriously the case situation that one bidder with an adjoining tract had superior information and made a poorly crafted attempt to characterize the mixed strategy of the bidder with inferior information, later solved in a better article by Milgrom and Weber. But a second article showed the effects of adverse selection in a symmetric game with both parties having imperfect information, later called the winner’s curse by Capen, that anticipated the later simulation results of Capen, Clapp, and Campbell that revolutionized how firms bid in such auctions. I then advised the brilliant PhD student Armando Ortega-Reichert on his excellent dissertation, which was never published but the working paper version affected subsequent research by many others. My work on auctions led Bobby Abraham at Weyerhaeuser timber company to have me present a series of lectures, for which I also devised a set of short hypothetical cases to exemplify how game theory could be used. This work attracted the interest of Darius Gaskins at the U.S. Department of Interior who hired me as a consultant for studies of bidding for exploration leases that continued for several years. After 1978 I also consulted with oil companies on bidding strategies and in the process developed explicit models with practical applications to bidding. The culmination was an article that showed how with many bidders in a symmetric auction with bidders having imperfect information about a common value the winning bid could converge to the true value, which is the classic problem of ‘rational expectations’ or alternatively the ‘efficient markets hypothesis’ that the market-clearing price summarizes all participants’ information. My analysis was awkward because with my limited skills I had to assume that the support of bidders’ estimates moved monotonically with the unobserved true value – later results by Paul Milgrom were more general, and his work with Robert Weber established rigorous foundations for much of what is known about auctions. I also published a model of auctions of shares of a fixed supply that led to later models of double auctions with both buyers and sellers bidding. I showed that, when traders have unit demands or supplies and independent value distributions, a double auction is an incentive efficient mechanism if the number of traders is sufficiently large, but I was frustrated that I needed that proviso. I was especially interested in Chris Avery’s dissertation showing that an auction that allows jump bids has many equilibria with revenue outcomes spanning a wide range. Over the next decade I occasionally did other theoretical work on auctions, and related markets run via posted ask and bid prices, wrote several chapters published in books, and started but never finished my own book. In the 1990s I was absorbed by practical design of spectrum auctions with Milgrom and then consulting on the design of restructured wholesale markets for electric power. Besides rational expectations, which even now I work on with Paulo Barelli and Srihari Govindan, another continuing interest is showing that a double auction is an optimal mechanism.  During the 1970s and 1980s there were other influences. One began in 1978 with consulting at EPRI with Hung-po Chao and Shmuel Oren on retail pricing of electric power, especially priority service contracts to implement demand-side curtailment in periods of short supplies, and then EPRI’s support for preparation of a book on the general theory of nonlinear pricing. In 1967 I spent a term at CORE in Belgium where I got more involved in standard economics. I had learned little from the few economics courses I took, all of which assumed some version of general equilibrium theory’s focus on existence of prices that cleared markets, so I began to learn it for myself, and in my own way focused on strategic behavior and the role of agents’ private information. In 1971 I used a sabbatical to reside in the Stanford economics department (and kept an office there for 8 years) with very interesting colleagues such as [Joseph Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/) and [Michael Spence](https://www.nobelprize.org/prizes/economic-sciences/2001/spence/facts/), and thereafter each summer to attend the series of IMSSS seminar series on economic theory organized by Mordecai Kurz. The attendees and their seminars were wonderfully stimulating, and I was greatly influenced by several fine scholars, especially [Leonid Hurwicz](https://www.nobelprize.org/prizes/economic-sciences/2007/hurwicz/facts/) who was developing his mechanism design approach to economic theory, and [Robert Aumann](https://www.nobelprize.org/prizes/economic-sciences/2005/aumann/facts/) and his colleagues from Israel who were doing the best work on game theory. It was a delightful way to get involved in the nascent reconstruction of economic theory by infusing it with models and methods drawn from game theory. I won a Guggenheim Fellowship and spent 1977–78 at the local behavioral sciences center but wasted it working fruitlessly on an integer programming algorithm, motivated by Herbert Scarf’s contemporaneous year-long visit to Stanford. **5. Late Career: 1980–2004** In 1978 my beloved associate dean Robert Jaedicke asked me to create a new economics group within the business school. This entailed hiring new faculty and our first hires were [Ben Bernanke](https://www.nobelprize.org/prizes/economic-sciences/2022/bernanke/facts/), Jeremy Bulow, Margaret Bray, and John Roberts, and later Anat Admati and Faruk Gül. It also meant creating new PhD courses and specifying requirements imposed on PhD students. We started right off with superb students and the new program was very successful and imitated elsewhere. Dave Kreps, Faruk Gül, and I did not actually join the new economics group until 5 years later when we were unanimously expelled from our old group, which then reverted to operations research and management.  In 1980 Dave Kreps returned from a sabbatical year in England eager to work on [Reinhard Selten](https://www.nobelprize.org/prizes/economic-sciences/1994/selten/facts/)’s ‘Chain Store Game’. This led to our work on reputation effects in dynamic games with applications to entry deterrence and to the finitely repeated ‘prisoners dilemma’, the latter with Paul Milgrom and John Roberts; and then to our refinement of Nash equilibrium called sequential equilibrium, intended as a generalization of backward induction and easier to apply than Selten’s refinement called perfect equilibrium. This work was influential, and we were pleased with ourselves until a summer day in 1984 when Elon Kohlberg visited to tell us his and Jean-François Mertens’ criticism of our refinement on the grounds that it violated basic principles like invariance and forward induction. Kreps remained unmoved (and argued why in an article with In-Koo Cho) but I was deeply affected and spent much of later decades working with Srihari Govindan to remedy the defects, resulting ultimately in our axiomization of Mertens’ definition of stability. In the meantime, I had fun working with Faruk Gül and Hugo Sonnenschein, who were visiting for a year, on the [Coase](https://www.nobelprize.org/prizes/economic-sciences/1991/coase/facts/) conjecture. This led to heightened interest in Peter Cramton’s dissertation that constructed a model of bargaining based on parties’ signaling their own values rather than screening through the other’s possible values; his model does not have the Coase property but better describes situations like a firm and union bargaining over wages. I did considerable work on bargaining without great success, except for a survey article written with John Kennan and a lecture published as one of the Schwartz Lectures at Northwestern. **6. Retirement: 2004–2021** After retiring in 2004 I continued teaching the two PhD courses and advising PhD students. But five years later the university banned emeritus faculty from being principal advisors on dissertations, which to my dismay diminished my student contacts.  I started working with Srihari Govindan on foundational game theory in 1995 and continue still in our search for the full implications of rationality in multi-person contexts. In one article we show that invariant sequential equilibria satisfy forward induction. A series of articles culminated in three axioms that characterize Mertens’ stable sets of Nash equilibria. That article was confined to extensive games with generic payoffs and two players, but recently it is extended to any number of players by strengthening the key axiom that excludes ‘framing effects’, and a slightly stronger axiom that characterizes essential sets of equilibria. Another recent result shows conditions for convergence, as the number of traders increases, of equilibria of asymmetric auctions and double auctions to a Walrasian ‘rational expectations’ equilibrium in which the clearing price identifies a hidden state. We also identify Walrasian equilibria that cannot be obtained as limits of equilibria of auctions.  We’ve also studied the infinitely repeated prisoner’s dilemma with alternating moves when the players have bounded recall. Our computational results show that the unique subgame-perfect outcome is perpetual cooperation when players’ recalls are no more than 4, but many attempts to prove that this is true for longer recalls has not yet succeeded. **7. Retrospective** My research in economics can be summarized by saying that I studied the effects of private information in markets, especially auctions, at a time when this was novel. My lasting contributions, neither about auctions, were co-authored with David Kreps and Srihari Govindan, on whom I relied for their superior skills and deep insights.  In 2020 the Sveriges Riksbank Prize in Economics Sciences in Memory of Alfred Nobel was awarded to Paul Milgrom and me. The diploma cites “improvements in auction theory and inventions of new auction formats”. In fact, the superbly talented Paul Milgrom, and his co-authors and descendants, proved the basic theorems that were the improvements in auction theory, and his genius produced the innovative features of the new designs of spectrum auctions. Earlier, I had provided formulations of various auction formats and studied examples, but I never proved a real theorem. I’ve been aware that my exploratory studies of auctions were influential among other researchers because they offered a new perspective that fit the growing interest in game theory as a tool for re-examining economic theory. So apparently, I shared the prize in my old age because my work as a youngster was influential (but never definitive) in the early stages of a process that has now matured.  I deeply value friendships with my colleagues Srihari Govindan and David Kreps, my former students Claude d’Aspremont, Peter Cramton, Bengt Holmström, Paul Milgrom, Jean-Pierre Ponssard, and Alvin Roth, and others I get to visit with less often. The plain fact is that I like and admire my students – including several I work with now.  Sadly, my wife Barbara died 6 March 2010 after 52 years of marriage. Since 23 March 2013 I am married to Mary Riemann, born 19 January 1957 in Sioux Falls, South Dakota. We are soulmates and supremely happy together. We share our love of nature, often hiking nearby and in the mountains.  I’ve had a wonderful life as a husband and father, and as a scholar and teacher. I was lucky to have it come to me accidentally. |
| Autobiographical |  |
| Podcast | **“I guess I was halfway content with the idea that somehow I’d become an economist”** Robert B. Wilson didn’t really see himself as an economist until he reached the age of 50. In this conversation, conducted in March 2021, Wilson speaks about his journey to becoming an economist, about how he received the news about his prize in economic sciences and the culture shock he experienced when he started to study at Harvard University.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0811=RW  Robert Wilson: Hello, I’m here.  Adam Smith: Good morning, my name is Adam Smith, I’m calling from Nobelprize.org. Many congratulations on the award.  RW: Thank you.  AS: We just spoke with Paul Milgrom and he said that he heard the news by you walking across the street and ringing his doorbell.  RW: Well that’s right because he had turned his phone off for the … to get a good night’s sleep, and so somebody had to wake him, and he lives across the street so I just walked over and knocked on the door. I roused him.  AS: I think … I think this must be a first in the history of the Nobel Prize.  RW: Yes, how many times does … first to have a knock on the door, which sounds like something from the 19th century, and secondly that in fact the two of us live only, what, 40 m apart.  AS: Precisely, and you breaking the news. So what did you actually say to each other, what was his reaction?  RW: Well, I said congratulations. He later told me that his reaction was that he thought that maybe he … only he had won and why wasn’t I included, which was very sweet of him. I have always thought of him as the leader in all of this so … but I take great pride in him because he was my PhD student so I take a lot of pride in having sort of at an early stage influenced him, and in this whole field of market design and auction design. This of course, by the way, is the third of my students who’ve won Nobel Prizes.  AS: That’s right because you also had [Al Roth](https://www.nobelprize.org/prizes/economic-sciences/2012/roth/facts/) and [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/), didn’t you?  RW: That’s right. So this is what I have been telling people, that if Paul won I would have a trifecta, that is you know the perfect combination of events. That’s turned out well.  AS: In cricketing terms we’d call it a hattrick.  RW: Ah, a hattrick, ah, very good.  AS: I asked … I asked Paul Milgrom this question also, but what is the magic between you that makes you so productive?  RW: Well, I’ve been more of a speculative thinker, and he is very precise. I mean the spectrum auction design relied a lot on very innovative elements that, that he put in. I was more of a traditional auction theorist. But he was so innovative in designing an auction that, what we call a package auction, that is a situation in which buyers can bid for packages of licences – there might be complements, there might be substitutes, it’s all very complicated – and in a sense he thought outside the box and created a really innovative design, with good attention to the … having the right kind of incentives for bidders to be truthful in their bid, you know, somewhat truthful, or of course you want an outcome that’s efficient. The people who value the licences the most should be the ones who would be winning them. So it’s a complicated design, and went on over time … that first auction, I think, started in December and ended in March. You know it involved billions of dollars … So I’m just getting a message here from Bengt. He just called on the other phone. That’s happy news from Bengt.  AS: Goodness. It’s, yes …  RW: I’ve always been very proud of Bengt and Al, and Paul. They’re wonderfully talented fellows.  AS: In these socially-distanced times it’s a bit difficult, but obviously what needs to happen is that you all come together again to celebrate this new event.  RW: That’s right, it would be nice.  AS: My warmest congratulations again.  RW: Let me say it’s so appropriate that your name’s Adam Smith.  AS: The name Adam Smith can of course get me into trouble because there have been Economics Laureates in the past who have rather disbelieved that Adam Smith would call on the day of the announcement. It’s been a huge pleasure speaking to you. I hope we can speak at greater length, but for the moment, wishing you a wonderful day.  RW: Thank you very much.  AS: Thank you. Congratulations again.  RW: Bye.  AS: Bye. |
| Interview |  |
| Q1 | What made you decide to pursue economic sciences? |
|  | It’s a rather strange circumstance. I wasn’t actually trained as an economist and I hadn’t gone to a business school back in college. I majored in mathematics and philosophy. But then there was a professor at my college. He was going to the business school. He was moving from a statistics department to the business school and I liked him a lot. So I went to the business school and from there I got a job after the doctoral degree.  There was a time in my career in the mid-sixties when I sort of got interested in economics and game theory. I sort of learnt it on my own and I didn’t have training in it, but I acquired an interest in it. It was more accidental. I was sort of led into it by, I was curious about the subject matter and I just had to learn more in order to understand it and do research in that area. I was always shooting multi-person problems. Game theory involves more than one person in a strategic situation. So even without game theory, I was still interested in how people coordinate and cooperate. |
| Q37 | Do you have a defining moment where you decided that this is what you wanted to do? |
|  | I took a visiting term at the Catholic University of Louvain in Belgium. There I was working with economists and I liked it. Two years later I had a sabbatical year, which I then spent in the economics department at Stanford. So that was a defining moment where I said, okay, I’m going to take a whole year. I got to study economics. I got to live with the economists, I’ll have an office at the economics department. I kept with it and from there on I was really heavily involved in economics. |
| Q3 | Is there a particular teacher or mentor that really has influenced you? |
|  | Later in my career, as I became involved in economics and game theory, I think there were these very influential older scholars that during the 1970s had a lot of influence on my thinking. One was [Lloyd Shapley](https://www.nobelprize.org/prizes/economic-sciences/2012/shapley/facts/), who was a game theorist and got a Nobel prize by the way. Another was [Robert Aumann](https://www.nobelprize.org/prizes/economic-sciences/2005/aumann/facts/) who would come every summer to Stanford and he too got a Nobel Prize. So those had a lot of influence on me. But before that, I can’t think of there being a particular mentor along the way. I sort of drifted through college without any particular direction or mentor. I had come from a very small town in farm country, in the Midwest. Going to Harvard was a shock for me, a cultural shock. I was unprepared for it. Most of the fellow students had gone into prep schools and had lots of prior work and I just drifted along without a focus. In some ways my undergraduate education was kind of wasted. It didn’t have any focus to it. |
| Q30 | Many of your previous students have become laureates and they have said you were a great mentor – what is your view on teaching and being a role model? |
|  | It’s a little mysterious as to why I’m considered a good teacher, because I actually haven’t done very much! I do have a course that’s apparently been very influential. For around 50 years now I’ve taught a PhD course that tries to convey what the frontiers of research are in the application of game theory to problems in economics. So of course, I worked very hard on that course, and I think it’s influenced a lot of the students. |
| Q10 | Do you have any advice that you usually give to your students? |
|  | As a teacher, mostly I’ve just encouraged my students. I like them. I admire what they’re doing. I encourage them to work on what they’re interested in and they usually just pursue it with a lot of interest in vigor because it is their own interests. Unfortunately, this is very difficult these days because young people feel they have to do empirical work in order to get a job and that the golden days of theory are kind of over. |
| Q22 | What qualities do you think a successful scientist needs to have? |
|  | There are different kinds of scientific endeavors. I mean, certainly the people doing empirical work, they’re just trying to find out what really is going on here, they have this curiosity about what is the truth. I’m more of a theorist and for me it just requires this kind of unrelenting curiosity. You have to be curious about something that’s really minor. When I started my work in economics, the prevailing model was what we call generally equilibrium theory. It was an elaborate theory that has a main purpose to show that there would exist prices that could make a demand equal supply in all or many markets.  The curiosity that drove me was where do these prices come from? Who sets them? Are they set by dealers and brokers or who are quoting prices?  The thing is that it only takes the slightest bit of curiosity to then lead you down a whole trail of interesting questions to study. I encourage young people to just have a little bit of curiosity about something basic from fundamental and then pursue it. |
| Q47 | Your three students that become laureates – can you tell us a bit about them? |
|  | All three students [Alvin Roth](https://www.nobelprize.org/prizes/economic-sciences/2012/roth/facts/), [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/) and [Paul Milgrom](https://www.nobelprize.org/prizes/economics/2020/milgrom/facts/)are extremely talented, intelligent and skilled people. It’s not surprising that they do well. They would do well working with anybody. It’s not clear that I had a particular role. We were just very lucky. I was very lucky to have such talented students. You can see they were very talented. It’s turned out that way.  I can tell you a story about Bengt Holmström when his time was up at Stanford. The money was running out and he had a post-doc fellowship to go to – he only had about eight weeks left and he still had not written his dissertation. He locked himself in a room and worked steadily for six weeks. He produced this magnificent manuscript all in one in this intensive period. Then he went, with this post-doctoral fellowship, to the University of Louvain. He traveled through Germany to there from Finland. As they traveled through Germany to get to Louvain in Belgium, they stopped in a little town. While the car was parked outside the hotel, his manuscript was stolen. It turned out that he put an advertisement in the paper, in that town in Germany, saying ‘This is a terrible thing. My briefcase that has my doctoral dissertation in it has been stolen.’ It turns out that the thief’s mother read that and demanded that her son returned it and he got his dissertation back |
| Q8 | Your co-laureate is a previous student of yours, Paul Milgrom. How did the two of you meet? |
|  | During the mid 1970s, I’m a professor at [Stanford] business school. After finishing college, Paul Milgrom worked seven years as an actuary. Then he came to the MBA program, and my colleague identified Paul Milgrom as this exceptional MBA student with strong analytical and mathematical skills. We invited him to transfer from the MBA program into the PhD program, which he did. My understanding of this story is that he asked Bengt Holmström which advisors he should choose and Holmström said, ‘well, you should work with Bob Wilson’.  I was the chairman of the committee for his thesis and that’s how it started. We had a little seminar series where we would meet at least every week. We would be pursuing topics, I was particularly interested at that time in auctions. I’d done some sort of exploratory work, but it wasn’t very deep rigorous, but Paul had these tremendous skills. He did very rigorous analysis of these topics in auctions. |
| Q8 | Are you still working together? |
|  | We haven’t done any joint work since that time but we’re friends. He and his wife Eva go hiking together with me and Mary. Eva’s quite a hostess so she invites us over for dinner and there’ll be large groups of people and an absolutely beautiful banquet of food. She’s quite the entertainer. |
| Q7 | How did you end up informing Paul Milgrom about the prize? |
|  | The Swedish Academy of Sciences called us, it was 1:40 in the morning. Since this is in the period before the election, I thought it was one of these political advertisements. So I turned off the phone, but fortunately the Swedish Academy knew my wife’s cell phone number and they called her. Then we talked to them and the chairman of the committee said, ‘well, we’re actually having problems reaching Paul Milgrom and we understand he lives across the street. Could you go over and wake him up?’ My wife and I put on clothes and went across the street. The reason there’s a video of this is that they have a system at the front of their house, that the doorbell where the chime that you would push the bell in order to ring at the door, it’s actually a camera and a microphone. As we approached the door, it’s taking a picture of us and recording what we’re saying. It’s sending all of this over the internet to Paul Milgram’s wife, Eva Myerson Milgrom, who was in Stockholm because she’s Swedish and visiting her son, granddaughter and mother. At that very moment, it was like 10 in the morning in Stockholm so she’s trying to talk to us, but we don’t hear her very well so we’re trying to get to through to Paul and knock on the door and say, ‘You’ve won a Nobel Prize!’ So that was a lot of fun actually and this video got distributed all over the world within hours. |
| Q7 | How does it feel to share the prize with Paul Milgrom? |
|  | It is fun. For the award ceremony, because of the pandemic, it could not be done any other way than here. The council came and it was held in Milgram’s backyard patio. The whole ceremony was filmed just in the patio. That was a lot of fun. |
| Q48 | You have been to a Nobel Prize award ceremony before, haven’t you? |
|  | I came to Holmström’s award ceremony in Stockholm [in 2016]. I dressed up in white tie and tails. My wife and I were his guests and we sat up at the third balcony for the ceremony. We went to the banquet and had a lovely time. |
| Q11 | How do you cope with failure? |
|  | There are lots of problems where I’ve worked on the problem for a year, failed to solve it, and then finally reluctantly given up on it. Since I retired, I have a problem I’ve worked on for nine years and I’m still working on that. Once I’m hooked on a problem and I think I know the answer, but I can’t prove it to be true, it’s very frustrating. It’s like a dog gnawing on a bone that won’t give it up, you know? So that’s hard. |
| Q28 | What do you like to do in your spare time? |
|  | These days, I’m 83. I used to be interested in gardening, I’m kind of too old to maintain the garden now. I have a gardener that does it so I don’t actually work in at that much anymore, but I used to do it.  Nowadays, we spend more time on birds. But hiking is our main thing. My wife and I like to go hiking. Normally every Wednesday we go on a hike – so that is probably what we will do after this call. |
| ID | 0812 |
| Biographical | **An accidental economist: a brief history** A series of accidents, mostly fortunate, made me the human being and economist that I am. It started with the accident of my birth, to a couple who were both economists. This, in my generation, was already a minor miracle – in the mid 1950s my mother somehow managed to persuade her parents, who were relatively conventional Maharashtrians of the time, to take the very unusual step of sending her to the London School of Eco­nomics. This is where she met my dad. My dad was really not meant to be there; he was a college drop-out who, bored with his clerical job in Kolk­ata, made his way to London with a goal of studying more. He took night classes at the Kensington Polytechnic while working various jobs as a manual laborer. With all that he did so well in his part 1 BSc exam that one of the examiners, Richard Lipsey, a professor at London School of Economics, went out of his way to seek him out and eventually secured for him a scholarship to go to LSE for his final year. But for this chance encounter with such extraordinary generosity, it is likely that he would have never gone to LSE and my parents would have never met.  As a child of economists, I knew that economics was one field I must avoid. My father was a famously charismatic teacher, who adored and was much adored by his many students. He would often talk about just how brilliant some of them were, and it was clear to me that I had nothing to gain and much to lose by inviting comparisons with them. In my deeply anti-intellectual high school, it was made very clear that we should all aspire to study engineering or medicine because they led to good jobs (the lure of jobs in finance came many years later). They made an occasional exception, in the case of an unusually brilliant student, for studying phys­ics. I had no desire to be an engineer or a doctor and prepping for physics required consorting with our physics teacher, a man who seem to take genuine pleasure in inflicting pain. What else could I do? I loved literature and history, philosophy and math; my parents were against the first three. Their stated grounds were that I could always go from math to those at a later stage but not the reverse, though my guess now is that they were not sure that I was good enough to make a living in the humanities, given the shape of the labor market. In any case their argument for math appealed to my instinct for trying to postpone all hard choices. Math it was going to be.  I applied and got into the undergraduate program at the Indian Statisti­cal Institute in Kolkata, reputed to be far and away the best place to study math (or mathematical statistics) in India. ISI, as it is popularly known, is – to my knowledge – the only Indian academic institution that was a true world leader in its field; many of the early leaders of the field of statistics were professors at ISI, along with many of prominent scholars across the discipline of pure and applied mathematics. Many of those teaching us in our entering classes were well-known scholars and the overall quality of teaching was very good.  But almost as soon as I got there it was clear to me that I needed to get out. It might have been my attempts to crack the occasional risqué joke that typically met with looks of disapproval or bafflement from my class­mates. Or the fact that classes started early in the morning (for me, any­thing before noon was kind of early) and continued till dusk, and then there were a few dozen math problems to solve for class the next morn­ing. Or perhaps the more general sense, seemingly shared by my teachers and classmates alike, that this was a monastery and we were novices waiting to be inducted into its mysteries. I liked and enjoyed math, but I was both too young and too undisciplined to accept to make it my entire life (or at least that is what it felt like). For those who know the classic musical Sound of Music, I was Maria – not meant to be a “credit to the Abbey”.  What then? The Indian system in those days was not forgiving of devi­ants. Deadlines for many of the standard options were long gone. I was back to trying to decide between literature, history or philosophy, when my parents tentatively sprung the idea of doing economics. I was aghast – my father was the head of the economics department at Presidency Col­lege in Kolkata, widely recognized as the best place to study economics in the country. I would have to take classes with him – what would he call me? What would I call him? And suppose I did badly …?  It turned out however that several of my close friends were applying. It seemed like a cool group to be with. I covertly picked up an economics textbook by Lipsey, the same man who was my father’s savior many years ago. On a quick read, introductory economics seemed almost entirely dis­connected from what I imagined economics would be about, the many fraught social issues that my parents discussed at breakfast and dinner. In particular, the word poverty never seemed to come up.  On the other hand, it seemed easy enough. I asked my friends about job prospects, wary of showing too many of my cards to my parents. One can do an MBA, I was told, which sounded dire. Or work for the media, which sounded more plausible – I kind of fancied myself as a writer of expansive op-eds about beauty and justice. My parents planted the thought that it is easy to move from economics to other fields. Satyajit Ray studied economics. So did another wonderful filmmaker, Shyam Ben­egal. [Amartya Sen](https://www.nobelprize.org/prizes/economic-sciences/1998/sen/facts/) himself was almost as much of philosopher as he was an economist. Perhaps I could do it too. At the very least it postponed making a choice. I was sold.  I hugely enjoyed my three years at Presidency. The teaching was very good; I found microeconomics rather bland, but my father, who taught it, was extremely entertaining. Mihir Rakshit taught us macro and managed to make the ideas crystal clear without trivializing them. One left with the impression that there was something more to learn. My favorite was eco­nomic history, beautifully taught by the chronically self-effacing Nabendu Sen.  But what made it truly special were my fellow students. There was always someone who knew much more than me about literature, history, philosophy, politics, art, music, theater or film – in other words everything that I really cared about. And they were passionate and keen to share; it was the most incredible learning opportunity. I read vora­ciously, attended every show and showing that was on offer, and argued with everyone, in part to test my growing understanding. And I discov­ered just how much I love getting to know new people – their passions and their interests, their anxieties and their hesitations, what makes them smile or frown. I was always gregarious, a result, I suspect, of the way we grew up. My parents were warm and hospitable people, and even when money was tight, welcomed the large and diverse community of their friends into our house. As a result, my brother and I grew up to be very comfortable around adults, even adults who had much too much to drink. That openness to people quite naturally extended to my own cohort in the very congenial environment of Presidency College. I now have more close friends than almost anyone I know; I have close friends whom I have known since I was five, and close friends I met a year ago, close friends who are twenty years older than me and close friends that are much, much younger. It is one of the unadulterated joys of my life.  Three years passed quickly and then there was another decision to be taken. What next? I still didn’t absolutely love economics, but I was doing well and in the spirit of why not postpone any decision that can be post­poned, I figured that a Masters degree would be handy. It also seemed exciting to move to a different city and live in a dormitory. The question was where – Delhi School of Economics was known as the best place to study economics in India and my father, in his gentle and always carefully balanced way, was nudging me to go there. The alternative was Jawaharlal Nehru University (JNU) in New Delhi, famous for its lively political life and Marxist economics. I was slightly leaning towards the latter – I had decided that I needed to learn about politics and given that I found con­ventional economics somewhat uninspiring, I had hopes that Marxism would deliver the punch I was looking for. However, what actually decided me was a recruitment visit to the Delhi School, where a brash young faculty member told me that DSE was the royal road to PhD pro­grams in the US. That made up my mind – I was looking to be inspired, not a prep school for some future step that I had no burning desire to take. I was on my way to JNU. I wonder where my life would have gone had he not taken on to convince me.  I have written elsewhere at some length about my JNU experience. That blinding moment of insight that I was waiting for never arrived. Marxist economics, the way we were taught, was much like all the other economics I had seen, just with some assumptions tweaked (and a bunch of self-righteousness thrown in for good measure). Since I had no par­ticular stake in the assumptions of standard economics, I found it easy to believe that one could make others, but I found it unsatisfying to be told that these assumptions were better because they were in the Marxist tra­dition. I had tried to read Marx as an undergraduate, and found him, in equal measure, illuminating and confusing – I saw no reason to take his words as a say so. The fact that the market system was not the paragon of all virtues that the standard micro textbook implied, seemed obvious to me – the question that troubled me was how do we get to a convincing diagnosis of where the problem lies? Is it all to be blamed on the capitalist class? Or are there other problems that might be with us even after revo­lution? How would one check? It was only some thirty years later that I caught, in the writings of my friends Thomas Piketty, [Daron Acemoglu](https://www.nobelprize.org/prizes/economic-sciences/2024/acemoglu/facts/) and [Jim Robinson](https://www.nobelprize.org/prizes/economic-sciences/2024/robinson/facts/) (and their many collaborators), a glimpse of where some genuinely creative Marxist thinking could take us.  The teaching at JNU, both from the Marxists and the more orthodox, was generally very good. But I was not particularly challenged, thanks to the excellent grounding in basic economic logic that I came with from Presidency. This combined with my growing disinterest in economics, meant that I learnt very little of the field in those two years. There were exceptions – Anjan Mukherji and Satish Jain, two of the most generous teachers I ever had, gave me a glimpse of what made high theory exciting to them and the wonderful Krishna Bharadwaj was a very convincing advocate for the History of Economic Thought – but on the whole I learnt much more from my many historian friends (including by occasionally sneaking into their classes with them).  Perhaps the most important thing I got from JNU was a sense of what politics in the Indian context meant. I learnt activism from the activists, pamphleteering from the master pamphleteers, slogans from the demon­strators. I listened to the Marxists talk about class, the Gandhians about caste and religion, the feminists about gender. The world was suddenly a more interesting and dangerous place.  Unfortunately, the two wonderful years were soon about to run out. My grades were good. My teachers and my parents wanted me to apply for a PhD. I dithered, but eventually decided that I might as well take the GRE and see if I was good enough to get in. I did well, which made it that much harder to not apply. I decided to go for it, but gamble on getting into one of the top places and if not try something else. Since applying was a huge pain – the forms were endless – I went for just a handful of places. To avoid being pressured to apply to too many places, I did not consult any­body; this is why I missed out on applying to Princeton and MIT, which were probably the two best places for someone like me. Instead, I applied to Harvard, which I knew from *Love Story*, and to Berkeley and Stanford, because I had heard of [Gerard Debreu](https://www.nobelprize.org/prizes/economic-sciences/1983/debreu/facts/) and [Kenneth Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), which was a bit like converting to Catholicism with the goal of making friends with the pope.  I guess I will never know why Harvard admitted me. My application was hand-written in my rather scratchy hand, and the research proposal was border-line nonsensical. I was a good student, even a very good one, but at a university which had never sent anyone to Harvard.  In any case they did, and I felt that not going would be rude to my pre­siding deity who was clearly looking out for me. In any case, it was not as if I had plan B. Nonetheless I must confess that I was slightly relieved when a student demonstration landed me in prison, and many people confidently told me that the Americans would not give me a visa. I applied for jobs at a few NGOs and shocked many well-wishers by being entirely nonchalant about the whole affair.  Bizarrely enough I did get the visa, despite the fact that we were all (quite unfairly) accused of various rather heinous crimes. My first days at Harvard were hard; I was homesick, pining for my girlfriend in India and unused to working hard. The teaching was at an entirely higher level than I was used to and two years at JNU had ruined my work-habits, never great to start with. Many of my classmates, I could tell from their ques­tions and comments during lectures, were way ahead of me. I was a part of a great class: Tyler Cowen, Mathias Dewatripont, Steve Kaplan, Miles Kimball, the late lamented Alan Krueger, Kiminori Matsuyama, John Nachbar, Nouriel Roubini and Rob Stavins were to leave their imprint on the profession in one form or the other, and other like Allen Sangines could have easily done the same but opted not to. Many of them came much better prepared than me and they all worked very hard and talked about it; I discovered that in the US, unlike in India, it was cool to work hard. I think this is what saved me; I decided that I need to try harder. Those work habits stayed with me; I still work long hours, interrupted by regular doses of distraction every hour or two – I play with the kids, chat with friends, cook or play a game of ping-pong.  It still took me a long time to properly catch up. I have always been obsessed by how every bit of what I know fits together with the rest, which means I get very confused whenever I meet something entirely new. Classes at Harvard were a mixed bag. I loved listening to Andreu Mas-Colell, who tried to get us to appreciate the simple, often geometric, logic behind a lot of the theorems in microeconomics and not to get bogged down by the notation and the bombast. He would eventually become one of my PhD advisors. David Kreps gave us an early and bril­liant introduction to both Game Theory and Behavioral Economics, but I was too unsophisticated to fully appreciate what he did. Larry Summers introduced us to empirical economics circa 1983 in his own unique way, a blizzard of intriguing insights and confusing words and math. Economet­rics was boring and similar to what I had already studied at JNU, so I decided to mostly skip it. It remains one of my abiding regrets – the result was that the only econometrics class that I (partially) attended was Time Series, the one part of econometrics that has since never crossed my life. Macro was deeply confusing – the application of “rational” expectations to various dynamic models generated a certain black magic that I learnt, painfully, to reproduce, but found entirely implausible. Nobu Kiyotaki, a macroeconomist who I admire greatly, who was then our kindly Teaching Assistant, and therefore the target of my many queries, once told me “you have many puzzles, don’t you”.  Predictably perhaps, I had trouble deciding what field to study. My original plan was to study economic history, but it turned out that eco­nomic history at this time was moving away from telling illuminating sto­ries, which I enjoyed (and still enjoy), towards more conventional applied micro. Development economics was in throes of computable general equilibrium models – the view seemed to be that we knew how the world worked in general, all we lacked was the computationaI capacity to figure out what happens in specific cases. A couple of lectures on computational methods decided me to look elsewhere.  Trade was another way into development, at least in those days, and I enjoyed the first class I took with Kala Krishna and Sue Collins and ended up being hired to work as a research assistant to Jeff Sachs, who was then flying around fixing up macro economies in Latin America. But Jeff quickly decided that I neither had the computational skills to crunch his data nor the personality to bang the desks in finance ministries, so, being a gentleman, gave me something that was mostly make work. I got the message. Development was not for me.  In the spirit of not committing to anything as long as possible, I stud­ied macro and theory as my main fields, telling myself that if I had a strong enough base I could decide later. It was clear to me however that the classes I most enjoyed were the theory classes, Game Theory with Dilip Abreu, General Equilibrium with Andreu and a Contract Theory class that [Oliver Hart](https://www.nobelprize.org/prizes/economic-sciences/2016/hart/facts/) taught at MIT. Andreu in particular, despite the for­midable formalism of his own research, was very good at making abstract ideas seem quite concrete and connecting them to the world, and Oliver’s lessons offered an object lesson in how to apply theoretical ideas to illu­minate very practical issues.  The challenge for me was to reconcile my urge to do something useful with my love of elegant arguments; my mother, who spent her life react­ing, often with extreme vehemence, to the unfairness and wanton waste that she saw everywhere in the world, was speaking in my right ear, while my father, who loved elegant arguments and clever mathematics, was speaking in my left. On the strength of what I heard in Oliver’s class and encouragement from Andreu, I decided that I could be useful as one of these applied theorists who tells stories about the world, in part to under­mine the many unfounded sureties that economists often carry with themselves. I obviously did not know how I would get there.  This is when [Eric Maskin](https://www.nobelprize.org/prizes/economic-sciences/2007/maskin/facts/) arrived from MIT. Eric, not knowing quite what he was getting into, encouraged me to formalize the ideas I had and show them to him. I, blissfully unaware of Eric’s reputation as one of the most intimidatingly brilliant minds of his generation, did exactly that. Eric saw many silly ideas, and patiently took them down. This is when I got lucky. One of the ideas I was playing with turned out to be more interest­ing than it had any right to be, and literally within days I had a paper on the “Economics of Rumors” to go on the job market with.  This was also when I was beginning to really enjoy economics. It helped that I had two wonderful playmates, Mike Spagat and Andy New­man, who were, like me, left-leaning theorists who wanted to use models to tell stories that usually do not get told. We spent hours together, part crafting models and part shooting the breeze. The work with Andy New­man on the foundations of poverty traps, which started in those years, was to eventually to change my life. I was married by then and waiting for my wife to finish her degree before looking for a job. Given that I had my job market paper in hand, this gave me a year and half with lots of time in hand. I filled it with coming up with an idea a day, usually worthless. I was having a great time.  I went on the job market in the winter of 1987. Everybody told me it would be stressful to be surrounded by so many others who are all looking for the same jobs, so I decided to distance myself from it. I parked myself at Mike Spagat’s house, some 20 miles from downtown Chicago where the meeting was, ignoring all advice to stay as close as possible. As luck would have it, there was a massive snowstorm on one of the days when I was sup­posed to be interviewed for a job, so I had to get up in the dark, walk a cou­ple of miles in knee-high snow, and managed to find some public transpor­tation that eventually got me there. But probably as a result of all that anxi­ety, I left my jacket in a taxi while going from one site to the other and ended up being interviewed in my overcoat (I was cold).  To compound my misjudgment, I had decided to avoid the days right after the job market when famously, job candidates spend their time anx­iously trying to figure out how everyone else’s market was going. I went to the UK, to visit Eric who was on sabbatical there, and work on our joint project. This was in the days before cellphones, and as I quickly discov­ered, it was a terrible idea. I did have an answering machine but calling into it involved feeding innumerable coins into the aging phones in the bright-red British phone booths, and often ended with half the message being inaudible. I almost missed my invitation to give a job talk at Stan­ford.  Overall, my job market performance was solid rather than spectacular. All the “top” schools, except Stanford, had decided to pass. When I reported that at Harvard, two of the kindest economists I know, Jerry Green and Kala Krishna, decided to go out and bat for me. This is how I ended up with a job at Princeton.  My wife Tuli was starting a PhD in French Literature at Columbia. We lived in Manhattan and I commuted to Princeton. It is while waiting for trains to Princeton that I noticed that there were days when a long queue would form on the platform at the wrong place – the door would open somewhere else and those not in the queue would get in and grab the seats. In trying to think of why these systematic and correlated errors occur. Partly inspired by my work on rumors, I wrote a paper called “A Simple Model of Herd Behavior”, which in many ways built my career. It somehow struck a chord and suddenly I was getting feelers from here and there, and eventually an offer to return to Harvard. This was great for me – it gave me exposure and made it possible for me to move to MIT the year after, in 1993. It was probably not good for Tuli, who was writing her dissertation and very generously agreed to move with me. The resulting relative isolation from the academic network at Columbia might have been one reason why despite her great charm and obvious brilliance, she had a very hard time finding an academic position. She eventually turned this around and is now one of the heads of MIT’s fund-raising team, but she suffered for many years, always with a brave face to the world, and I fear I was not always as supportive as I should have been.  The move to Harvard made me a development economist. I remember Andrei Shleifer asking me what advanced PhD class I would want to teach, since the theory classes, which is what I taught at Princeton, were already covered. On an impulse, I suggested development. It is a tribute to the generosity of the existing development faculty at Harvard – leaders of the field like Dwight Perkins and Jeff Williamson, as well the younger Jon­athan Morduch – that they allowed me to take over a PhD class, despite the fact that I had never studied development and my main claim to research in the field was a theory paper with Andy Newman with develop­ment in the title. As I said, I moved to MIT the next year, and continued to be allowed to teach PhD development. I had chanced upon on an oppor­tunity of a lifetime.  Looking back, I think it was actually an advantage being an utter nov­ice. Development at the time was a mine field, with many strong personal­ities and powerful minds who did empirical research and did not get along. I knew nothing of these battles and as a result, taught a course that was very different from what was standard, both more theory driven and more skeptical of what passed for empirical research. In particular a lot of the empirical work seemed too obsessed with showing that markets (or non-market institutions) delivered efficient outcomes, which I found hard to credit. I started to think about how to get more credible evidence, and my one insight was that we needed fresh data, collected with the relevant theory in mind. Kaivan Munshi and Maitreesh Ghatak, two of my earliest PhD students, were my accomplices in this enterprise, which Chris Udry at Northwestern and Rob Townsend at Chicago were also pursuing.  Around this time, there were two more critical accidents that entirely reshaped my life. First, there was another opening in development teach­ing and I somehow persuaded Michael Kremer, who had just finished his PhD and joined us at MIT as a brilliant young macroeconomist, to take it on. That was quite a coup, since macro was a serious field and develop­ment, quite plainly, was not, as one of my senior macro colleagues kindly pointed out. The result was that I was exposed to Michael’s rather unique perspective on the world; in particular I remember a conversation where he wondered aloud why we economists don’t do randomized controlled trials (RCTs). It turned he was doing one. This was I think in 1995. I went home and thought hard about it and was entirely persuaded. RCTs offered so many possibilities that one could never get to otherwise. Emboldened by the fact that I had some confidence in our ability to collect data, I started organizing my own first RCT – and persuaded Michael join me. This was in 1996.  The second accident was that Esther Duflo applied to our PhD pro­gram. With the sort of perfect irony that one would never dare to make up, I rejected her application, but thankfully Thomas Piketty rescued it. In the Fall of 1995, I taught development to Esther, Eliana La Ferrara, Jishnu Das, Asim Khwaja, Stuti Khemani, and Chris Spohr, all whom have made major contributions to the field. Development was on its way. In a little while Esther asked me if she could work for me as a research assistant, which is how I really got to know her and learnt to appreciate the way she, better than anyone I know, combines deep understanding of theory and empirical techniques with intense practicality, pitch-perfect common sense, infinite energy and boundless generosity. She might be my student, but she taught me a big chunk of what I know about empirical research.  As my work with Esther and RCTs in general flourished, we had another lucky break. A white envelope arrived from the MIT administra­tion. Esther, Sendhil Mullainathan and I, were offered 100,000 dollars each to promote our research. The unspoken goal was to keep us at MIT. We decided to pool the money and build an institution around RCTs, and somehow that idea caught the fancy of Susan Hockfield, who was MIT’s new president. This brought in Mohammad Jameel as a donor and a sup­porter – I believe that he saw more potential in us then, than we ourselves did. That was also true of [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/), our brilliant department chair and the wonderful Rachel Glennerster, Michael Kremer’s wife and a friend, who I persuaded to give up her lucrative IMF job and join us – convincing her is the one part of the success of the Abdul Latif Jameel Poverty Action Lab (J-PAL) that I take credit for. The rest is history.  Amidst all the lucky breaks that shaped my life, exactly as the J-PAL story unfolded, a tragedy was brewing. Tuli’s and my son, Sasha, was going from a wonderfully precocious boy to a troubled adolescent. That, among other things, contributed to our breakup. Sasha eventually moved in with me. We had an extraordinarily close relationship – we would talk five times a day or more, but I, and Tuli, who also tried her best, and his community of devoted friends failed to persuade him that life was worth fighting for. Sasha died on the 10th of March 2016.  I have learnt to live with his absence, though there are days when I still refuse to believe it. That makes me all the more grateful for the other piece of unbelievable good fortune I have enjoyed, my extraordinary and wonderful wife Esther and our two intensely adorable children, Noémie and Milan. Touch wood, or as we say in Hindi, *chasme baddoor*. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0812=AB  Adam Smith: Hello, my name’s Adam Smith, calling from the website of the Nobel Prize, Nobelprize.org in Stockholm. Many congratulations on the award of the prize.  Abhijit Banerjee: Thank you.  AS: As you may have heard from Professor Duflo, I spoke to your wife a little earlier, and she mentioned that after hearing the news from Stockholm you went back to bed!  AB: Yes.  AS: It seems a very sensible and collected thing to do.  AB: Yeah, I mean, it was early, very early in the morning. I’m not an early morning person. I figured it would be a fault of the system if I don’t continue my sleep.  AS: And you managed to sleep after hearing the news?  AB: Yeah. Not long as I kept getting calls after a while. Once the press conference happened and news spread to India and Europe I think, I had no chance But I had a 40 minute interim period when I slept.  AS: I doubt any Laureate has prepared for the day better than that. It’s a prize which recognises work focussed on the world’s poorest people, and that is very special.  AB: I agree. And it, I guess, reflects the, on the fact that somehow that while we often pay lip service to the welfare of all, this is something that not always the immediate focus of a prize like this. I think … I’m delighted that some attention was shown this way. Not that I think all the other things that they give prizes for aren’t important, but it does make people who work in this area I guess feel a little more enthused. It’s lots of people in this world of people who do real things, not people like us, who do real things, and it’s somewhat of a prize for all of them.  AS: And the lesson from all your experimental studies is really, I suppose, that you cannot impose your own rationality on others but you have to listen to what those people will teach you.  AB: That’s right. That’s certainly one lesson. And then maybe you should also … I mean, I think it’s a two-way relationship: I think you should not have too much faith in your own rationality. You should not have too much faith in the rationality of, you know, anybody else either. We all learn together about the way the world is, and I think it’s a sort of antidote to wishful thinking of all kinds.  AS: Nicely put. But the way you describe it, it makes it sound like a wonderful collaborative relationship which is very hopeful.  AB: That’s been my experience. I’ve been … I have learnt an enormous amount from talking to people on the ground. The set of people I really owe an enormous amount to is the people who are kind of … both I think the people with whom we work whose, whose lives we study in many ways. But also the people who work with them, and I think we’ve learnt just a huge amount from organisations like [unclear], for example. In my personal experience these organisations that work on a very large scale with very poor people has certainly been very important for us.  AS: I must let you go, but let me just ask you one thing, let me ask you about this rare thing that you have been awarded this prize as a married couple, I think only the fifth time that has happened in the history of the prize. It makes it special in some way?  AB: Yeah, I guess it does, in that it’s sort of been our entire family enterprise in a sense, this whole between J-PAL the research and working at MIT, you know, makes it a … You know, there’s lots of dimensions of the work, that just become much more pleasant when you do it with your partner.  AS: Nicely put. Well thank you very, very much and congratulations again. |
| Interview |  |
| Q20 | Can you tell us about your childhood? |
|  | I grew up in a middle-class family in the city of Kolkata. I, just by accident more or less, happened to live right next door to one of the biggest slums in Kolkata, so I had a slightly resentful childhood in the sense that I was surrounded by kids who didn’t go to school, who was playing all day, whereas I had to go to this extremely boring school every day. My parents had different expectations and so I would see the people who I would play marbles with, playing while I go to school, and playing when I came back from school. The exposure, I think, early exposure to the fact that people, poor people – my parents sort of explained to me that this was poor people, that they have very different lives – was, I think, in some ways it kept with me in the sense that I would say that I think I became more of an economist in trying to understand how I can connect the economic theory that I knew which really said nothing about why the poor would be different in any way. To the ideas that, you know, all the exposure, to the kinds of facts of the world that I came with. These two languages were not particularly commensurate and so it took me a little while to understand that, how to translate one into the other. |
| Q1 | How did you end up in economics? |
|  | I ended up in economics by accident. I wanted to study mathematics and I went to study mathematics, there is a special dedicated, very well-known institute in Kolkata called Indian Statistical Institute where they give you an undergraduate degree in mathematics and I went there and I realized that I wasn’t cut out for it. It was not because I was not good at maths, because I was actually pretty good at maths, but because I didn’t like the social life of that place, it was too serious, too kind of ‘heavy’. And so I decided I need to study something else, and in India, if you don’t kind of plan your strategy, the only way you can change fields is by you losing a year, and I didn’t want to do that, so I decided to find out study something else. What can I get into at this late date? And the answer was economics, so I went into economics. |
| Q49 | What’s your relationship with India today? |
|  | I think I my mind I have always been an Indian, I mean, it doesn’t matter where I live or what affiliation or citizenship. It is really much more about what’s the social milieu that directly inspires you, what the social milieu you react to. Who are the people who you think of when you do anything good or bad, you think of certain people and those people for me are, I mean lots of them are in India and there’s somehow that India matters to me in a sense in which I think no other country matters to me. |
| Q24 | How can we better understand poverty? |
|  | To be honest I think the sense from our research has been that actually someway the same insights goes for poor people in many places. Maybe not in Sweden where maybe it’s just very different to be poor because of the social welfare networks are much better, but I have worked in Indonesia, in Ghana, in Kenya, in India, which are very different places. I would say, the similarities are more striking than the differences. I think people are both creative and often, sort of … I think the idea that the poor are sort of fixed machines which have no choice, is all wrong. People are both, I mean, poor people have enjoyed their lives, they look for opportunities, they try things, they are also scared by them, they make mistakes, all of the things that we do, they do. So, in that sense I don’t think that one needs a different sense of psychology to study the poor.  The poor, I think, in fact I would say, what we often miss with the poor is precisely the opposite which is that they have the same psychology as us. We often underestimate how important that is which is that we think the poor, they are starving so they must only want to eat nutritious food. But in fact, they don’t, because they also want to have fun, so they might sacrifice some nutritious food to eat something that’s tasty. And I think that’s the psychology that we have as well, we would not going to just stay on a diet eating, whatever, quinoa every day, we are going to have something delicious sometimes. I think in that sense, I don’t think the psychology is deeply different. And I don’t think it is deeply different across the world either. |
| Q25 | How is your work connected to climate change? |
|  | I think the question of climate changes is central to the challenges that the poor will face in the next fifty years. Being from Kolkata, I always think of what’s going to happen in the southern Bengal delta which stretches across India and Bangladesh. I’m guessing 150 to 200 million people live there, all of that would be under water, unless we do something drastic. Where will these people go? There’s 200 million people, that’s many times the population of Sweden, where will these people go? It’s really no one has an answer, nobody really thought about it, suddenly it’s not easy to think of them as emigrating somewhere else, that many people, so what’s going to happen to those people? It’s one of these completely frightening questions. They are mostly poor people living on small amounts of land which is all going to disappear. What’s going to happen to them, what’s going to happen to their living? I don’t think that it’s a more pressing question from the lives of the poor than finding a life to deal with climate change.  On the other side, I would say that our learning from the work with the poor and in general from experimental work, is that it is not that costly to make the adjustment to a different lifestyle. I think people once they see that other people are doing it they often just do it and if they do it then it sort of sticks. It’s not that hard to get people to change their mind, if you just send people a notice which says: “You are among the top ten percent of energy users among your neighbourhood”, that reduces energy consumption. It’s a letter they read, it is okay, fine, we can switch of the light or whatever. I don’t see any reasons why in America you have to have 75 degrees inside in the winter and 65 in the summer. That seems backwards somehow. There is just so many things that seems gratuitous in the way we consume energy that it seems to me that it is also a matter of political will and of finding creative solutions that people don’t think of as onerous. I think if you put all this together I would say that on one side I feel that this is absolutely critical moment to act, the other side, I really don’t believe it so hard to do so. |
| Q28 | How important is it to have fun in your life and work? |
|  | Enormously important, I must say that I get bored with everything I do unless I have some relief. I try to arrange my day so that it is at least one or two hours when I am doing something that I’m genuinely enjoying, usually more than one or two hours. Three or four hours when I actually enjoy whatever I am doing. I love cooking so I cook almost every day for an hour and a half or so. I like playing racket sports so I usually arrange my day to either play tennis or ping pong or badminton or something like that. And I like being with my kids, so I try to arrange my days so there is enough relief, otherwise I get bored out of my mind very quickly. |
| Q3 | Do you like teaching? Was there a teacher that inspired you? |
|  | I really love teaching and one of the things in which I have invested a lot of my life is in kind of changing the way our field of development economics has had a start. I think one of the things that I would say I take some credit for is in the way, in many places now the courses start this, I see my students have gone and they teach the same kinds of things. I do think that teaching is extraordinary important and, in my life, I think, you know, both my parents are economists and they taught me an enormous amount among others. There were other teachers that were inspiring too, but my parents in very different ways. My mother is a kind of a passionate activist and my father is a very cool collected, very careful debater, where both have been, both very salient influences in my life. |
| Q50 | Can you tell us about your work in films? |
|  | I got into filmmaking out of a sudden frustration with my ability to capture in my research the things that I was hearing. There were so many wonderful stories that people tell. If you interview people the things that’s striking is that of course, you ask them: “How many times did you go to the health care centre in the last six months?” and they don’t just say: “Four”, they say: “Oh yeah, I went three times, but those are the other times when I went and the door was closed, does that count?” or “I went three times and there was the other time when the doctor shooed me away, so does that count?”. You hear that piece, and you say: “Where do I record that?” and I think the sense of that we have a whole narrative around each data point, and those narratives are interesting by themselves, and there’s a way, there’s a need to capture them. That was what inspired and an attempt to make documentaries. |
| Q25 | What’s your hope for the future? |
|  | My hope for the next 15 years is very much that I continue to do what I am doing. I enjoy very much what I am doing, I think that this might open some more doors, give us a chance to do some more useful interventions or study some more useful interventions, make the case that these things actually matter. Maybe people will listen to us a little bit more as a result of the prize. I’m not envisioning a chance in my lifestyle. |
| ID | 0813 |
| Biographical | I was born in 1972 at the Port Royal hospital, the “baby factory” of Paris.  I grew up with my parents, Violaine Duflo (a pediatrician) and Michel Duflo (a math professor) in Asnières, a western suburb of Paris. I went to the local public schools until grade 11, and transferred to Henri IV, a magnet high school in the center of Paris, for the last year of high school. I was not stellar at anything in particular, but very organized, quite solid in every school subject, and generally keen to please. As a result, I was a good, uncomplicated student, well-liked by teachers, and with a great set of friends. I have two siblings whom I adore, my brother Colas who is four years older than me, and my sister Annie who is 6 years younger. My parents were busy, but our house was always filled with Au Pairs, cousins and friends. We grew up with a lot of freedom and joy.  I was very tiny as a child. My mother says that this is how I became so confident: I only learnt to read at six, but since I looked like a four-year old, adults thought I was really smart. I was also what you then called a tomboy, and would I guess now be called gender-fluid. I wanted to be a boy, I dressed like a boy, and cut my hair short; all my friends were boys. At adolescence, both things changed: I grew enough to be counted at the short end of “normal-sized”, and I became comfortable being a girl. But I retained both my self-confidence and the desire to never let my gender define what I would or could do.  I was fortunate to be made aware at a very early age of the diversity of life circumstances in the world. My mother volunteered across the world, helping children who were victims of war. In school, in the magazines I subscribed to, in the various youth groups I belonged to, we were always exposed to the lives of others. I was amazed and somewhat awed by my luck: how come I, Esther, get to be born in this middle-class, intellectual family, with loving parents, decent schools, and all the food and books I need, while some other kids are born in Congo, in the middle of a war, and are forced to carry a Kalashnikov to fight?  I was also disturbed by the French education system’s propensity to rank everyone according to their grades and treat harshly the students who did not conform. My best friend throughout primary school was just as small as me, but – in my opinion – much smarter. Yet he struggled in school and was constantly humiliated by teachers and other children. He eventually had to repeat a grade. I theorized early that everyone must be excellent at least ONE thing, but do not necessarily get to find their spe­cial talent. This made my luck even more flagrant: since my one talent was to be a jack of all forms of schoolwork, I did not have to look very hard to find it: I was rewarded for it every day! In contrast, the boy with the Kalashnikov may never live to discover his talent. Closer to home, perhaps my friend would have been an amazing programmer or cook or artist or tennis player, but the school system was not allowing him to dis­cover what he was good at.  I felt that the only way I could ever repay this huge cosmic debt to the world was first, to nourish and exploit my own unremarkable talent, and second, to play some role in helping others get the opportunity to find and nurture their talents. The meant I resolved very early that I would be an academic, and also that I needed to pick a course that allowed me not to specialize early (since, once again, I was not particu­larly stellar at any one thing, but was able to keep many balls in the air). The French system allows good students the chance to stay in high school for their first two years of college. This gives them access to small classes and time to prepare for competitive examinations with the goal of entering the *Grandes Écoles*. This is what I did after finishing high school in 1990. As a field, I chose the social sciences, because it allowed me to combine math with philosophy and history (and sociol­ogy and economics, but I considered those as prices to pay to keep math and history in the mix). It served me well and I got admission to the *École Normale Supérieure*.  There I decided to study at least two subjects, to avoid specializing for as long as possible. I was determined to study history, but the choice of economics as a second subject was a (fortuitous) accident. On the day of admissions to the École Normale, in June 1992, Daniel Cohen, the profes­sor in charge of economics, was out recruiting students. Daniel is one of the most charming economists I know. He persuaded me that economics was the perfect discipline for a jack-of-all-trades like me.  To accomplish the second goal of helping others, I did what I could on the side: as a child, I helped my classmates with homework, I helped out in soup kitchens; in college, I volunteered in a prison for a year. But I found all of this rather unsatisfactory – it was hardly making a dent. Meanwhile, I started to find history really too remote from anyone’s life to be useful. And economics was just dreadfully boring.  All in all, at twenty, I was ever more fortunate and privileged, being a member of the elite of French students, but my project of improving the world was not going well. I felt I was not living up to my own luck. A little despondent, I seized an opportunity the school offered me to spend a year as a French teaching assistant in the new Social Science university in Moscow. In 1993–1994, I happily gave up economics and moved to Mos­cow. I wrote my history masters dissertation there, which at the time was sufficient to get me my degree and it also gave me a year to clarify my ideas on what to do to accomplish my second goal.  Indeed, I could not have anticipated how much the year in Moscow would clarify my ideas. Daniel Cohen, who showed great forbearance, given how undisciplined a student of economics I had been, hired me for one of his projects, and also got me a job in the team that Jeff Sachs had put together to advise the Ministry of Finance. I knew no economics, but I spoke Russian well, and I was ready to be enterprising, so I made myself useful (one of my early tasks was to locate a directory of plants and draw a stratified random sample, which I did by making up an algorithm to do it mostly manually). This gave me a unique ringside view of the process of economic reforms in Russia. 1993-1994 was a very difficult year. The political process was bitter and angry, the economy was tanking, people were literally starving while others were starting to build empires by buy­ing back their shares in the national industries. I did not think any of the competing teams of economists working in Moscow at time was neces­sarily doing the right thing. But I was incredibly impressed by their influ­ence (especially perhaps given how little they knew). I knew that I wanted their job, in pursuit of my second goal.  In Russia, I also met Thomas Piketty, who was at the time teaching at MIT. He told me that MIT would teach me the economics I needed to be useful in the world. He also ensured, when I was applying the second year, that I could get in, by convincing his colleague that I was worth gambling on despite my unusual background (No one had heard of my multidisciplinary program at the time, and an admissions officer called Abhijit Banerjee had initially put my file in the reject pile, I later came to know …).  I came to MIT in 1995 as a graduate student (one year after completing my masters at the ENS, at “Delta” – which would eventually become the Paris School of Economics). One of my very first classes was development economics. Abhijit Banerjee and Michael Kremer were teaching it.  The class was very small: it was taught jointly with Harvard students (in the spring we took Jonathan Morduch’s class at Harvard). Eliana La Ferrara, Asim Khwaja and Jishnu Das from Harvard, and Chris Spohr, Stuti Khemani and me from MIT. Every single one of us is a development economist today.  For me, it took precisely one lecture to be hooked. This class was noth­ing like I had ever experienced. They were not just teaching. They were searching. Searching for what made this field different. Building on the work of – among others – Dasgupta, Ray, [Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/) and his own research with Andy Newman, Abhijit was busy building the theoretical framework for how to think about development and how to teach it. An essential part of this was the idea of poverty traps: being poor changes people’s oppor­tunity set, and sometimes this means that they stay poor. This theory res­onated with my childhood theory of undiscovered talent. It also offered a way out. Perhaps it is indeed possible to find the right lever to unleash people’s opportunities?  And then, of course, there were Michael’s early forays into randomized controlled trials (RCTs), with the famous textbook projects. We students saw the start of this movement unfold in real life. The 7 schools, the unex­pected results, the larger sample of schools, the results still unexpected, the new tests, and results still were not falling in line!! The most impor­tant lesson was perhaps that this research was hard. The second was that it is worth experimenting. Quite often, you’d be surprised.  Most of his colleagues thought Michael was a little crazy to do this and indulged his experimental forays as a cute way to waste his time before returning to his real talent (macroeconomics). But Abhijit immediately understood the power of RCTs, not just as a tool to evaluate programs, but as a way of turning development economics on its head by giving us the freedom to put any theory to the test. He started running RCTs as well, and even more importantly, creating the partnerships that would allow them to be broader than just a few researchers.  What Michael and Abhijit were trying to do was to bring their pas­sion, their intellect and their hard work to the project of transforming development economics, not just for the sake of economics, but for the sake of poor people. I had found my role models. Now I needed to emu­late them.  But they were also realizing that they could not do it alone, not if they were going to make a real difference. So, their project was intimately tied with teaching and advising. Michael and Abhijit were doing their bit to discover and nurture the talent of others (us, the students), with the greater goal of making sure that, worldwide, the talents of the poor don’t get wasted. The MIT economics department was a wonderful place to do that, for it places graduate education at its very core. I had a wonderful experience there. Abhijit became my advisor, along with [Joshua Angrist](https://www.nobelprize.org/prizes/economic-sciences/2021/angrist/facts/).  Angrist is one of the architects and main troublemakers of the “credi­bility revolution”. He taught me to think about data in the real world through the lens of an experiment: “What would be the ideal experi­ment?” He would ask. “And how does your set-up differ from that?”  In 1999, I got my PhD, and immediately joined MIT as an assistant pro­fessor. It was not common to be hired by your own department, but Michael had unexpectedly decided to move to Harvard and the depart­ment felt that Abhijit needed a colleague to continue building the field of development in the department. I felt this would be a place that would let me do whatever it takes to discover exactly what my talent was (within economics), without giving me too much direction. This freedom, and Joshua Angrist’s rigor and clarity of thinking, gave me the courage to take the plunge and conduct my first “real” experiment as soon as I got my first paycheck and a little bit of research money.  In 2003, Abhijit Banerjee, Sendhil Mullainathan and I were all mulling over various outside opportunities, and the institution decided to do something to humor us. We proposed to the chair of our department, [Bengt Holmström](https://www.nobelprize.org/prizes/economic-sciences/2016/holmstrom/facts/), the idea of creating a center that would focus on doing randomized controlled trials and spreading the results to policy makers. From the very beginning we conceived of a poverty action lab (later renamed J-PAL) as a network. There was so much work to do that what we needed to do was to support the work of all those ready to use RCTs to improve the policies affecting poor people in developing countries. Bengt saw the value of this proposition much more clearly than we did. He secured some funding for us from MIT, we hired Rachel Glennerster, and the Poverty Action Lab was created.  Rachel Glennerster (who happened to be Michael Kremer’s wife) was absolutely instrumental in the creation and the development of J-PAL. She gave it a name; she gave it a mission (“Our mission is to reduce pov­erty by ensuring that policy is informed by scientific evidence. We do this through research, policy outreach, and training”) and kept us on it, and she oversaw a massive growth from our humble beginnings (two barely heated offices, three staff members besides Rachel, and eight affiliates) to the moment she left to become the Chief Economist at the Department for International Development (DFID), in the UK.  Even with Rachel at the helm, with only the initial funding provided by MIT, it would have been difficult to go very far. We were fortunate that Susan Hockfield became MIT’s president. She met us very early on and also understood our model, and why it made sense. She decided to make the Poverty Action Lab a priority for fundraising. This is how we were brought to the attention of an alumnus, Mohammed Jameel.  Mohammed, a pragmatic enthusiast who had chosen the fight against poverty as one of his key philanthropic pursuits, thought the project was worthwhile, and in turn challenged us to affect 100 million lives with bet­ter policies in our first ten years. This sounded like a lot, but since my early days as a very short six-year-old, I don’t lack confidence. Rachel, Sendhil, and Abhijit are not the kind of people to say no to a challenge either. We agreed.  Mohammed endowed our lab, which became J-PAL (the Abdul Latif Jameel Poverty Action Lab). Today, J-PAL is a network of 194 affiliated professors and just as many other invited professors from around the world. We have 998 ongoing or completed projects in our database, run by our affiliates or invited researchers in 84 countries. And at least 450 million people have been affected by policies scaled up after a J-PAL eval­uation found them effective.  I have stayed at MIT ever since I got that first job there. I have wonder­ful friends and colleagues there, and this has been a great home for J-PAL Global and J-PAL North America (led by Amy Finkelstein and Larry Katz). The other J-PAL offices are scattered in academic institutions around the world, and I feel that those are also a little bit my homes.  As I mentioned, when I first met Abhijit Banerjee as a first-year stu­dent, I found him unbelievably inspiring. He was kind and a bit aloof, but I knew crossing his path had changed my life. Little did I know that many, many years later, I would not only become a colleague, but also, eventu­ally, a life partner. I continue to find him just as inspiring, and just as kind, although the aloofness is gone. We have two delightful children, Noemie and Milan. I continue not to believe how lucky I have been throughout my life. That my path crossed Abhijit’s was definitely the most amazing part of it all. |
| Autobiographical |  |
| Podcast | Esther Duflo’s research improves our ability to fight global poverty. In just two decades, co-laureates Duflo, Banerjee and Kremer have transformed development economics with their innovative experiment-based approach, which is now a flourishing field of research. Thanks to their work we have clearer perspectives on the core problems within areas such as education and health.  In this podcast episode Esther Duflo talks about how her drive to understand and fight poverty began at an early age. She discusses the world in coronavirus times, and the fears and prejudices connected to migration. Duflo also shares her tips for managers and her best collaboration techniques.  The host of the podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0813=ED  Ester Duflo: Hello?  Adam Smith: Oh hello. My name is Adam Smith. I’m calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  ED: OK.  AS: We have a tradition of recording, as you may know, very short interviews with new Laureates.  ED: You don’t want … we can do it a little bit later with my husband.  AS: Are you with your husband at the moment?  ED: No, no. He’s still … he went back to sleep. [Laughs]  AS: How very wise of him. [Laughs] It must be very nice to be awarded in each other’s company?  ED: Yes. As long as you’re woken up at night it’s good that you are both woken up for the same reason.  AS: And indeed with Michael Kremer because you’ve worked very closely with him as well.  ED: Yes. No, no, this is wonderful. He was not with us today but it was wonderful this, you know, Micheal Kremer is the first who went to Kenya and started running this experiment, and very early on we all learned from his … from what he was trying to do and his mistakes, and it was … it’s been a wonderful ride ever since then.  AS: And the key is that you need to understand the lives of the people you’re trying to help.  ED: Exactly, and trying to understand them a little bit more deeply in order to devise effective policies to help them get out of poverty.  AS: Because human behaviour is often surprising and not what one would expect.  ED: Exactly. Without spending some time understanding the life of … the intricacy of the lives of the poor and why they make the choice that they make, and why something that might seem at first surprising makes a lot of sense in a particular logic, it is impossible to design the right approach. So the intuition that we might have about a particular problem – this is what needs to be done, there is no text book in schools so we need to give text books. That was the first experiment that Michael Kremer did, and it didn’t work. And in a sense people, of course, running the first experiment was in itself was a wonderful idea, but the fact that it didn’t work was almost as important and revealing. Because it made him realise, and then the rest of us, like ‘Oh, things are not as straight forward as you might think’. The obvious solutions are not always the real ones because we misunderstood the issue. The problem was not the lack of textbooks. The problem is that what is in the textbook is not what the kids need.  AS: Does your work make you hopeful, or also worry you that it’s just … every solution has to be so carefully tailored to the people who it’s being applied to that the work seems sometimes sort of insurmountable.  ED: Oh no, the work makes us tremendously hopeful because the… in a sense there is a combination of specific and general. So the specific is you have to understand what’s the nature of the problem, and the nature of each problem is different. But what is much more general is the lessons that you can draw from human behaviour, once you understand what exactly is the lesson, can often be carried from context to context. So if you take for example the case of learning that Jakob Svensson was talking about today, we understand that the problem is not resources, the problem is not textbooks, the problem is that the children are taught something which is much, much too far for them to understand. They are not taught at the right level. And so the solution becomes simple. It’s ‘Oh, we have to teach them at the right level. If they cannot read we teach them to read’. Which is what this organisation we worked with for many years, PRATHAM, kind of pioneered. But then you … Once you discover that, that is actually a tremendously robust insight across contexts we’ve discovered. We keep running into the same problem from place to place to place, in the [unclear] of India, in Africa, even in France, we have the same problem. And the solutions, in a sense, then can be the same. So you’ve learnt something which is very general, and then from this very general finding you can, you can extract lessons that you then tailor to each individual context, or that the policymakers will tailor to each individual context. There has been tremendous progress done against poverty in the last several decades, and not just against income poverty but also against the problems that are related to poverty, like infant mortality, maternal mortality, low immunisation etc. And to a large extent these are because better policies have been undertaken. And to at least some extent it’s because people … policymakers have been much more rigorous about thinking ‘What are the real problems? What are the real solutions to this problem? What works, what doesn’t and why’. So there is plenty of reasons to be hopeful.  AS: Yes, that constant interplay between experiments and policy is extremely important isn’t it. The topic will of course come up so may I just ask you about the fact that you are only the second female Laureate in 50 years of the Economics Prize, and the only one now living. It’s a very hopeful sign even though the statistics are somewhat depressing.  ED: You know this is … hopefully, it’s onward and forward from now on. I think it does reflect the fact that the field is not … there are not enough women in the economics profession. Period. So it’s not just … you see this problem at all level. Not enough women go into economics as graduate students. Not enough women continue to become assistant professor and then get promoted and then subsequently get recognised for their work. So we really have a problem in economics that is structural and fundamental, that this, you know, lack of women receiving Nobel Prizes before, relatively small number ‘til today, reflects, and … So first of all I think it is going to change because there are more women among the younger cohorts so it’s going to improve, mechanically it’s going to improve. But second of all not enough, not fast enough. I think it’s … the profession is starting to realise that in part it’s the general climate, and the way we treat each other is not conducive for having more women in the profession. It’s not just about promotion but it’s about the general environment and how people talk to each other and address each other in seminars and things like that, that we need to work on a culture that is more respectful and that will be more acceptable for many women who think that they don’t want to play the games of shouting at each other. So we need to make progress there. We also need to make progress in showing to the younger people that economics is relevant for problems that they care about. Because I think women … but it’s also true for minorities. We are talking about women but other minorities is even worse. We see very, very few people who are non-white in the profession at all, and it’s partly because they don’t go into the profession, partly because the perception is that economics is not about real problems for the real world I think. I’m hoping that … I’m hoping that this could also make a difference.  AS: Indeed, well hopefully today’s prize has been inspiring in both ways – both inspiring women to come into the profession, and also showing people that economics can be relevant to directly helping people.  ED: Yes, I think both of these will attract more women to be honest. I think many women do not see themselves as like thinking about finance, or… But they might be more likely to see themselves as doing things that are directly relevant to have an influence in the world against poverty or social problems more generally.  AS: Thank you very, very much indeed, that was a lovely conversation.  ED: You’re most welcome.  AS: It’s been a great pleasure speaking to you, congratulations.  ED: Thank you, bye bye.  AS: Thank you very much, bye bye. |
| Interview |  |
| Q1 | How did you become interested in economics? |
|  | I grew up in a suburb of Paris, in a family with three children. My father is a mathematician and my mother is a pediatrician. Nothing really remarkable about my childhood except for the fact that my mother was involved in a small NGO of doctors who were helping children victims of war. She used to have her regular work in Paris but then from time to time she would spend a few weeks away to do this work, to help the children of the war. I think very early on we got from that a sense that there were some children who had very different experiences growing up and that gave us children a sense of how fortunate we were and maybe how responsible we were of doing something with this good fortune. From this experience I always hoped that some part of my life would be devoted to fighting poverty but I didn’t really know which part. Growing up I thought I would have some career and then on the side I would work on an NGO, very much like my mom. But after some point in the middle of college, I thought maybe this is not entirely satisfactory, that my entire life should be devoted to this and not just something I would do on the side.  I thought maybe I would go into politics or I wasn’t quite sure. I decided spend take a year to think about it, I went to Russia for one year and there I was doing various things, but one of the things I was doing is to be an assistant for economists who were helping with the transition in Russia at the time from communism to capitalism. And in this context I realized how influential economists could be and I thought this is actually perfect, because I had always envisioned myself as an academic, because I am more like the sort of quiet, reflective type and I wanted to do science and I thought with this job I can do my science and at the same time when I have something to say I can really have a role in policy. So that is when I really decided to go into economics. |
| Q49 | Can you tell us about your relationship with India? |
|  | India is the place where I first went as a graduate student to start doing field work. And I kind of learned that in Calcutta as a graduate student in my early 20s with no understanding of anything really, and with a lot of misconceptions about what it was to be poor, in particular the centered around Calcutta. Because somehow I was a bit obsessed about poverty in Calcutta as a child having grown with [Mother Teresa](https://www.nobelprize.org/prizes/peace/1979/teresa/facts/) and the like. And I arrived in Calcutta and I was kind of shown how different the life of the poor really is and how they were kind of going about their business and being under-pricing and having issues but not being like the helpless people that I kind of had in the corner of my mind. From then on; it was kind of a lot of my education as a development economist and as a human being was through the work that I did in India. And then of course now, my husband is Indian, my in-laws are Indian, most of my work is in India so that is kind of a second home for me. |
| Q51 | What have you learnt from your travels? |
|  | I think you learn that people are not so different, maybe it is one of the biggest things that I have learnt, that people live in very different circumstances and that of course has implications on what they can achieve and what they can do with their lives, but at heart we have the same type of strengths as human beings and of weaknesses. They just happen to be dipped into different contexts. |
| Q52 | What can economics tell us about migration? |
|  | I think the biggest misconception that people who are against migration have are two. One is that everybody wants to move from poor countries to rich countries and profoundly if there was no restriction this is the first thing that would happen. And the second is that should they come this would have a large impact on the wages of people here and particularly low-skilled people here. And it turns out both things are just factually incorrect. In practice, a lot of people who have the opportunity to move don’t, so even within country migration from one place to another is very low. People don’t move from the rural area of India to the urban area of India so they are not going to move to Sweden.  People don’t move from the places that get hit by economic shocks in the US to other places that don’t get hit by the same shocks. Fundamentally, people prefer to stay at home. And if you were to remove all the migration barrier the flows would not be that big. That is the first thing. The second thing is that even when there are big migration flows, which usually comes because of a crisis, for example the Syrian crisis did push a lot of people out. It is not that they wanted to go out, it is that they had no choice. But even when that happens all of the history suggests that when there were such episodes, they weren’t big impact on the wages of the natives. If anything in some cases you can see that it leads to a small increase in native wages and in employment, not decrease. So that is the second misconception. So once you remove these two misconceptions there is really no reason to be that scared of migration one way or the another. |
| Q2 | Do you feel that being a woman has affected your work as a scientist? |
|  | I do think that women tend to have different interests than men. I don’t think it is coming from biology, but it is probably coming from early education of girls versus boys. I certainly see in the field of economics, development economics, the study of poor countries, it is the only field that is really more or less equally men and women working in this field. A lot of women come to economics because they want to study questions that have direct implications on the lives of others and can have direct impact on how to do better policy – development economics, public economics, health economics, education. So I do think that being a woman has influenced, probably is one reason why I am interested in development economics. That is of course the most important but apart from that I don’t think there are necessarily vast differences in the way that being a woman specifically has influenced my work.  Of course I have a very specific way of working but it is hard for me to say where it is coming from. But certainly I think one reason why there are few Nobel Prize winners in economics is that there are few women in economics, very few. And I think one of the reasons is that many young women don’t think to even go into the field because they do not realise that those topics are also part of economics. And I do hope that giving the prize to development, not only to a woman among the laureates, but just even the topic is going to make them aware that that is a possibility, those are part of the topics that economists study and therefore are going to serve as kind of a role model for more women to come into the field. |
| Q12 | How can we encourage more women to pursue economics? |
|  | I think there are already two ways to increase the fraction of women in economics. The first one is making people aware that economics also addresses those issues, social issues, climate, poverty, all part of economics and you know not just interest rate, making banks bigger. The second thing is the culture itself in economics is not very friendly. It is a very aggressive culture, very in-your-face culture which many women don’t particular like. There I think the profession can improve by being mindful of having a somewhat more civil way of engaging with each other. And I do think that actually this is already changing, that in the last few years there has been a realization that it is not okay to be aggressive and rude etcetera, one has to … the whole field has to become more gentle. |
| Q25 | How is your work tied to climate change? |
|  | The work on poverty is intimately connected to any work on climate change for two reasons. The first one is that climate change is going to affect, is already affecting the poor countries more than anywhere else. First of all because the poor countries are in the south where it is already hot and second of all because there are fewer ways to adapt. For example it is in Bangladesh places are literally going to go under water. It is in India, where some parts of India are going to become inhabitable because it is too hot and so on and so forth. So that is the fact that it is in the south. And the fact that there are less ways to adapt is for example when there is a heat wave in France it ruined … Last year there was one last summer, it doesn’t kill anyone because we know what to do and we also have the means to adjust to it so we can make sure that everybody is in a cool place and there are enough places with air condition where the old and fragile people can be protected. And it is a matter of organization, so the first heat wave killed many people and the second one didn’t.  In India there are just fewer air conditions for people living in rural area, where are they to go? They have to continue living and the agriculture is not resilient to the heat and so on and so forth. So they have less ability to adapt and therefore it is much much more costly. And the same number of hot days in India kills more people than it does in America or Sweden if you have hot days sometimes. So that is one reason why I think the change in the planet’s climate could easily undo all of the progress or most of the progress that has been made in fighting poverty in the last few years.  The second reason although most of the responsibility for climate change lies with the rich countries there might be some opportunity in poor countries to do things that are going to preserve the environment. For example the forest is not depleted yet in some places so you could try and keep it. There could be ways to compensate people to keep it etcetera. So there are still areas that not have been made yet in the poor countries in the way that they are going that could have an influence on climate change in the future. That is why I think for these two reasons you can’t really be a development economist and not be worried about climate and vice versa, you can’t be concerned about sustainability and not think about the south. Let me just add that with that said I think that the effort and really the bulk of the action in terms of sustainability world will have to be a reduction in consumption in rich countries, because that is really where we are, just consuming too much, period. |
| Q53 | What can we do to create a more sustainable world? |
|  | I have one hope which is that peoples … Sometimes economics tend to think that people always … that all that people want is to consume more and have bigger cars and more air condition in the summer and more heat in the winter. But I think that people’s likes and preferences and needs are much more influenceable than that. So when you see a movement like the youth movement, for example Greta’s work here, it could be enough to create enough of awareness of the issue that people become aware that it is not okay to let the planet warm and it is not just about polar bears, if you don’t care about polar bears, it is also about people.  Most economists tend to be a bit distrustful of the ability of individuals to care about other individuals, particularly people who are far away, but I actually don’t think that is true. I think that people can easily be taught to care. Once they are taught to care I think they can also relative easily adjust to a change in their own consumption because we are creatures of habits and I think if we change a little bit the way we behave then we can get used to that so that it doesn’t become that difficult. The consumption per capita of someone in Sweden is much lower than someone in America and I don’t think the Swedes are unhappier. It is just that they got used to take the subway, the tramway to work. I think we can get there in the sense that I think it will require changes in the way we behave here but I don’t think these changes are necessarily sacrifices. |
| Q11 | Can you tell us about your approach to solving problems? |
|  | I think it is very important to ask questions that can be answered. And that can be answered in as rigorous and scientific way as possible. The question can be broader or it can be narrow but it should be specific. For example there are questions you can ask through experiments that have vast implications, for example “Is private school better than public school?”. That doesn’t seem to be a narrow question, but it is a pretty well defined question. And you can set up an experiment that compares the expense of children in a system that is mostly private school and the same in mostly public school, by creating an experiment where some villages have private school voucher offering and some villages don’t. So that is not a narrow question, that is a broad question but that is one you can set up an experiment for.  I think to me it is very essential to try to ask these questions that are very well defined and on which you can train your kind of gaze as a scientist in trying to answer them rigorously and that involve usually not answering the whole problem you would like to answer at once. I don’t have answer on whether aid work, I don’t have answer on whether all countries should be like China. Because to me the questions are not even very well defined. But I can answer a question on “Would kids to better in a system where they have free access to private school?” or “Would kids do better in a system where school is provided by the government?”. That’s kind of how I see the differences. And once you have your very well defined questions, you can set up an experiment, very much like you test the effectiveness of a drug in medicine, you can look at the effect of the program or the effect of the intervention or the effect of structuring your school system for example. |
| Q28 | What are your hobbies? |
|  | I would say I am an ex-climber, I used to climb a lot before having children. But now I think, first of all I have less time and second of all, my husband think that I should be sure to be alive for my children, so I should not take some unnecessary risk. So now I do all sorts of … I still climb but indoors, and I do all sorts of other activity like running, playing tennis and hiking. |
| Q8 | How is your working relationship with your co-laureate Michael Kremer? |
|  | Michael was a young assistant professor at MIT when I came as a graduate student and was really the kind of visionary in seeing how you could start using experiments in the field. They existed as a technique but almost nobody had used them. And Michael, and then Michael and Abhijit together started doing this work. First of all really feeling their way around and as a student I was watching this happening and this was really incredibly exciting to know that was like a tool that could be exploited to us. As soon as I could, as soon as I got my PhD I got involved in this work as well. And since then, of course we have been working, Michael was one of the first few affiliates of our network, J-PAL. We were eight researchers at the beginning when we created the network and he was one of them. Of course, Michael’s wife Rachel Glennerster who is now the chief economist at DFID and was for fifteen years the executive director of J-PAL and grew it from these eight people to 200 affiliates and 200 fellow travellers. I have a lot of work with Michael, I worked with him in Kenya on several projects and we are still involved in a work, in a big project in Ghana together. And it is kind of, I think we are very different the three of us, in what we bring to the table so it is always a huge pleasure to work with them. |
| Q26 | What does the prize mean to you? |
|  | I think what the Nobel Prize gives us is a chance for leveraging the work we have done so far and give it even more presence. Really the way you achieve any success in the fight against poverty is by working with development country governments. It is not really NGOs or foreign aid or one of us who can make a difference, everything happens in the development country governments themselves when they, if their policy work better and they are able to design better policy to achieve the problem that they are thinking about. And we have already started on this journey where we really work more and more with governments, not just to give them suggestions but to kind of learn together. And I think this will help even more in terms of opening doors and making, reassuring various governments with whom we are not yet working, you know this is legitimate and not a strange undertaking by some crazy people so I think that will help a lot. And I also hope that it is going to contribute to bring many more people to the field, researchers and also to bring money to do all this work so the whole thing should kind of move on to one more level of intensity I am very much hoping. And then the second thing is related to these climate discussions we were having a little bit earlier, I would hope that a lot of work also gets focused on these issues, both in rich countries and in poor countries. |
| ID | 0814 |
| Biographical |  |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0814=MK  Michael Kremer: Hello?  Adam Smith: Oh hello. My name is Adam Smith. I’m calling from Nobelprize.org, which is the website of the Nobel Prize in Stockholm. Congratulations on the award of the Prize in Economic Sciences.  MK: Thank you so much.  AS: Well it’s an awkward moment to catch you at. Are you actually in the UK at the moment?  MK: That’s right, I’m in London right now.  AS: Right, okay. How did the news reach you?  MK: I had … I had a skype message from a Swedish colleague saying that he wanted to talk to me urgently and I started to send him a note to say that this was a phishing attempt, that he should be aware that somebody has hijacked his account and it was a phishing attempt, and then somebody here at the LSE, Steve Pischke, knocked on my door and told me congratulations, and that explained it so …  AS: The randomised evaluations which the three of you work with were something that you really pioneered in Kenya in the 1990s. And I know that they’d been used before but they really took off when you started working with them. What was the key to their success in that period in Africa?  MK: I think that one real key is that I think a lot of people think of economics as only about the stock market but a lot of people go into economics because they care about practical problems and poverty. One of the … you know aside from the methodological side of randomisation, this type of work involves on the ground engagement and trying to engage in a practical way with problems, and it combines that with intellectual rigour and I think that’s led to a real flourishing of the field, and lots of understanding now about practical steps to address poverty. I think that makes the field very exciting but I think it’s also very exciting for the world. And I think there had … There can be a tendency to see the problems of poverty as intractable, and I think we’re learning that there’s a lot of practical steps that can be taken to address poverty.  AS: That’s the point – your work is leading to all these small success stories around the world which together become a great source for hope.  MK: I think that the work in the field is, more broadly … I mean it’s just very exciting to go to conferences these days and see the fantastic work being done. And I think that this is both intellectually exciting, helps us improve our understanding of the world, but I think it also can help lead to practical improvements on the ground as well. And has.  AS: Yes indeed. And that link with the policy making is absolutely key isn’t it.  MK: Yes. I think that’s … I think that’s important.  AS: You do sound as if you’ve been quite surprised by the news.  MK: [Laughs] Yes, very much so.  AS: Had you any inkling that this might happen?  MK: No, none at all.  AS: That’s nice. And it’s very nice that the three of you awarded are all such close colleagues.  MK: Yeah, it’s wonderful, wonderful to share the prize with them. They’re wonderful scholars and wonderful people to work with.  AS: Have you been able to speak with them yet?  MK: I’ve just spoken with my family and that’s it. I’m a bit in shock here and taking it one step at a time.  AS: It’s really very kind of you to speak to us, and we very much look forward to speaking more when you come to Stockholm in December.  MK: Okay, wonderful. Well thank you so much.  AS: Thank you. Bye bye.  MK: Bye. |
| Interview |  |
| Q1 | How was your interest in science sparked? |
|  |  |
| Q4 | How did your experimental approach to economics begin? |
|  |  |
| Q8 | Can you tell us about your relationship with your co-laureates, Abhijit Banerjee and Esther Duflo? |
|  |  |
| Q54 | Do you enjoy teaching? |
|  |  |
| Q55 | Are women and girls more affected by poverty? |
|  |  |
| Q28 | What do you like to do in your free time? |
|  |  |
| ID | 0815 |
| Biographical | I first saw light in Albuquerque, New Mexico, USA, at the dawn of World War II. My earliest memories are of the warm climate, skiing in winter, trout fishing in summer, and a fragrant alfalfa field outside my window.  How did I get from an alfalfa field in New Mexico in the 1940s to Stockholm in 2018? This essay recounts the intellectual history of my involvement with climate-change economics, or in the words of the Prize citation, “integrating climate change into long-run macroeconomic analysis.” I will concentrate on the people who taught me, the colleagues and mentors who inspired me, the bureaucratic obstacles encountered, the critics who nudged me toward improvements, and above all the pure joy of the half-century voyage of discovery.  My interest in the environment can be traced back to my father’s passion for skiing. Robert J. Nordhaus had joined the Tenth Mountain Division, troops on skis, and fought in the Italian campaign in World War II. When he returned to practice law in Albuquerque, he decided he needed to develop a ski area called La Madera (“the timber”) on the Sandia Mountains east of Albuquerque. When I describe this to friends as a “ski area in the desert Southwest,” they wonder just how crazy it was to build a ski area above a town that averages 10 inches of snowfall a year. As a boy, I once tried to predict future snowfall using some quixotic mathematics because I knew that the difference between snow and drought was between happiness and boredom. Skiing has been a most favorite sport of all lineal descendants of my father.  I am often asked how I became interested in the economics of climate change. Was it my boyhood passion for skiing, with its dependence on weather? Perhaps, in part. However, as Voltaire wrote, history is a fable agreed upon. I have no surviving diary of this period, so I will accept this origin story as plausible, much in the spirit of the founding fables of cultures that do not have written literature.  Before leaving skiing, it is worth using this as an example of the impacts of climate change and the offsets of technology. Because of global warming, the snowline in ski areas will rise about 1,000 vertical feet. If nothing else changes, my boyhood ski area of La Madera will no longer offer skiing.  Several technological developments more than offset the warming, however. The most important is “snow-making,” which allows production of snow at above-freezing temperatures from water through high-powered nozzles. The net effect of warming-plus-technology is that the coverage of slopes for many ski areas has actually increased rather than decreased in the last half-century.  Skiing is a good example of how “managed systems” can adapt to climate change and reduce damages. Much of the economy of high-income countries has similar adaptive capabilities. For example, those who study agriculture, such as MIT’s John Reilly, find that adaptation can reduce much of the negative impact of warming up to 2°C or so. Similar studies apply to some but not all of infrastructure.  However, there are large and important parts of the world that are “unmanaged” or “unmanageable.” Vulnerable unmanaged systems include rain-fed agriculture, seasonal snow packs, river runoffs, threatened species, and natural ecosystems. Ocean carbonization means that ocean crustaceans are doomed.  I grew up in a family of six. My older brother, Bob, was a lawyer and became a leading draftsman of energy and environmental legislation for the US House of Representatives; we like to think of him as the architect of the deep state. The second born, Dick, was more the artist and landed as a successful architect. I was the third boy. My parents were fortunate on the fourth draw, and I have a younger sister, Betsy, who has lived around the world and now resides in Israel with her extended family.  Much has been written about the importance of birth order, but my experience differed from the psychological formulas. I was the third of three boys. I don’t suppose I was a huge disappointment, but a third boy was probably unwelcome given that the first two were rowdy. My father, who in later years was a most beloved grandfather, in my early years was an Army captain with a German-Jewish background and a firm sense of who was in charge and should be called sir (he was). He commanded the first born, Bob, to be a lawyer (he did), to join the Army (he did), and to go to Yale (he refused).  By the time I arrived and was growing up during World War II, my parents were bored with boys. So, they paid the bills but largely ignored me. This family arrangement was heaven and set my course for the rest of my life. I came to believe that if I stayed within the bounds of rules and civility, I could do anything I wanted, fail or succeed, have fun or struggle, laugh or cry. I didn’t have to join the Army or be a lawyer, but in one of the great windfalls of my life, I did go to Yale.  In hindsight, Yale in the early 1960s was a paradise for minds coming to the age of abstract reason. I was especially interested in the humanities and social sciences, while the natural sciences held little attraction (which fortunately matched Yale’s comparative advantage). I spent the first two years in a program called Directed Studies (DS), which was an interdisciplinary program focusing on the classics of literature, art, and social sciences. The courses were taught by awesome teachers like Vincent Scully (history of art), Leonard Krieger (history), Victor Brombert (literature), and Richard Bernstein (philosophy). These scholar-teachers were role models for my future academic life.  After two years, I became restless with Yale and decided to spend my junior year in Paris. That was another fortunate diversion. One sunny afternoon, at Café de l’Univers, I encountered my fellow student and future wife, Barbara Feise. We spent the year as *flaneurs* in Paris, where we studied and went to concerts together, she taught me about fine food, and I enticed her to ski. We continue to enjoy all these activities together today. When we moved to New Haven, Barbara began her career as a clinical social worker and then psychoanalyst at Yale’s fabled Child Study Center (in those days, the medical school’s equivalent of the economics department’s Cowles Foundation).  The intellectual environment in Paris was a sadder tale. Paris, and Sciences Po where I studied, was still in the intellectual ruins of World War II and a backwater for economics. My teacher had not yet made it to Marxian economics but still taught Ricardian economics (with analytics from around the 1820s). But with all the other fascinating things to do, I just ignored my schooling for a year and soaked up the culture.  When I returned to Yale for senior year, I became serious about economics. I took macroeconomics with [James Tobin](https://www.nobelprize.org/prizes/economic-sciences/1981/tobin/facts/), economic history with Henry Broude, and political economy with Ed Lindblom (using his book with Robert Dahl). When I graduated, I was thrilled to be admitted to the best graduate economics program in all the world and of all time, at MIT.  MIT was Arcadia for young economists. Teachers included [Bob Solow](https://www.nobelprize.org/prizes/economic-sciences/1987/solow/facts/), [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/), [Ken Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), and a host of others and they provided me the tools of the trade. But my real teachers were my fellow students. The ones I was closest to were [George Akerlof](https://www.nobelprize.org/prizes/economic-sciences/2001/akerlof/facts/), Bob Gordon, Bob Hall, [Joe Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/), and Marty Weitzman. I was fearful of looking stupid to Ken Arrow, but my fellow students were kind enough to pardon my offenses.  In tense times, I muse that being a married graduate student is the best stage of life. No responsibilities, no stress, no wails from the crib, no calls from the school principals, no employers haranguing you, no committees. But lots of time for talk and reflection and playing Go, and all this paid for by scholarships. After three and a half years of graduate study, I was done and ready to get a real job, and in 1967 I moved back to Yale as an assistant professor. **Evolution of climate-economy models** One of the key tools emphasized in modern economics, and in graduate programs such as MIT in the 1960s, is the importance of *models*. A model is a simplified, but not oversimplified, picture of a more complex reality.  The first model I built was of snowfall in New Mexico, and it was just a fantasy model. One of the joys of academic economics is model-building. I built models of railroad profits, of the US macroeconomy, of the patent system, of inflation, of productivity, and of induced technological change.  I then turned to energy models.  The 1970s were the high-water mark of modern Malthusianism. We were awash in books like *The Population Bomb* and *The Limits to Growth,* which predicted stagnation, the decline of living standards, and spreading famines.  One of the doomsday books was by MIT’s Jay Forrester, called *World Dynamic*s and published in 1971. He was a computer genius, but I was too young to care and was appalled by the lapses in Forrester’s study and the later famous offspring, The Limits to Growth. I then built and published in 1974 a model of the Forrester model to show that the results were highly sensitive to many assumptions and that any of several alternative assumptions would lead to continued growth in living standards. History was not kind to the predictions of any of these studies, and Malthusian thinking proved as inappropriate to the late 20th century as it had been in the early 19th century.  My other response to the flaky *Limits* literature was to take it seriously. Clearly there are limited resources of oil, gas, copper, and clean air. Just as clearly, technological change is finding substitute processes that replace scarce resources with superabundant resources. Perhaps the best example is fiber optics (light plus silicon) as a substitute for copper wire. The quadrillion-dollar question is whether technology will outpace depletion.  It was here that working at Yale sent me down a path that was different from the standard economic approach. Much brilliant work of this time relied on econometric analysis and modeling. Colleagues at Yale, particularly [Tjalling Koopmans](https://www.nobelprize.org/prizes/economic-sciences/1975/koopmans/facts/) and Herbert Scarf, persuaded me that better methods were activity analysis (whether linear programming or LP, which I started with, or computable general equilibrium or CGE, which came later).  These approaches combine the fundamental constraints of resource availability (of oil, gas, coal, uranium, and so on) with society’s preferences to determine the efficient path of resource extraction and depletion. Along the way, they also compute the efficiency prices, such as the price of oil. Theoretical studies showed that, if calculated correctly, the efficient outputs and prices of the LP solution correspond to the outcomes of competitive markets, this being “the correspondence principle.”  Using the programming approach to energy systems, I developed a model called the “Bulldog model,” named after the mascot of Yale. The model found that the efficiency price of oil was close to the actual market prices of the 1960–1972 period. The model suggested a gradual transition from oil and gas to coal and synthetic fuels and then to superabundant resources, which were then thought to be nuclear power.  I was assisted by a brilliant Yale undergraduate named [Paul Krugman](https://www.nobelprize.org/prizes/economic-sciences/2008/krugman/facts/) and planned to present and publish a paper with the results in the fall of 1973. Then, in October 1973, the first oil crisis erupted. Energy markets went haywire, oil prices went up 300%, other resource prices followed upward, inflation soared, stock markets tanked, recessions followed, and the world entered the Era of Scarcity. **The energy crisis** It is difficult today to reconstruct the mindset of people in the mid-1970s. Energy prices rose sharply, there were long lines in gas stations, many advocated wartime rationing of gasoline, and it was hard to find staples like toilet paper. *Newsweek* had an issue with an empty cornucopia and a cover, *Running Out of Everything.* The economic crisis was shadowed by the American political crisis of the collapsing Nixon administration, followed shortly by a Carter administration that foundered in another energy/oil crisis, inflation, sky-high interest rates, and a sense of malaise.  But the fortunes of economists are counter-cyclical. For a young energy economist working in the 1970s, there was music in the cafés at night and revolution in the air (Dylan 1975). My work on energy convinced me that the crisis was primarily one of a mismanaged energy regulatory system and a rigid capital stock. It was definitely not the Limits or Newsweek scenario of running out of everything. I reviewed the issues in a short paper on resources as a constraint on growth in 1974. I concluded that energy and other subsoil resources were unlikely to be major constraints, nor were environmental problems on a national level. **First stirrings of global concerns** However, global environmental issues were a different problem. I first encountered global environmental issues in a visionary 1970 MIT report by a group of scientists, *Man’s Impact on the Global Environment: Assessment and Recommendations for Action.* This report stirred my interests in global environmental issues, and my next research took up the issues outlined in that report.  I went back to reread the 1970 report to prepare the present essay. Because we know so much today, it is hard to remember how little was known in 1970. There was only one estimate of the impact of rising levels of carbon dioxide, CO2. Specialists thought the globe was cooling and that rising levels of particulates would further exacerbate cooling. The first study of the economics of climate change examined the impact of cooling, not warming. The only alternative to fossil fuels was thought to be nuclear power, which was viewed by many scientists with suspicion. It was many years later, perhaps only in the 1990s, that the measured changes in climate began to match the model projections.  Until that time, climate change was based on fundamental science, and only in the last two decades has the science been validated by observations. **A waltz to Vienna** My life took an unexpected and fortunate turn in the summer of 1974 when our family spent a year in Vienna at IIASA, the International Institute of Applied Systems Analysis. IIASA was an international research organization, with scholars from both sides of the Iron Curtain, devoted to global problems of mutual concern. It attracted a group of talented scientists and economists and was led by Harvard’s Howard Raiffa, a pioneer of decision theory. We thought Vienna was the last romantic city left in Europe, and that was half-true, with beautiful music but a gritty environment.  I was encouraged to go to IIASA by Tjalling Koopmans, and he was a source of inspiration and support throughout this formative period. My group was devoted to energy studies and included an analyst and algorithmist (Alan Manne from Stanford), a font of futuristic ideas (Cesar Marchetti), and a young economist from Belgrade with whom I have co-authored four books (Nebojsa “Naki” Nakicenovic).  Scholars shared offices at IIASA, and by lottery I joined Allan Murphy, a distinguished climatologist from Oregon State University. We shared stories about our interests, and he encouraged me to think about the impact of the economy on climate systems. Thus, began my study of the economics of climate change.  I was familiar with economics from my own studies and from having taught the entire subject (macroeconomics and microeconomics) at the introductory, intermediate, and graduate levels at Yale. But I knew nothing about the scientific aspects of climate change, zero. I set out, beginning at IIASA, to learn about the relevant sciences. I later thought it was fortunate I had not studied the sciences in school because they would be outdated, like the biology course I took in 1958 that did not mention DNA. My self-taught courses involved the writings of scientists like M.I. Budyko, Lester Machta, Suki Manabe, H.H. Lamb, and Charles Keeling. It then involved tutorials, when I was on committees of the US National Academy of Sciences, from leading lights such as Roger Revelle, Joseph Smagorinsky, Stephen Schneider, Jesse Ausubel, Paul Waggoner, Ram Ramanathan, the polyglot economist Tom Schelling, and a young star environmental economist, Gary Yohe. **The IIASA model** The first step in integrated modeling of climate change was actually quite simple. It involved beginning with the Bulldog energy model, adding emissions of CO2 (a simple set of linear equations), and then adding a Markov distribution matrix among seven carbon reservoirs (following the work of Lester Machta). We used a high-end LP program in that period’s large computer and stacks of IBM cards. After a couple of months of programming, clearing computer jams, and fixing bugs and mistakes, we ended up with the first integrated assessment model of climate-change economics.  While the modeling was simple, I encountered a major obstacle. The leader of the energy program was a nuclear physicist and a fierce proponent of breeder nuclear-power reactors as a long-run solution to energy scarcity. Breeder reactors are a second-generation design that produces as much fissile material as it uses. It was a complete economic dead-end because of high costs and the subsequent decline in first-generation reactors, but the breeder has a certain expensive elegance.  When I described my proposed research program to include CO2 to the leader, he ordered me to stop the work. It was not, he declaimed, on the work plan of the energy project, i.e., on his work plan. It was obvious to me that taking climate seriously would be an advantage to nuclear power, but he did not see that yet, so it also was not in his work plan.  In skiing, if you see a bare spot, you go around it. So too with the leader. I went around him to the director of IIASA, Howard Raiffa, and explained the problem. Being a deep advocate of undirected research, he simply assigned me to another program, on methodology, where I would find encouragement. It left some residual bitterness between me and the leader, but the work was not slowed more than a couple of hours.  This research produced the IIASA model, which was an early integrated analysis, including projections of atmospheric CO2. Because of the LP approach, it allowed analysis of constraints (such as limits on CO2 concentrations). Most interesting were the results for shadow prices, and which eventually were interpreted as carbon taxes.  The concept of shadow prices originated in the mathematics of Lagrange and later in the Economics Prize-winning work on linear programming of Kantorovich and Koopmans. We are accustomed to market prices (such as the price of gasoline), which represent the costs to producers and the value to consumers. Shadow prices are the social equivalents, that is, the social costs and values, but they are costs that are not captured by markets because of externalities. In climate change, the shadow price of CO2 emissions is the social cost that is not reflected in the price that we consumers pay for our emissions.  Our energy/climate models calculate the shadow prices of CO2 emissions.  The shadow price indicates the cost to the economy of an extra ton of emissions; perhaps the shadow price is $40 per ton of CO2. The correspondence principle states that, in a properly functioning economy with no externality, the market price would be $40 per ton. The $40 price would reflect the value to consumers and the cost to producers of the last unit.  I knew about shadow prices from work on the Bulldog energy model, but at first I was puzzled by the shadow prices on CO2 in the IIASA model. Eventually, I figured out that the CO2 shadow price is the social cost of CO2 emissions. When the study was published in 1977, I explicitly recognized this as a fiscal question, interpreting the shadow price as a carbon tax to implement policy on a decentralized level. This idea lay fallow for many years and then resurfaced as the “social cost of carbon,” a concept that is now central to climate policies.  The IIASA study was my first attempt at integrated-assessment models (IAMs) of climate-change economics. It had two major shortcomings.  One was that it was a partial-equilibrium model; that is, it took the overall conomy and interest rates as given. Second, it did not really close the loop because it had no damage function. Rather it imposed an artificial constraint on CO2 concentrations, or in a later version on temperature. This leads to an interesting question of the evolution of temperature limits. **Two degrees of modeling** While the IIASA study in 1975 contained no estimates of damages, it did attempt to close the loop from emissions to policies by considering different approaches to setting standards for the control of CO2. It noted that there was no serious work on standards, and that standards need to be set in terms of society’s technology and preferences.  The resolution was in retrospect a wild guess at a standard: temperature limits. I wrote that as a first approximation temperature increase should be kept within the normal range of long-term climatic variation. Studies at that time (1975) suggested that a temperature increase of more than 2 °C would take the planet outside the range of temperatures experienced over the last several hundred thousand years. Since the IIASA model did not have a temperature module, I estimated that a 2 °C target was consistent with a doubling of CO2 concentration, so that was used as a proxy for temperature increase.  At the same time, I emphasized that this was the starting point, not the ending point: *“It must be emphasized that the process of setting standards used in this section is deeply unsatisfactory, both from an empirical point of view and from a theoretical point of view.”* (Emphasis in original)  I will not follow the subsequent discussion of temperature targets, which is beautifully described in a paper by Carlo Jaeger and Julia Jaeger. The target of 2 °C was endorsed in the 2015 Paris Accord and is the aspirational standard around the world. My reservations have not changed over the years. As I wrote four decades later, “We cannot realistically set climate-change targets without considering both the costs of slowing climate change and benefits of avoiding the damages.” **From early stirrings to the dice model** Over the next two decades, I moved from model to model like a pilgrim trying to reach the Promised Land. The main goal was to fix the two major flaws in the IIASA model. These were to develop a general-equilibrium framework, and the second was to develop the modules of the climate externality.  Accomplishing these two goals took nearly two decades, and what finally emerged was the DICE model (Dynamic Integrated model of Climate and the Economy). The first major study was rejected by economic journals but accepted and published in *Science* in 1992.  I have always loved the name of the DICE model. It is easy to remember and visualize. DICE also conveys a shiver of risk and danger. It alluded to the Faustian bargain that we make as we continue down the path of unchecked climate change, the Walpurgis Night of reveling without reckoning on how the devil of damages will come to drag us to a hellish future.  The first major change was to scrap the large energy model and move to a Ramsey model of optimal economic growth. I learned growth theory from Sukhamoy Chakravarty, Karl Shell, and Bob Solow at MIT. But writing code for a computerized model is a different animal than solving differential equations. That step had been taken by Alan Manne, Rich Richels, and others for a standard economy without climate starting in the early 1980s.  But I wanted to have a closed system, which required developing, estimating, and programming the externality equations along with the Ramsey model. The required externality equations were for emissions, a carbon cycle, a climate module, a damage function, and an emissions-control variable. None of these five major modules existed in a way that was suitable for an economic model to solve the climate problem in its fundamental form. Finding satisfactory modules was the pilgrimage that took two decades.  Rather than going through the evolution of each, I will take the examples of the climate and damage modules. For climate, the issue is how to go from the path of CO2 concentrations in each year to the path of global mean temperature in each year. Climate models do this, but they have hundreds of thousands of variables and take a month to solve on a supercomputer. Moreover, they are recursive, and the economic approach is optimization, which is computationally much more complex. I needed a few climate equations, certainly less than a dozen, not a dozen thousand.  I stumbled through several iterations. I would ask climatologists, but they generally were not interested in economics or in simple models. I would try simple empirical equations, but they would fit poorly and were methodologically unsatisfactory. Moreover, I wanted something that was not only simple but acceptable to the climate community.  It was then, around 1990, that I encountered the dazzling scientist-advocate Stephen Schneider, at NCAR, then Stanford. When I told him what I needed, he said that he had just what I needed. It was the Schneider-Thompson model, which was a two-equation climate model. I studied it, decided it was the right scale, and adopted it in 1992. The same basic structure, with different parameters, has been part of the DICE model since then. The genius of the Schneider-Thompson approach was to rely on radiative forcings, or the earth’s heat balance, rather than CO2 concentrations for climate change. This allowed closer integration with other high-resolution models over the years. Steve died in 2010 of a vicious form of lymphoma, but I send him a silent greeting and murmur of thanks whenever I think of him.  Each of the other major modules has its own story. The other difficult one was the damage function. Damage functions originated in environmental economics in the 1970s. Estimates of damages from climate change began to appear in the 1980s, and by 1990 it was possible to make estimates of damages along a climate path. My earliest estimates were that damages were a linear function of output and temperature, and later I changed this to quadratic, which is the form that has survived to this date.  Some of the most impressive work on damages has been done by my Yale colleague Robert Mendelsohn. He and I did the first paper using the controversial Ricardian method for measuring damages in US agriculture, and he and others have applied that approach more widely. This approach solves a key problem, which is to distinguish climate from weather. Some of the current approaches forecasting high damages, often cited by alarmists, rely on weather shocks, which are entirely different from climate change.  Another persistent issue in the damage function is the treatment of catastrophic impacts. This question was highlighted by Harvard’s Martin Weitzman. He argues that the likelihood of extreme outcomes might dominate the analysis because of “fat tails” in the distributions. We see fat tails when the stock market goes down 23% or when a Japanese earthquake is many times larger than any in the recorded history of Japan. Under a normal distribution, it is extremely unlikely to see a seven-foot-tall man walk through the door (perhaps one chance in a million). In a fat-tailed distribution, we might occasionally see a twenty-foot person. Similarly, for climate change according to Weitzman, we might have catastrophic changes that would be civilization-ending. The point, emphasized by Weitzman, is that fat tails would make DICE-type analyses irrelevant for climate policy because it would be looking at weather forecasts in the last minutes of the *Titanic*.  Arguments about damage functions have been going on for almost three decades, and they are just as heated today as in the earliest times. If anything, further research has increased rather than reduced the uncertainties.  Since 1992, the DICE model has spawned many versions as well as spin-offs. It has been extended to a regional model (RICE, joint with Zili Yang), a probabilistic model (joint with David Popp), a model with induced innovation (R&DICE), and one to understand climate clubs (Coalition-DICE). Former students such as Lint Barrage (now at Brown) have pushed the boundary to include other issues, such as taxation. It is used around the world, and I get emails virtually every day from scholars or college students asking about its structure or critiquing its assumptions. My friend and colleague Martin Weitzman has remarked that updating the DICE model will provide lifetime employment until we solve the climate problem, which means lifetime employment. **From dice to climate clubs** All economists who have worked on climate change knew it was “The Hard Problem” of economic policy. Global public goods are difficult because there is no market or political mechanism to solve them, and climate change is the Colossus of all global public goods. Despite the best efforts by economists, scientists, and politicians to forge effective international agreements, progress on this Hard Problem has been slim. The effective global carbon tax in 2018 is about one-tenth of my estimated target, and perhaps one-one-hundredth of the Stern approach.  The reason for the failure is clear, as has been emphasized by the work of Columbia’s Scott Barrett. Our climate agreements are ones without penalty for non-participation or non-compliance, so the result is that there is free-riding and minimal abatement. This is the lesson of economic theory and environmental history, and it has been proven accurate for climate agreements.  While I was generally familiar with work in this area, the failure of climate agreements was becoming clear as the Kyoto Protocol was dying in the 2010–2012 period. I remember exactly when the neurons connected. I was discussing the Greek economic crisis in my macroeconomics class, and I wondered, “Why does Greece stay in the Eurozone? Why not just pull out, and perhaps out of the EU as well?”  The answer to the Greek conundrum was clear. The EU is a “club” with dues and privileges. The dues are adherence to the rules, such as regulations and rules on democracy; the privileges are enjoyment of an enormous borderless economy, with free trade in goods, services, and finance. I began to study the theory of clubs, look at other political clubs such as NATO, the United States, and the World Trade Organization. Each of these has the same structure – dues and privileges. The successful ones have benefits greater than costs as well as coalition stability. The unsuccessful ones (such as the League of Nations, the [Kellogg](https://www.nobelprize.org/prizes/peace/1929/kellogg/facts/)–[Briand](https://www.nobelprize.org/prizes/peace/1926/briand/facts/) Pact that outlawed war, and the Kyoto Protocol) were ones where free-riding and low benefits led to their demise.  Building on other work, I developed the idea of a “climate club” in my 2015 presidential address to the American Economic Association. An important aspect of the climate club, and a major difference from current proposals, is to create a strategic situation in which countries act in their self-interest to enter the club; they undertake high levels of emissions reductions because of the structure of the incentives. In the language of [John Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/)’s beautiful theory, change the structure so the non-cooperative strategy yields an equilibrium with high participation.  Although it is easy to design potential international climate agreements, the reality is that it is difficult to construct ones that pass two tests: to be effective and stable. Effective means abatement that is close to the level that passes a global cost-benefit test. Coalition stability means that no country or group of countries can improve its welfare by changing its participation status. Existing approaches from Kyoto to Paris fail these important tests.  The proposed climate club is simple: A coalition of countries form a group, similar to the EU or the WTO, in which they agree to set a target of a minimum domestic carbon price of, say, $50 per ton. Countries who decline to join the club are penalized with duties on imports into the club of, say, 3% on all imports. This is conceptually the same structure as the European Union.  I built a model of coalitions (the Coalition-DICE model) to test different carbon prices and tariffs. The major empirical findings were these. First, a club with a zero-penalty tariff would have no members (which is true of the Kyoto Protocol). Second, for prices up to $50 per ton of CO2, modest penalty tariffs of less than 5% would induce most countries to participate (as they do in the WTO). It cannot be proven mathematically, but it seems clear that some version of a climate club is our only hope for inducing widespread participation and abatement in a strong climate treaty.  In December of 2018, Barbara and I were joined by our children (Jeff, Monica, and Rebecca), son-in-law (Will Curry), grandchildren (Annabel, Margot, and Alexandra), family (Barbara’s brother Chris Feise and my nephew Ted Nordhaus), and close colleagues (Jesse Ausubel, Lint Barrage, Lars Bergman, Nebosja Nakicenovic, and Gary Yohe) in Stockholm. This was a wonderful celebration of our many decades together. **Looking backward and looking forward** In Edward Bellamy’s futuristic 1887 novel, *Looking Backward*, Julian West wakes up 113 years later in Boston to survey the society in 2000. He finds a socialist society, complete equality, no pollution, and nationally owned industry. The economy is managed much like an idealized version of Soviet central planning. Money has been replaced by cardboard credit cards that have punch holes like IBM cards. By 2000, the well-oiled machinery produces a cornucopia of nineteenth-century products.  The economic fantasies in *Looking Backward* are basically all wrong. Bellamy’s vision did not foresee air travel, nuclear weapons, computers, the Internet, cyberwarfare, the miracles of modern medicine, or climate change. The vision of comprehensive central planning collapsed with the wall in 1989. Even with modern computers, the economy is too complex to be managed by the largest and most idealistic of hierarchies. Instead, countries have found the formula for prosperity in the mixed economy: the rule of law and government support for basic science, alongside profit-oriented research and innovation of the market.  *Looking Backward* reminds us of the profound difficulty of predicting the structure of our societies far into the future. If we go to sleep today and wake up at century’s end, what will we find in 2100? Will it be a dystopian landscape where the successors of today’s thuggish leaders find new tales to spin about climate change? Will ocean crustaceans be a footnote in the cookbooks? Will the Western forests of the United States be replaced by char and savannah? Will Weitzman prove a prophet as the outlier events are realized and are much worse than we imagine today?  It need not be so. We should instead gather strength from our valiant high-school science teachers, our great research universities, and forward-looking scientific prizes. We should insist that the data-based findings and theories of natural and social scientists replace fake facts and false narratives. We should persuade nations to look to the EU, the WTO, and similar club-like organizations as models for global governance in human rights, non-proliferation, and climate control. The emissions and temperature curves then might point down rather than up.  Our futures are not in the stars but in ourselves. I will not be here in 2100 to witness the results of our efforts. However, my grandchildren are likely to be here, with their children and their grandchildren. May they be looking backward with appreciation. I hope they will say that this generation had the resolve to overcome the obstacles and take the steps necessary to preserve our unique and beautiful planet. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0815=WN  William Nordhaus: Hello.  Adam Smith: Oh hello, may I speak with William Nordhaus please.  WN: This is he.  AS: My name is Adam Smith. I’m calling from Nobelprize.org which is the website of the Nobel Prize in Stockholm.  WN: Good morning.  AS: Good morning. Well congratulations on the award of the Prize in Economic Sciences.  WN: Thank you.  AS: How did the news actually reach you?  WN: The news reached me when one of my children called, actually a few minutes ago. We were asleep and then we woke up, the phones were ringing, and talked to my daughter.  AS: What a lovely way for the news to come.  WN: Yes it was, it’s a very nice way.  AS: It’s a striking coincidence that the news of your prize comes on the same day that we’re hearing about the IPCC special report highlighting enormous transformations that are going to be needed if we’re going to keep within reasonable temperature rises.  WN: If you called on almost any day there would be a story somewhere in the newspaper and the science journals, in the economic journals about climate change and its causes and its impacts, and that’s a sign of its ubiquitous nature. So I know this is a report, it’s a useful report and it’s … it’s collecting together what we have been studying and learning over the last, actually, 30 or 40 years, and it’s an important reminder of the dangers we face and the things that need to be done.  AS: And does your work make you hopeful that we can do what needs to be done?  WN: I’d say my most recent work has made me somewhat concerned about the fact that we’re doing so little. The most recent work I’ve done is studying actual trends in abatement and in policies, suggests we’re doing much less than needs to be to reach any of the targets, whether it’s a 1.5 degree, or 2 degree, or even a 3 degree target. There’s … I think the policies are lagging very, very far – miles, miles, miles – behind the science and the need to … what needs to be done. So it’s hard to be optimistic. And also we’ve fallen behind because of the policies – we’re actually going backwards in the United States with the … with the disastrous policies of the Trump administration. I never use the word ‘pessimism’, I always use the word ‘realism’, but I’d say it’s a kind of dark realism today.  AS: Yeah, and it’s not just the United States that may be pulling out of these accords – there are others on the horizon.  WN: Yeah, different places are doing different things. It’s not too late, but the steps we have to take are more difficult now than if we’d started earlier.  AS: And would you highlight one thing as being the single most important thing for people to do?  WN: Well, the first thing is that people have to come to grips with the difficulties we face. I think the scientists have, and many of the people have, but the governments have to. And then the second thing that’s most important is that we take some kind of economic steps. I have advocated for many years a carbon tax as a way of implementing policies. And then the third thing is we’ll have to have a significant technological transformation. Of course those first two would help the third but those three have to go together. You can’t do it without public support, but you can’t do it without some kind of economic signals in the form of a carbon tax. And then all of those will help induce the technological changes that are necessary to make a transition to a low carbon world.  AS: And all that is difficult enough to achieve if you believe that this is what needs to be done, but if you don’t believe, goodness, where do you start?  WN: Well, as I say, that’s the … the first thing, you can’t do it without, without the public awareness. I think … I think actually we have a fair amount of progress on public awareness if you look at what the surveys suggest and what people say. I think it’s actually the governments are lagging behind the people here.  AS: Yeah. I do want to just ask what was your daughter’s reaction when she told you the news?  WN: Oh, she said … she said: ‘This is so nice’.  AS: Says it all!  WN: You know, I can’t capture the tone of voice. It was a very sweet, a very sweet announcement.  AS: I think it probably goes down as one of the best.  WN: [Laughs]. Yes.  AS: We very much look forward to Stockholm in December.  WN: OK.  AS: So, thank you very much.  WN: Look forward to seeing you.  AS: Thank you. Once again, congratulations.  WN: Thank you. |
| Interview |  |
| Q1 | What’s the source of your passion for economic sciences? |
|  | So my passion for economics. It comes from many sources; I think the biggest one is for the benefit of humanity or mankind as we used to call it. Humanity, and also not just for humanity but for the broader world around us. I thought when I first began working on economics that it was clear that there were many economic problems that were unsolved. I think the ones I was most interested in when I first started was poverty and macroeconomics for business cycles. I still am interested in those, but I wird off into other areas particularly environmental economics and resource economics later, but the big passion was I thought economics was a tool that one could use, like a carpenter uses a tool to build something, it was an intellectual tool to be used to improve people’s well-being. |
| Q49 | How has your upbringing shaped you? |
|  | My upbringing in New Mexico, I’d say was very focused on the outdoors. My father was interested in the outdoors, interested in skiing, fishing and building things, interesting things like tramways. He was interested in Indian law at that time and in the later part of his life became one of the nation’s leading Indian lawyers, so all this had to do with the outdoors. And so, when I moved east to a more urban environment, I had that in my background. |
| Q25 | What interests you about climate change? |
|  | My interest in climate change was a natural outgrowth of interests that evolved over time. So, when I first became seriously interested in economics in graduate school, I was primarily interested in economic growth. I think it is quite interesting that the prize this year, which is really about economic growth, was what my interest, actually has been my interest all my life in economics. And I started working on that, working on the technology, and for four or five years I worked on exactly the same areathat Paul Romer got his prize for, but I gave it up, it was too hard, so it was just too hard for me so I moved on. And then I started worrying about … But it was the same issue that the technology Romer deals with which is that markets cannot solve certain problems, markets are very good at some things, they allocate bread, they allocate gasoline, but they don’t allocate technology efficiently and they don’t allocate energy and environmental issues efficiently.  I was always interested in the by-products of economic growth and so I worked first on energy and then, well, I have always worked on energy, I should say. And then gradually looked at some of the by-products of energy production and that very quickly led to climate change. I think that the place that sort of crystallized that for me was in 1974. I went to a new institute that had been formed in Vienna called the International Institute for Applied Systems Analyses or IIASA. At that time, it was just an extraordinary crop of brilliant, brilliant people. Director Howard Raiffa from Harvard, my colleagues [Tjalling Koopmans](https://www.nobelprize.org/prizes/economic-sciences/1975/koopmans/facts/) and Alan Manne from Stanford being among them, and there was a man named Allan Murphy, a brilliant climatologist, I shared an office with and so we naturally got started, and he said: “Why don’t you think about this?” and so I started thinking about climate change. |
| Q18 | How can your work be used to take better care of the planet? |
|  | I think the main message of the work, not just my work, but the work of people in the area of climate change economics, the integrated assessment modelling as it is called, a technical name, is really twofold; one is it to see the climate change problem as a by-product, an unintended by-product of economic growth and then the second thing is to understand that what we need to do is we need to correct that by fixing the price of emissions so now emissions are free in most places, not in Sweden, but every place else in the world outside of Western Europe. When you admit a ton of carbon dioxide you don’t probably know you’re doing it or think of doing it probably, you drive your car, or you have a product that has combustion behind it, that you are putting in the atmosphere, and you are causing damage, but you are not paying anything for it. So, the inside of economics is when you go through that and you analyse it to say we need to put a price on these, and that way give people incentives to reduce their use of carbon fuels or the product that use carbon fuels and thereby reduce their emissions.  The key message of economics is that this is a flaw in the market and it’s correctable. If governments take steps to change the price from zero to a high enough price, then you can meet your target. So that data is pretty easy to explain, but then the next step is, what is that price? And I think the most interesting thing about all the work I’ve ever done, is building models of this – the price just comes out of it. It’s just one of the by-products of the … it’s called linear programming, it’s a technical name for it but it has another technical thing, called a dual variable, which is, this is the price that you should set on carbon dioxide emissions if you’re going to meet your goal. I think of it as kind of a mathematical miracle in the sense that you can do this and out pops the price that the government should set. |
| Q18 | What can an individual do to help the climate? |
|  | My message is, you by yourself can do nothing. There are too many people doing too much for too many generations of people so even if I’m the most ethical person or even if I’m not just an ethical person that I’m going to represent ten or hundred other people who are not so ethical or don’t care about it, the effect I alone, I want to emphasize the word alone, will have is insignificant, it’s like a billionth of a billionth or trillionth of what is necessary. So, what is necessary is that people act through their governments and their governments act together with other nations to take steps to solve this problem. I don’t want to say there’s a single step you can take, because there are multiple things to do, but I’d say the message is you need to exercise your duties and responsibilities as citizens, as voters on places where they’re not voters, where there’s no democracies as protesters, work through governments, work through non-profit organizations, work through schools to persuade people that we need to take steps to do this. There is nothing wrong with taking the step by yourself to curb your own emissions, to turn the heat down or whatever. That’s fine, but that by itself is not sufficient. |
| Q56 | What is your outlook for the future of climate change? |
|  | I’ve been working on this long enough to see that things change and change slowly. I’d say over the period since I have been working, I started work on this in the 1970s, there has been a lot of progress in the science, the social sciences more than natural sciences. I’ve seen considerable changes in viewpoint of professionals, of scientists, interested scientists, concerned citizens. I think there are now a great realization of the problem, the solutions and so on. So in that respect I think it’s been a good moment. At the same time the politics has moved backwards in the last, say, last couple of years, maybe more, maybe a little bit more, with people, particularly the American president, who makes ludicrous statements about climate change, but I think it spreads, it is a little spread so in all of our big countries we have problems of leaders who are really not thinking about global problems, not even thinking about problems often in a sensible way. So what I would like to say is two steps forward one step backwards, maybe it’s a big step backward now, but I’m hopeful that we will get over that and move forward again. |
| Q56 | How important is education in relation to climate change? |
|  | People often learn their social, political, scientific views when they are in school and they get hardwired into their brain and their emotions and it’s sometimes very difficult for people to change. Often our leaders will be taught things 20, 30, 40, 50 years old and so they’re not really ready for the challenges of today, they are not ready for the challenge of the climate change. Climate change is one example, another one is cyber security. I think the people have, the people in leadership in the world don’t actually know anything about information technology so how can they perceive the threats of it. And I think it is now changing a little bit, but when it first came these people know nothing about computers, how can they possibly understand the threats of cybersecurity, so it’s little the same as with climate change. They grew up, they went to college, so I view the most important job that I do is teaching because people at college age, I think of 18-22, they have the most amazing open and plastic minds. I’m just absolutely astounded.  The students will come in at the beginning, let’s say teaching macroeconomics, and will know nothing, they know nothing about international trade, nothing about how the exchange rate works, they will know nothing about how the economy is affected. But by the end of the time they basically know what a professional economist knows. It’s just so easy and the they will forget half of it, of course, but somewhere deep in their brain they will know it, so when they run into it five or 10 or 20 or 30 years later, say: “Yes, I learned that at Yale”. This is the generation I love working with because there is a brilliance of minds but also a plasticity and an ability to absorb new ideas and to challenge also when poor ideas are put to them. So that is what teaching means to me. |
| Q3 | Did you have a teacher who inspired you? |
|  | I had so many of them. I was just blessed, both at Yale as an undergraduate and at MIT as a graduate student and then in a way also in my professional life as having so many teachers, teachers and often later colleagues and co-authors. I think the one that I, in this area, in climate change, the one I particularly point to, would be Tjalling Koopmans. Tjalling Koopmans who’s Dutch, and he left Holland right before World War II, he was very much aware of the Nazi menace in Western Europe. He went to America, he worked in America, in mathematics and he invented his very important linear programme technique when he was working on shipping convoys in WWII. He went to Chicago and then he went to Yale and he was a colleague at Yale. He influenced me, not just because he was interested in energy and climate change, but because of his enthusiastic … and also his mentorship, he wasn’t really mentor at that stage, it was more his enthusiasm for the subject and his enthusiasm for my work. And the techniques he used were different from what everyone else was using and what I pointed out to earlier, this business about the shadow price, the dual variable that gives you the price of carbon, came out of work he did. If I hadn’t used that technique, I wouldn’t have found it. Actually, I think he was the first person I ever found mention climate change in a Nobel Lecture or speeches. In December 1975 he actually mentioned it in his [toast at the banquet](https://www.nobelprize.org/prizes/economic-sciences/1975/koopmans/speech/). I just mention him because he was a wonderful colleague, a brilliant, brilliant Nobel Prize winning economist and just a very generous man as well. But there were many, many others, but since in this context I would particular associate with Tjalling Koopmans. |
| Q8 | How does it feel to share the prize with Paul Romer? |
|  | I think it’s wonderful, it’s wonderful to share this with Paul Romer for lots of reasons. One is that I have a great admiration for his work. What Paul Romer got the prize for as you will explain, is opening up the box of technological change. In the earlier work that had been done in economic growth, the work that I learned in graduate school, technological change was taken as a given, it was known as exogenous, falling from heaven and not produced by humans, it just comes to you. And everybody knew that’s wrong, everybody knows we produce great technologies, we produced the steam engine, the idea for the steam engine and the steam engine, the idea for the transistor and the transistor and modern electronics, the idea for the telephone and then the telephone, and now the telecommunications. Everybody knows it didn’t just come floating down from some intellectual heaven, but it was invented by people, but it wasn’t in our models.  We knew this, I actually tried to work on that myself, as I said, it was too hard, so I gave it up. But that was the problem that Paul Romer tackled and solved and produced this really brilliant new way of thinking about technologies. I think it’s really nice for me to share with Paul, partly because that was a problem I thought about, worked on and failed at when I was a graduate student. And partly also, coming to today, because it’s clear that the solutions to problems like climate change involve exactly what he is talking about, finding ways to induce new technologies, finding ways to get people to think about them, to invent them, to develop the products and processes to make sure they are commercial, to get them out in the world. This is the issue he saw and using his insights in the climate change area, marrying those two, is a critical, critical need over the next few years if we are to succeed. |
| Q57 | Where do you do your best thinking? |
|  | One of the things to understand, it’s not a linear process, it’s not, you sit down with a blank piece of paper. I know nobody thinks this, but just to emphasize, you sit down with your paper and you say, climate change and underline and say, number 1, number 2, number 3, and you say there is the problems, let’s go solve them. Often things will occur while you are outside walking around, when you’ll see something, when you’ll talk to somebody or where you are maybe sometimes, actually an art exhibit will stimulate some thinking. A problem, obviously a problem, when you see problems, an example of a problem was when in 2002-03 when the US was about to go to war in Iraq. We were just marching, marching on as back WWI, Europeans will know the history from WWI, marching, sleepwalking in the history so to speak, and we were just marching along. It wasn’t 100 %, but it was pretty clear, and the question is, how costly this is going to be, and so I started working on the costs of the Iraq war, a pretty big study on that. But that was one where there is a problem just in front of you and you say: Well, how much is this going to cost, have we thought about the costs of this? Talk to people: Well, yes, it is not going to be very expensive, but of course, instead of 50 billion dollars, it’s like 2, 3, 4 trillion dollars – I’m just vastly, vastly underestimating. But that’s something where it is a problem.  Other times it’s just curiosity. An example of curiosity was I was thinking about light and whether we are measuring light correctly. It was just something I thought about and then I forgot about it, then thought about another year later, and then something would happen and I would see something and: Oh, that‘s how to do that. And then I got interested and worked intensively on it. So usually there’s, sometimes there’s a problem, sometimes just pure curiosity about something when you want to … was it measured correctly, and the lighting is a good example. With lighting, the background of lighting was it was a big controversy whether the price indexes were being measured correctly. This was particularly in the United States and there was a big report by a Stanford colleague named Mike Boskin, and the Boskin report, he argued that the consumer price index in the United States was seriously mis measured and that looked right. Part of the problem was we weren’t measuring quality change correctly, so that looks right. And I looked at some other price indexes, like the European price indexes are even worse that the US price indexes. So then at that point I said, what about light, are we measuring the price of light correctly, so at that point then I would say how we are going to measure, we can look at different sources, oil and electricity, compact fluorescence, that was before the LED bulb, and then you say we are measuring incorrectly because we are measuring them by the price of the light bulb, rather than how much it produces or certain number of lumen hours. I would say that was just pure curiosity that drove then. |
| Q4 | How has the world interpreted your award? |
|  | I receive obviously as many Nobel Laureates do, a lot of e-mails, in these days e-mails, a few phone calls, a few letters and then personal congratulations from colleagues that I know in New Haven, friends. I think the most interesting thing about it is that this particular award for climate change, yes it’s economics, but people think this is an award for somebody who’s working on climate change. People think this is like a light in a dark time and they see this as very important institution, really the premier scientific award institution of the world, saying to the world and particular America, they are Americans, I think, particularly useful to Americans, to take heart, even if, I don’t think the Nobel, I don’t say you are saying, this is what they think you are saying, the people, and why they are so happy about it is because they think that you are saying take heart, there is good work out there, there are people who care about you, the United States and the rest of the world and just stay with it, keep heart and we have people who are working for good. So it was very interesting, and I think, I’m very grateful for that particularly, that it gives a kind of courage and heart to the people who are on the right side of history. |
| ID | 0816 |
| Biographical | *Paul M. Romer did not submit a biography.* |
| Autobiographical |  |
| Podcast | Protecting the ship, building relationships and organising surprise weddings – in this conversation, conducted in February 2020, Paul Romer discusses everything from the special moment he experienced just hours before collecting his prize to the importance of unity, purpose and inclusion.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0816=PR  Paul Romer: Hello.  Adam Smith: Hello, may I speak with Paul Romer please.  PR: This is Paul.  AS: This is Adam Smith calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  PR: OK.  AS: Well, first of all many, many congratulations on the award of the Prize in Economic Sciences.  PR: Yes, well, thank you.  AS: You sound nicely calm.  PR: [Laughs] I think it takes a while for the … it takes a while for this to all sink in. So …  AS: I can imagine. I won’t ask whether it’s a surprise because of course a couple of years ago NYU released a press release saying you’d been awarded the prize by mistake.  PR: Well, I wasn’t sure I should mention that, but yeah that’s … unfortunately this is something that universities tend to prepare for, and I’d sort of been through this trail, including this, this release. It’s actually not the first time that happened. There was an email that went out when I was at Stanford … [phone rings] So, anyway, I’ve got some experience with this, but nobody, I think nobody prepared a press release at any university this time, and I was … sound asleep so it caught me off guard.  AS: Well, never mind they can dig out the old ones.  PR: I guess so.  AS: You’ve been awarded the prize for analysing the relationship between innovation and the economy. We all know that innovation drives growth in GDP, but what do you think is the most important thing that your work teaches us?  PR: Well, it teaches us that what happens with technology is under our control. If we collectively set our minds to improving technology of a particular type we can do that, and it takes some collective action, some support for research, or some provision of patent protection, or a mixture of the two, and some focussed energy. It takes even policies like a commitment to open up systems of university education to everyone. But if we set our minds to improving technology, we can improve it in a direction that seems important to us and even at a faster rate. So instead of treating it as something that just happens to us like the weather, we should treat … we can treat it as something that we control.  AS: And is fostering research and development enough or do you need more to make it work?  PR: Well, one of the things I should tell you is that if you look at the very long sweep of history what you see is that the rate of growth has been speeding up, the rate of progress, and that’s because there’s more and more people who are all engaged in this process of discovery. And then once anyone discovers something they can, they can share it. So a very important part of supporting this kind of research is making it possible for as many people as possible to know what we’ve already discovered, to communicate what they’ve discovered and to share this process of learning what we know, and then going out and making new discoveries. So it isn’t just a question of what any one nation does, it’s also a question of how effectively we connect with everybody anywhere in the world and share all the insights that are discovered anywhere. But when I think about, say, a pharmaceutical that might help keep my mind sharp in 20 years or 30 years, I don’t care if it’s discovered in the United States or someplace else in the world. I just care that somebody discovers it and if there are a lot more people all over the world working on things then it’s much more likely that we’ll discover them.  AS: Indeed, and there seems to be a race among nations to become the technological leaders of course, but then one also has to think of nations that are left behind perhaps?  PR: Yes, well but this notion about this possibility of sharing is a very optimistic result for countries that are left behind. They don’t have to reinvent the wheel – they can take advantage of what’s already known. And so the challenge of development is figuring out what are the impediments that prevent that flow of knowledge to those countries, and what can policymakers do to remove those impediments and then take advantage of this very rapid growth that countries get when they start catching up.  AS: Lovely, thank you. Well hopefully we’ll get a chance to talk about all this and more when you’re in Stockholm in December to receive your prize.  PR: Yes.  AS: Thank you so much, and once again congratulations.  PR: OK.  AS: Thank you.  PR: Good, thank you. |
| Interview |  |
| Q58 | You have previously said that, “the most important invention is the development of the city.” Why do you think this? |
|  | The reason the city was the most important invention is because cities facilitate cooperation. If we hadn’t invented cities, we wouldn’t be able to cooperate with large numbers of people like ourselves. It’s this potential for cooperation at scale that explains all of the success that humans have had. As the number of people goes up, the income per capita or resources per capita or standard of living that’s possible goes up. The challenge is to create the conditions where a large number of people can cooperate and all get the benefits from that. Cities let us do that at scale, starting from a hundred people up to nowadays 10 million or so. |
| Q59 | How do you think cities will develop in the future? What is important to take into consideration when making decisions about that evolution? |
|  | It’s important to remember that there are people who will be affected by current decisions and elections who don’t have a vote – the people who will be born in the future. They’re affected by our decisions, but they don’t have a say. I think it’s useful to have that same kind of mentality about cities.  Most people, when they think about cities, think about the existing set of cities and how they will evolve. Part of what I’ve been trying to encourage everybody to think about is that new cities are still emerging and will continue to emerge. Probably the most important question we can ask about the future of the cities, is what’s the future for these new cities. |
| Q4 | Why are you so interested in the development of new, rather than existing cities? |
|  | The future for existing cities is in some ways more boring because so many things are already established and would be very hard to change. But with new cities you have more freedom to try other kinds of arrangements. Also when most people then think about the future of a new city they assume that only rich people get to live there. But the new cities that matter are going to be the new cities that let people who have almost nothing move in and live and work there.  The reason I’m so focused on new cities that welcome people who come bringing just themselves is that this is a problem of enormous practical significance. This is not just a matter of science fiction or fantasy. According to the Gallup organisation, there are 750 million people around the world right now who say that they would move to a new country if there were some place that would accept them.  If you imagine new cities that could grow to 10 million people, which is not large by today’s standards, you’d need you need 75 of them to accommodate 750 million people. It’s not inconceivable that the world could develop 75 new cities like this, but it’s a pretty substantial undertaking. |
| Q60 | How could we develop 75 new cities that could accommodate that number of people? |
|  | Well how did we get the cities we already have? We got the cities that we have because the people who moved there used their talents and their labours to build them. So you need to have a vision of how the people who move to a new city start building it, but starting from ground zero. |
| Q61 | And what do you think these new cities would offer that our current cities couldn’t just by growing? |
|  | First, they would accept many new residents. Most existing cities won’t do this. Second, what the successful cities would offer are the things that matter most to the people who want to move. You could have lots of different cities offering lots of different things, but the ones that succeed will be the ones that meet the demand that people have.  I suspect based on existing refugee and migration flows, that one of the most important things people are looking for is a place where they will be safe. The second thing that I think they want is a place where they can work – people are ambitious, they want to build things. And finally a place where they can stay for a long period of time, make longer term investments and that will offer opportunity for their children. |
| Q62 | You mentioned making decisions now for people who are not able to have a say, like those that are yet to be born. What do you think are the best things we can do with the cities we currently have to ensure a better future for incoming generations? |
|  | One of the things that I think all cities and all societies have to confront is the problem of inequality. The thing that first made me think about this idea of new cities was not international migration, but it was living in Chicago and getting to know a woman who lived in public housing there. She was a single mom with two children and I got to understand what it was like for her to live in conditions of high crime. The thing that struck me was that if you’re affluent, you can protect yourself from the threat of crime and the costs of crime. But when you have less income and fewer choices crime is an incredible burden. So these are the kinds of things that I hope, if there were more options of places people could move to, we would see better opportunities in terms of lower crime and better education opportunities. |
| Q63 | I just have one last question for you – what is your favourite city? |
|  | Maybe it’s because I lived there, but my favourite city now is New York. The thing I love about New York is that it was a gateway that welcomed millions of people who moved there from around the world. They brought nothing but themselves and got their first job in conditions that sometimes were not very attractive. But that was the first step that let them build a better lives for themselves and their children. New York is the demonstration that we can do this. It’s not a fantasy, we just have to do what we’ve done before. |
| ID | 0817 |
| Biographical | **1.Early days** I was born in East Orange, New Jersey, on September 12, 1945. My father, Alan, was an actuary for Prudential Life Insurance, where he would spend most of his career. He had graduated from the University of Toronto, majoring in math and physics. Alas, I did not inherit his mathematical prowess. My mother Roslyn attended the now defunct Upsala College in East Orange. She was an elementary school teacher before having children: myself and my younger brothers, Donald and Maurice. Aside from a two-year stint in Los Angeles, where my father was temporarily transferred, I spent my youth in New Jersey. We lived in Chatham after returning from California.  My career as a student was undistinguished. A combination of mild dyslexia, a habit of daydreaming, and a dislike for tedious work were all contributing factors. My father was always frustrated with what he considered to be my carelessness. I remember an episode in Chatham when I was young, I am guessing around age eight, when my father attempted to teach me discipline. He instructed me to copy the first two pages of Mark Twain’s book, *Tom Sawyer*, verbatim. When I did it perfectly I would be allowed to go out and play with my friends. Although this seems to be an easy enough task I never quite got it right. In my defense, the opening does have a lot of dialogue with punctuation I could never quite get perfectly.  “TOM!”  No answer.  “TOM!”  No answer.  “What’s gone with that boy, I wonder? You TOM!”  After several days filled with much yelling and frustration by both of us, my father finally gave up on this task.  When I was finishing middle school my parents decided that the answer to my scholastic problems was to attend a more rigorous school, so they sent me off to Newark Academy, a private day school for boys then located in Newark. (It moved to Livingston shortly after I graduated.) I commuted to school on the train along with many men in suits, including my father. The plan of sending me to a harder school worked to some extent. I did work harder, and got lots of practice writing, but the same limitations kept me from being a top student.  For college I went to Case-Western Reserve University in Cleveland. There I met my first wife, Dianne (“Dee”) Shiff, with whom I had three children: Greg, Maggie and Jessie. My favorite subjects at college were economics and psychology and I decided to go to graduate school in economics on the theory that it was more “useful” in case grad school did not work out. I chose the University of Rochester because they then had a reputation in mathematical economics and I was naïve enough to think that this is what I wanted to study, having enjoyed a course using the classic text by James M. Henderson and Richard E. Quandt. However, my first semester in graduate school convinced me that I was not going to be a theorist. (My friend and classmate Jerry Green gave me a good standard to which I could compare myself, and I knew I was not in his league.) Instead I focused on more applied fields, including labor, public finance, health economics and economic history. For the latter subject I was lucky enough to study with [Robert Fogel](https://www.nobelprize.org/prizes/economic-sciences/1993/fogel/facts/), who almost made economic history come to life, but not quite enough to get me excited about spending my career looking at old documents in the library. The Rochester economics department was a striving place in those years. Several stars passed through on their way to careers elsewhere, including Robert Barro and Rudy Dornbush. The faculty I spent the most time talking to were Stan Engerman and Rudy Penner.  Eventually I focused on health economics working with the then department chair, Richard Rosett, whose wine buying habits were later featured in my research. We had picked a potentially interesting problem for my thesis, which was to try to explain why infant mortality rates were so much higher for African American mothers than for whites. A regression putting all of the plausible explanatory factors on the right-hand side (including income, education, whether the birth took place in a hospital, etc.) could only reduce the discrepancy by about half. Truthfully, it did not make a great job market paper, though I think a later version of me might have been able to give the article a more interesting spin by calling the result a puzzle. Since this was just my fourth year in graduate school, waiting to leap into the job market would have made sense, as is commonly done. But in those days lingering was not an option. My funding was ending, and I had a family to support so had little choice. Despite some promising fly-outs, I did not end up with a job offer at an attractive research-oriented university, and I decided to take a job in the Washington D.C. office of an economics consulting firm whose headquarters were (and remain) in Princeton, New Jersey.  Late in the week before I was supposed to start work I got a call asking if I could fly down to Princeton from Rochester to talk about my new job. When I arrived, I was shocked to learn that the purpose of the meeting was to fire me. It seems that the Washington office was not doing as well as they had hoped. They offered to pay for my moving expenses to Washington and give me a month’s pay. This was on Thursday; the moving trucks were due to arrive Monday. Meanwhile, there was a hiring freeze in place for the Federal government. After unsuccessfully pleading my case, I rushed back to Rochester to figure out what to do. It made no sense to move to Washington with no job prospects, but the lease on our grad student housing apartment was also running out on Monday, so we were about to be jobless and homeless. Not good.  Fortunately, the last two years that I was in graduate school in the economics department I had been teaching at the Rochester Graduate School of Business to earn some extra money. I had gotten to know the Dean, Bill Meckling, and so I called him in a panic begging for a job for a year. It turned out that they had a couple courses that were unstaffed, and Bill made me an offer the next day. Someone found us another apartment to live in and the movers ended up just taking us across campus rather than down to Washington. We survived, though a few days later I came down with a strange illness that made me as sick as I have ever felt. Stress is like that.  This bit of bad and good luck gave me a second chance at writing a thesis and I decided to make the best of it. I had an idea for a research project that I was intending to work on as soon as my thesis was done, and I decided to make the rash decision to just abandon the first thesis topic and switch to my new idea: how to put value on saving a human life.  This research idea came from the course on cost-benefit analysis I had been teaching in the business school. Many government programs, from highway safety to environmental regulations to health care provisions have, as one of their outcomes, a change in the life expectancy of some segment of the population. What is that worth? The method then in place was to value the lives saved (or prolonged, really) by calculating the loss in human capital their deaths would entail — i.e., how much earnings they would lose minus what they would have consumed. This struck me as a preposterous idea. For one thing, prolonging the life of a retired person would have no value, or perhaps a negative value, since retired people consume more than they earn. I had read an article by [Tom Schelling](https://www.nobelprize.org/prizes/economic-sciences/2005/schelling/facts/) (1968) that sketched out what seemed to be a much more sensible frame-work based on willingness to pay for changes in the probability of death, but no one had been able to figure out a way to estimate a value based on this conceptual framework.  My thought was that it might be possible to estimate the value people put on their own lives by looking at how much people had to be paid to be willing to take risky jobs such as mining or logging. The main problem I had was to find a source for occupational mortality rates. It finally occurred to me to ask my father, the actuary, whether he might be able to find such data. He promised to look around and struck gold. He soon sent me a study sponsored by the Society of Actuaries that had exactly the data I needed, published in a thin red book. I was in business.  This project had turned into a labor economics exercise, so I went to talk to Sherwin Rosen, then a young professor in Rochester who had received his Ph.D. from Chicago. He liked the problem and agreed to become my new advisor. As we worked on the project the more general problem of what he called “hedonic prices” captured his attention, and this led to his well-known paper on the subject (Rosen, 1974). We co-authored a paper based on my thesis (Thaler and Rosen, 1976) that became quite well-known, and the technique for valuing lives saved that we advocated is now widely used. (See Viscusi and Aldy, 2003).  While working on my thesis, I decided that it might be interesting to do an informal survey of people’s attitudes toward the risk of death. However, I was not sure whether I should ask about people’s willingness to pay or their willingness to accept so I did both, sometimes using a within-subject design in which people answered both questions, such as these:  Assume you have been exposed to a disease which if contracted leads to a quick and painless death within a week. The probability you have the disease is 0.001. What is the maximum you would be willing to pay for a cure?  Suppose volunteers were needed for research on the above disease. All that would be required is that you expose yourself to a 0.001 chance of contracting the disease. What is the minimum you would require to volunteer for this program? (You would not be allowed to purchase the cure.)  Theoretically the answers to these two questions should be approximately the same, with differences only attributable to income effects.1 But I was stunned when I saw the results. Typical respondents offered answers that differed by one or two orders of magnitude, and a non-trivial number of people said they would not agree to participate in the program described in version B for any amount of money. I thought this was interesting and showed the result to Sherwin, but he was unimpressed and told me to go back to work on my econometrics exercise.2  While I followed Sherwin’s instructions to get back to work on my thesis, I was intrigued by my survey results, and it started a new interest. I began collecting examples of people behaving in ways that were inconsistent with economic theory and put a list of them on my office blackboard. The topics included other examples of buying and selling prices diverging, people failing to ignore sunk costs, and struggles with self-control problems. Most of the economists with whom I shared these examples found them more annoying than interesting, and I was not at all sure that there was anything resembling “research” that could come from this collection of stories.  Then I had a lucky break. In the summer of 1976 Sherwin and I went to an interdisciplinary conference held at Asilomar, a rustic conference center near Monterey, California to present some new research on the value of a life. At the conference there were two psychologists, Baruch Fischhoff and Paul Slovic, who hailed from an independent think tank in Eugene, Oregon and were presenting some research on how people (mis)perceive risk. At the end of the conference I offered Fischhoff a ride back to the San Francisco airport, which gave us a couple hours to talk. I learned that Fischhoff had done his graduate work in Israel at the Hebrew University. I told him about my list of peculiar economic behavior and he said that I might want to read some of the research by his two thesis advisors, whom I had never heard of: [Daniel Kahneman](https://www.nobelprize.org/prizes/economic-sciences/2002/kahneman/facts/) and Amos Tversky. I wrote down their strange sounding names so that I would not forget them.  When I returned to Rochester (where I now had a job as an assistant professor at the Graduate School of Business) I went to the library, found the psychology section, and started devouring the early Kahneman and Tversky papers on human judgment. Reading those papers created the sort of “ah ha” moments that one imagines are routine in academic life, but in fact are rare treasures. The insight was that when people use simple rules-of-thumb or “heuristics” to make predictions, they make systematic errors. The key phrase was “systematic error”.  This phrase was the key to making use of the idea that people have limited rationality, as famously explored by Nobel laureate [Herbert Simon](https://www.nobelprize.org/prizes/economic-sciences/1978/simon/facts/) a decade earlier. At some level the idea that people are only boundedly rational was unchallenged even by the most ardent defenders of rational choice theory. (They knew plenty of individuals they thought to be quite irrational …) But no one could specify how the behavior of boundedly rational agents differed from those with complete rationality. Here was Kahneman and Tversky’s key insight: if people make *systematic mistakes*, their behavior is *predictably different* from that implied by rational choice models. That seemed to me to be a big deal.  I wrote to Fischhoff to thank him for the tip and share my excitement, and he told me about a new paper that Kahneman and Tversky (K&T) were working on, something they called “value theory”. He thought that economist Howard Kunreuther might have a copy. I called Howard who sent me a copy complete with his comments scrawled in the margins. Value theory turned out to be the working title for what later became Prospect Theory, and in this paper Kahneman and Tversky were working on issues directly related to the items on my list. Specifically, the value function at the heart of the theory had “loss aversion” as an integral component. Loss aversion leads directly to a discrepancy between willingness to pay and willingness to accept. The answers people were giving to my value of a life questionnaire were a little less mysterious, and I was captivated.  I soon learned that K&T were planning to spend the following academic year, 1977–1978, visiting Stanford. I had already been planning to spend the summer of 1977 at Stanford working with Sherwin on some new projects, so I set out to find a way to stick around for at least a semester to meet my new heroes. In another bit of good luck I met Victor Fuchs, the now legendary health economist, who was then running the west coast office of the National Bureau of Economic Research (NBER) where Sherwin and I would be working. For reasons that I will never know, Victor agreed to put me on his grant, which allowed me to stay for the fall, and he eventually paid for my visit for the entire year. That year literally changed my life, and rather directly led to this prize. **2. Going all in** In June, 1977 the Thaler family (Dee, Greg and Maggie aged 11 and 8 – Jessie was not yet born) loaded up our station wagon and took a leisurely trip across the country to eventually arrive at Stanford. One of our last stops was in Eugene, Oregon where Fischhoff and Slovic were located. I met with them along with Sarah Lichtenstein, who also worked at their organization, Decision Research, and Maya Bar Hillel, another K&T student who was hanging out for the summer. They all became the initial members of my psychology support team. By this point I had written a draft of what would eventually become my first behavioral economics paper (Thaler, 1980) titled “Toward a Positive Theory of Consumer Choice.” I was beginning my process of sending the paper to journals and waiting for it to be rejected. K&T arrived at the end of the summer and we were soon introduced.  It turned out that Danny (as I learned everyone called Kahneman) was visiting the Center for Advanced Study in the Behavioral Sciences, which was located about 200 meters up the hill from NBER. I soon began visiting Danny on a regular basis and we would often take long walks in the hills nearby talking about psychology, economics, and life. (It would become a life-long conversation that we still pursue.) It turned out that Amos and Danny knew roughly as little about economics as I did about psychology, so we were able to learn a lot from each other. Later in the year I audited a Ph.D. course Amos was teaching in the psychology department on judgment and decision-making. I had no teaching responsibilities that year so I had plenty of time to think, and about once a week Victor (my benevolent benefactor) would stop by to ask me what was new. It turned out that Victor was an excellent inducer of guilt, so I was under a lot of pressure to have something new to talk about the next time he dropped by.  At this point I was a third-year Assistant Professor at Rochester and I had a big decision to make. Along with my thesis paper with Sherwin I had written a couple papers on the economics of crime, but it was clear that my passions were captured by this tantalizing combination of psychology and economics. At some point during the fall I decided that I was going to commit my research efforts to that topic full time, and that it would be wise to look for a job somewhere other than Rochester. I had two reasons for wanting to move. First, most of the faculty at the Rochester business school was not enthusiastic about my new line of research. They had recently been described in some magazine article as a University of Chicago “farm club” (a baseball expression for a minor league affiliate). [Milton Friedman](https://www.nobelprize.org/prizes/economic-sciences/1976/friedman/facts/) was considered a god, and Michael Jensen, the most prominent faculty member, was a recent Chicago graduate with a strong belief in rational choice models and efficient markets. My second reason for looking to move was that I realized this new research agenda was highly risky and progress might take time (especially since my first paper kept getting rejected) so I was hoping to land somewhere and restart my tenure clock.  I ended up getting an offer from the business school at Cornell, just a two-hour drive southeast from Rochester. The job opening for which I had applied was to teach public economics, which was consistent with my recent publications, but I wanted to make sure that any potential employer would not be surprised by my new peculiar research interests, so the paper I used for my job talk was an early draft of “An Economic Theory of Self-Control” (later published as Thaler and Shefrin, 1981). I figured any school that would hire me based on that paper would give me the freedom I would need going forward.  I stayed at the Cornell business school for 18 years, seeing it become the Johnson School of Business Administration, and it proved to be an ideal place for me to take the risks that would be necessary to try to pursue a new way of doing economics. I soon made friends in the psychology department (especially Tom Gilovich and Dennis Regan), hired some likeminded economists at Johnson (Bob Gibbons, Mike Waldman, and Bob Frank, recruited over from the economics department) and found some good students to work with. **3. Finance** My first graduate student was Werner De Bondt, whom I had recruited from the Cornell MBA program. I had gotten him interested in behavioral economics, but his specialty was finance, so he wanted to write his thesis on some topic in financial economics. I had picked up some knowledge of finance while at Rochester since it was a focus there, but had never had a course on the subject so Werner had to do the heavy lifting. We published a paper based on his thesis (De Bondt and Thaler, 1985).  The way that paper came to be published is an interesting story. My co-author on the self-control papers, Hersh Shefrin, had started dabbling in finance as well, and had been asked to organize a session at the American Finance Association annual meeting. At that time, one issue of the *Journal of Finance* each year consisted of a selection of papers that had been presented at the annual meeting. The current president of the Association, which that year happened to be Fischer Black, picked the papers. Black, of course, is the co-inventor of the Black-Scholes formula, and someone who would certainly have shared the Nobel Prize won by [Robert Merton](https://www.nobelprize.org/prizes/economic-sciences/1997/merton/facts/) and [Myron Scholes](https://www.nobelprize.org/prizes/economic-sciences/1997/scholes/facts/) had he still been alive. Fischer had eccentric tastes (his presidential address that year had the one-word title “Noise”) so he picked our paper (with the help of a nudge from Hersh), which meant it was published without the usual referee process. I am convinced that sped up the process of publication by at least two years.  The paper caused a bit of a stir in finance and my papers were still being largely ignored in other fields, so I was up for writing more. De Bondt and I collaborated on two more and then I wrote a series of papers a young colleague, Roni Michaely, and with a series of Cornell doctoral students including Charles Lee, Kent Womack, and Shlomo Benartzi. The paper with Charles Lee was joint with Andrei Shleifer.  Over time Benartzi became my most frequent co-author. There are two reasons for that. First, he is a very creative guy who comes up with a ton of interesting ideas. Second, he excels at getting me to work. Others have tried to learn his secret, but he holds his cards close to his chest. **4. Another year with Danny** After our year at Stanford, Amos and his wife Barbara took jobs in the Stanford psychology department and Danny and his wife Anne Treisman moved to the University of British Columbia in Vancouver. After six years we all had earned sabbatical leaves (and I had managed to get tenure). Because the year at Stanford had been so productive I naturally thought about trying to visit either Amos or Danny, but there was no real choice to make. Amos was planning to spend the year back in Jerusalem and my wife Dee vetoed a visit there, whereas Danny was planning to spend the year at home in Vancouver because Anne’s youngest daughter Deborah was in high school and did not want to leave. I asked Danny if he was up for having a visitor and he quickly agreed. We would have another year to spend thinking, talking, and taking long walks (when it wasn’t raining).  It turned out that when I arrived in late summer 1984, Danny had just begun collaborating with the economist Jack Knetsch, who also lived in Vancouver, teaching at Simon Fraser. Jack and I had more or less independently stumbled upon the discrepancy between buying and selling prices, what I called the “endowment effect”, and he had run a clever early experiment cleanly demonstrating the phenomenon. And though we would later return to that shared interest, the project they had just started was on a new topic: fairness.  Specifically, the question we were interested in was finding out what actions by firms people consider to be unfair. Another way to put it would be: when do firms make people angry? The technique we used was to devise scenarios of interest and then make use of some free telephone polling services Jack had procured to find out what (Canadian) people think. Coming up with interesting stories is a skill that is rarely in high demand in economic research, but for this paper it was crucial, so I had no trouble contributing to the team. We also had a lot of fun. And for the only time in my life, the referees liked the paper at least as much as we did, so it was accepted at the American Economic Review with barely a revision. (It helped that one of the referees was [George Akerlof](https://www.nobelprize.org/prizes/economic-sciences/2001/akerlof/facts/), another iconoclast.)  The three of us also began another project that returned to the interest Jack and I shared in the endowment effect. The goal of the experiment was to show that the endowment effect could survive a setup in which the subjects traded in markets for real money and had opportunities to learn. I ran an early version of our design when I got back to Cornell, and went over to the bookstore to look for some inexpensive object we could use in the experiment that students might like. Somehow a coffee mug with the Cornell insignia caught my eye and we used a set of those for subjects to trade. For some unknown reason, university coffee mugs have become a mainstay in the dozens of subsequent versions of this experiment. I had kept one of the original mugs as a memento so when I was asked to donate something to the Nobel Museum I had an easy choice. **5. Anomalies** Shortly after returning from Vancouver I was having dinner with Hal Varian, the economist who went on to become the chief economist at Google. Hal was telling me about a new journal the American Economic Association was launching called the *Journal* *of Economic Perspectives*. The first editors were going to be [Joe Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/) and Carl Shapiro. The idea for the journal was to publish readable articles that were aimed at a general audience of economists rather than specialists in a specific subfield such as international trade or corporate finance. Like any science, economics has become increasingly specialized and it can be hard for those in one area to understand the latest research in fields outside their own. In a brilliant move, the journal also hired Timothy Taylor, an economist who can write, as managing editor, a job he still holds nearly 30 years later. Tim makes every article that appears in the journal better.  Hal, who was a member of the editorial advisory board, told me that the journal wanted to include some recurring features, and they were looking for suggestions. At some point we came up with the idea for a regular column on “Anomalies” that I would write, recruiting co-authors as appropriate. Hal pitched the idea to Joe, who happily agreed. I would be committed to produce one column each issue, meaning four a year. The columns were about 12 journal pages so there was room to go into some detail. This was going to be a lot of work, and it was risky. More than one senior faculty member told me that these publications, since they would not appear in a “refereed” journal, would not “count”, whatever that means. I suppose if I did not already have tenure this might have dissuaded me, but fortunately that hurdle had been cleared and I loved the format. This was a chance to write articles without having to spend endless time trying to satisfy hostile referees. More important, I thought that if I did this right, I would have a unique platform to reach new readers, particularly young readers whose opinions were still malleable.  My goal was to cover a lot of different topics, each detailing empirical facts that appeared to be inconsistent with economic theory. The topics varied from seasonal patterns in the stock market, to betting odds at the racetrack, to industrial patterns in wages (why some industries pay receptionists more than others), to cooperation in games. The best part of writing these columns was that instead of referees I got intelligent, constructive comments from Carl, Joe and Tim. In the end it was a gamble that paid off. Lots of economists have told me that they first became aware of behavioral economics by reading these columns. **6. The Russell Sage Foundation** That year in Vancouver was made possible, in part, by Eric Wanner, whom I had met a couple years earlier. He would later become a longterm supporter (and constructive critic) of behavioral economics and a good friend. Eric, who had a Ph.D. in psychology, was working as a program officer at the Alfred P. Sloan Foundation and had gotten the idea that it might be smart to support research that tried to combine psychology and economics. He met with Amos and Danny at a conference to talk about the idea and Danny famously told him “well, it is not a project on which one could spend a lot of money honestly”. Danny, bless his heart, has a well-deserved reputation as a pessimist. But Amos and Danny did suggest that Eric talk to me, so I made the trip down to New York to meet him. Sometime later he made his first grant, which was to support my year in Vancouver. But Eric was just getting started.  Eric soon left to become the President of the smaller Russell Sage Foundation (RSF) and brought his interest in psychology and economics with him. RSF soon started a modest program in behavioral economics, which included hosting occasional conferences. I remember one meeting where we tried to interest psychologists in talking to economists and we attracted an all-star lineup including the intellectual giants Leon Festinger, Stanley Schachter, and Walter Mischel, along with Amos and Danny of course. But, although the meeting was thoroughly enjoyable, this ended up being a blind alley. Aside from Amos and Danny and a handful of other psychologists such as Eric Johnson, Drazen Prelec and Eldar Shafir, behavioral economics has become a field dominated by economists. One plausible explanation is the high entry barriers to psychologists who want to publish in economics journals. Another is that many people just find economics (and economists) boring.  Another conference that started as a one-off meeting turned into a surprising success. This was a meeting on behavioral finance that [Robert Shiller](https://www.nobelprize.org/prizes/economic-sciences/2013/shiller/facts/) and I organized. Bob and I did this for a few years at RSF but we outgrew their meeting space and shifted the conference to the auspices of NBER, where we had meetings twice a year. Bob and I organized this conference for nearly 30 years.3 Many of the most important contributions to behavioral finance were first presented at those meetings.  At some point the Russell Sage board created a group of key members of the behavioral economics community, gave us a small pot of money and told us to spend it in whatever way seemed best. This sort of laissez-faire grant making from foundations is highly unusual, which is too bad because it led to two highly successful programs. One designated an amount of money that would be given out in small grants to graduate students and new faculty members. Typical grants were just a few thousand dollars and many led to good publications. The other idea was more ambitious, and it came from Danny. We started a two-week long bi-annual summer institute for graduate students, known to all as the Russell Sage summer camp. Colin Camerer, Danny and I organized the first one in 1994 in Berkeley, and with no track record we managed to attract an amazing group of students. Those included Linda Babcock, Christine Jolls, David Laibson, Sendhil Mullainathan, Terry Odean, and many more. We also had Matthew Rabin, a young assistant professor, serving as a sort of counselor in training. The faculty was not bad either. It included (according to my memory) George Akerlof, [Ken Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), [Vernon Smith](https://www.nobelprize.org/prizes/economic-sciences/2002/smith/facts/), Richard Zeckhauser and the psychologist Lee Ross. The camp continues running every other year to this day, usually around the time of the soccer World Cup. For a period, Colin Camerer and George Loewenstein were the organizers but for more than a decade David Laibson and Matthew Rabin have organized it. I have yet to miss one. There is a tradition of making t-shirts for each of the summer camps. That early morning in October when I got a 4 am wake-up call from Stockholm I was told to get some coffee and get ready for a telephone press conference in 45 minutes. I showered, and put on one of those old camp t-shirts for the press conference.  Russell Sage also hosts a visiting scholar program. In 1991-92 it had been another seven years since Vancouver and Danny and I applied to be visiting scholars at RSF. We were joined by Colin Camerer and Amos came for periodic visits. Danny and I tried to recreate the magic from our previous stints, but we were unable to pull it off. It didn’t help that during the year his Berkeley home had burned to the ground in a massive fire and I was going through a divorce, but I think mostly we had both gotten too busy to work seven days a week on one thing. Nevertheless, in various combinations, Colin, Danny and I wrote three papers that year on what we called “narrow framing”.4 We also managed to have a lot of fun. **7. Chicago** During my year in New York I began dating a marketing professor at MIT named France Leclerc. The following year I spent a semester visiting MIT, then she came to Cornell for a year, then I went back to MIT for a year. Apparently someone at the University of Chicago noticed this “co-movement” and we both received offers to join what is now called the Booth School of Business. We married the following year. Since then France has changed careers and has become a photographer seeking out and capturing images of ancient cultures while it is still possible to find them.  The University of Chicago may be the most intense intellectual environment in the world. I had been hired to help build a group of behavioral scientists that would include cognitive and social psychologists and behavioral economists. In the years that I have been there I have learned a lot of psychology from that group of colleagues. But for me the best part of being a faculty member at Booth is that it is so easy to cross disciplinary borders. I continued writing finance papers but now with new Booth collaborators such as Nick Barberis and Owen Lamont, and I could get the efficient markets take on any idea just by wandering down the hall to Gene Fama’s office, or better yet, joining him for a round of golf.  Oddly, my most important collaboration at the University of Chicago was with a law professor who was the first person to reach out to me when he heard I was coming: the incomparable Cass Sunstein. We were colleagues at Chicago from 1995, when I arrived, to 2008 when he went off to work for President Obama and then later to join Harvard Law School. Our first collaboration was on a paper with former summer camper Christine Jolls that was titled “A Behavioral Approach to Law and Economics” (Jolls, Sunstein and Thaler, 1998). That paper was at least as controversial at the Chicago Law School as my papers with De Bondt had been over in the finance department at Booth.  The University of Chicago was the spiritual home of law and economics with scholars such as [Ronald Coase](https://www.nobelprize.org/prizes/economic-sciences/1991/coase/facts/), Richard Epstein and the formidable Richard Posner. Posner, a prodigious scholar, for many years did double duty as a Justice on the U.S. Court of Appeals, the court one step below the Supreme Court, and as a “part-time” professor who wrote at least one book a year. The seminar where we presented our paper was as raucous as any I have ever attended. Many in the audience seemed to think that using coffee mugs to test the Coase theorem was tantamount to heresy. Later Cass and I would go on to write the book *Nudge*. Writing that book changed our lives and, somewhat miraculously, caught the fancy of both academics and policy makers around the world. As I write whenever anyone asks me to sign a copy of the book, I hope they all use what we wrote to “nudge for good”. **8. Friends and collaborators** I have been a very lucky man. The academic life can be solitary, especially when many of your colleagues think you are nuts. I have been fortunate to have had a fabulous set of collaborators, all of whom became friends. Without supportive friends in graduate school, especially Dipankar Dasgupta and Robin Mukerjee, I would not have survived the first year. Without Sherwin I would not have written that thesis. Then there was Hersh Shefrin, my fist behavioral co-author on the self-control work, and Tom Russell, another friendly voice at Rochester until they both abandoned me for sunny California. Of course, none of this would have happened without Amos and Danny. We lost Amos all too soon in 1996 at the age of 59. Danny and I remain dear friends.  I am admittedly a rabble rouser who enjoys stirring the pot and challenging conventional wisdom. But taking on that role brings an emotional burden: it is not easy having economists you admire dismiss your research because it does not follow existing norms. Throughout my life, my friends, family, co-authors, and colleagues have given me more than just ideas and feedback; they have sustained my courage and moderated the volatility of my ego.  To everyone who has helped over the years, I can only say thank you. I wish you could have all joined us in Stockholm. And as Danny always says when we finally have to stop talking: “to be continued”. **1. To mitigate liquidity constraints in some versions I offered people the option to finance their payment in the willingness-to-pay condition with a thirty-year loan at zero interest.****2. Sherwin and I remained friends and were colleagues for a while at Chicago some years later, but he never became a fan of behavioral economics. One day I teased him that if he wasn’t nice to me I would write a book and say that he had taught me everything I knew.****3. You can find the programs on his website:** [**http://www.econ.yale.edu/~shiller/behfin/index.htm**](http://www.econ.yale.edu/~shiller/behfin/index.htm)**.****4. Linda Babcock, Colin, George Loewenstein and I wrote a paper on the behavior of New York City taxi drivers (Camerer et al, 1997), Danny and Dan Lovallo (Kahneman and Lovallo, 1993) wrote one on managerial decision making, and Benartzi and I wrote about the equity premium puzzle (Benartzi and Thaler, 1995)… The same theme was the basis for the only paper Amos, Danny and I ever wrote together (Thaler, Tversky, Kahneman and Schwartz, 1997) …****References** De Bondt, Werner F. M. and Richard H. 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| Autobiographical |  |
| Podcast | Nudges, sludges and the connection between stubbornness and success – listen to a conversation with Richard Thaler, conducted in February 2020. His work has helped us to understand how people make choices in the real world and has also given us tools to nudge people towards better decisions.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0817=RT  [Richard H. Thaler]: Hello  [Adam Smith]: Hi, this is Adam Smith, calling from Nobelprize.org, the website of the Nobel Prize in Stockholm.  [RT]: Yes.  [AS]: Well first of all, congratulations on the award of the Prize in Economic Sciences.  [RT]: Thank you.  [AS]: May I ask how the news reached you?  [RT]: It woke me up.  [AS]: [Laughs]. Nice way to be woken.  [RT]: Yeah. It’s good to see Sweden on your cell phone.  [AS]: Of course. People will perhaps best know you for your book *Nudge*, published almost a decade ago. For those who don’t know could you describe what a nudge is?  [RT]: A nudge is some feature of the environment that changes the behaviour of humans but would not change the behaviour of rational economic agents, what we call Econs. So, for example, the research I was talking about in Stockholm a couple of weeks ago was about two nudges in the Swedish pension system, one was creating default funds that people would take if they didn’t make a choice, and then the other was an advertising campaign encouraging people to not to take the default. The paper that we’re now writing is sort of a battle of those two nudges.  [AS]: Right, yes, What’s your favourite example of a successful nudge?  [RT]: Well you know, I would say probably the most successful has been the use of what we call automatic enrolment in pension plans. Meaning the default is to join rather than not to join.  [AS]: Right.  [RT]: For example this has been used in a recent roll out of the national pension saving plan in the UK, and the enrolment rates are well over 90%.  [AS]: It’s 15 years since your friend Daniel Kahneman was awarded the Prize. I suppose since then what we’ve seen is an absolute blossoming of the field of behavioural economics. Has it reached a point do you think where it can be used for making tools for setting public policy?  [RT]: Well sure. I mean that’s what somebody asked me to come over to Stockholm to talk about three weeks ago. And I think our research has greatly changed pension systems all around the world. The idea of Save More Tomorrow where you invite people to commit themselves to saving move sometime in the future has been quite successful. We think there may be as many as 25 million people in the US involved in that programme. Countries all around the world, starting with the UK, have started behavioural insight teams, often referred to as nudge units. And they seem to be doing lots of good.  [AS]: Just a last thing, we all like to think we’re different don’t we? But somehow your work brings us all into a unifying theory. Is there a kind of disparity there between people’s individual belief in their own individuality and …  [RT]: Oh sure. People are different. The key finding from Kahneman and Tversky’s research is not that everybody is the same but that on average we tend to err in the same direction. So we all think that we’re going to finish projects sooner that we will. Although some people procrastinate more than others.  [AS]: Yeah. I think that describes me.  [RT]: Yeah. And my home contractor at the moment.  [AS]: [Laughs]. You sound very calm, how do you feel?  [RT]: Uhh. Well, not calm.  [AS]: Master of understatement. Will we look forward to welcoming you to Stockholm in December?  [RT]: Uh yes. I had the pleasure of coming with Prof. Kahneman and he keeps telling me I better win it soon because he wants to go back. So it’ll be a pleasure to ask him to join me again.  [AS]: Oh that’s gorgeous. And you will reverse roles …  [RT]: Yes.  [AS]: … See it from different perspective. OK, well, we very much look forward to having you here. Once again, congratulations.  [RT]: Thank you.  [AS]: And thank you for speaking to me. Bye bye. |
| Interview |  |
| Q7 | Did you ever think that you would get the call from Stockholm? |
|  | You are not really allowed to think about it, but one can always hope. A friend of mine told me that he had won a bet. In London I was going at 20 to 1 odds, so that suggests it could have happened. |
| Q6 | What does this prize mean to you? |
|  | I think many Nobel Prize winners, especially in economics, were sort of stars the moment they arrived on the scene and might have been counting the days until this call came. That is not really the career I had. I was not a great student. My thesis advisor famously said that when interviewed about me, of my time in graduate school, that: “We did not expect much of him”. And you know the research that I won the prize for has been quite controversial from the beginning. I would say it’s becoming less controversial recently, so that is quite gratifying. |
| Q6 | What are you most looking forward to during Nobel Week? |
|  | I am having a dinner the night before the big day with all my friends. It is a little bigger that it might have been because there are a few young economists who somehow got invited so our dinner will 23. |
| Q18 | How does it feel to be called the ”father of behavioural economics”? |
|  | It is very gratifying. You know as most ideas there are, there’s more that one father. Adam Smith was a behavioural economist so he was there before me. As I argued in my recent book *Misbehaving*, economics was behavioural until about 1940. So Keynes would now be considered a behavioural economist. And then when economics started to become formalized and after World War two people like [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/), and [Ken Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), and [Bob Solow](https://www.nobelprize.org/prizes/economic-sciences/1987/solow/facts/) … Not necessarily those people *per se* but the movement they started of writing down mathematical models. The easiest models to write down are ones of rational behaviour because you are just solving for the right answer. And then you are assuming that the people you are studying also get that answer. If you think about that, you realize that there is a bit of a flaw there, so you can have an economist work for three months to solve some very difficult problem and then just right assume that people maximize this formula that it took the world’s leading authority three months to solve. It might be that others would not solve it the same way. |
| Q64 | How do you tackle resistance? |
|  | I always felt that the answers to these debates has to be data. You can’t win arguments and you also can’t really change anybody’s mind. So for people who had made their minds up – so be it. I concentrated on young people and the young people that found these ideas exciting are really what has made the field thrive the way it has the last couple of decades. |
| Q1 | How did your interest in behavioural economics start? |
|  | My interest started before I knew anything about psychology. I just noticed when I was in graduate school that people would do things that seemed inconsistent with the theories that I was learning. There is a story I tell about a friend of mine and I were given tickets to a professional basketball game about an hour’s drive away in normal times and then there was a big snow storm and my friend said: “There is no way we are driving in this snow, but if we had paid for those tickets then we would be going”. This is what economists called the sunk cost fallacy. The idea is that, suppose we had paid full price for those tickets whatever that might have been at that time, let’s say we were out 50 bucks each, that money is gone, there is no way to get it back. It’s sunk and you can read in any economic principles book that you should ignore sunk costs and that you should and that people do. And what I was seeing in stories like that was, people don’t. And in fact we have to teach people when they take economics for the first time, no, that is a sunk cost, you should not pay attention to that. So I started a list of funny things people do and that’s all it was for two or three years.  Then I met two psychologists in 1975 I think this was, who introduced me to the work of [Daniel Kahneman](https://www.nobelprize.org/prizes/economic-sciences/2002/kahneman/facts/) and Amos Tversky, psychologists who were then teaching at Hebrew University in Jerusalem. I had told that this psychologist’s name is Bob Fischhoff who had been a student of theirs and I told them about what I had been thinking about and he said: “You know you might be interested in the research that my thesis advisors have done”. And so when I got home I went to the library as one did in the 70s. There were actually books and journals, dusty ones. I had to go search for the psychology section of the library which was a whole new thing for me, and I started reading these papers and getting very excited and it is not that their research had anything to do with economics. Later they wrote a paper called *Prospect theory* that was aimed at economists but this early work in the early 70s had nothing to do with economics, but it had one idea, one central idea which was not only do people make mistakes, but that the mistakes they make are predictable. The reason why that was important was up until that time when I would bring these kinds of things up, economists would say: “Look, we know people make mistakes but those will wash out and we can handle mistakes”. But if the mistakes are all in the same direction or mostly in the same direction then it is a bigger problem for economics.  So I began to rethink some of the things on my blackboard. For example, one of the things I was interested in was the not very remarkable idea that people have self-control problems. To anyone but an economist this does not seem to be a deep observation. It is in the Bible, so it has been around for a long time. There is the story of Odysseus tying himself to the mast so we are up to ancient greeks. This is not a new idea. Thaler discovers self-control problems! Everybody knows that but there is an assumption in economics that comes with the fancy term consumer sovereignty. What consumer sovereignty means is that no one can know better than you what’s best for you. There are two parts to that. One is you are capable of making rational decisions and two, that you know your tastes. And then maybe there is the third one which is that you will then act on those preferences. We know say 20% of the population is obese, many people are smokers. If you ask them: “Would you rather not be a smoker?” they would say: “Yes I have tried to quit 20 times”, so people do have self-control problems and what is this matter for economics? One of the problems people have to face is saving for retirement and that is a problem that is a cognitively very difficult. Figuring out somebody your age trying to think how much they are going to make over their lifetime and then how long they are going to live and what kinds of rates of return they are going to get: that is hard! And then even if you could solve that, there is that very cute sports car that is begging you to buy it and you know it is not really consistent with this long run plan, but you are only young once, so these self-control problems are important to understand. |
| Q8 | How long have you known your fellow laureate Daniel Kahneman? |
|  | After meeting my friend Fischhoff who told me about their work I heard they were going to – they being Kahneman and Tversky – were going to spend a year at Stanford 1977-78, so it is exactly 40 years. I decided I was going to somehow figure out a way to go spend a year at Stanford too. So I went sometime in the spring for a few days and went around to various people and begged and pleaded, and finally a guy called Victor Fuchs took pity on me and set me up with the fellowship so I ended up spending that year there in an office very nearby to where Danny Kahneman was sitting and got to know both of them. Then we all took new jobs at the end of that year. Amos did at Stanford, Danny went to University of British Columbia, I went to Cornell and that meant we were all on the same sabbatical schedule.  Seven years later I had a sabbatic coming up and I had had this great year visiting Amos and Danny and decided I would try to do the same thing. Danny was staying in Vancouver for his sabbatic. His wife Anne Treisman’s daughter was going to still be in high school so they were sticking around for her. So I decided: “Danny, would you like a visitor for a year?” and I went and spent another year with him and we wrote two very important papers in this field that year. It probably helps that I am an optimist because Danny is a pessimist. He claims to be a rational pessimist. So you know, in one of their theorises is that losses hurt more than gains, so he says if he expects the worst then he won’t experience any negative surprises. But being a pessimist is a really dumb idea. I have been trying to convince him of that unsuccessfully for forty years. But I think I served the job of cheering him up and telling him maybe things aren’t as bad as all that. This is getting to be more and more difficult in the times we are currently living in. But somehow miraculously we have stayed friends all these years. We have had a third year, we spent together seven years after that in New York City and now we still talk very frequently. |
| Q18 | What is the greatest impact of your work? |
|  | At an abstract level I think the work has given economists permission to think about models that are not perfectly rational and to say this is a legitimate exercise. Creating that legitimacy required doing research in a wide variety of areas. I started studying financial markets and the reason is that economists were certain that you could not find any evidence of what I call misbehaviour in financial markets because the stakes are so high and you have all these professional traders. A friend of mine … you know the Frank Sinatra song *New York, New York*: “If you can make it there you can make it anywhere”, and he used to refer to my efforts in finances, the New York, New York theory, that if I could find anomalies in financial markets then that would carry the day. So 1985 I had a student who wanted to do research in finance and we wrote a paper together called *Does the stock market overreact?*. The answer is yes, and that paper caused a big stir in financial economics which encouraged me to write more of those papers and since then I have done research on anything from game shows to American football to laboratory experiments with people playing games and then a large part of my work has been in studying saving behaviour and how to help people save more.  Pensions, first of all pensions are new for human beings. For most of the millions of years we have been on earth saving for our retirement was not anything to worry about because you were going to die first and if you manage to live then you would move in with your kids. So pensions are kind of a 20th century invention. And the first ones were these so-called defined benefit pension schemes, that you worked at your company and then they would give you an annuity when you retired that was based just on what you made and how long you worked. No decisions to make. And then companies realized that those pensions were very expensive and started switching over to the so called defined contribution plans where companies and workers both contribute to a pot of money and then it is up to the worker how much to save, how to invest it, then how to draw it down and that is a really hard problem. I have worked with co-authors especially another of my students Shlomo Benartzi. We have worked on trying to make that problem easier for people. I have been working with companies and countries in devising retirement savings plans that are easy for humans.  One of the first innovations was something called automatic enrolment. It used to be in order to join the plan you would have to fill out a big pile of forms. People do not like filling out forms so many people would just fail to join even if their company or their government was giving a match. So we had the idea of making enrolment automatic, so changing the default, so when you first became eligible for a pensions plan you would get a pile of forms and instead of having to fill them all out the first page would say: “If you do not fill out all these forms we are going to enrol you at this saving rate and in this fund”. That’s become very popular and as you no doubt know was used in a recent pension reform in the UK. Essentially a national defined contribution retirement saving plan and it employed automatic enrolment and signups have been over 90%. So there was great scepticism. Lord Adair Turner was responsible for this reform and he decided to go with this idea and people did not think it would work but it worked. Then the second idea Benartzi and I had is what we call Save More Tomorrow. And the idea there is that we all have more self-control in the future. We are planning diets after New Year’s, so our idea was to give people the opportunity to commit themselves to increase their saving sometime in the future like. Like when they get the next raise and we linked it to raises because of loss aversion so they would not see their pay go down just it would go up a little less. And then there is a lot of inertia in these plans so once you joined this Save More Tomorrow plan it would keep ratcheting up until you hit your goal and it took us years to get somebody to try this, but finally found one company in Chicago to try it and we tripled saving rates in three years and then wrote a paper about it and now in the US among large companies the vast majority use automatic enrolment and automatic escalation, which is the generic term for Save More Tomorrow. |
| Q65 | Can you describe the idea behind a ”nudge”? |
|  | The idea of a nudge is that you can point somebody in a helpful direction without forcing them to do anything. So that is the definition of a nudge. It is some feature of the environment that improves decisions but doesn’t force anybody to do anything. One of my favourite examples, one that has saved my life on numerous occasions, are those helpful signs “Look right” when you are crossing the street in London. You know, you drive on the wrong side of the road there. I wish you would do something about that. But given that you are not, it helps to warn people that those double decker buses come the wrong way. Oh yes, it is coming from over here! So that is a nudge! People get confused about this. They think that nudging is kind of a nanny state that the government is trying to get people to do what the government wants. You know the “Look right” sign, yes. the government would rather you did not kill yourself and it will cause a bit of a mess, but you do not want to get hit by that bus. And we are very clear that the goal of nudging has helped, helping people achieve their own goals. I’d like to think of GPS as a good analogy. I have the worst sense of direction of anybody. If I am not with my wife I am certain to get lost. But now I have Google maps on my phone and even wandering around the West End of London I do not get lost too much. The thing about a GPS system is you pick the destination. I want to go to the British Museum, I plug that in and it says “Turn right” and furthermore the instructions are not mandatory. If I see something interesting over to the left I can go to the left. The Apple recompute that, that’s our goal. To be the GPS for life and we do not want to tell people where they should go. |
| Q66 | You have joked that you’ll spend the prize money as ”irrationally as possible”. |
|  | I should say that I made this comment at 4.45 am. Well, I do not have the money yet so I have not lived up to that promise. I do intend to throw a good party Saturday night and then try to spend it to make as many people happy as possible. |
| ID | 0818 |
| Biographical | I was born in London in 1948. My parents were both doctors. My mother was a gynecologist at a time when women doctors in the U.K. were relatively uncommon; she was a German-born Jew, who had left Germany in 1933 just after Hitler came to power. My father was an epidemiologist of some distinction whose particular interest was TB. He also was one of the key players in the 1948 Streptomycin trial, which put randomized control trials on the map. He came from a long-standing Anglo-Jewish family. We lived in comfortable but by no means luxurious middle class circumstances in the Hampstead area. I was an only child. My parents had had a son about a year before who died a few hours after being born, and so my arrival was particularly welcome. Perhaps because of this my parents were quite protective of me. Since my mother worked, a caregiver, called Mrs. Shealey, helped out during my childhood. My father was 48 when I was born – very old for a father at the time – but he lived to 106. My mother died at 93. I had a close and loving relationship with both of them.  My first regular school was a progressive one. My parents were left-wing and they thought that a non-traditional school might be a good option. At that time children in England took the eleven-plus, a forbidding test that sorted people out into “academic” and “non-academic.” My memory is that when I was eight, only one person in the class of eleven year-olds in my school passed the exam. Given that they mostly came from backgrounds like mine, my parents panicked and decided to go in absolutely the opposite direction: they entered me for admission to a “public” (i.e., elite, private, expensive) school in our neighborhood, called University College School. Part of the admission process, I recall, was a personal interview with the head-master of the junior school. He asked me to solve a long-division problem and when I showed him my answer he said there was a mistake. I disagreed and turned out to be right. Soon after my parents learned that I had been admitted, and I entered the school just before my ninth birthday.  I take a few things away from the admission incident. I already had some intellectual self-confidence and was stubborn (even though I was actually quite shy). I had learned something after all at my progressive school. I was reasonably good at mathematics.  U.C.S., as it was known, provided me with a very good education. The quality of the teaching was high, the school was intellectually serious, and many of my peers were bright. I liked mathematics and there were plenty of opportunities to advance in this area. I moved to the senior School at 13, and shortly after won a scholarship, so that my parents did not have to pay any more. At age 15, having passed several (nationally administered) O-level exams (including Latin and Greek!), I specialized for my remaining time in mathematics, physics and chemistry, leaving the school at 17. I continued to like mathematics best, and had some minor interest in science. I spent as little time as possible on history or literature (which was very easy to do after age 15).  As I look back, I have generally good feelings about U.C.S., but also some reservations. It was all-boys, which I do not think was good for me. I could have benefited from feeling comfortable with girls earlier in my life. Also, although by English public school standards the school was quite liberal, one still had to call one’s (predominately male) teachers “sir,” and there was always the threat of corporal punishment although this was rare. I managed to avoid it. Interestingly the headmaster of the senior school did not like to carry out corporal punishment and so it was left to the aptly-named Vice Master to administer a beating or two when the headmaster was away (the Vice Master was actually one of the most intellectual teachers in the school and I got on quite well with him).  At age 17 I was admitted to King’s College Cambridge. I had hoped to win some sort of scholarship, which although not worth much was prestigious and guaranteed that one’s name was memorialized on a wall at my school. But that did not happen. The senior tutor at King’s at the time thought that I would be better off not continuing with mathematics – that was for “Rolls-Royces,” I recall him saying – but, stubborn as ever, I ignored his advice and spent the next three years studying for a mathematics degree, which given the Cambridge system meant doing nothing but mathematics. I was good enough to receive a middling degree in 1969 – a class 2. I recall that on the day the results were announced – they were posted on the wall of the Senate House (and later printed in the London *Times* – things were very public in those days), I decided to sleep in, but one of my fellow mathematicians went to look at them and by arrangement came to my room to report. He told me that I’d got “what was expected” (he received a first). I remember thinking both that he was exactly correct and that I wouldn’t have put it that way.  King’s was an exciting place to be in the late 1960s. The students were a gifted group. Among the people I knew well in my class, either then or later, were Mervyn King (later to be Governor of the Bank of England), Martyn Poliakoff (a cousin of mine, and a distinguished chemist and foreign secretary of the Royal Society), Ben Friedman (a colleague now in the Harvard economics department), and Tony Judt (the historian, who sadly is no longer with us). Unfortunately, King’s was all male at the time, which postponed my feeling comfortable with women even further.  The worst thing about my education both at school and later at Cambridge is that I never learned to write. Since others did, this must have had a lot to do with me. But I think that I could have benefited from less specialization in mathematics and sciences early on. Perhaps some of that time spent solving differential equations in fluid dynamics and mechanical engineering could have been devoted instead to literature or history. Still I have to say that the grounding in mathematics has served me very well in my career as an economic theorist.  I graduated at a time of student rebellion and the idea of a job did not seem attractive. The answer was, of course, further study. But in what? People told me that mathematics was being used in economics, and I had a second reason for choosing that field. I was quite left-wing at the time and liked to argue about politics. But I found that at some point my fellow-debaters brought in some consideration like the balance of payments and at this point I lost the argument. Clearly I had to learn something about this field.  So study in economics it was, and I applied and was admitted to a master’s program at Warwick University. The M.A. was a one-year course, but since I knew no economics the plan was that I would spend the first year catching up on basics and the second year doing the real thing. Warwick was a new and relatively small university at the time, which meant that the classes were quite intimate compared with large Cambridge lectures (although not with Cambridge supervisions). I felt immediately a vitality about economics that I had not felt about mathematics. Mathematics at Cambridge had been taught in a very sterile way: Theorem, proof, theorem, proof, lemma, proof, etc. Nothing was ever said about when anything had happened or who had done it. Was that result in Group theory proved three hundred years ago or last week? I had no idea.  Economics was refreshingly different. Since it was still a relatively new field, one felt that the frontiers were not that far away (this is less true nearly fifty years later). Names and dates were thrown around all the time. I liked this and felt an immediate affi nity with the subject. I learned macroeconomics from Dick Sargent, international economics from John Williamson, and mathematical economics from Richard Clarke (who sadly died young). Some of the mathematics that had seemed very dry seemed much less so when I saw how it could tie in with the world.  I owe Warwick a great deal because it gave me the start in my new life. However, much as I liked the economics department, I was quite lonely. There weren’t too many graduate students and many of my peers were married and showed up to class and then went home. I think that things are very different now.  In my second year I started to think about what to do next. John Williamson, who had a Ph.D. from Princeton, encouraged me to consider graduate work in the U.S. I had worked in New York for a few weeks in the summer of 1970 and toured some of the country with a friend by Greyhound bus, and the idea of spending more time there was appealing. So I decided to apply. In those days I was not very well organized and by the time I put in my applications it was close to the deadline, and, by bad luck, a postal/mail strike in the U.K. had just started. In those days there was not much alternative to regular mail and so my applications were destined to languish for weeks.  But then John Williamson made a wonderful suggestion. He was going to Princeton for a few days, and he offered to take my application by hand. A few weeks later I heard – the postal strike was now over – that I had been admitted to Princeton and offered a decent financial package. I also received responses to my snail (almost literally)-mail applications. I think that I was admitted by Penn but I was rejected by MIT, Harvard, and Yale.  At the time I put my MIT, Harvard, and Yale rejections down to the fact that my applications had arrived late. Having been on the other end of the process I now know that deadlines are often waived for strong candidates. My record, good at Warwick but less good at Cambridge, was probably not attractive enough for the top places. (Princeton was less competitive then than it is now.)  So in September 1971 I arrived in Princeton. I remember immediately liking the place. Yes, it was mock Oxbridge, but it was also beautiful. And I liked the steamy, hot weather. It made such a change from the almost non-existent summer in England (global warming has changed things a bit). As soon as classes started I also realized that the Princeton canvas was bigger than what I had experienced before in economics. There were many more fields covered, the faculty was larger and more diverse. It was exciting.  Perhaps the biggest difference between Princeton and anywhere I had been before was the Graduate College. Built like an Oxford or Cambridge college, and located a mile or so from the main campus, this was where many of the graduate students lived and, amazingly, there were both men and women. So Princeton came with a rather pleasant social life. One met one’s fellow students from many fields over dinner, which was quite good particularly compared with British student fare of the time. There was a certain formality – gowns had to be worn – but the atmosphere was anything but.  One of my fellow graduate students was Rita Goldberg, who was studying comparative literature (and who later successfully got the college to abandon gowns!). We became a couple in the spring of 1972, married in 1974, and have been together ever since. We now have two sons, Daniel and Benjamin, two grandsons, Gabriel and Jamie, and a daughter-in-law, Ellen. So I have very fond memories of the Graduate College and of Princeton!  I made many friends at Princeton, some of whom I still see or am in touch with. Although the department was not as strong as today, there were some very good students and faculty. I found that my background in mathematics and my exposure to mathematical economics at Warwick helped me to navigate the program and I was able to finish all my exams after one year in spring 1972. Early on I linked up with one of the professors, Dwight Jaffee. He was looking for a research assistant and I volunteered. We ended up writing a paper together on financial intermediation – my first publication. In the fall of 1972 Michael Rothschild arrived from Harvard as a professor. This was a hugely important event for me. Before Mike’s arrival I was not really aware that there was an area called economic theory that was distinct from mathematical economics. This was the time when work in asymmetric information was at its peak and Mike was of course a central figure. For me it was an eye-opener to see the same degree of rigor that I was familiar with from general equilibrium theory (which I had learned from Richard Cornwall) being applied to “small” models.  But I did not at the time pursue the small model path. I had become interested in the question of the objective function of a firm. What is the generalization of profit maximization in a world of uncertainty? This led me to the theory of general equilibrium with incomplete markets and I became aware of an important paper by Peter Diamond on the efficiency of a stock market economy. At some point I realized that Diamond’s results did not generalize once one moved beyond the two-period, one-good economy that he had considered. I think that this must have been in the early fall of 1973. From one day to the next I had (the main part of) my thesis. Of course, it took some time to work out the details and write it all up, but I was on my way as a researcher. I received my Ph.D. in 1974.  I have jumped over an important episode. Mike Rothschild was a very supportive advisor and, given that there were few theorists or mathematical economists at Princeton, he thought that I would benefit from attending a sixweek (or so) summer workshop in mathematical economics at the University of Massachusetts organized by Hugo Sonnenschein. Hugo was kind enough to invite me and in the summer of 1973, Rita and I moved up to Amherst. It was the time of the Watergate hearings and the two of us watched the proceedings on TV in the evenings after I had attended the conference sessions during the day. This was the first conference that I had been to as a nascent researcher and the experience was wonderful. Many interesting papers were presented by many interesting people, and I lapped it all up. Hugo and his wife Beth were wonderful hosts and have remained life-long friends. Rita and I also met Andreu Mas-Colell there (and his wife Esther a year later), and Andreu and Esther have remained life-long friends too.  I even presented a paper at the summer workshop. It was some joint work with Harold Kuhn on a proof of the existence of equilibrium without the free disposal assumption (later published in the *Journal of Mathematical Economics*). Harold had a joint appointment in the mathematics and economics departments at Princeton, and was another important influence on me. He was a mesmerizing teacher and it was exciting to work with him even though what we produced had no great significance.  In the fall of 1973 I started looking for a job. My parents had been pushing me to return to England and Rita, who was still working on her Ph.D. thesis, and I decided to try it out. U.K. universities did not have a presence in the U.S. job market in those days, and so candidates had to apply individually for positions. Michael Rothschild knew Tony Atkinson, a professor at Essex University, well, and I applied and was offered a job there. So in September 1974 Rita and I decamped to Wivenhoe and I started teaching at Essex in October. The department at Essex was quite good – as well as Tony, Christopher Bliss, Ken Burdett and Peter Phillips were colleagues (so was Peter Hammond, but he was away that year) – but morale was not high. This was partly because U.K. universities were going through bad times (these only got worse over the subsequent ten years), but also because Essex had a reputation dating back to the 1960s as a bastion of left-wing student activity, which did not put it in good stead with the funding authorities. I vividly remember during my year there that someone came in to my office and removed one of the bulbs in my ceiling lamp as an economy measure. (Sometimes I feel that this was a dream but I don’t think it was.)  In the summer of 1974, I had met Frank Hahn at a summer workshop at Stanford, and he wrote to me and said that there was an assistant lecturer position at Cambridge and would I like to apply. Given the uncertain situation at Essex I decided to do so, and got the job. Rita and I moved to Cambridge in September 1975 and that was our home for the next nine years or so. I also became a Fellow of Churchill College.  Returning to Cambridge was interesting, exciting and enjoyable in many ways. First, I was going back in a different field and to a different college compared with my undergraduate days. Second, given its status and the college system, Cambridge was better able to withstand the poor economic climate than most other U.K. universities. Third, although the faculty was divided into different economic camps (neoclassical, Marxist, neo-Ricardian, etc.), Frank Hahn had managed to assemble a small, but superb, group of theorists around him, including David Newbery and Roger Witcomb as regular teaching staff, and Douglas Gale, Eric Maskin, David Kreps, Louis Makowski and Mark Machina as research fellows or visitors. So there was constant discussion of ideas and the intellectual environment was extremely stimulating. The period 1975–1981, when I taught at Cambridge first as an assistant lecturer and then as a lecturer, was one of the most exciting of my life.  I have mentioned that when I graduated from Cambridge in 1969 my writing skills were non-existent. I had to learn to write when I started to do economic research, particularly at Princeton, and it was a painful experience. Rita, with her knowledge of literature, was incredibly helpful. I bothered her endlessly about comma placement and the like. Semi-colons were a revelation. But gradually I started to make some progress.  I do not know whether my lack of writing experience can explain one very unpleasant incident that occurred in my early years. I had written a paper on portfolio theory – not part of my thesis – when I was at Princeton and I submitted it for publication in the *Review of Economic Studies*. They accepted it with alacrity. I was not very good at titles. The first title was very long and I decided to shorten it before submitting the final version. The galley proofs arrived with the shortened title and I returned them. At some point I showed the paper to a senior economist, and he said that he liked everything but the title! I realized that in shortening the title I had made it misleading. Was it too late to change it? I phoned the production editor, who said, it’s not too late, just send me a letter with the new title. I remember feeling some concern, but thought that the production editor must know what he was doing, and went ahead. In October 1975 the journal landed in my letter-box and I discovered with horror that one word of the new title had been printed incorrectly (in every place in the journal). Interestingly, the replacement word did not change the meaning of the title but made it incredibly clumsy.  I recount this at some length because the experience was extremely humiliating. A clumsy title does not matter to others but it does matter to the author, particularly one who is feeling his way on how to write. The unpleasantness has remained with me for years.  One way to think about this is that the problem arose because the contract between journals and authors is incomplete. I wish that I could say that this prompted my interest in incomplete contracts, but I fear that this is not the case. Turning to more pleasant things, a major intellectual event in my life occurred in 1976 when I attended the IMSSS summer workshop at Stanford (for the second time). Mordecai Kurz, who ran the workshop, decided that it would be good to have a session on finance, and designated Sanford Grossman and me to run it. I had met Sandy briefly in 1975, and knew that he was a *wunderkind*, but that was our only contact up to that point. We started working together for the session. I still remember when I first learned from Sandy, who was already well known for his work on price informativeness, that he was almost five years younger than I was. There followed what seemed a long silence. I was literally speechless. Fortunately, I was able to recover and we worked together for the next twelve years or so.  Sandy and I worked on many topics, including the objectives of firms, takeovers, capital structure, and the principal-agent problem before embarking on work on incomplete contacts, the topic for which I have been awarded the prize.  Sandy was intellectually mature beyond his years and I learned a great deal from him, particularly about the Chicago approach to economics. Sandy is one of the most brilliant people I have ever met. At some point he decided that he wanted to apply his economic ideas in the financial world and he has been extremely successful at that, but I feel that his departure was a great loss for economics.  Promotion in the Cambridge economics faculty was almost impossible in the late 1970s and early 80s, partly because the faculty could not agree on anything. At some point it was time to move on and I was offered and accepted a professorship at the London School of Economics. I started teaching there in January 1982 but continued to live in Cambridge. Commuting was not easy. As I started my new job there was a national rail strike.  It felt good to have a senior position and L.S.E. was a stimulating place to be. Among my colleagues were Tony Atkinson, Partha Dasgupta, [Christopher Pissarides](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2010/pissarides-facts.html), Ken Binmore, Richard Layard, and David de Meza. While giving a seminar at L.S.E. shortly before I arrived, I met John Moore, who was finishing his Ph.D. there. In 1983 we started to work together. Meeting and working with John has been the second great intellectual event in my life. Over a roughly 25-year period we continued the work on incomplete contracts that I had started with Sandy, taking it in new directions, including foundations, corporate finance, and the introduction of behavioral elements. John is a brilliant economist, with an extraordinary mathematical mind, who on several occasions was able to establish propositions and theorems that I could never have proved myself. John also provided important psychological support to me. We have had a close and intense friendship over many years, which extends to Rita and his wife, Sue.  As I have said on several occasions the work for which I won the prize could not have been done without Sandy and John.  In early 1984, Rita and I started to think seriously about whether we should move to the U.S. There were several reasons for this. Rita’s fellowship in Cambridge had come to an end and she was looking for a job. The possibilities in the U.K. in her field did not look good. Indeed, given the cuts imposed by the Thatcher government, academic prospects in the U.K. did not look good for me either. In 1984 I took a visiting position at the Massachusetts Institute of Technology and this was converted into a permanent position in 1985. The family moved to Lexington, Massachusetts, where Rita and I still live.  When I arrived MIT had probably the best economics department in the world. It was thrilling, but also quite intimidating, to be a professor there. Greatness was all around. Paul Samuelson still came into the department, Bob Solow was still teaching, and Franco Modigliani was only a couple of floors away. The younger people were very impressive too. Among my colleagues were Peter Diamond, Eric Maskin (who soon left for Harvard), Jean Tirole, and Drew Fudenberg. I spent nearly nine years at MIT, and it was a very productive period for me. Much of my work on financial contracting was done during these years. But, eventually, for various reasons, a change made sense, and in 1992 I was offered and accepted a position at Harvard (not too far away, but a shorter commute from Lexington!). I started there in July 1993.  Anyone reading this far will probably think that I was restless, and there is some truth to this: I did move around a lot. But since arriving in the Harvard economics department, and I have been there now for over twenty-three years, I have never felt a desire to move again. This is home. The main reason for this, I think, is that not only do I have great colleagues and great students, but also the place is really friendly. There is no hierarchical structure and people are keen to help. The place is also run democratically (at least among the senior faculty!). Decisions are not made behind closed doors, everything is argued out at meetings, and the people who are best prepared and make the best arguments win, whether they have been there for years or are new. This is refreshing.  I have formed close intellectual bonds with several of my colleagues, particularly Andrei Shleifer, Philippe Aghion, Elhanan Helpman, Pol Antras, [Eric Maskin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/maskin-facts.html), and Jeremy Stein, who have all contributed to the incomplete contracting agenda in one way or another. Andrei suggested in the mid-1990s that incomplete contracting ideas could usefully be applied to the question of whether services paid for the government should be provided by private contractors or public employees. This led to a paper that the two of us wrote with Robert Vishny, which included an application to prisons. The prize committee put quite a lot of weight on this paper and since the prize was announced I have found myself talking about it more than any of my other contributions. It is a highly topical issue that journalists and the public can relate to.  My relationship with Andrei Shleifer transcends work. He is an extraordinary person, who has been a constant source of encouragement and wit for more than twenty years. I have had my down periods and Andrei has always pushed me up, telling me that my work was really important when I doubted it.  Philippe Aghion has also been tremendously supportive over the years and, together with Luigi Zingales, Mathias Dewatripont and Patrick Legros, organized a wonderful conference in Brussels in 2011 to celebrate the twenty fifth anniversary of my 1986 paper with Sandy Grossman. This led to a book published in 2016 by Oxford University Press.  Being at Harvard has had many other benefits. I have formed a close relationship with members of the Law School, particularly Lucian Bebchuk, Louis Kaplow, Kathryn Spier, and Stephen Shavell. I co-run a law and economics seminar with several of them. My exposure to these colleagues and to law and economics more generally has improved my understanding of contracts, and contributed immensely to my intellectual life and development. Cambridge is also a large intellectual community and I have continued to have close ties with people from MIT, including Bob Gibbons, Bengt Holmström (who moved to MIT in 1994), Birger Wernerfelt, and Michael Whinston. George Baker was also an active member of this community before he left Harvard Business School for the private sector.  My relationship with Bengt Holmström has been very important. We have written two papers together but more than that we have been close friends for many years and have talked not just about economics, but about our lives, and our hopes and disappointments. This has been invaluable.  As I look ahead to the next period of my life I feel a sense of anticipation. Being awarded the Nobel Prize is wonderful in many ways, and I am excited to explore some of the new possibilities that have opened up. At the same time many aspects of my life will stay the same. In the last few years I have embarked on a new line of research with Luigi Zingales and I hope to continue this. I plan also to spend a lot of time with my family. Rita and I are very lucky to live near one of sons, our daughter-in-law and our grandsons, and we see a lot of them, and vacation with the whole family, including our other son, each summer on Martha’s Vineyard, which is fantastic. I hope to continue to swim regularly for my physical and mental health, and to play the piano. I used to play as a kid, stopped for decades and then returned about eight years ago with the help and encouragement of my teacher, Jennifer Baverstam Weitzman (the wife of my colleague, Marty Weitzman). I keep thinking that with a bit more practice I could rise above the mediocre and although this is probably not so I will keep trying. |
| Autobiographical |  |
| Podcast | Imagine you’re married, but you never discussed children with your partner beforehand. Then imagine your partner doesn’t want children, but you do. Your wedding day contract made no mention of kids, and legally everything is fine – but you’re still disappointed. Contracts are everywhere in society, and the example of children and marriage is just one example that shows that many contracts are – as Oliver Hart would say – incomplete.  In this conversation, conducted in April 2020, Hart explores the importance of words and language for a researcher, how being good at economics is about learning to THINK like an economist and how his parents influenced him to think that anyone who’s not left-wing is an idiot. The host of the podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0818=OH  Oliver Hart: Hello.  Adam Smith: Hello, this is Adam Smith, calling from Nobelprize.org the official website of the Nobel Prize in Stockholm, Sweden.  OH: Yes.  AS: Congratulations on the award of the Prize.  OH: Thank you very much, I’m absolutely thrilled.  AS: How did you hear the news?  OH: I got a call at about, I think two minutes past five.  AS: It woke you?  OH: I have to admit I was awake. I hadn’t been awake for long, but you know such is life. I think I woke at about 4:40 and was wondering whether it was getting too late for it to be this year, but then fortunately the phone rang.  AS: Gosh, what a thrilling feeling of excitement and relief then, when it came.  OH: It was, yes it was rather amazing.  AS: What was you first action after hearing?  OH: My first action was to hug my wife, wake up my younger son who is up for the weekend, so in the house, and then I actually spoke to my fellow Laureate, Bengt Holmström, who’s a friend of mine, a great friend of mine. I knew he knew and I gave him a call, so we had a chat.  AS: Am I right in thinking in was your birthday yesterday?  OH: It was, yes it was.  AS: That’s a nice present then, to receive, a little late.  OH: It’s a lovely present. Friends of mine said I have a decent a shot at this for a few years and it’s always around my birthday, which actually makes celebrating my birthday a little tricky. This is always in the background, and that was certainly true yesterday. So it’s going to be, you know, one of the smaller benefits of winning the Prize, is in the future I will be able to celebrate my birthday without thinking about it. A minor thing, but still.  AS: Interestingly, you’re the fifth Laureate this year to have been born and educated in the UK but then to have moved to the US.  OH: Yes, I noticed that. Yes, and that was true of the one last year. Angus Deaton was in that category too. I think it says two things. The British education system is really quite good, but certainly there was a period when the opportunities to actually develop one’s work were better in the US.  AS: Just one last question. This is for your contributions to contract theory, so it will introduce lots of people around the world to contract theory. What do you hope it brings to the field, the award?  OH: I think contract theory … contracts are just an incredibly powerful way of thinking about parts of economics. I mean they’re just fundamental, the whole idea that trade is a quid pro quo, there are two sides to a transaction and that any transaction that you have in economics, whether it’s between a buyer and a seller or an employer and an employee or a creditor and a debtor, that the way the thing is structured, you know, is very useful to think of it as being done to increase efficiency so both sides have an incentive to construct the transaction in the most, so that it generates the greatest value. You know, even a simple thing like people often think that one side dictates the terms to the other side. It may look like that but actually even then the side dictating the terms wants to choose terms which are acceptable to the other side and generate the most value because if there’s money left on the table then you can write a better contract, so in a way it’s a very good side to economic transactions, and you see it through the design of the contract.  AS: Thank you.  OH: I don’t know whether that made any sense? I think it’s something that people truly lose sight of.  AS: It made a lot of sense. It was beautifully explained and especially beautifully explained so early in the morning after receiving such news.  OH: That’s right. Well, I could do better later in the day. Thanks a lot, nice to talk to you.  AS: Congratulations.  OH: Bye bye now. |
| Interview |  |
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| ID | 0819 |
| Biographical | I was born in Helsinki in 1949. My sister Marianne was born in 1946. Our parents were married when the war against the Soviet Union ended in 1944. My father had spent five years on the front like so many young Finnish men. The post-war years were challenging socially and economically. The government had to arrange homes for over 400,000 refugees from regions lost in the war. In addition, Finland had to pay onerous reparation fees. Despite these hardships, I remember my early childhood as a happy time. There were plenty of children around to play with. We entertained ourselves with games of all sorts. There were few toys but our imagination more than filled the void. It taught me that material wealth is not essential for happiness.  Finland is a bi-lingual country. My father belonged to the Swedish speaking minority and Swedish became my mother tongue – a misnomer, because my mother had spoken Russian, German, Finnish, but hardly any Swedish in her home. Her mother was Estonian and her father, born in St Petersburgh, was of Baltic and Mediterranean origin. My mother, still alive and alert at 92, went to work when she was fifteen to ease the financial burden of her family, spending most of her career at her brother-in-law’s law firm. Pragmatic, principled and when necessary demanding, she was the bedrock of the family. Her unconditional love gave me a sense of security and the belief that life would turn out well whatever I decided to do.  My father got an MBA after the war and served in various executive positions in the shipping industry throughout his career. Had it been financially possible, he would most likely have become an academic. He was a renaissance man, with a big and varied library. It included a copy of [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/samuelson-facts.html)‘s Economics text, which he referred to when he needed to justify spending on something that wasn’t necessary (he was supporting the Finnish economy.) He mastered six languages, including Italian and Latin. He was fascinated by American literature and politics and subscribed to Time Magazine to keep himself informed. Our dinner conversations were wide-ranging and animated. My mother recalls that I asked a lot of “stupid” questions; my sister remembers that I would not easily give up my point of view. But we rarely ended up angry at each other. I’m very grateful for the open, multicultural atmosphere that prevailed in our home. **My early education** I learned basic arithmetics and reading early and therefore started elementary school a year ahead of schedule at age six. I failed my physical examination, because I couldn’t reach my left ear with my right hand going over the head – a basic test of physical maturity. But my mother’s persistent pleas got me past this potential hurdle. The consequence was that I was always the youngest student in class and for a long time also the smallest. It didn’t bother me at the time, but in retrospect it may have contributed to my focus on school work at the expense of learning social skills by interacting more with my class mates.  I went to a private, Swedish-speaking middle/high school (Munksnäs svenska samskola) and got a very good education. I liked school, because I was curious and did well in most subjects, especially mathematics. At the end of high school there was a nation-wide matriculation exam that the Ministry of Education administered each spring. The exam covered four mandatory topics and up to two elective ones. The answers were graded by independent examiners, which made this a test not just for the students, but also the teachers and the school. As one of the top students in my class I was expected to pass with flying colors, but I failed essay-writing and had to retake that part in the fall. This was a major embarrassment, but I wasn’t entirely surprised. Writing had been a challenge for me throughout high school; I was slow and often needed extra time. I wrote about the causes of over-fishing (we were given ten titles to choose from) without knowing much about the empirical facts. I recall being pleased with my theoretical arguments, but speculative theories were not what the examiners were looking for.  Luckily, I met my future wife Anneli (née Kuusakoski,) soon after failing the exam. Rather than brooding over this I spent a wonderful, fun-filled summer together with her. In the fall I passed the essay exam without much difficulty. **Undergraduate studies** Because I didn’t graduate in the spring I couldn’t apply to the Helsinki University of Technology and become an engineer as I had hoped. Instead, I ended up studying mathematics, physics and statistics at the University of Helsinki, which accepted students without an entrance exam if they had passed with distinction the mathematics part of the matriculation exam.  The years at the University of Helsinki were enjoyable and edifying. The mathematics and statistics departments were excellent with some fine teachers. Seppo Mustonen in statistics was a highly original, creative professor, who viewed the domain of statistics broadly and made many practically and important contributions. Olli Lokki was another professor I greatly admired. His course in applied mathematics introduced me to operations research and game theory. These were topics that were novel, exciting and offered good prospects for a non-academic job. I wrote my master’s thesis with him: a non-linear programming algorithm applied to quality control. Neither Lokki nor Mustonen were pedagogical stars – far from it – but their lectures were eclectic, inspiring and delivered with passion. They taught me the importance of putting your soul into teaching.  Undergraduate studies at the university were rather unstructured in those days. There were degree requirements, of course, but no one really monitored student progress. One did not have to attend classes to take exams. Exams were offered multiple times a year in all subjects and there was no limit on the number of times one could retake exams if one failed. With such weak incentives, the average time to graduation was long and many failed to finish their degree.  Socially, life at the university was engaging. The student rebellions were in full swing across the Western world and had just reached Finland by the time I entered. Established views were being questioned, debated and protested against. Normally I don’t like to go with the crowd, but I did participate in the occupation of the Old Student House in Helsinki – an enduring legacy of the student protests. The rebellions led to profound changes in the governance of universities, adding student and staff representation to the traditional academic boards of governance. Today I think most of these decisions were misguided and I have been actively engaged in trying to undo some of the damage. **Ahlström** After graduating in the spring 1972 Anneli and I got married. I began working as an operations research analyst at Ahlström, one of the ten largest firms in Finland at the time. It was a family controlled conglomerate with around 30 factories, a few of them outside Finland. Ahlström had embraced the use of computers early. Under the leadership of Jarl Engblom, the CFO, the firm had developed a large-scale linear programming model to aid management with its long-term strategic planning process. I was hired to fine-tune the model and collect the data to implement it. It was a great way to learn how Ahlström functioned and how the financial system was structured.  But it did not take many trips to the factories to realize that the project was going to be very difficult to implement. Linear programs ask for data that firms do not routinely collect, making the input unreliable. More importantly, the factory bosses were suspicious of a model that would influence how headquarters would distribute funds for investments and other shared resources. It was apparent that they were trying to figure out how to game the model to fund projects they were convinced were right for their factory. After some months of data collection I became convinced that the quality of the data was so poor that we could not trust the model and therefore we should stop the project. This was not welcome news for the CFO, but he understood and reluctantly accepted my judgment. (As it turns out, large-scale corporate planning models, so popular at the time, went soon out of fashion.)  As a substitute, I began working on smaller models to serve the factories rather than the headquarters. This was more successful for two reasons. It alleviated the incentive to distort information, because now the factories were in charge. The second reason was that I used the model in a way an economist might use it – initially as a descriptive rather than prescriptive device. If the model’s answer did not align with what they were doing I explained how the model was reasoning and asked what it was missing. That built trust. Through a process of feedback and adjustments I would eventually get sufficiently close to their production plan so that we could discuss whether their resources could be used better. The human mind is very bad at figuring out the opportunity cost of resources. With simple linear programs, I could describe the logic behind the model’s thinking in a way that the managers understood and appreciated.  My two years at Ahlström led to and influenced my academic research on incentives and organizational design. I have drawn on my work experience when judging the relevance of models and several of them, for instance on career concerns, were directly inspired by what I saw at Ahlström. I was very lucky to be given the chance to work with a fantastic CFO at such a young age. **Stanford** Towards the end of my second year at Ahlström I won an ASLA-Fulbright grant to enroll in the Master’s program in operations research at Stanford University. I left for California in September 1974 and was joined one month later by Anneli and our newborn son, Sam. Soon after arriving I began to think about doctoral studies either in the Operations Research Department or in the Graduate School of Business. I enrolled in a couple of doctoral classes to see if I could handle the material and to get a reference for my applications. I was thrilled when I heard that I had been accepted into the Decision Sciences program at the GSB, formally starting in the fall 1975, but de facto transitioning into it right away.  Studying at Stanford was so different from what I was used to in Finland. The course load was much heavier and the pace much faster. I realized I could have finished my undergraduate studies in two rather than four years. I was also amazed by the interaction between students and teachers in class and outside.  In the math department, Karel deLeeuw taught functional analysis using the Socratic Method. He assigned readings in advance and spent the entire class time discussing illustrative examples and issues. Typically, he would pose a problem and let us figure out how to approach and solve it. He knew full well whether an approach would be successful or not, but he would never tell us and simply nudge us along if we got stuck. Walking down wrong paths was an essential part of the exercise. It was a masterly, engaging performance and a wonderful way of learning.  In the operations research department George Dantzig’s class on linear programming was equally memorable. As the father of the subject, he told stories about how he discovered the simplex algorithm, including the many dead-ends he had tried out in search of a faster method. Context and stories were an integral part of his teaching. In the final exam I got full credit for a proof that was incomplete. In the margin he had written “Had God been just, your method would have worked!!” For him the journey was more important than reaching the destination.  At the GSB Bob Wilson was a towering figure. Not yet 40 years old – and looking ten years younger – Bob’s influence extended well beyond the Decision Sciences group. The first economics class I attended was Bob’s iconic Multi-Person Decision Theory course. It was comprised of new papers on various topics that Bob thought were interesting. Each year was different (I took the class three times). Bob’s teaching style made a lasting impression on me. He didn’t spend time on criticizing the papers or going through them in detail. Instead he focused on why the question posed in a paper was interesting and how that problem was transformed into an economically relevant, mathematically tractable model. “Formulation is 90% of the analysis,” he used to say. It took me a long time to fully appreciate the significance of this message.  Early in the class I asked Bob what I should read if I wanted to understand incentive problems of the sort I had encountered at Ahlström. Bob suggested that I look at Groves’ recent paper on incentives in teams.\* I read it immediately but found it disappointing. With my operations research background, the model looked hopelessly unrealistic. I did not understand that the art of modeling is to figure out what can be left out of a model without losing relevance, not what can be fit in and still solve it (paraphrasing Mike Rothschild’s insightful words to me). Eventually I came to appreciate the economic approach, but not before I had wasted a semester working on a potential thesis on integer programming. Bob’s informal reading group was of much help. The regulars were my fellow students (and life-long friends) Takao Kobayashi and Froystein Gjesdal, who worked on incentive problems; William Thomson, who worked on social choice problems; and Roger Noll, Barry Weingast and Linda Cohen who were visiting political scientists from Cal Tech. The meetings had no set structure or readings. Whoever had something to say would go to the black board and sketch a model, discuss a problem or just bring up an interesting issue worth thinking about. It was part fun, part scary, and always inspiring.  As an advisor, Bob was extremely generous with his time. He liked to put his students on a regular schedule: discipline was essential for good work (something that regrettably hasn’t stuck with me). In the beginning I met Bob weekly.  It was a great incentive scheme. I didn’t dare to go to his office empty-handed and usually managed to produce something at the last minute. Bob rarely expressed strong views on what I presented (as he put it, ideas were like delicate plants that one should not step on), but I learned to read his delicate signs of approval or disapproval. His comments were deep and penetrating and his ability to see the connections between my results and earlier work were invaluable. Just as importantly, his vision of where the field was headed and what role incentive and information theories would play in economics were an inspiration for his students. It’s no exaggeration to say that the use of modern game theory would not be what it is today without the foresight and insight that Bob provided for a several generations of young scholars around the world.  There were many other professors I benefitted from at Stanford. At the GSB Joel Demski enriched my understanding of agency theory and through its connections to accounting. Dave Kreps and Mike Harrison, who were the young stars in Decision Sciences, taught a lovely course on diffusion processes. Dave was on my thesis committee and has continued to offer me advice throughout much of my career.  Stanford provided an exceptional education. My only regret is that I rushed through my studies in three years, which was possible, because there were few course requirements (as I recall, the main requirement was to take five or six MBA courses!). There would have been much more economics to learn. Fortunately, I’ve had the opportunity to return to Stanford many times to draw on the insights and inspiration from my teachers, colleagues and friends there. It remains my intellectual home. **Returning to Europe** The rules for the Fulbright stipend required that I leave the U.S. after completing my degree. So I didn’t go on the regular job market, but instead took a one-year post-doc position at the Belgian research center CORE in 1977–78. The intellectual atmosphere was stimulating under the exemplary leadership of Jacques Dreze, and academically the visit was a success. For Anneli and Sam it was a challenging year, because CORE had just moved to Louvain-la-Neuve before we arrived and none of us spoke French. But the Belgian food never failed to lift her spirits.  I got a junior faculty position at the Hanken School of Economics for the following academic year.\*\* It was nice to be back in Finland with family and friends, but academically the bureaucracy was stifling. Without the company of Björn Wahlroos, who left academia early for a stellar career in banking (currently the Chairman of Nordea), it would have been much worse. We became good friends and I have continued to benefit from his valuable insights and feedback especially on the relevance of my work on liquidity.  Around Christmas I got a letter from Steve Shavell, asking me whether I would consider returning to the U.S. and if so, would I be interested in Harvard? To my surprise Anneli was ready to return (for a year or two, she would be quick to add). In the end Harvard decided not to invite me to talk, but CarnegieMellon, Northwestern and Yale did. My first talk, at Yale, went poorly, but the visit was saved by Steve Ross, who dispelled my disappointment with charm and humor. We became immediate friends. Thanks to him, the two other talks went much better and in the end I got job offers from all three places. **Meds and Northwestern** I chose Northwestern. My parents wondered whether I knew what I was doing; Yale had such a superior reputation in Europe. But MEDS had the talent I was looking for. The department had hired an exceptional group of applied game theorists including Ehud Kalai, John Roberts, Mark Satterthwaite, [Roger Myerson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/myerson-facts.html), Nancy Stokey and [Paul Milgrom](https://www.nobelprize.org/prizes/economic-sciences/2020/milgrom/facts/). The excitement in the department was palpable. Dean Jacobs, who I saw right after my seminar, sealed the decision by offering me a job on the spot.  My four years at MEDS – which now feel more like a decade – were wonderful both academically and socially. As befits junior faculty, we were competitive and worked very hard to outperform each other. But we also greatly appreciated each other’s work. We exhibited a peculiar mix of insecurity about our tenure prospects at the same time as we felt very excited about our research. It was a time with an abundance of problems to solve and ideas to pursue and things to talk about day in and day out. With Roger Myerson I spent endless hours thinking about the right definition of efficiency under asymmetric equilibrium. It was (and remains) a slippery problem that we were lucky to get some closure on. With Milt Harris, who joined the finance department a bit later, I studied wage dynamics. Milt’s style of working was a revelation. We would sit and do everything together, including writing the paper. It was both productive and fun. Much credit for the MEDS magic goes to the “elders” that provided adult supervision: Stan Reiter, Mort Kamien, Nancy Schwartz and David Baron. Mort used to walk around the department to see how we youngsters were doing. Once he came in when I was in an especially sour mood over my poor teaching. He suggested I go to the board and present the lecture I just had given. I had barely begun the mock lecture when he interrupted me: “That’s your problem.” “You said ‘this is easy’. Never say something is easy. It makes the students nervous that they may not get it. And if they get it, there is no bonus, because you said it is easy. Say nothing, or say that it is difficult. Then there is only upside for the students.” This remains the most memorable and possibly the most valuable advice I’ve ever gotten about teaching. Mort was a wise and generous colleague and mentor.  There was an additional bonus from being at Northwestern – it’s proximity to the University of Chicago. I visited the economics department in the winter quarter 1981. The visit opened my eyes to the power of the Chicago school of economics as exemplified by [Becker](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1992/becker-facts.html)‘s, Rosen’s and (a little later on) [Coase](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1991/coase-facts.html)‘s work. At MEDS we mainly discussed game theory and information economics – how to formulate and solve technically demanding problems. It was a natural continuation of what I had learned at Stanford. I found the positive approach to economics that permeated Chicago very appealing. After my visit I often went down to Chicago to participate in and give seminars. I liked the aggressive give and take, because it was stimulating and done in earnest. **Yale SOM** The MEDS department began to lose people in my third and fourth year there. First John Roberts and then Dave Baron moved to Stanford. Both were instrumental to the MEDS success. When Paul Milgrom visited Yale SOM and decided to stay there, I was ready to leave. I worried (prematurely as it turns out) that MEDS would lose its magic. When Yale made me a tenured offer I accepted it.  Paul and Steve Ross were the biggest reasons for moving to Yale. Steve was the intellectual leader. He was very accessible and frequently held court in his office for whoever happened to drop by, dazzling us with his lightning quick mind. Paul’s brilliance I knew from Northwestern. We had talked almost daily, often walking to or from the office together. But we had never cooperated on a project. I hoped that we would hit on something interesting once we were in a quieter place.  We did indeed. I had been bothered by the fact that the models we had used to characterize optimal incentive contracts, produced unrealistically complicated answers. These models could not explain the wide-spread use of simple piece rates, for instance. Soon after arriving at Yale, I went to discuss this issue with Paul. In a single, intense session we sketched out a bare-bones model, based on the intuition that linear contracts provide robust incentives. The model was meant to be a first stab at explaining piece rates, but proved far more interesting than we ever imagined. The model was very tractable and soon took us in a number of different directions.  The most important, and unexpected, payoff came when we realized how important it is to study multitasking – the case where employees work on several tasks simultaneously and have to decide how they allocate their time between these tasks. If some tasks are easy to measure (such as output produced) while other tasks are hard to measure (such as quality of the output), then muting the incentives for the easy-to-measure task is an indirect way to provide incentives for the hard-to-measure task. Multitasking can explain why firms so commonly use low-powered (or no) explicit pay-for-performance incentives. It also leads to the insight that job design, bureaucracy and promotions are potent, cheaper incentive instruments within firms. Our quest for understanding simple piece rates led us to a much broader, richer theory of incentives, in which piece rates play a limited role.  Needless to say, without Paul I would not be writing this essay. The exhilaration we felt when we worked on the sequence of papers described briefly above, and in more detail in my prize lecture, is hard to put in words. **MIT** We loved living in Guilford, in a modest house near the water. Seeing and walking along Long Island Sound every day was precious. But after eleven years at Yale and Sam gone to college we were ready to move to MIT in 1994.  It was the first time that my primary appointment was in an Economics department rather than a business school. The first thing I noticed was how harmonious the department was. Differences in opinion were vented vigorously at times, but people did not hold grudges and there was a shared sense of responsibility for maintaining the excellence of MIT Economics. That’s how it still is today after a successful generational transition. The level of excitement among our stellar, young faculty is at least as palpable as it was at MEDS, but broader as it reflects the rapidly progressing frontier of all empirical work. It tells a lot about the MIT culture that the shift from theory to empirical research has gone so smoothly and successfully.  Moving to MIT gave me the opportunity to work more closely with [Jean Tirole](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2014/tirole-facts.html), who visits MIT six weeks of the year. We had begun a project on financial crises after my sabbatical in Finland in 1991–92, when the Nordic countries, and especially Finland, were hit by severe recessions due to collapses in their banking systems. We wrote a string of papers focused on the demand for insurance (liquidity) and the resulting scarcity of collateral. At the time, people seemed skeptical about collateral shortages, but today the shortage of “safe assets” and the role of government supply of safe assets are vibrant research topics.  In 2008 I was invited to the yearly central bank conference in Jackson Hole to discuss an inspiring paper by Gary Gorton entitled “The Panic of 2007.” This led to a second line of research on financial crises, focusing on the role of debt in money markets. Together with Gary and TriVi Dang I have studied the optimality of information-insensitive debt in the production of private money by banks. The project has been very exciting, partly because it is controversial. Our work suggests that opacity in money markets is a logical consequence of optimal contracting. This contrasts with the widely held view that transparency is essential in money markets and for the credibility of the banking system. The issue is still unsettled, but I think our point of view has won more support over the years.  My work on financial crises led to a deeper interest in policy issues. MIT, with its broad footprint and strong network of alumni has given me ample opportunities to connect with policy makers and central bankers. MIT’s reputation as the leading university of technology also gave me the opportunity to spend thirteen interesting, edifying years on the board of Nokia. I got an inside look into corporate governance, which was intellectually interesting and valuable for my work with Steve Kaplan. Nokia also provided a unique window into the rapidly changing world of ICT and most interestingly perhaps, what the young people around the world are thinking and feeling. The entrepreneurial spirit and activities at MIT continue to be the perfect springboard for keeping abreast with new technological trends and how they will impact organizations.  I have had the pleasure to make close friends in the community of economists and scientists at MIT and Harvard. The list is too long to include here, but I want to single out Oliver Hart, my closest intellectual partner and close friend throughout these years. We have debated approaches to contracts and organizations ad infinitum. We have written a few papers as a result. But more important than the papers, is the special friendship and trust that has developed between us. Without Oliver, my time in Cambridge would have been much less rewarding.  Let me close with a story about Paul Samuelson, which epitomizes his spirit and thereby the spirit of MIT. Occasionally he would invite me on Friday afternoons to share a glass of sherry in his office, which was next door to mine (I trust that this won’t get me into trouble). Talking to Paul was always inspiring. One Friday when I went for another edifying chat, I was met by a sharp question: “Do you ever change your mind?” I didn’t know where he was going with the question, but it sounded ominous. I hedged my answer with a sheepish “sometimes,” whereupon he proceeded to remind me of a conversation we had had about executive pay shortly after I arrived to MIT. I had defended the use of incentive options based on my work with Steve Kaplan and he had accepted my argument (sort of), noting that “it’s never too late to learn something new.” But the financial crisis had made him furious at the banking executives and their over-sized bonuses, which in his view had played a key part in the catastrophe. He wanted to know if I was ready to acknowledge that I had been wrong fifteen years ago. I had changed my mind, but the message of the story is the exceptional passion and memory of Paul at 94, still fully engaged with the political and economic issues of the day. **Gratitude** I feel extremely fortunate to have had the opportunity to interact with brilliant economists like Paul Samuelson, [Ken Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/arrow-facts.html), [Bob Aumann](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2005/aumann-facts.html), Bob Wilson, [Peter Diamond](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2010/diamond-facts.html) and many of MIT’s brilliant young minds. I am deeply honored and grateful to the Royal Academy for bestowing me with this prize and for making the week in Stockholm so memorable for me and my family.  To my wife Anneli and my son Sam: Thank you for your love and support. You have shared the pain and you deserve to share the gain from the award. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0819=BH  Bengt Holmström: Hello.  Adam Smith: Oh, hello. May I speak to Professor Holmström please?  BH: Speaking.  AS: Ah, this is Adam Smith calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm. Congratulations on the Prize in Economic Sciences.  BH: Yes, thank you.  AS: First of all, you are a Finn but of the Swedish speaking minority population. Does that make it especially meaningful to receive this Swedish prize?  BH: Yes, of course, I mean the Nobel Prize is very special, but being a Finn and a Swedish-speaking Finn, and seeing Stockholm is my second home town, you know, it’s very special for me. My family, a big part of my family lives there.  AS: How did you actually hear the news this morning?  BH: Yeah, I was woken up and I thought it was a reminder of taking my medication, and then I learnt that it was not about medication, it was about the Prize.  AS: It’s a nice surprise.  BH: It was a very nice surprise, yes.  AS: Oliver Hart said that he phoned you early.  BH: Yes, Oliver Hart, I’m so glad that I won it with him. He’s my closest friend here, and you know we have worked together and talked together over the years, and he has been a great inspiration for my research.  AS: One thing I read was that you are a proponent of using the blackboard, the chalkboard, when teaching. You never use slides. That seems quite surprising these days.  BH: I use slides when I give seminars, but I would say that I like to look at people that teach from the blackboard and I teach myself from the blackboard.  AS: I suppose it aids thinking.  BH: Well it’s much easier for the people to think, and it gives you the freedom to go wherever the lecture goes. I’m not a person who plans exactly what I’m going to say. It depends on what questions people ask and what they want to talk about. My lectures are never the same even though the title may be the same.  AS: Could you extend that process to talking about your own research, that you just see which avenues you follow next, you allow yourself freedom?  BH: Well I think you have to have a goal, but yes research that takes its own path, an unexpected path, that’s a very essential part of doing research. So research that exactly goes where you expected it to go is uninteresting on the whole. You need to start travelling somewhere, you know you have to decide you want to get to Stockholm, but if on the way, you know, you see Paris you may want to stop there, to give you a sort of metaphorical answer. Research is a trip, and you have to be attentive to all the things you see and be able also to move away from the planned path.  AS: The Prize will of course focus the world’s attention on contract theory, and I imagine it is increasingly important given the growing public debate about incentives, and corporate governance, and public versus private provision of services etc.  BH: Yes. But I want to emphasise that it’s not just about money. People have a very narrow view of incentives, but one can say that almost everything in economics is about incentives, but if you look at incentive theory what has happened is that instead of just focussing on some ways to pay people so that they do certain things, it’s very much about structuring their jobs or structuring the organisation in a way that motivates. So the issue of motivation is hugely broader than just asking you know how should people get the CEO to behave in a particular way, and financial monetary incentives are in some sense too effective often. They are very powerful in sending signals as well as, of course, rewarding finically. And so one has to be very careful in their use.  AS: That’s very interesting. So one must think more broadly.  BH: Yes. So not paying people is also an incentive, if one wants to put it that way. Sometimes no financial incentive is the best incentive.  AS: Now you’re in for a day, I suppose, of constant interviews and conversations. How does that strike you?  BH: Well, it’s another twist. You know I take it where it goes. I follow where it goes. I have no idea what’s ahead.  AS: Well I wish you every success and joy in your journey into the unknown today.  BH: Thank you very much.  AS: We very much look forward to welcoming you to Stockholm in December.  BH: Yes, I am fully intending to come. Thanks so much.  AS: Thank you.  BH: Bye bye. |
| Interview |  |
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| Biographical | **Scotland** I was born in Edinburgh, in Scotland, a few days after the end of the Second World War. Both my parents had left school at a very young age, unwillingly in my father’s case. Yet both had deep effects on my education, my father influencing me toward measurement and mathematics, and my mother toward writing and history.  The school in the Yorkshire mining village in which my father grew up in the 1920s and 1930s allowed only a few children to go to high school, and my father was not one of them. He spent much of his time as a young man repairing this deprivation, mostly at night school. In his village, teenagers could go to evening classes to learn basic surveying and measurement techniques that were useful in the mine. In Edinburgh, later, he went to technical school in the evening, caught up on high school, and after many years and much difficulty, qualified as a civil engineer. He was determined that I would have the advantages that he had been denied.  My mother was the daughter of William Wood, who owned a small woodworking business in the town of Galashiels in the Scottish Borders. Although not well-educated, and less of an advocate for education than my father, she was a great story-teller (though it was sometimes hard to tell the stories from gossip), and a prodigious letter-writer. She was proud of being Scottish (I could make her angry by saying that I was British, and apoplectic by saying that I was English), and she loved the Borders, where her family had been builders and carpenters for many generations. The region has a rich history; centuries of cattle-stealing along a lawless border left many good tales, and the memories of (mostly losing) battles against the English were well tended, particularly Flodden Field, fought in 1513 but which felt more like 1913. My mother knew and could sing many of the local songs. Sir Walter Scott’s Abbotsford was nearby, and he is said to have visited an ancestor in the cottage where we lived after we moved from Edinburgh; the house had once belonged to my great-grandmother. I have a vivid memory of a long walk in the woods near the River Tweed guided by the friend – a printer in Galashiels – who had introduced my parents to one another during the war. He had a spellbinding gift of turning his knowledge of local history into long rhyming ballads, composed in the moment, and he would point out places where someone had done something, or had something done to them (“and when her legs were cuttit off, she fought upon her stumps”), a mesmerizing treat for an eight-year old. In late afternoon, suddenly revealed in a clearing, but quite invisible from anywhere else, stood an enormous sandstone statue of Sir William Wallace. I suspect that today’s relentless meritocracy, whatever its other benefits, would not leave my historian minstrel as a printer in a small Scottish town.  In Edinburgh, where we lived until I was nine, I attended James Gillespie’s Boys School a few hundred yards from our home on the edge of the meadows, a large open space south of the center, then still dotted with wartime “allotments” where the locals grew vegetables. I didn’t care for school much – it was very strict, corporal punishment in the form of the “tawse” was common and unpredictable, and I was often afraid – but I believe that I did well enough; indeed, my mother always regretted that I had not stayed long enough to become the “dux,” as the best pupil was called. We learned a lot of history and geography, as well as arithmetic and reading, with lots of drill.  The educational highlights I remember were not in the classroom. My father spent a lot of time with me when he could. He taught me how to take square roots, a skill I have retained, but do not use often, except to check that I still remember. At weekends, he took me to Edinburgh’s great zoo, to museums, to the botanical garden (with a giant hothouse, and whose bus stop was right by Robert Louis Stevenson’s childhood home) and to the harbor at Granton, which had a fishery fleet (trawlers unloading fish and loading ice and salt), a lighthouse supply ship (the Pharos), and which imported esparto grass from Portugal for making high quality paper. Looming in the distance over the eastern end of the botanical gardens was an enormous castle, adorned with hundreds of grotesque gargoyles, which my father wistfully explained was Fettes College, Scotland’s most exclusive (and expensive) school where he had (impossible) dreams of sending me. I was lonely when my father had to go on long civil-engineering assignments away from home, and I remember being even more lonely, and desperately bored, when I caught scarlet fever, and had to spend seven weeks in a darkened room, with no books and only an infinitely dull radio for entertainment. I detested Mrs. Dale’s Diary ever after, and was glad to hear of its end in 1969. Even my beloved jigsaw puzzles were off limits. Boredom and loneliness have been familiar visitors throughout my life, though I have come to (reluctantly) accept that the turning inwards that they bring is linked to creativity, at least for me.  In those days, children could walk unattended to museums and libraries, as well as being sent to the local grocery store to buy food, or as we said to “do the messages.” What was then called the Chamber Street Museum was a mecca of exotica, mummies, totem poles, taxidermy, science, exhibits that moved when a button was pressed, clocks, ships, and mines. The children’s library on George IV Bridge had both delights and dangers; my parents did not read and could not guide me, though they once tried to cut off my Biggles addiction. I read much that terrified a seven-year-old, Edgar Alan Poe’s *Pit and the Pendulum* and Dickens *Christmas Story*, as well as the much more reliably enjoyable Stevenson. I dreamed of coral islands, with or without treasure, of adventures in the South Seas, and tried to imagine how long it would be before I would visit Africa and India, still (mostly) colored a proud pink on the maps of the 1950s.  In 1955, my father qualified as a civil engineer, and we moved to the village of Bowden in the Borders. The village is old; it has a 12th century church. The ruins of a fundamentalist Free Church were still present in the center, but the pub had closed a century before, though it was often referred to. I loved the escape from the city, and Dave Preston, a plumber who worked for my father and who was a member of Scotland’s international fly-fishing team, took me fishing. Like nearly all other fly-fisherman I have ever met, he was far too busy fishing to teach me; it is not for nothing that many states in the United States have laws that prohibit paid guides from fishing themselves. Fly-fishing, like boredom, which it frequently resembles, has provided me with thousands of hours of dreamtime, where the inchoate muddle in my head is given a chance to sort itself into something that might resemble an idea. Fly-fishing in Scotland, at least for trout, was inexpensive, though not free, and I recall that the cost of the license was an occasional source of conflict at home. There never was much money and my father worried about it often.  My sister Mairi and I went to the local school in Newtown St. Boswells. I passed the dreaded exam at 11 plus, and went to High School in Hawick, a knitwear manufacturing town about 15 miles away. (Those who did not pass the exam were doomed to three years of gardening, cooking, or car repair depending on sex. Mairi, four years younger, elected to go to High School in Kelso.) At Hawick I learned Latin (a Bowden villager explained to my father, there were three languages on the curriculum, Latin, French, and “Algeebra”) and its precision greatly impressed me. The declension in Latin frees word order from the burden of carrying meaning as it must in English and so gives great flexibility to poetry and rhetoric. The powerful idea that precision and beauty could be combined came from my Latin classes, though Algebra and the King James Bible played a part too, even if the Bible was less strong on the precision.  It turned out that Fettes College admitted two Foundation Scholars a year (out of a class of about 90), and several teachers at Hawick High School donated their time to train me for the competitive examination. They must have done this out of dedication to teaching; certainly my father had no money to pay them. I worked very hard over many months, becoming quite sick at the time of the exams, but won one of the scholarships. Even then, the incidental expenses were a strain for my family, and there was some difficulty as I tried to keep up with much richer boys. I remember being the only boy with a Scottish accent (though it seems likely now that there were a few others) and the social life was not always easy, especially at first. Fettes strengthened an older feeling that ordinary Scots like me were not full citizens in our own country, compared with a landholding English elite who spoke with a different accent, and who set boundaries that I could not cross (though perhaps I was too much influenced by the access rules for trout fishing). In any case, the feeling of being an outsider is one that still comes more easily to me than it should. And it is not without advantages; it helps me not back down when I am trying to argue a position that only I believe. (Of course, it is less helpful when that position is wrong.)  The teaching at Fettes opened up new landscapes in many directions. I specialized in mathematics and physics after two years, but that only opened up time for other activities. I continued to play the piano (with some ability), the pipe organ (not so well), and the double bass (not well at all, but it got me into the orchestra.) I played rugby seriously for a while, which helped get me into Fitzwilliam College at Cambridge, but I probably spent most time in (entirely optional) English classes. This lack of forced learning was of great benefit to me; I learned to browse, working only on things that seemed interesting, guided only by my (sometimes temporary) enthusiasms and by always willing and talented teachers. When I later became an economist in Britain, I had the same freedom; in the United States, newly minted economists must first find a field and a peer group, and then stick to them relentlessly in order to get a job, and to keep it. In Britain, I never had a field, I took no courses in economics, and escaped going to graduate school, so I could continue to work as I had done at Fettes, browsing across areas, learning new things that often seemed irrelevant, but were always interesting, new, and which with (perhaps not so) surprising frequency, would eventually come together to open up new insights. For many years, I regretted my lack of formal training, envying my peers who had taken tough courses, and who understood things that I did not know existed, but feel now that those regrets were misplaced. When I learn something that I want to learn, and do it my own way, I often make mistakes and it is usually slow, but when it is done, it tends to stick (like taking square roots), and there is always the chance that I find something that is not so well known after all. Fettes also taught me that people like to share what they know, and that they love to be asked. Being willing to confess ignorance and to listen is a fast and joyful way to learn; I sometimes worry that our competitive American graduate schools make such confessions difficult for many students. **Cambridge** This happy story fell apart when I got to Cambridge. Mathematics, it turned out, was not what I wanted to do if I had to do it fulltime, especially in a teaching program that was appallingly badly organized, and with fellow students who were better and much more dedicated mathematicians than I. Shopping around among other areas was fine if you were doing well in the subject that counted, but I was not. Rugby collapsed too in the face of the wanton and sometimes sadistic violence of those who took rugby seriously in my college. I requested a transfer to history of science, but was denied by a risk-averse advisor; I still think that it would have been a good choice for me, even though I would not be writing this particular biography. Eventually, in desperation, my tutors told me that I had to stop doing mathematics and take up what they clearly thought of as a last resort for ne’er-do-wells, a previously unconsidered option called economics. I did so, with no expectation of anything other than a degree, and the lights came back on.  When I left Cambridge, I worked briefly at the Bank of England as part of their new graduate intake. But the Bank did not know quite what to make of graduates in 1967, and they did not have anything very useful for me to do. So I learned, perhaps incorrectly, that I didn’t want to be a banker, and I have never since left academia. I returned to Cambridge as a Research Assistant for my college economics tutor, Jack Revell, who was constructing wealth accounts for Britain. I spent several months in dusty archives, copying down information on the assets of friendly societies; I did not mind the work, and sometimes even regret today’s easy availability of data. It is impossible not to think about the numbers, however dusty, to wonder what they mean, to look for patterns, even to test halfformed hypotheses, and when they are assembled into something that can be analyzed, I am protected from some of the stupider mistakes that are all too easy when I know nothing about the data. Today’s equivalent – and it is undoubtedly better as long as it is done – is the ability to use computer graphics to visualize the data; I do not miss the graph paper, the erasers, and the endless starting all over again. But I never quite learned to use research assistants, or at least to sleep at night when they are working for me. It is not that I myself do not make mistakes, indeed I am a poor research assistant for myself, but someone else’s mistake is not lodged deep in my brain where it can wake me up in the night.  My main reason for going back to Cambridge was not academic, but that I wanted to be with Mary Ann Burnside, an English major, originally from Wichita, Kansas, via Evanston, Wellesley and Berkeley. We were married before I left the Bank; we bought a small pretty house in the village of Barton outside of Cambridge (then reputed to have many Nobel laureates), and we had a daughter, Rebecca, and a son, Adam. Mary Ann died of breast cancer in 1975, and is buried in the old churchyard in Barton. Rebecca and Adam have families of their own now, Rebecca and her son Julian live in Chicago and Adam, his wife Sabina, and their daughters Celestine and Lark live in New York. Julian, Celestine and Lark, the Deaton *barnbarn*, had the times of their lives in Stockholm, where they were much photographed and televised.  All this lay far in the future. Soon after I arrived in Cambridge, Jack Revell left to take a chair in Wales, I was rescued from the dusty archives, and largely left to myself. Which was when [Richard Stone](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1984/stone-facts.html) brought me into his orbit. He led a larger project in which I was nominally employed, and he somehow decided that I was a kindred spirit who could be asked, not only to run regressions and fetch numbers, but also to come to dinner and, though he hardly designed it that way, to pattern myself on him. Stone had a passion for measurement, for modeling, and for clarity and transparency in writing. He had worked with [James Meade](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1977/meade-facts.html) and with Keynes on national income accounting during the war, for which work he received the 1984 Nobel Prize. It was under his guidance that I first started to think about saving and about demand analysis, but the guidance came by example, not by instruction. With my own students, I have tried to do the same, though if I had it to do again, I would probably give a little more purposive direction, at least on occasion.  In those days, Cambridge was still run by the Keynesians; Joan and Austin Robinson, Nicholas Kaldor and Richard Kahn were powerful figures. Meade and Stone were there, but were less than appreciated by the Keynesians. They had little taste for the often robust and frequently *ad hominem* arguments, and kept very much to themselves; the withdrawal of Cambridge’s two future Laureates taught me one of the most important lessons of academic life: withdraw from the academic politics and get on with the work. The coffee room was a place of lively conversation, often very loud lively conversation; I remember Joan shouting, “What *do* you mean, Nicky, the international *pig* standard?” having misheard “pig” for “brick.” Cambridge taught me much, and gave me an acquaintance with the ideas of the intellectual left. I didn’t know until many years later that there were economists to the right of the Fabian socialists, and when I came across [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/stigler-facts.html)‘s piece on why the professional study of economics makes one conservative, I thought that the *Journal of Political Economy* had committed an egregious typographical error. Joan Robinson was fond of saying that neoclassical economics was an apologia for American capitalism, and while I did not believe that then (nor today), it is a perspective that is often worth keeping in mind. It was later balanced at Princeton by a distinguished colleague’s fondness for declaring that “government is theft;” it is a poor political slogan, but after 33 years in the United States, I am frequently reminded of its empirical relevance for some activities, such as crony capitalism.  Cambridge was full of good young economists around that time. [James Mirrlees](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1996/mirrlees-facts.html), [Amartya Sen](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1998/sen-facts.html), [Peter Diamond](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2010/diamond-facts.html), [Joe Stiglitz](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2001/stiglitz-facts.html), Tony Atkinson, Christopher Bliss, Geoff Heal, Mervyn King, Hashem Pesaran, and [Eric Maskin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/maskin-facts.html) were all there or spent time there during my seven years. The first economics talk I ever attended was Tony Atkinson presenting his famous paper on inequality, and I expected all seminars to be of similar quality. When Tony went to Essex to become a professor at age 26, not only were his fellow young economists delighted at his achievement, but we were all energized by the possibilities for ourselves. My chance came in 1975, when I was offered and accepted the Chair of Econometrics at the University of Bristol. Mary Ann had died a few months before, I had two small children, and the additional income meant a lot to us.  I was still at Cambridge when I met John Muellbauer, who was a lecturer at the University of Warwick, and who had recently returned from completing his PhD in Berkeley; he had known Mary Ann there, which is how we met. We discovered a common interest in consumer behavior, and quickly found that we both knew a lot of not so well-known material, though from different perspectives, I by wrestling with the writings of Terence Gorman, then at the LSE, and he through the teaching of [Dan McFadden](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2000/mcfadden-facts.html) at Berkeley; he was also much further along than I in his own writing. We became good friends, and worked together for nearly a decade.  Terence Gorman “adopted” me; he became a friend, and would talk to me for hours. I was both delighted and terrified; listening to Terence was always like a tantalizing glimpse through a mist, and feeling that, if I could only understand, many of the things I had puzzled over would be revealed. He would talk about things I’d thought about, like representations of additive preferences, which I knew I did not understand, but I would finish up knowing only that he understood it but could not explain. Yet he never flagged in making it clear how highly he thought of me, and that, if I did not understand, it was his fault, not mine. When he invited me to talk at the London School of Economics, very early in my career, I could not have been more petrified; it was my first talk, and the audience contained not only Terence himself, but many luminaries including Frank Hahn, Dennis Sargan, Jim Durbin, Amartya Sen, Michio Morishima, Partha Dasgupta, David Hendry, Ken Wallis, Richard Layard and the eccentric but brilliant John Wise. My paper was much better than I could have known (it later won the inaugural Frisch medal from the Econometric Society), but almost all I can remember is the terror. The now Lord Layard remembers that talk too, and recalls wondering where this self-possessed young man that no one had ever heard of could possibly have come from, so I must have managed to put on a brave face. **Bristol** At Bristol, I was once again fortunate with my colleagues. The chair, Esra Bennathan, became a close friend, and was a stalwart supporter of my appointment in the face of the skepticism of those who had been professors since before I was born. Martin Browning came to Bristol as his first academic job, and we worked together on labor supply and consumer demand over the life cycle. We also hired Ian Jewitt, one of the funniest people I know, and a thinker of startling originality, as well as John Broome, who was still trying to decide whether to be an economist or a philosopher (in the end, he chose the latter). From John I learned how to think about the ethical issues underlying economics and public policy; he and Amartya Sen are responsible for a part of the way I think about the world. The drifting apart of economics and philosophy, which were close in the 60s and 70s, has surely brought harm to both; recent signs of a rapprochement are to be welcomed.  After I visited Princeton for a sabbatical year, Orley Ashenfelter came to Bristol to visit in turn, and those years were the beginnings of a lifelong friendship. Bristol, with its long history of wine importation and many wine merchants was the first inspiration for Orley’s path-breaking interest in the economics of wine. Orley’s work is as original as he is and he has an uncommon curiosity about everyday phenomena, whether it be labor markets, wine, hamburgers, or prohibition. His empirical skepticism and commitment to matching theory and data marked us as kindred minds from the first time we met, at a conference in Urbino in 1976. Orley brought a young graduate student with him to Bristol, David Card, who eventually went home in frustration with Bristol’s computer facilities, only to be deported to Canada at the US border. The computer arrangement was indeed imperfect, and I wrote a number of theory papers there.  At Bristol, my collaboration with John Muellbauer flourished, there, and in London, where John had moved to Birkbeck College. We worked on the development of the Almost Ideal Demand System, tinkering with candidate functional forms, trying to shape one that would be “ideal;” we came close. We also realized that we could bring a unified and relatively new approach to consumer behavior that would be useful to others. I had good students in Bristol to try out the material, and the eventual result was our book *Economics and Consumer Behavior*. I remember that the first few key chapters were written very quickly, over a few months, and typed by our expert typing pool (how I miss the typing pool!). Each of us focused on the *other’s* main areas of expertise; if we did not understand each other’s ideas, how could we expect our readers to do so? Of course, we hit diminishing returns, and some of the later chapters held us up for a long time. The collaboration with John is one of the highpoints of my intellectual life. Our time together was full of learning from each other, from threshing out things that it turned out neither of us fully understood, and the exhilaration of kids who thought we were going to show the world. We knew more than we deserved to know, or so it seems it retrospect.  Bristol is a wonderful place to live. I, my children, and my second wife Helge lived in a tea-merchant’s house built in the 1840s, not as elegant as some in Bristol or nearby Bath, but whose architecture came from the age of upstairs-downstairs. There was a servants’ staircase, a basement kitchen and laundry, with an “area” that gave tradesmen access to the servants. The city is small, with theater and concert halls nearby, fine wine merchants, and the spectacular countryside is very close, especially the Mendips to the south and the Wye Valley in South Wales to the west; London is a fast train ride away. But at the end of my time there, money was short in the university, departments were being closed, and a lot of unpleasant time was spent in figuring out who would be next. Princeton seemed like an idyllic paradise with fabulous colleagues, students, and wealth. As indeed it was. **Princeton** Almost the first person I met in Princeton, at a party after giving my job talk, was Anne Case. It was a memorable meeting for both of us, but it was only fourteen years later, in 1997, after winding and separate journeys for both of us, that we became a couple and were married. It is impossible today for me to imagine a life in which we are not joined at the hip; we have offices a few doors apart, we often travel together, we sometimes – but not always – work together, we cook together, we go to the opera together, and best of all, we fly-fish together. In both cooking and fly-fishing, I was originally the teacher, but have been long surpassed by the pupil; on a river, Anne has an apparently natural but deeply mysterious gift for sensing just what is about to happen. Off the river, we have recently been working together on an important and large scale project on mortality and morbidity among middle-aged white people in the United States. A marriage that encompasses all of our lives is a rich gift.  As promised, Princeton brought collaborations with both students and colleagues. Christina Paxson came to Princeton not long after I did; she had always wanted to work in development economics, but had not been able to do so in graduate school. So we decided she would come to Princeton and that we would learn development economics together, which we did. We had a mutual interest in saving, and we worked on life-cycle consumption in rich and poor countries. We also wrote about health, mortality, consumption inequality, and a series of food puzzles. Our work is prominently discussed in the Nobel citation. Chris is now President of Brown University. This is sad for me, but not for her; she always wanted to be an administrator and she has a great talent for it.  John Campbell was also a junior faculty member at Princeton. He had worked with [Bob Shiller](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2013/shiller-facts.html) at Yale, and had written a breakthrough thesis about saving for a rainy day and opened up many new ideas. Together, we thought about the puzzles of consumption and saving that were then in the air; I remember a pleasant day at the Engineering Library as we tried to figure out what the spectrum at zero might be, and how it might be relevant for the relationship between earnings and consumption.  At Princeton, I joined the Research Program in Development Studies, which had been founded by [Sir Arthur Lewis](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1979/lewis-facts.html) who, although retired, was still around on a regular basis, and who befriended me; he always called me “chief,” though I have no idea why. Princeton thought of me as a partial replacement for Arthur, a daunting idea; I knew little about the subject, and my quantitative approach could not have been further from his deep historical wisdom that he had accumulated in the Caribbean, in England, and in Africa. He was unhappy in those years with how history had been marginalized in economics, and the mainstream’s lack of interest in the persistence of poverty around the world, the topic that was central to his intellectual life. So it would have been understandable had he resented me, but his attitude was entirely the opposite. Central to the development group then was Mark Gersovitz, who had worked in every area that can lay claim to being part of development economics. He was my guide to the subject, and generously shared what he knew. Another great influence on my development work, especially in India, has been my friend and collaborator Jean Drèze. Jean is a scholar and activist, who argues and agitates for policies to help the poor. He brings to our joint work an unequalled knowledge of the life of farmers and laborers in India; he is also one of the finest analytical economists I know.  Over the last decade, I have worked with the Gallup Organization, occasionally advising on data collection, but more often simply analyzing the data that they have collected on wellbeing in almost every country in the world. Gallup is an impressive and unusual company that is deeply interested in the intellectual underpinnings of its work, and in making its own contributions to science. I was introduced to them by [Danny Kahneman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2002/kahneman-facts.html), then my Princeton colleague, who was working on and thinking about wellbeing, and had advised Gallup on measuring life evaluation and hedonic affect. Danny and I worked together on Gallup’s data, most famously on a project that showed that, in the US, hedonic affect improved with income, but only up to a about $75,000 a year, while life evaluation continued to respond to income even beyond that limit. Gawker.com deftly summarized the project: science shows poverty sucks.  I have had a long relationship with the World Bank, beginning even before I moved to Princeton. The Bank’s work constantly throws up good problems. Most are insoluble, but occasionally it is possible to come up with a better measure, or to see that something is not being thought about in the best way. For me, it is always useful to be presented with other people’s problems, an escape from the risk in academia of small group self-referential research. In the early 1980s, I worked with Graham Pyatt, who was part of Richard Stone’s team in the early days of the growth project, and who was starting the Living Standards Measurement Surveys, a still ongoing program to develop household surveys throughout the world. When Nick Stern, a friend since college, was Chief Economist, he asked me to think about the Bank’s methods of measuring poverty, which greatly stimulated my interest in the topic. Most recently, I have worked with the technical advisory group of the International Comparison Program (ICP), which is currently hosted by the World Bank. The ICP is perhaps the world’s most ambitious statistical undertaking, and it presents immense (and not fully solved) theoretical and empirical difficulties, all the way from index number and statistical theory, through to which prices to collect and how. The people who work on the ICP include national income accountants, subject specialists (e.g. construction, or education), survey statisticians, as well as economists. I have made several good friends and mentors in the program, especially Alan Heston, one of the founders of the ICP, and Bettina Aten, now at the Bureau of Economic Analysis.  Finally, two institutions and their leaders have helped shape my work. One is the National Bureau of Economic Research whose President, Martin Feldstein, generated vast public goods for economics as a whole, and was a lifelong supporter of me and of my work. Much of my research on consumption and saving first saw the light of day at the Bureau, either in the macro seminars, or in David Wise’s group on aging with which I have worked for more than 20 years. Marty’s successor, Jim Poterba, continues the good work. At the National Institute on Aging in Washington, the late Richard Suzman’s creative energy, ingenuity and enthusiasm was responsible for bringing a generation of social scientists into health research, including me; his legacy is the change and advance that he brought to both fields. His friendship is sorely missed.  I will become an Emeritus Professor at Princeton in June of 2016. To Princeton, and to its units to which I belong, the Economics Department and especially the Woodrow Wilson School of Public and International Affairs, I express my gratitude for providing me with such a profoundly supportive environment and home for most of my working life. |
| Autobiographical |  |
| Podcast | Angus Deaton dreamed of being a pianist, a rugby player or a mathematician – but he just wasn’t good enough. After these setbacks, however, he discovered economics, and in 2015 he was awarded the prize in economic sciences for his analysis of consumption, poverty, and welfare.  Listen to a conversation conducted in February 2020 about some wild ideas, beautiful places and the role trout fishing can play in problem-solving. The host of the podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0820=AD  [Angus Deaton] Yes.  [Adam Smith] Hello, my name is Adam Smith and I’m calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  [AD] OK.  [AS] Congratulations on the award of the Prize.  [AD] Thank you.  [AS] Fist of all, how did you hear the news?  [AD] By the traditional telephone call.  [AS] Were you asleep?  [AD] At 6:10am this morning.  [AS] Were you asleep when it came?  [AD] No, I wasn’t asleep. We get up pretty early around here.  [AS] Right. And what was your first …  [AD] It was very early though, I mean we had not begun our day.  [AS] No, sure. What was your first thought?  [AD] Oh, my first thought was ‘Oh my goodness’. It’s really happening. I mean, you know, there are many, many, many distinguished economists out there who deserve this prize and so you always know it’s a possibility but that’s very different from … you know it’s a possibility but you also know it’s a fairly remote probability. And so I was just absolutely delighted.  [AS] Lovely, lovely, who did you most want to tell on hearing the news?  [AD] Well, of course the person I most wanted to tell is my wife and kids. My wife was with me and she could hear over the line, so that was the wonderful thing, we could share it. I didn’t have to go and tell her and say ‘Oh, guess what just happened’. So, she actually picked up the phone and she I think had a pretty good idea of what it was.  [AS] Gosh, what fun.  [AD] And so we shared that moment. And I’m now sort of texting and trying to get hold of my kids.  [AS] Of course. What fun it would be to be a witness to such a moment.  [AD] Well, one’s not actually dressed appropriately!  [AS] I don’t know, it’s an interesting question. What is the appropriate dress to receive the phone call in?  [AD] Right, exactly.  [AS] Your work seems to have been really a combination of a desire to measure and increase well-being and also a love of solving puzzles.  [AD] Yes.  [AS] Is that a fair summary?  [AD] That’s a very, very fair description. Just trying to figure stuff out, and also to try and bring data to bear on those puzzles and get some illumination. It’s a murky world out there and it’s hard to figure things out sometimes. You know, the best moments are when, together with … you bring information, you bring data to bear in a way that helps illuminate something that you just don’t really understand. Even if it doesn’t completely clarify it, it just, you know, helps bring it together.  [AS] So much of your work seems to have focused on collecting data at the very local level, at the household level. Why is that so important?  [AD] Well, you know, it’s individually … it’s about people in the end, and if you don’t understand … you have to understand what makes people tick, and you have to understand, you know, what’s good for them. And for me it’s always been about trying to understand behaviour and to try to infer from that behaviour, you know, how people are doing.  [AS] Happily we’ll have a chance to speak more about this when you come to Stockholm to receive the Prize in December. But I just wanted to ask, you’re a Scot, you were born and bred in Scotland, educated in Scotland. Now you’re in the US. Are you a Scot or an American, or a combination?  [AD] All of them, all of the above. I was very glad that, in the end, that the referendum did not make me forced to choose between being British and Scottish. You know I feel very Scottish, but you know my father was born in England, my mother was born in Scotland, and as I think someone else said in Britain at the time, you know I would feel like I would be being personally dismembered. And so I like the idea of being England and Scotland being a part of Britain.  [AS] Nice. And one last thing, the award of the Prize will of course mean everybody’s asking you for your advice on things. How do you feel about that prospect?  [AD] It’s OK. I’m old enough and I’ve been around the block enough enough to be able to say not embarrassing things … about those things.  [AS] Well yes, you sound well in control and very measured. Very good. Anyway, it’s a great pleasure to speak to you, congratulations again. We look forward to welcoming …  [AD] OK, thanks very much indeed. I appreciate that.  [AS] OK, thank you, bye, bye.  [AD] Bye, bye. |
| Interview |  |
| Q1 | What’s your story? |
|  | What’s my story? What brought me to this scientific field? A lot of luck, a lot of accidents. I certainly didn’t start out to study economics. I think, you know, when I was in high school I studied mathematics because it was relatively easy and it gave me a lot of free time to do things I liked to do, which is play rugby, listen to music and play the organ and things like that. But when I got to Cambridge I discovered that mathematics was a little more serious than I wanted it to be so I became an economist, more or less by accident, and I loved it from the beginning. I think I have been curious ever since I was a kid, I just wandered through economics finding things out. |
| Q4 | Has luck played a role in your life? |
|  | I think you say it right, how do I think about luck and why is it so central in the way I think about it? I think it has been central in my life and what happened to me and my family, but it has also played a big part in my work so that I could talk a little bit about both of those. My father grew up in coal-mining village in England and he would have still been there if the luck of the second world war had not drafted him into the army. It’s a funny sort of luck, but it worked out well for him and then he was going to go on a raid, a commando raid in the army in Norway in which everyone was killed and he didn’t go because he had the luck to get tuberculosis. There are two big pieces of luck without which I would not have been born in the first place and I almost think people who are successful tend to understate just how important luck is and what happens to them. I think generally we tend to underestimate how important luck is in our lives, especially those of us who are successful and also in my work I have tended to focus on one of the things that’s a very … I think a lot about inequality for instance. One of the forces that’s very important in generating inequality is just luck. If you and I start out with exactly the same amount of money or talent or education and then we are just bombarded by, you know, you get out of bed on one side in the morning, I get out of the bed on the other side in the morning. After a few years we are very different and that is just the cumulated facts of luck and I think that’s a force in life over time that’s creating a lot of inequality. |
| Q18 | Describe your Nobel Prize-awarded work in one minute |
|  | I have to describe my Nobel Prize-winning work in one minute. Wow, I have been trying to concentrate the time to 30 minutes for my talk on Tuesday, but one minute is much more of a challenge. I think I have spent a lot of my life trying to understand how people behave and why they behave the way they behave. Some of that is the classic problems of economics, which is if market prices change, how do people respond to those market prices and people have been studying that in economics for 200 years at least, maybe 300 years, a long time anyway. I find maybe better ways of doing that, this is a classic subject in economics so people worked on it for a long time, so I am not going to change everything, but maybe I made a little few improvements here and there. I also tried to study. When you look at how people behave you can say something about whether they are becoming better off or worse off, and I am always being concerned with human wellbeing or a human welfare and how that relates to the way that people behave in markets. That’s what I really do and that’s what the prize was about. Less than a minute. |
| Q67 | When do you do your best thinking? |
|  | It’s hard to tell, I do quite a lot of thinking when I am asleep, I think. You wake up in the morning with a solution to something you didn’t know. Or worse, and maybe more frequent, you go to bed thinking of a solution and you wake up in the morning realising it’s not a solution. But I love to go fishing and I have loved to go fishing since I was a kid and you spent time in Scotland, and you know how people like to go fly fishing for trout. Even a poor kid in Scotland can do that, you don’t need a lot of money to do that. I used to go fly fishing for trout and when I was fly fishing for trout I would … It’s a very strange experience because it’s a totally absorbing activity. I would go fly fishing and I would come home in the evening and my mother would ask “How was your lunch?” and I would say “I forgot to eat it” and she said “You have been out there for 12 hours, how could you forget to eat your lunch?” and I said “I was so busy fishing.” But at the same time I was thinking about things and once again it’s sort of like being asleep. I come home from fishing and maybe I would have solved something or I had thought of some new way of doing something or some new way into some problem and I still do that. Anne and I go fishing together a lot and, you know, we think about things. |
| Q68 | What does intelligence mean to you? |
|  | I don’t know, I’m not prepared for that question at all. It’s interesting because when you are a kid you’re always being tested for intelligence or that sort of things, so you think of intelligence as getting the highest score on an IQ test or something. It’s always been the case for me that I have certainly never been the most intelligent kid around. When I was younger I used to be very envious of people who were much quicker than I was. I think as I have gotten older I have understood that intelligence is important, but certainly it may not even be the most important thing. Curiosity is tremendously important. We talk in our profession a lot about what we call bearing down and that means that when you have a problem you don’t run away from it. You just try very hard to find out what that problem is about and get a solution or something like that. Bearing down is I think a very very important part of academic success. Intelligence I think is important, but only one of many things, because we see students all the time who are just smarter than anything you have ever seen. If you teach at a place like Princeton you see undergraduates who would completely break your IQ scale or whatever, and some of them are very successful, but most of them are not. Who knows, you need a lot of other things. |
| Q3 | Who was your most inspiring teacher? |
|  | I never took a course in economics and I never went to classes in economics, but I had people that I tried to imitate. There were people I wanted to be like and people I much much admired and some of those were people I knew, some of those were people whose work I read and I wanted to be able to write things like that. But the one I knew the best that I wanted to be most like and who used to wear bow ties like me and I really imitated all my life is [Richard Stone](https://www.nobelprize.org/prizes/economic-sciences/1984/stone/facts/) who got the Nobel Prize in 1984. I knew him very well and very early in my career, when I was just a research assistant, he saw something in me that he thought was like him and so he always used to say, “You and I are on the same side of the movement.” I never quite knew what that meant but I loved it. It was just wonderful to be told that. I always wanted to be like him and I have achieved some of those things. He would have been very happy to see me getting this award, after many … 31 years after him. |
| Q18 | What’s the biggest challenge facing the world today? |
|  | Part of the problem there is just picking which of the many things that really threaten us, but one of the things … I wrote a book a couple of years ago called *The Great Escape*. I am answering this question in a somewhat circuitousway. We all read the newspapers every day and the newspapers are full of terrible things, and the newspapers are full of all these threats. We have the threat from global warming, which has come up in past, we have the threat of the destruction of the eurozone from migration pressures or from financial pressures. There are terrible threats out there that we don’t always appreciated like the threat of antimicrobial resistance for instance, which I think is a very serious threat. The reason I came back to my book is because amidst all those threats we don’t realise in some ways how well we have done and just how much better the world is now than it was 250 years ago, how much better the world is now than it was 50 years ago. I was born in Edinburgh in 1945, the infant mortality rate in 1945 was higher than it is in India today. When my father was born in the Yorkshire coalfields in 1918 the infant mortality rate in England was about as same as it in Sub-Saharan Africa today and we think of Sub-Saharan Africa as being so far behind us in terms of health and wellbeing, but it’s not so long ago since we were there. We have made an enormous amount of progress, so when you think of these threats we should see these threats in context as threats to the progress we have made rather than sort of an existential threats, though some of them, like global warming, could of course be existential threats, but there is an awful lot out there that threatens us. |
| Q2 | What advice would you give your 20-year-old self? |
|  | Oh, I don’t know, what advice would I give to my 20-year-old self? I think I would tell him not to worry as much as I did when I was 20 years old, things will come out alright in the end and to do what is fun to do, to follow your curiosity. I was always worried that I didn’t know what I wanted to do and even when I was a little kid when people said “What do you want to do?” and I said “I want to be an engine driver” and after you stopped being an engine driver you never knew where I was going to be after that. I certainly didn’t know I was going to be an economist, but it worked out well in the end and I thought within economics that I was too scattered and worked on too many different things so when this prize was announced it was almost as if the committee had discovered the pattern of my life’s work in a way that I didn’t know it was. I think, tell the 20-year olds to follow what you are good at, to follow your curiosity and let the rest of things look after themselves. You can’t control it, you got to let it come. |
| Q11 | What’s the toughest challenges you’ve faced? How did you overcome it? |
|  | Oh, I don’t know, what’s the toughest challenge I have faced in life? I don’t know, like everybody else there have been personal challenges that have been very hard to overcome. I am not sure that I want to talk about those very publicly, but I think for all of us, everybody, again if I am addressing young people who are thinking about going into scientific careers certainly won’t absolve you of all the bad things that will happen in life and you know we all have a common humanity which means there are bad things and wonderful things which will happen to us too. You just have to trust and live them through. |
| ID | 0821 |
| Biographical | **My Educational Background** I was born and raised in Troyes, a town located east of Paris and north of Burgundy. Troyes was the capital of Champagne in the middle ages; its fairs hosted trade between Northern Italian cities and Flanders among others; Troyes has accordingly preserved a rich cultural heritage. My father, who passed away in 1992, was an obstetrician/gynecologist; my mother, who still lives in Troyes, taught French, Latin and Greek in high school. My parents as well of some of my teachers taught me the value of knowledge. I have two sisters, Marie-Claude and Laurence. My youth was a pretty uneventful and happy one.  I left Troyes after my baccalaureate to carry out preparatory studies at the Lycée Henri Poincaré in Nancy (1971–1973). I then entered the *Ecole Polytechnique* (1973–1976), followed by the *Ecole des Ponts et Chaussées* (1976–1978), a “doctorat de troisieme cycle” (a degree intermediate between a Master degree and a PhD, which has since disappeared) in decision mathematics from *Université Paris-Dauphine* (1976–1978), and finally a PhD in economics from MIT (1978–1981).  In high school, I was particularly interested in mathematics and social sciences (history and psychology in particular). I kept a strong interest in sciences at the *Ecole Polytechnique*, especially in mathematics with professors such as Laurent Schwartz, a Field medalist for his theory of distributions. Economics was not an obvious choice of study given my family background and the rather weak economic culture in France. As a matter of fact, I was rather unknowledgeable about the topic. I attended my first course in economics at *Ecole Polytechnique* at the age of 21. I was fascinated by the issues and liked how it combines rigorous analysis and social sciences. I started thinking about becoming an economist.  In 1976 I joined the engineering “corps of roads and bridges” (*Corps des Ponts et Chaussées*), an applied corps of civil servants dating back to 1716 and that most members enter after the *Ecole Polytechnique*, and accordingly studied at the *Ecole des Ponts et Chaussées*, a school created in 1747. The corps has been very generous with me, supporting me while I studied for a PhD in the US and thereafter in my career in France. I am still a member of the corps, and I’m obviously grateful for its letting me accomplish my passion for research.  This choice of the *Corps des Ponts* may seem an odd choice for someone about to become an economist; yet, the corps has a long tradition of excellence in economics since Jules Dupuit, who in 1844 discovered – as he was studying public infrastructures and railroads – the notion of consumer surplus (the difference between the price consumers as a whole are willing to pay – the “willingness to pay,” a measure of which can be derived from the demand curve – and the actual price they do pay) and the principles underlying market segmentation (through non-linear pricing and by offering a range of different qualities). This tradition of excellence was continued by François Divisia and René Roy, and many others since. Co-authors of mine who originate from that corps include Roger Guesnerie (whom I met during classes he gave at the *Ecole Polytechnique*, at the *Ecole des Ponts* and who was my adviser in Paris; he just retired from the economics chair at the *Collège de France*), Roland Bénabou (Princeton) and Bernard Caillaud (*Paris Sciences Economiques*). *Corps des Ponts* members have included five Presidents of the Econometric Society, the most prestigious international society for economics. The 70 former Presidents of this society (founded in 1930) include Irving Fisher, Schumpeter, Keynes, [Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/arrow-facts.html), [Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/samuelson-facts.html), and many Nobel Prize winners. *Corps des Ponts* “engineer-economists” outside the academic sphere have also always been very active in the public debate. **Why Economics?** Research in economics offers a twofold opportunity: addressing demanding, intellectually absorbing theoretical problems and contributing to decision-making in the public and private sectors. On the latter point, economics is a positive discipline (in so far as it seeks to document and analyze individual and collective behavior), but it is also, and ultimately, normative: Its goal is to “make the world a better place” by recommending economic policy measures. This strong normative content is, in my opinion, an important factor behind its appeal.  My engineering degrees (which in the French tradition were fairly mathematics-oriented) may not have been the most direct route to economics, but they were not as distant as might be thought.  My research deals both with methodological aspects and with applications to different areas of economic life. Applied economic theory offers some analogies with engineering sciences. The starting point is a concrete problem, either already identified or gleaned from observing reality or listening to decision-makers, public or private. Then follows a detour through abstraction. The essence of the problem is extracted in order to focus on its key aspects. In this simplification process lies much of the difficulty of the exercise; for tractability, the researcher cannot take everything into consideration. (S)he must select what is important and sort out what is anecdotal (i.e., its omission has little chance of changing the analysis). The experience of the researcher and discussions with practitioners are very useful at this stage, although ultimately a study of the robustness to underlying assumptions is highly desirable. Then, the model can be tested: econometrically if past data are available, and in the lab or in the field as well. One cannot underestimate the interaction between theory and empirics: empirical work needs theory, both to guide it and to make it useful for policy. Theory needs empirical work to strengthen the confidence in policy recommendations and also to suggest key omitted ingredients. **The PhD Years** In 1978, I traveled across the Atlantic to undertake a PhD in Economics at the Massachusetts Institute of Technology (MIT), graduating in 1981. MIT had a relatively low profile in the field of economics until the arrival of Paul Samuelson in 1940, but in the 1960s it became the best economics department in the world. The intellectual vibrancy, the professors’ dedication to teaching, and passion for an economic discipline which is both rigorous and application-orientated are all part of the culture of MIT. Ever since then, I have been deeply convinced that the field can advance only through mutual respect across fields and across research styles.  An anecdote here: being a civil servant, I was given two years to obtain my PhD and return to France. This was of course highly unrealistic. I rushed to pass waivers in the first-year core courses and (like my classmate Drew Fudenberg) to pass my generals at the end of the first year. Having been raised in an examintensive environment, I succeeded in doing so at the detriment of understanding economics. Indeed, after a core macroeconomics waiver, Rudi Dornbusch wondered how I succeeded in passing, understanding so little to the field; he wisely advised me to attend the course nonetheless (without taking the final exam), which I did. During this year of PhD coursework, my four fields of specialization were theory, public finance, econometrics and international economics. I then started a thesis under the supervision of [Eric Maskin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/maskin-facts.html), a 29 year-old MIT professor. At the end of my second year at MIT, I received a letter saying that I was allowed to stay in the US for a third year. This pressure-free third year turned out to be a fantastic opportunity, as I could devote the time to deepening my understanding of economics, take courses in fields I was completely unknowledgeable about, and start on new research projects.  At that time, Eric Maskin initiated MIT PhD students to game theory and information theory. Outside of regular class time, he very generously spent many hours every week in tutorials with fellow Maskin-advisee Drew Fudenberg and me. You can imagine how overjoyed Drew and I, like all former Maskin students, were when Eric deservedly received the Nobel Prize in 2007.  As I explain in the Nobel lecture, I was also very lucky to discover at MIT, thanks to Drew Fudenberg, industrial organization and regulation, fields I was not aware of prior to starting my PhD. I never took these fields for credit, but I sat in classes given by Paul Joskow and Dick Schmalensee and started writing papers with Drew on game theory and industrial economics (later, we also wrote a PhD-level textbook for doctoral students called “Game Theory”).  My thesis, as is often the case now, covered a range of topics: investigating the possibility of bubbles in financial assets (a bubble exists when the value of a financial asset exceeds the “fundamental” of the asset, in other words the discounted value of dividends, interest and rents it will yield today and in the future). Drew Fudenberg and I conducted a first analysis of sequential bargaining under asymmetric information and the resulting inefficiencies (“sequential bargaining” describes a situation where two or more parties bid until one accepts the offer of the other. The study analyzed the dynamics of concessions and the length of the bargaining process before an agreement is reached, when the parties do not have the same information). Finally, the thesis covered research (also in collaboration with Drew Fudenberg) on pre-emptive strategies through the accumulation of productive capacity in oligopolistic markets. None of these subjects were directly within the scope of Eric Maskin’s research topics but, like all great teachers, he never pushed his students to follow and refine his work, but rather encouraged and supported their own initiatives.  While Eric Maskin was instrumental in my development as an economist, I interacted with a number of professors other than my thesis advisor, as well as several talented fellow PhD students. Equally important was the opportunity to take a number of specialization courses (macroeconomics, public economics, international economics, etc.) in fields not directly related to my thesis. In economics, research fields change quickly and multidisciplinary knowledge is often essential to bring fresh thinking.  From 1980 to 1983, I also often spent time at Stanford, especially during the summer. While MIT and Harvard were the best departments for general economics, Stanford University and Northwestern University, whose researchers also often visited Stanford, were at the cutting edge of the revolution of game theory and information economics, ahead of their rivals on the east coast (with of course a few exceptions, such as Eric Maskin). **The Post-PhD Years: Research & Teaching, And an Important Encounter** After my PhD in 1981, I did not go on the job market and went back to France to work as a researcher at the *Ecole Nationale des Ponts et Chaussées (ENPC)*. At the time, a small economics research centre, CERAS, was being established with, notably, Serge Kolm as Director, Roger Guesnerie working there part time and Bernard Caillaud (with whom I have since often collaborated) as student.  In 1982 I began my work on the regulation of network industries and public procurement with Jean-Jacques Laffont, whom I had met in Rio in 1980 during an Econometric Society conference. Jean-Jacques Laffont, who had resisted the call of major American universities and was starting to establish a school of economics in Toulouse, regularly came to the ENPC to work on this subject. A student of Ken Arrow at Harvard (like Eric Maskin, with whom he had already done very innovative work), he had already contributed fundamental papers on information theory and public choice theory.  We thus began to talk about structural reform in sectors such as telecoms, electricity, postal services and the railways. The performance of incumbent operators in most countries across the globe was unsatisfactory, and economists and decision-makers were reflecting on potential reforms which could make companies more accountable for their costs, and on reforms that would facilitate competition in non-bottleneck segments. Jean-Jacques Laffont and I felt that the new theories of information and industrial economics could add an important perspective on this type of reforms and their limits.  In the fall of 1982, I received an offer to teach as an associate professor at MIT, which I accepted. Having just met my future wife Nathalie (we got married in 1984), who had to finish her master degree in law at the University of Paris in 1983–1984, I started to teach at MIT in September 1984. **1984–1991: Back to MIT, the Consolidation Years** The MIT years were seven years of research and teaching in a perfect environment: a reasonable teaching workload focused mainly on doctoral courses, no administrative work, and above all an exceptional intellectual environment. The economics department had a very collective and congenial atmosphere. The tone was set by professors such as Paul Samuelson, [Bob Solow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1987/solow-facts.html), [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/modigliani-facts.html), all Nobel Prize winners, who showed humility, refused to set themselves above others and emphasized the ability of the younger faculty to identify promising areas for recruitment.  Beyond the obvious intellectual attraction, I learned at MIT about mechanisms for good governance in a department and university, which later helped me think about potential reforms in the French university system.  MIT has always treated me very nicely. When in 1992 I decided to stay in Toulouse, I was invited to be a visiting professor for six weeks per year (four of them during July, when I teach a minicourse for PhD students). This year will be the 24th year of this yearly arrangement, from which I have benefited a lot. I always return there with much pleasure, even though older mentors such as Paul Samuelson and Bob Solow and many of my collaborators or people I discussed most with during my faculty years there have moved to other academic or non-academic institutions, retired or passed away: Olivier Blanchard, [Peter Diamond](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2010/diamond-facts.html), Rudi Dornbusch, Stan Fischer, Drew Fudenberg, [Oliver Hart](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2016/hart-facts.html), Jerry Hausman, Paul Joskow, [Eric Maskin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2007/maskin-facts.html), Dick Schmalensee, to mention only a few senior faculty, and many others as well. The key point is that a top department is more than its members; it is a culture. When I return, I am struck by the observation that the spirit, the creativity of research and the devotion to students has remained intact despite a substantial turnover. **1991: A Pivotal Sabbatical Year, the Jean-Jacques Laffont Effect** In 1991 I took out a sabbatical year in Toulouse to finish a book, “A Theory of Incentives in Regulation and Procurement,” with Jean-Jacques Laffont. In the 1980s we had discussed his plans to make the University of Toulouse 1 Capitole one of the best European universities in the field of economics, in particular thanks to the creation of an Institute for Industrial Economics (IDEI) which would be financed largely by partners from the public and private sector. The IDEI was established in 1990. Jean-Jacques Laffont and the group of friends who helped him in this initial phase had already managed to bring in a handful of leading researchers to Toulouse and wanted to use IDEI to provide a few more resources to develop a top-level European department. Jean-Jacques Laffont was the director of IDEI until 2002 (Jacques Cremer, Patrick Rey and Hervé Ossard have directed it since) and I have been IDEI’s scientific director since its creation.  In 1992, my wife Nathalie and I decided to stay in Toulouse. Our daughter Naïs was born in Boston in 1989; our two other children, Margot (1992) and Romain (1996), would be Toulousains. I returned to the Corps des Ponts, working at the University, and later developed a secondary affiliation with the Ecole des Hautes Etudes en Sciences Sociales (1995) and became a member of the *Académie des Sciences Morales et Politiques* (2011).  My motivation for moving to Toulouse was not to work with Jean-Jacques Laffont, since we were already working closely together despite being separated by the Atlantic Ocean. Rather, I was attracted by the collective ambition and the desire to improve the university environment in France (on a small scale of course). I had total trust in Jean-Jacques Laffont, who apart from his wellknown intellectual capacity (he would have been a serious candidate for the Nobel Prize), had remarkable human qualities as well as being a highly competent manager, which is quite unusual in the research world.  The project has been successful and many talented researchers have since joined the group. In the mid-1990s an economics doctoral program was created (entirely in English to attract the best students, both French and foreign – who learn French during their thesis) with a second year of doctorate courses added (a year of courses after the former advanced studies diploma, the DEA, now called M2); this second year, which exists in leading US PhD programs, was uncommon in Europe; and because it has no legal existence in France, we have been financing it on our own funds. It provides students with greater autonomy to write their thesis and a stronger foundation for research.  After my return to Europe, and even though my academic activities always take precedence, I also got more involved in policy-making over the years, through various economic committees such as the French Council of Economic Advisors (*Conseil d’Analyse Economique*), a non-partisan body that issues reports and opinions related to current policy issues, of which I have been a member since 1999, or other committees dealing with higher education and research. In the academic world, I have been long associated with the Econometric Society and the European Economic Association, which I presided over in 1998 and 2001, respectively. **Meeting the Institution-Building Challenge** In 2004 Jean-Jacques Laffont died of cancer in Toulouse. A terribly sad time. At his funeral, Eric Maskin spoke about his friend Jean-Jacques through a short description of a scrabble game played at the Laffont family’s summer house in Lacanau in the early 80s. Within two minutes, the scrabble game described many of Jean-Jacques’ expressions that were familiar to all his friends; it ended in Jean-Jacques’ winning with the word “magnifique.”  Chances were that the group would disband after such a blow, as Jean-Jacques Laffont had played such an important role in the ex-nihilo construction of the Toulouse economics department and Toulouse had no strong tradition in economics before him. The best tribute to him is that the group did not break apart and actually worked entrepreneurially to further his organizational project.  Beyond the obvious intellectual attraction, I learned at MIT about mechanisms for good governance in a department and university, which later helped me think about potential reforms in the French university system.  In 2006, we won a national competition to create 13 centers of excellence across all fields in France. This provided some financial resources and most importantly enabled us to create a private foundation, the Fondation Jean-Jacques Laffont/Toulouse School of Economics (TSE), which I directed until 2009 (at which stage vice-director Christian Gollier took over) and have been chairing its board since. This foundation has received, in roughly equal shares, public and private money. It enjoys the flexibility of private management, while being overseen by the French General Accounting Office (*Cour des Comptes*). Its board is almost entirely external (13 directors out of 15) and its scientific council fully external: two substantial departures from internally governed French higher education institutions. With the help of the university (Toulouse 1 Capitole) in which we are located, we were thereby able to introduce a number of other policies that may seem trite viewed from abroad, but have been innovative in the French landscape: for instance, creation of tenure tracks for junior faculty (who since the mid-1990s have never been recruited internally); new recruiting processes; creation of a “grande école” within the university system … Like other research-intensive departments in Europe, we also very much benefited from the creation of the European Research Council (ERC) in 2007. The ERC’s goal is to help Europe compete for international talent in an environment in which academic careers often lack attractiveness. It now plays a major role in keeping talented researchers in (or bringing them back to) Europe.  I have long been interested in other social sciences and done research at the border of, in particular, political science (independent agencies, party organization and electoral strategies), sociology (stereotypes and collective reputations, real and formal authority, cliques and collusion, modes of communication, leadership and influence) and psychology (longstanding research agenda with Roland Bénabou related to the psychological aspects of incentives on the one hand, and to motivated beliefs and identity on the other hand). I was therefore very happy when, in 2011, our project to create an “Institute of Advanced Study in Toulouse (IAST)” was selected by an international jury and funded within the program “Investissements d’Avenir,” At the date of its writing, this institute, directed by Paul Seabright, is still a start-up, but has got off to a very promising start. It successfully brings together political scientists, lawyers, psychologists, sociologists, anthropologists, biologists, economists and historians in a fruitful cross-disciplinary exchange.  The history of the Toulouse economics group is that of a team. I must say that this collective institution-building work has been very rewarding and I am grateful to my colleagues – too many to cite here – for what they have contributed. **With Gratefulness** This Prize is so much the outcome of a collective effort that it is impossible to acknowledge all those who contributed to it in their own ways: my wife and family for their unfaltering support; my teachers, colleagues and students for making me a better economist (they still are); the staff at TSE and MIT for their cheerful and very professional help (for instance my assistants Pierrette Vaissade, Emily Gallagher and Marie-Pierre Boe); and all those who have, often anonymously, helped us build our department in Toulouse.  A researcher’s main motivation is the pleasure of discovery, as described by the French mathematician Henri Poincaré: “Thought is only a flash in the middle of a long night. But this flash means everything.” But like everyone else, the researcher is not indifferent to peer recognition. I have received many more honors than I deserve, but I am definitely grateful for all of them. Some were particularly emotional as well as highly honorific: my first honorary degree in 1989 (8 years after my PhD) at the Université Libre de Bruxelles, a very risky choice but an incredible show of trust from ULB, and the one honor that my father was able to witness; the ceremony for CNRS gold medal in 2007 (the second awarded to an economist after Allais in 1978), to which numerous friends came and at which Eric Maskin, on his way back from receiving the Nobel Prize in Stockholm, delivered a hilarious speech explaining to the crowd in the Sorbonne’s Grand Amphitheatre that I actually was an impostor; and many other very memorable moments as well …  But there is of course nothing quite comparable to the Nobel experience. After the phone call from Stockholm and my calling Nathalie and my mother, students and faculty flocked to the building and gave me an incredible reception. Zillions of friends as well as strangers sent warm congratulations, many of them very touching; to the point that, under the general overload, the server broke down and a number of messages were lost.  There is something magic about such moments, as everyone, from the closely related to the more distant, rightfully shares the excitement. The following two months were a series of kind gestures of friendship, culminating with a concert given in my honor by Tugan Sokhiev and the Capitole National Orchestra in the evening of December 4, the day before my leaving for Stockholm. The Nobel week was, needless to say, very special. Sharing these moments with my family and friends was wonderful. Colleagues who had contributed to my receiving the Prize came to Stockholm, even though some of them could not even attend the ceremony and banquet due to the limited number of slots. Many others could not come, but shared the moment nonetheless. **In Conclusion** Research is largely a question of motivation and passion. The intellectual environment is absolutely vital, not only for learning and updating knowledge, but also for motivation. During my career I have been extremely fortunate to “find myself at the right place at the right time” and also to benefit from working with exceptional colleagues and students of the highest caliber, from whom I have learned very much and who often became dear friends. I may just have been very lucky, but I never regretted my early choice of a career as a researcher. As I said in my [Banquet speech](https://www.nobelprize.org/prizes/economic-sciences/2014/tirole/speech/), wisdom now forces me to return as soon as possible to my previous activities, to the colleagues to whom I am indebted for the Prize, in short to the wonderful life of a researcher. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0821=JT  [Jean Tirole] Hello.  [Adam Smith] Yes, hello. My name is Adam Smith calling from the website of the Nobel Prize, Nobelprize.org.  [JT] OK.  [AS] Congratulations first of all on the award.  [JT] Oh thank you.  [AS] How did you receive the news?  [JT] Well, I was called by the Swedish Academy, and I was very surprised. I was incredibly surprised at the honour and it took me half an hour to recoup from the call. I still haven’t recouped yet, but I immediately thought to all those who helped me, you know, with my career, and my family of course, and also my colleagues and students who have played a big role in my career. And in particular the person who started here in Toulouse, the Centre in Toulouse, Jean-Jacques Laffont who passed away and probably would have been, would have deserved to be with me today in this Prize for regulation and competition policy.  [AS] Yes, you worked together very closely but he died ten years ago, is that right?  [JT] He died ten years ago from cancer and he was my mentor and also a dear friend, yes.  [AS] Nice to remember him today then. You’re the first French economist to be awarded the Prize since [Maurice Allais](https://www.nobelprize.org/prizes/economic-sciences/1988/allais/facts/), over a quarter of a century ago. You must be very proud.  [JT] Well, I’m very proud, this is true, I mean. But, you know, it’s also being with the right people, in the right place, at the right moment. And, you know, it’s a team work too. It’s true Maurice Allais got the prize I think in 1978 or ’77 and he was a great mind, and it is very, yeah it’s very impressive for me.  [AS] And I suppose the timing could be interesting because, I mean, more and more governments are opening up their public monopolies to private stakeholders these days so your work is more and more relevant year by year I suppose.  [JT] Well, that has been a trend and we have been working with Jean-Jacques Laffont and my other co-authors to try to understand what regulation should look like in such industries. So, you know, opening access to entrants in a way that is going to keep the infrastructure built. That’s actually a difficult task. But it’s true that we need competition. That competition doesn’t come about easily in such industries by definition, so that’s why you need an economic framework to analyse this.  [AS] Who was the first person you told after you heard the news?  [JT] Well, I told my wife, and I told my mother too, and …  [AS] What did she say?  [JT] Oh, I first, to be honest, she is 90 years old and I first ask her to sit before I told her of the news. [Laughs] So, but yes, she was, my mother used to be a teacher, French and Latin and Greek teacher. You know, knowledge is very important to her, very important. And of course, for my wife and my children also. I see one of my daughters is on Skype with me from London and in fact it is actually quite moving for the whole family of course.  [AS] Yes, indeed, probably the whole world are trying to get hold of you now so I should leave you to their attentions. But for now our congratulations and we look forward to welcoming you to Stockholm.  [JT] OK, thank you, bye bye.  [AS] Bye bye. |
| Interview |  |
| Q18 | Could you please explain your Nobel Prize awarded work in simple terms? |
|  | My work, which has been awarded the prize, is on the regulation of industries. The regulations of industries include what we call anti-trust which mean that churches and courts and anti-trust authorities check on behaviour of large firms, to check there is no abuse of a dominant position so that the large firms don’t abuse their power. It also includes regulation, regulation of what’s called network industries, so the telecom industry, electricity industry, railroad, post offices and also a little bit of regulation of banks which of course has been a very typical issue lately. |
| Q19 | At what point did you realize your work was a breakthrough? |
|  | I am not sure that my work is a breakthrough. I have done a lot of different pieces of work because this field actually is very diverse and very rich. Two industries are not the same, all industries are different. For example, the payment card industry when you use your Visa or American Express card doesn’t work at all like Google or doesn’t work at all like the cement industry or the telecoms industry – they are all different so we have to build a number of series and /---/ work so as to understand those industries better. So it is more like a accumulation of work, not only by me but also by my colleagues and the entire profession which has made it easier for regulators and competition policy, for central banks, for regulators to deal with those large firms, but I don’t think there is really a breakthrough, it is more like an accumulation of different contributions by me and many other people. |
| Q1 | What brought you to science? |
|  | What attracted me about economic sciences, well, when I was a student, I didn’t study economics to start with. When I was in high school, I liked mathematics because it is very challenging and intrigued and then I liked social sciences, but I didn’t know anything about economics, I liked history, I liked psychology. And then when I was 21, I was studying engineering and mathematics, I discovered economics and I loved economics because it basically combined the rigour of mathematics and this human aspect of social sciences so then I decided to do some economics and I went for PhD at MIT in the US, but it was very late actually, I discovered economics. |
| Q3 | Who is your role model, and why? |
|  | I have a lot of role models. My adviser of course, [Eric Maskin](https://www.nobelprize.org/prizes/economic-sciences/2007/maskin/facts/), who got the Nobel Prize in 2007, he is a fantastic researcher but also a wonderful person, human being. And Jean-Jacques Laffont actually was my mentor. He started the old school in Toulouse, he was a great scientist, but instead of being at a top university in the US, he decided to be in Toulouse in France and to build something. He was a very dear friend. He passed away in 2004, he was a very dear friend. I learned a lot from him, both in terms of work but also, just like with Eric Maskin, as a human being. And of course, there are lots of other mentors, [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/), [Ken Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/) – you know, all the great economists and many others as well, you know, standing on the shoulders of science as Newton used to say. |
| Q7 | What were you doing when you heard you had been awarded the Nobel Prize? |
|  | It was strange, I was working with a collaborator with a grand proposal for the European Research Council and I was not expecting the prize, so I didn’t hear the phone. It was on the vibrating mode. So, the committee actually tried to call, and I understand they tried to call my secretary as well and so on and so forth. And at the end I heard the phone, it was vibrating on my desk and I took the phone. The number was +46, so I said: “Uh-oh”. I was very surprised and that was wonderful news. |
| ID | 0822 |
| Biographical | My grandparents on both sides immigrated to the United States from Sicily in the early 1900s, so I am a third generation Italian-American. The sec ond generation, my parents and aunts and uncles, were intelligent people, but they reached maturity at the start of the Great Depression, when there were few opportunities, so all worked manual jobs, when they could find them. I was the first in the lineage to go to university.  My father was a truck driver, but during the Second World War, he worked in the holds of battle ships at the Boston shipyard. The ships were lined with asbestos, so although my father never drank or smoked, he eventually died at age 70 of asbestosis, a form of lung cancer. My mother died earlier, at age 60, of cervical cancer, caused by high doses of hormones that were commonly prescribed in those days for women going through “the change of life.”  I was born in Somerville, Massachusetts, a suburb of Boston, but soon thereafter my parents along with one of my father’s sisters and her husband together purchased a two-flat building across the Mystic River in Medford. I doubt they had money for a down payment, but the whole neighborhood was in foreclosure, so my guess is that they just had to make the monthly payments to keep the house. I went to a Catholic grammar school (St. James) in Medford (grades 1 through 8). All the children in that school were from working class families like mine. There were 60 children in each class, but all learned to read, write, and do arithmetic, and many eventually went to college, all of which confirms recent research evidence that there is little relation between academic performance and class size or expenditures per student.  I went on to Boys Catholic High School, also known as Malden Catholic, in the city of Malden, which is next to Medford. The school had fewer than 500 students spread across four years. The teachers were Xaverian Brothers. High school sports are a big deal in the Boston area (as they are in Chicago). For some reason, which I’ve never understood, relatively small Catholic high schools are prominent in sports. I played basketball (poorly), ran track (second in the state meet in the high jump – not bad for a 5’8″ kid), played football (class B state champions in my junior year), and baseball (state semi-finals two years). I claim to be the inventor of the split end position in football, an innovation prompted by the beatings I took trying to block much bigger defensive tackles. I am in my high school’s athletic hall of fame.  I entered Tufts University in 1956, intending to become a high school teacher and sports coach. At the end of my second year, I married my high school sweetheart, Sallyann Dimeco, now my wife of more than 55 years. Sally was a student at Girls Catholic in Malden, just across the street from Boys Catholic. Our high school prom picture is below. We have four adult children (see picture below) and ten delightful grandchildren, all but two also adults. In an interview at the Nobel banquet, my daughter Elizabeth commented that the research success of an Economics Laureate is almost always enabled by a spouse who provides a nurturing family environment. In our family this is clearly Sally, whose family contributions dwarf mine.  At Tufts I started in Romance languages but after two years became bored with rehashing Voltaire and took an economics course. I was enthralled by the subject matter and by the prospect of escaping lifetime starvation on the wages of a high school teacher. In my last two years at Tufts, I went heavy on economics. The professors, as teachers, were as inspiring as the research stars I later profited from at the University of Chicago.  My professors at Tufts encouraged me to go to graduate school. I leaned toward a business school Ph.D. My Tufts professors (mostly Harvard economics Ph.Ds) pushed Chicago as the business school with a bent toward serious economics. I was accepted at other schools, but April 1960 came along and I didn’t hear from Chicago. I called and the dean of students, Jeff Metcalf, answered. (The school was much smaller then.) They had no record of my application. But Jeff and I hit it off, and he asked about my grades. He said Chicago had a scholarship reserved for a qualified Tufts graduate. He asked if I wanted it. I accepted and, except for two great years teaching in Belgium, I have been at the University of Chicago since 1960. I wonder what path my professional life would have taken if Jeff didn’t answer the phone that day. Serendipity!  During my last year at Tufts, I worked for Harry Ernst, an economics professor who also ran a stock market forecasting service. Part of my job was to invent schemes to forecast the market. The schemes always worked on the data used to design them. But Harry was a good statistician, and he insisted on out-of-sample tests. My schemes invariably failed those tests. I didn’t fully appreciate the lesson in this at the time, but it came to me later, in the evolution of work on market efficiency.  During my second year at Chicago, with an end to course work and prelims in sight, I started to attend the Econometrics Workshop, at that time the hotbed for research in finance. [Merton Miller](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1990/miller-facts.html) had recently joined the Chicago faculty and was a regular participant, along with Harry Roberts and Lester Telser. Benoit Mandelbrot was an occasional visitor. Benoit presented in the workshop several times, and in leisurely strolls around campus, I learned lots from him about fat-tailed stable distributions and their apparent relevance in a wide range of economic and physical phenomena. Merton Miller became my mentor in finance and economics (and remained so throughout his lifetime). Harry Roberts, a statistician, instilled a philosophy for empirical work that has been my north star throughout my career.  Miller, Roberts, Telser, and Mandelbrot were intensely involved in the burgeoning work on the behavior of stock prices (facilitated by the arrival of the first reasonably powerful computers). The other focal point was MIT, with Sydney Alexander, Paul Cootner, [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/modigliani-facts.html), and [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/samuelson-facts.html). Because his co-author, Merton Miller, was now at Chicago, Franco was a frequent visitor. Like Merton, Franco was unselfish and tireless in helping people think through research ideas. Franco and Mert provided an open conduit for cross-fertilization of market research at the two universities. Both eventually became Laureates in Economic Sciences.  At the end of my second year at Chicago, it came time to write a thesis, and I went to Miller with five topics. Mert always had uncanny insight about research ideas likely to succeed. He gently stomped on four of my topics, but was excited by the fifth. From my work for Harry Ernst at Tufts, I had daily data on the 30 Dow Jones Industrial Stocks. I proposed to produce detailed evidence on (1) Mandelbrot’s hypothesis that stock returns conform to non-normal (fat-tailed) stable distributions and (2) the time-series properties of returns. There was existing work on both topics, but I promised a unifying perspective and a leap in the range of data brought to bear.  Vindicating Mandelbrot, my thesis shows (in nauseating detail) that distributions of stock returns are fat-tailed: there are far more outliers than would be expected from normal distributions – a fact reconfirmed in subsequent market episodes, including the most recent. Given the accusations of ignorance on this score recently thrown our way in the popular media, it is worth emphasizing that academics in finance have been aware of the fat tails phenomenon in asset returns for 50+ years.  My thesis and the earlier work of others on the time-series properties of returns falls under what came to be called tests of market efficiency. I coined the terms “market efficiency” and “efficient markets,” but they do not appear in my thesis. They first appear in “Random Walks in Stock Market Prices,” paper number 16 in the series of *Selected Papers of the Graduate School of Business*, *University of Chicago*, reprinted in the *Financial Analysts Journal* (Fama 1965b).  The discussion above is a short history of my personal life and my early professional life. A full description of the work cited in the Economic Sciences Prize award is in the printed version of my Prize Lecture, “Two Pillars of Asset Pricing,” which will soon appear in the *American Economic Review* Vol. 104, Number 6, pp. 1–20 and is also found in a slightly edited version in this volume. A more complete review of all my research in finance is in “My Life in Finance,” *Annual Review of Financial Economics*, 3 (December 2011), 1–15. **Vita** February 2014  Born: February 14, 1939 – Boston, Massachusetts Marital Status: 55 years married – four children, ten grandchildren **Education** Undergraduate: Tufts University, Medford, Massachusetts; B.A., 1960.  Graduate: Graduate School of Business (now the Booth School), University of Chicago; 1960–63. MBA, 1963; Ph.D., 1964, Dissertation: *The Behavior of* *Stock Market Prices*. **Main Honors and Activities** The Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel, 2013  Fellow, American Academy of Arts and Sciences, 1989.  Fellow of the American Finance Association, January 2001. First elected fellow.  Deutsche Bank Prize in Financial Economics, 2005, first recipient.  Morgan Stanley American Finance Association Award for Excellence in Finance, 2007, first recipient.  Onassis Prize in Finance, April 2009, first recipient.  Chaire Francqui (Belgian National Science Prize), 1982.  Doctor of Law, University of Rochester, 1987.  Doctor of Law, DePaul University, 1989.  Doctor Honoris Causa, Catholic University of Leuven, Belgium, 1995.  Doctor of Science Honoris Causa, Tufts University, 2002.  Fellow, Econometric Society.  March 2001. Membre correspondant, Acadèmie des sciences morales et politiques, section Économie, politique, statistique et finance, de l’Institut de France.  Smith-Breeden Prize (with co-author Kenneth R. French) for the best paper in the *Journal of Finance* in 1992, “The Cross-Section of Expected Stock Returns.”  Fama-DFA Prize for the best paper published in 1998 in the *Journal of Financial Economics* in the areas of capital markets and asset pricing, “Market Efficiency Long-Term Returns and Behavioral Finance.”  Jensen Prize (second place) for the best paper in corporate finance and organizations published in the Journal of Financial Economics in 2001. “Disappearing Dividends: Changing Firm Characteristics or Lower Propensity to Pay,” (with Kenneth R. French)  Nicholas Molodovsky Award from the CFA Institute, 2006, presented for “outstanding contributions to the investment profession of such significance as to change the direction of the profession and raise it to higher standards of accomplishment.”  CME Fred Arditti Innovation Award, April 24, 2007.  Jensen Prize (second place) for the best paper in corporate finance and organizations published in the Journal of Financial Economics in 2006. “Profitability, Investment, and Average Returns,” (with Kenneth R. French).  Graham and Dodd Best Perspectives Award from the Financial Analysts Journal, 2012  At Tufts: Dean’s List (1956–60); Society of Scholars (1957–60) – a group consisting of the top two students in each of the sophomore, junior and senior classes; Phi Beta Kappa; Omicron Chi Epsilon; Class of 1888 Prize Scholarship (1959) – given each year to the school’s outstanding student-athlete; graduated Magna Cum Laude with honors in Romance Languages.  Malden Catholic High School Athletic Hall of Fame, 1992. **Work Experience** 1963–1965 Assistant Professor of Finance, University of Chicago, Graduate School of Business. 1966–1968 Associate Professor of Finance, University of Chicago, Graduate School of Business. 1968–1973 Professor of Finance, University of Chicago, Graduate School of Business. 1973–1984 Theodore O. Yntema Professor of Finance, University of Chicago, Graduate School of Business. 1975–1976 Visiting Professor, Catholic University of Leuven and European Institute for Advanced Studies in Management, Belgium. 1982–1995 Visiting Professor (Winter quarters), Anderson Graduate School of Management, University of California, Los Angeles. 1982– Board of Directors, Dimensional Fund Advisors. Member of the Investment Strategy Committee. 1984–93 Theodore O. Yntema Distinguished Service Professor of Finance Graduate School of Business, University of Chicago. 1993– Robert R. McCormick Distinguished Service Professor of Finance, Graduate School of Business, University of Chicago. **Professional Activities** American Economic Association, American Finance Association. Associate Editor, *Journal of Finance* (1971–73, 1977–80). Advisory Editor, *Journal of Financial Economics* (1974– ). Associate Editor, *American Economic Review* (1975–77). Associate Editor, *Journal of Monetary Economics* (1984–96) |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0822=EF  [EF] Hello?  [AS] Oh good morning, my name’s Adam Smith, calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  [EF] Oh yeah, I’m watching it.  [AS] (laughs) We were ringing to offer our congratulations and also to ask whether we might able to record just a two or three minute interview with you? We have a tradition of speaking to all new Laureates on the day of the announcement.  [EF] Okay.  [AS] Thank you very much. Well, as I say, first of all, our congratulations. And, how did you hear the news that you’d been awarded the Economics Prize?  [EF] I just got a call from you ([AS] laughs), from the … I was so frozen up, I don’t remember his name actually, but I remember Per Strömberg talking to me.  [AS] And how long ago was this? Just a few minutes?  [EF] Yes.  [AS] Yes. What was your first reaction on hearing the news?  [EF] Well, I was thrilled (laughs).  [AS] Have you … have you ever thought how you might react to this news, if it came?  [EF] Ah, I don’t know … I didn’t want to ever presume that I would win, so …  [AS] No, of course.  [EF] I didn’t really think about it a long time (laughs). But I knew I’d be thrilled, of course. Who wouldn’t?  [AS] And, your work stems from work you did during your PhD thesis. [EF] Right. Well, I mean, some of it, right?  [AS] Exactly. But you studied French initially, so what was it that turned you on to the study of asset pricing?  [EF] I … in my Junior Year at college I switched … I was getting kind of tired of French, and so I took an economics course and I loved it, and the rest of my two years in college I spent in economics. And then I came to the University of Chicago, got a PhD from the Business School, where I had the privilege of having Professors [Merton Miller](https://www.nobelprize.org/prizes/economic-sciences/1990/miller/facts/) and Harry Roberts, and Lester Telser and Benoît Mandelbrot coming around often, and the work on asset prices was just starting to warm up at that time, so, it was a good area for a young person to get into.  [AS] And this is, yet another Economics Prize for the University of Chicago. What is it about the environment there that is so productive?  [EF] Well, we have good people – you need that, obviously (laughs). And it’s a very interactive environment; people help one another out a lot. I mean, I couldn’t do what I did without the help of my professors at the time and colleagues since then and students since then. It’s a very interactive place, where everybody contributes to everybody else.  [AS] Wonderful.  [EF] Very unusual.  [AS] Just let me ask you one more question, please. The Prize will focus attention on the area of asset pricing very widely, so, of course your work is very well known to those in the Finance community. But people who have never encountered this before will suddenly be exposed to it. What, fundamentally, would you say was the relevance of your work, to the world at large?  [EF] Well, for this particular area, it’s really, the idea is really ‘how do you measure risk?’ And if the market is pricing things correctly, what is the relation between the expected return, which is the compensation for risk, and the risk. So …  [AS] Ok, perfectly said. Thank you very much indeed.  [EF] Okay.  [AS] Okay, we’ll look forward to speaking again soon. Thank you.  [EF] Sure, bye.  [AS] And congratulations. Bye bye. |
| Interview |  |
| Q18 | Could you explain your awarded work for young students? |
|  | If I would explain my work, I started back in 1962 working on the general topic is how do prices incorporate new information. It’s called the efficient markets hypothesis, but that’s it basically, how do prices incorporate new information. I devised a lot of tests about how you go about doing that, that’s one branch of asset pricing for which the award was given. The other one is what we call the asset pricing models which are really stories about how you measure risk and then what is the relation between risk and expected return, so they are basically risk return story. Those are kind of the two main branches of asset pricing which I have been working on for the last 53 years. |
| Q7 | What were you doing when you got the message of being awarded the Prize in Economic Sciences? |
|  | It was 6 o’clock in the morning so I was doing my back exercises which I do every morning pretty much. I was preparing a class because I had a class later that day, at 9 o’clock actually. Of course, I was surprised by the call, lots of people could get the prize, I was surprised, it was a pleasant surprise. I was kind of chocked, I went in to talk to my wife, I wanted to tell my wife and I told her “Well, you are going to have to go to Sweden” and she said “What for?” and I said “Because I got the Nobel Prize” and she said “No” and I said “Yes”. She said “No”, because the phone didn’t ring in her room, so she didn’t hear it. She thought I was putting her on, but in the end, she started crying. |
| Q1 | What brought you to Economic Sciences? |
|  | I started out studying French, I was going to be a French teacher and a sports coach. When I was very young I was very good a sports, my high school’s athletic hall of fame, so I thought that’s what I was going to be. Then I started to get tired of studying languages and I took an economics course and it was a revelation, so that’s what I have been doing ever since. What I tell young people now is “Don’t think you know what you want to do”, because you don’t know all the opportunities that are out there. You have to have a range of experiences before you choose what you really want to do and you better choose something you like because you are going to spend a lot of time at it. If you enjoy it, it makes your life a lot easier and more pleasurable and more satisfying than if you don’t enjoy it. |
| Q19 | At what point did you realize your work was a breakthrough? |
|  | From very early on, because there wasn’t anything new, everything was a breakthrough, so it was kind of easy in those days to do new stuff. Nowadays the young people coming along have it much harder because there is huge body of work they have to be familiar with. I didn’t have to be familiar with anything except basic economics and statistics in order to proceed with my work. At that time, the reason it all started at that time is kind of silly because what started it was the fact that computers came and computers made it easy for people to do work with data, that was very difficult to do before that. They had to use these old mechanical calculators that the people watching this have probably never seen in their lifetime. It was very difficult to do things accurately even. Then computers came and they were not as powerful as your cellphone is now, but they were a revelation at that time. All kinds of people got interested in doing work with computers and stock prices are one of the most easily available data that you can get so lots of people started to work on stock prices and returns and that’s where I started too. |
| Q3 | Who is your role model, and why? |
|  | My professor, I had two main professors when I was a student, one was [Merton Miller](https://www.nobelprize.org/prizes/economic-sciences/1990/miller/facts/), he was a Nobel Prize winner, another one was Harry Roberts, he was a statistician and those two people were my inspiration. I didn’t learn anything in classes at that time. When I started writing my thesis, that’s when I started to really take things into my memory and to work with them and keep them there. Basically, it was one-on-one work with these two people that did it. I was married at 19 and we had one child and one child on the way so that was inspiration, I wanted to finish my thesis pretty quickly so I could get a job. |
| Q18 | Can you tell us a little about your work in finance? |
|  | I started before finance was even a real discipline so there wasn’t anything, there weren’t any journals in finance or anything like that, everything was new at that time. It took a while initially before it made its way into practice, but then it was kind of like an explosion, waterfall. It took maybe ten or fifteen years before initially the applied world, the investment world, came around to the new way of thinking, but then when they did, now it’s basically instant so they are looking over our shoulders all the time and now to see what is going on, and see how they can incorporate it into their products. Finance is really very applied as a discipline, the research gets applied very quickly if it works, if it has anything to say. |
| ID | 0823 |
| Biographical | My name sounds very Nordic. Seven of my great-great-grandparents were born in Denmark and two more in Sweden. My first name comes from my Swedish great-great-grandfather, Lars Toolson, whose son also had the first name Lars. My middle name is from my great-grandfather Peter Hansen, who was born in Denmark. Fifteen of my sixteen great-great-grandparents, emigrating from Denmark, England, Sweden and Wales in the mid-nineteenth century, settled in Cache Valley, Utah.  My maternal grandfather George Rees was a country doctor who was accustomed to making house calls and often took payment in kind. Given his profession, he was one of the first owners of a car, a Ford Model A. My mother recently reminded us of that car: her first trip to Yellowstone National Park was made while riding in the rumble seat of that car, eighty years earlier. My grandfather attended the University of Chicago 70 years before I first worked there; he enrolled in a joint program between Rush Medical College and the university in 1911. During this time, my grandmother Veda Rees supported his medical training by working at Marshall Field’s, an iconic department store in downtown Chicago.  My paternal grandfather, Willard Hansen, was a proud farmer, committed to helping his sons launch their own successful farming ventures. When my father, R. Gaurth Hansen, told my grandfather Willard that he wanted to continue his studies beyond a four-year degree, my grandfather said, “If so, then be sure to study something useful, and become a doctor or a veterinarian.” My father was very close to his mother, Syble Toolson Hansen, and she quietly urged him to follow his academic and intellectual interests. Rather than a career in farming or animal care, my father became a biochemist and an expert in nutrition.  According to my maternal grandparents, my mother, Anna Lou Rees Hansen, knew my father from infancy. As the story goes, her father took her along when he made house calls to my grandmother Hansen and her infant son Gaurth. My mother grew up having the responsibility of taking important phone messages from patients who needed her father’s care from a very young age. Later, my mother and father were classmates in a one-room schoolhouse in Smithfield. Both of my parents went to college at Utah State University, my dad for three years before going on mission for the Mormon Church and my mother for four years and graduating in 1942. It was indeed a close-knit community in which everyone knew each other; both of my grandfathers served as mayor of the town of Smithfield, Utah.  My father received his bachelor’s degree from the University of Wisconsin, eventually leaving with a Ph.D. in 1948. His academic contributions included enzyme analysis of galactose metabolism in micro-organisms and animals, and painstakingly interviewing patients for family histories to seek clues to genetic transmission of galactosemia. In addition to applying his training to conducting nutrition surveys and consulting for the U.S. Department of Agriculture and the U.S. Public Health Service, he played a pivotal role in consolidating biochemistry at Michigan State University. I learned much about academic organization and management from dinner time conversations about my father’s administrative adventures.  The third of three sons, I was born in Urbana, Illinois. My oldest brother, Roger, is seven years my senior and has spent most of his professional career as a water resource engineer with the Bureau of Reclamation. My second brother, Ted, is five years my senior and is a geneticist and immunologist who just recently retired from Washington University in St. Louis. Ted took an active interest in my intellectual and athletic development at an early age. My athletic exploits ended after my second year in high school, but Ted’s interest in my studies had a long-lasting impact. Roger and I became closer later in life, when he came to Utah State University during my last years in college after serving in the Coast Guard.  In grade school, I developed a speech impediment, suffering from severe stuttering, especially in public. I still remember losing an election for class president in grade school in part because I had trouble speaking to the school. My reaction was to avoid public speaking for many years. Of course, as an academic one cannot really avoid speaking, so I have improved by practicing.  When I was almost 16, my parents moved back to Utah when my father was given the opportunity to become first Academic Vice President and later Provost of Utah State University. The university is in Logan, Utah, in Cache Valley and near Smithfield where they were born, so this was a return home for them. Although the move to Logan made it possible for me to get to know my grand-parents better, it was a difficult adjustment for me. I led an undistinguished academic career at Logan High School. Once I brought home double check marks under “does not respect authority.” In part, this reflected my independent thinking, but I was not an easy student for some of my teachers. My parents were patient with me, hoping I would eventually turn things around. My parents arranged for time off from school in order to work part time in a chemistry lab.  Once I entered Utah State University in 1970, I began to really apply myself. To my parents’ surprise, I was able to pay my way through my undergraduate education. I worked various jobs, including one in an animal science lab, a political polling venture, and working my parents’ “hobby farm” of about eighty acres in Cache Valley. Our crops included barley, oats, and alfalfa, all requiring manual labor to redirect water from mountain streams to the crops. Academically, I pursued interests in social science, majoring in political science, along with mathematics. Many faculty members took an interest in me, and three stand out. Michael Windham helped me better understand and appreciate the field of mathematics. Doug Alder, who taught an honors European history class, gave me some advice that caught me by surprise. He told me, “Lars, do not just follow the course of others but find your unique talents and aim to do something different.” The idea that I even might have unique talents had not occurred to me. By the end of my sophomore year, I decided to try combining my interests in mathematics and political science with the field of economics. A third professor, Bartell Jensen, helped me design an accelerated economics curriculum allowing me to take essential undergraduate classes in economics during my junior year, and some graduate classes by the time I was a senior. The fast pace helped me prepare for a Ph.D. program in a short period of time. Obtaining some distinction in mathematics and political science with my college degree, along with an accelerated curriculum in economics, enabled me to proceed to the University of Minnesota for a Ph.D.  At Minnesota, I was lucky in two ways. First, I was allowed to augment my economics classes with those from the graduate mathematics department. I pursued an eclectic mix of analysis, probability theory, and statistics classes that has served me well. Second, at Minnesota I met my intellectual mentors Chris Sims and Tom Sargent, when they were young scholars. Both had launched their stellar academic careers and were becoming recognized as major contributors to macroeconomics and econometrics. While Tom and Chris have different research perspectives, there was also much complementarity between their approaches. My training and experience at the University of Minnesota in the 1970s helped to shape my research and nurtured my interest in economic dynamics and time series econometrics. I learned to think of macroeconomic models as restrictions on stochastic processes, random processes that evolve over time.  Influenced by some lectures by Chris, I also began working on large sample approximation arguments; some of this research grew into my paper on Generalized Method of Moments (GMM) estimation. I found Tom’s willingness to engage me in joint research while I was still a graduate student very stimulating, and this grew into a long-standing collaboration. Tom and I have written many papers on such topics as rational expectations econometrics, present-value budget balance and robustness in decision-making.  Leaving graduate school, Carnegie Mellon University was an ideal first job for me. The faculty there in the late 1970s was skewed demographically: there were few senior faculty, and the younger faculty members were given much responsibility for the Ph.D. program and for running research workshops. Looking back on those years, I see what a valuable environment it was. I had the opportunity to share ideas with many young scholars who went on to distinguished careers.  Carnegie was where I first learned about asset pricing theory and began to explore with co-authors how to study empirical implications of these models in novel ways. I learned much from my collaborations: Bob Hodrick helped me explore international finance, Scott Richard worked with me on testable implications of asset pricing models, and Marty Eichenbaum collaborated on preference parameter estimation for dynamic macroeconomic models. My work with Ken Singleton was particularly valuable, developing novel ways to study the consumption-based capital asset pricing models. Working on these papers helped me to better motivate and reinforce the development of GMM estimation.  I visited the University of Chicago in the fall of 1981, and have been there ever since. The research environment at the University of Chicago is uniquely rewarding. It has historically been a highly interactive environment in which economics is taken very seriously. When I arrived, I had [Gary Becker](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1992/becker-facts.html), [Jim Heckman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2000/heckman-facts.html) and [Bob Lucas](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1995/lucas-facts.html) as senior colleagues. Each went on to win Nobel prizes and each set incredible examples of scholarship along the way.  After arriving, José Scheinkman and I shared many students and collaborated on papers. I gained great insight from our interactions and was sorry when he left later for Princeton. José and I had complementary perspectives on the field of finance. Even after he left, José and I continued our collaborations and have recently been working on methods for characterizing the term structure of risk prices. Surprisingly to me, the eminent labor economist Sherwin Rosen was another great colleague, proving to have a substantive interest in economic time series. I always enjoyed our interactions. Jim Heckman and I have had many conversations over the years on an extremely wide variety of topics related to economics. Jim served as a continual reminder that the best econometrics is grounded in economic analysis and empirical evidence. I can easily expand the list of influential colleagues. At Chicago, criticism can be intense but also very valuable.  The nearby business school has strong intellectual roots in financial economics, proving to be a valuable place to exchange ideas. Scholars there, like John Cochrane and later John Heaton, share my appreciation for economic dynamics and time series econometrics as fundamental ingredients in the study of asset valuation. In 2007, I was asked to join the Statistics Department at the University, further broadening my interactions on campus. My interdisciplinary research interests have been bolstered greatly by these connections across campus and diverse interactions as I explored productive connections between macroeconomics, finance and statistics.  With my father as an example, I have served the department in various capacities, including as its department chair and in helping to launch the [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1976/friedman-facts.html) Institute, which eventually became the Becker Friedman Institute for Research in Economics. Opportunities at other universities occasionally presented themselves, but I always decided to stay.  I have also had the opportunity to advise many excellent students. Starting with Ravi Jagannathan at Carnegie Mellon, I’ve advised sixty students and served on committees for many more. These former students now hold a wide range of posts in academia, the private sector, and government, with an equally wide range of accomplishments and interests. I greatly benefited and learned from my interactions with them while they were students; it has been rewarding to watch their careers flourish over the years.  Shortly after moving to Chicago, I met my future wife Grace Tsiang, a graduate student in economics at the time. I asked her out in part because she understood my unique sense of humor. We were married in 1984. It is impossible to explain adequately her influence on my life in a short space; she has been a continual source of support and encouragement. With her Chicago price-theoretic training, she has often pushed me hard to write better and think more broadly. She challenges me to explain why my seemingly abstract approaches to economic analysis deliver important practical insights. It has been a joy to watch how Grace has played a lead role in building the undergraduate program, the most popular major at the University of Chicago. Grace has helped many students discover their personal path to success by helping to design a curriculum to suit their personal interests, goals, and strengths. This example has helped me in my own student advising.  Grace’s influence in my life is far-reaching. We introduced Grace’s parents to the scenery and wildlife of Yellowstone and Grand Teton National Parks on a summer driving tour. While we were walking by Old Faithful, we passed other tourists on the narrow wooden walkways who recognized my father-in-law. A young couple from Taiwan had seen news stories there in which Grace’s father, Sho-Chieh Tsiang, discussed his economic policy recommendations. While I was initially surprised, in retrospect I should not have been. Grace’s father, along with Ta-Chung Liu, both Cornell University economists, were key economic advisors to the Taiwanese government in advance and during the “Taiwan Miracle,” a time period of rapid economic growth. This set quite an example for how to use economic analysis in the policy sphere.  My parents’ interest in farming and projects influenced how I spent time away from the office. Shortly after Grace and I were married, we acquired a log cabin in the countryside where we could relax and our son Peter and our energetic dog Rufus could run around endlessly on the weekends. The log cabin was on ten acres of wooded land near Harvard, Illinois, and provided many opportunities for outdoor projects. My father helped us build a garden, and our property included two ponds stocked with fish that provided food and habitat for wood ducks, blue herons, and snapping turtles. Near the end of the century, with considerable reluctance, Grace and I sold the property; we decided to spend future vacations in the mountains and national parks of western Wyoming, hiking, skiing and participating in other outdoor activities. While time away from Chicago includes fun activities with the family, it also serves as a valuable opportunity to regroup, recharge, and rethink whatever problems occupy my mind at the time. I have made progress on many research projects by having these blocks of time to think and write.  We have lived in Hyde Park all of our time together in Chicago. Hyde Park is a local community in Chicago that includes the University of Chicago and home for many of its faculty. Over the years we have enjoyed many interactions with others in this community, getting to know other families – many with ties to the University of Chicago. Our son Peter was born in 1992 in Hyde Park. It has been rewarding to watch him grow and thrive. The three of us have traveled all over the world and have I enjoyed these trips and our long conversations together. Peter will graduate as a mathematics major at the University of Chicago in the spring of 2014. Grace, Peter and I will march in the academic procession to honor the occasion.  Over the years, I have received some recognition for my professional accomplishments. I remember my dad, the biochemist, attending the ceremony when I was inducted into the National Academy of Sciences. He was proud, but was also still coming to grips with the notion that social sciences – including economics – should be part of the National Academy. I tried to convey that the scientific method can be applied even when it is difficult to verify models with testing and experimentation. I still jokingly refer to the social sciences as the truly “hard” sciences for exactly this reason when talking to biological and physical scientists about the issue. My two brothers, a sister-in-law and my mother-in-law joined our family in Madrid when I was recognized by the BBVA Foundation in 2010. In 2012, Utah State University gave me an honorary Doctor of Science degree. My mother, a USU alum, and many other family members were able to come to the ceremony – making this a very special event. Although my father was no longer living, this was all the more gratifying because my father received this same degree in 1991. I tend to shy away from the spotlight, but sharing these honors with family has always meant a lot to me.  Looking ahead, I will continue to investigate a variety of research related to the implications of uncertainty – both as a modeler of dynamic economies and as an econometrician. The real consequences of uncertainty are important for a variety of questions in economics with policy relevance. I look forward to advising more students and learning from them. I hope I can encourage young scholars to advance research in fields to which I have contributed. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0823=LPH  [LPH] Hello.  [AS] Oh good morning, I’m sorry to call so early. Is it possible to speak to Lars Peter Hansen please?  [LPH] This is he.  [AS] Oh hello. My name is Adam Smith and I’m calling from Nobelprize.org, the official website of the Nobel Prize, in Stockholm, Sweden.  [LPH] Yes.  [AS] We have a tradition of interviewing new Laureates on the day of the announcement just for two or three minutes, would you be happy to speak?  [LPH] I would be.  [AS] Thank you. Well first of all, many congratulations on the Economics Prize.  [LPH] Thank you.  [AS] How did you hear the news?  [LPH] I just got call a phonecall ten or fifteen minutes ago.  [AS] Were you sleeping when it came?  [LPH] No, I’m an early riser so I’d already taken my dog out and was eating breakfast and was about to head out to exercise.  [AS] And what was your first thought on hearing it?  [LPH] Very surprised … I was, of course, very pleasantly surprised. But I was very surprised.  [AS] And the prize links you with Eugene Fama and Robert Shiller.  [LPH] Yes.  [AS] That must be nice.  [LPH] Sure, yeah. I have great respect for both scholars and I’ve certainly been influenced by their work as well.  [AS] And …  [LPH] But they’re quite old, I guess I’m the youngster in this crew.  [AS] [Laughs] And your work really deals with the question of uncertainty and how you deal with uncertainty and risk …  [LPH] Yeah.  [AS] Yes, and for those who are not in the finance community and haven’t really encountered this before, how would describe what you do?  [LPH] I think there’s two aspects to it. One is , I have done work on, what’s called [inaudible] econometrics, which is statistics in economics and I did work, that I think of in a very simple way as trying to show how you can do [inaudible] without having to do everything. It’s kind of a funny way to put it, but if you want to study a dynamic economic system, what you’d like to be able to do is focus on the linkages, say, between asset markets and the macro economy without having to model everything at the same time. So my econometric work was kind of devoted towards that and that opened the way to think about studying and testing a variety of different asset pricing models and models that link the macro economy and security markets. So for me it’s in … and what certainly comes into play is that you need ways to certainly address the fact that investors, they respond to information and struggle with what the right view of the world is, and you need to have ways to capture those struggles and how they reflect in asset markets.  [AS] And how well do you think we can do that now?  [LPH] We’re making a little bit of progress, I think there’s a lot more to be done. I think the challenges have been, the modelling challenges have been made clear, and have opened the door for a lot of exciting future work.  [AS] Thank you. And you work at Chicago, and there have been so many economics prizes for the University of Chicago. What is it about the place that is so productive?  [LPH] Yeah, it’s interesting. I’ve been here for quite a while, so I’ve actually watched many of colleagues in the past get Nobel Prizes. And I’m very close to some of those. Its a few things, certainly having such distinguished faculty set great examples for me. A lot of my colleagues have been people with broad interests in economics, not just narrowly focused interests. And really convinced that economics is there to explain stuff, to understand the real world better, not just some intellectual game, and completely committed to that kind of mentality. And people here just don’t slow down. I’ve got senior colleagues like [Jim Heckman](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2000/heckman-facts.html), [Gary Becker](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1992/becker-facts.html) and [Bob Lucas](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1995/lucas-facts.html), they remain incredibly active and they remain incredibly energetic and for me have been remarkable role models. So I think it’s kind of the breadth and the fact that you really want to take economics seriously and it’s there to do something meaningful.  [AS] Thank you very much indeed. We would, if we may, like to speak to you at a greater length, in a couple of weeks time perhaps. But for now we should leave you to get on with what’s looking like it’s going to be a pretty busy day for you [Laughs].  [LPH] It will be, I don’t quite know what’s ahead for me, but I’m sure you’re right.  [AS] I guess the world’s press are going to descend on you. What do you feel about that? How do you feel about publicity?  [LPH] I haven’t really processed that part yet. So I guess I’m going to have to wing it.  [AS] [Laughs] Best of luck with it all and, once again, many congratulations.  [LPH] Sure.  [AS] Thank you.  [LPH] Bye.  [AS] Bye. |
| Interview |  |
| Q18 | Could you describe your awarded work for young students? |
|  | The way that I like to describe my work is that it’s developing statistical methods to do something without having to do everything. Let me elaborate a little bit on that. I am particular interested in these linkages between the overall macroeconomy and financial markets. One could imagine that to do this with a model you’ve got to have a detailed model of the macroeconomy, you’ve got to have a detailed, inside that a detailed model of the how financial markets work inside that economy in order to get off the ground and it’s very handy to instead just be able to do pieces of that without having to do the whole thing simultaneously. The methods that I developed were aimed being able to understand that piece without having to have a full-blown model of both, financial markets and the entire economy. Eventually one has to put all pieces together, but it is nice to be able to look at a portion of it and then want to come back and add much more details along this various different dimensions. |
| Q7 | What were you doing when you got the message of being awarded the Prize in Economic Sciences? |
|  | I am an early raiser and there’s two tasks I had that morning. The first was we had recently purchased a puppy so shortly after five o’clock that morning I took the dog out for a walk and then because I regularly exercise shortly after six o’clock I was in between taking the dog for a walk, came back, was just thinking about getting ready to go out and exercise. At that point in time the phone call came in – is this really happening? – so the first was, well the first indication that this was a call from Stockholm and the second person conveyed to be the message about the award. I started to believe it when the third person got on, the third person was someone who I knew for several years, Torsten Persson, and when he came on and congratulated me and I said: “Wow, I guess this is just really happening”. My wife could kind of sense that something was happening, she was in the kitchen so she quickly came out of the kitchen and witnessed this and figured it out. |
| Q1 | What brought you to Economic Sciences? |
|  | When I went to college I did a lot of experimentation across various different subjects. Early on I was a chemistry major, but I wasn’t very into doing lab work. I was actually working inside a chemistry laboratory and I just decided that probably wasn’t what I wanted to be doing. I became interested in the social sciences and I became very interested in the mathematics also at the same time. I explored political science, but by the junior year of collage I convinced myself that economics would be a good place for me to put together my interests in both social problems and economics and statistics. |
| Q19 | At what point did you realize your work was a breakthrough? |
|  | I worked on statistical methods and then I was very lucky early on in my carrier to have some great collaborators and some work that I did with Ken Singleton started getting a lot of attention and I guess at that point in time was the first time I had some feeling that this is an important research agenda. Maybe it was 1985 when Ken Singleton and I were given the Frisch prize. [Ragnar Frisch](https://www.nobelprize.org/prizes/economic-sciences/1969/frisch/facts/) was the first Nobel Laureate in Economics, /- – -/so it is called the Frisch prize and so on. At a fairly young age I was able to share that, I guess that was an indicator that the work was getting some attention. I remember being very much kind of enthusiastic and excited by the research, but it was very hard to tell which portions of the research were going to command the biggest attention. In fact even now when I work on research project, sometimes they get very low attention and sometimes they get a lot and while I am doing it, it is not always so easy to tell which of the ones will have the bigger influence. |
| Q29 | What message you would like to confer to the young people? |
|  | The one thing that was important to me was the fact that the education system was very tolerant of a late bloomer. In high school I would bring home so-called checkmarks for “does not respect authority” and I was not a very … my performance in high school was erratic. Certain teachers picked up on the fact that I had some talent, but I was not … I did not perform particularly well. The university that I went to happened to be the one where my dad was provost, but it’s a local university and that admits a large number of people. Just being able to go to a university, having some key-faculty members identify you as a person that might have some count and interest and then invest in and kind of allowing me as a late bloomer to still engage and embark in a career was tremendously important, so I guess the message would be first of all don’t give up on yourself early on and it’s good to be in environments that can tolerate late bloomers. |
| Q69 | Can you tell us a little about the University of Chicago? |
|  | Chicago is a very intense environment, I tell lots of people that, I don’t go to work looking for compliments because we are very critical of each other’s work and so. But there is a notion that economics is to be taken very seriously and it’s to be addressing important problems. It’s supposed to be rigorous and at same time relevant. That intensity, I think, has been very important for that environment and it dominates lots of interactions and I think it’s really been part of the reason Chicago as an economics department or as an economics community has been so productive. Economics is a field that advances and as it advances it necessarily gets more specialized. Chicago has been very good at resisting too much specialization. There is lots of interaction across areas and the like that I think can really be important for a creative research. |
| ID | 0824 |
| Biographical | In reflecting on my own life history on the occasion of the Nobel Prize, I find myself wondering about some traits of my research, about the kind of colleagues I have chosen to associate with in research, and why I even went into economics. I have used this occasion to think about the true origins of these inclinations and life directions.  I began my professional career in economics as an econometrician, producing a Ph.D. dissertation focusing on the econometrics of rational expectations models, Bayesian statistics and distributed lag estimation. Throughout my career I continued to be an applied econometrician, interested in the interface between theory and data, with an abiding appreciation of the importance of models and their careful testing.  However, as years have gone by, I have developed a research style that finds opportunities in avoiding so much specialization in any one field as narrow as econometrics. I increasingly tended to think that, for me, these econometric methods are best augmented with other approaches, if I am really to be useful in adding to an understanding that allows for better economic policy and practice. In doing so, I believe that some aspects of my research have evolved so as to be described by some as going down the wrong road. I have been more willing than most to entertain inventions or ideas that may seem eccentric. I have also tended to be relatively eclectic, borrowing more from other social sciences, violating economics profession norms. I have been more eager to go out and collect data (as for example by doing questionnaire surveys) that many people might dismiss as uninteresting, and happier to do mundane or low-brow research for little more reason than that it interests me and no one else seems to be doing it. I have also apparently tilted from most of my academic colleagues in choosing to devote some of my time to journalism, writing scholarly-trade books instead of purely scholarly books, and writing regular newspaper columns I believe that the experience of doing such diverse work has made me a better researcher even from a purely scholarly point of view, though I have to admit that others, with a different inspiration, may thrive more on specialization.  The aspect of my research that was stressed by the Scientific Background for the Nobel Prize, my econometric work in asset pricing, was a bit eccentric by some standards. When it was first published, and for at least a decade, I encountered considerable hostile criticism from some quarters. In fact, after I won the Nobel Prize, I received a postcard from a colleague who recalled talking with me at the AEA Convention in 1982, when I told him I wished I had never written a paper that now is cited as a centerpiece of my work. Over the years, the pain of the rejection I felt by many in my profession has faded, but it seems that as a young economist it was quite uncomfortable to be attacked for work that was seen as so out of line with professional conventions. Others in the academic world have had similar experience when their research seems to offend the norm, only to be recognized later.  In thinking about my early life, I can see some of the experiences and inclinations that preceded my career as a researcher who pursued somewhat unusual directions. In writing my life history I will work to create an understanding of formative life experiences and inborn personality traits that contributed in significant ways to my life course. **Family History** All four of my grandparents, Jurgis Šileris, Amelia Mileriutė, Vincas Radzvilas and Rozalia Šerytė came separately to America 1906–10 from Lithuania. They joined the Lithuanian-American community and within that they met and married here.  Two of these last names are Lithuanian spellings of German names, and my Grandfather had a decision to make on how to spell his name in America. Everyone agreed the name was German, and he lived in the town Gaurė in part of Lithuania that was substantially German in origin, and close to the Prussian border and the city of Königsberg (now Kaliningrad), but he had no known family history to link to Germany and he spoke only minimal German. He chose to spell his name George Shiller in America, while his brother coming around the same time chose Michael Schiller. The others became Amelia Miller, Rosalia Serys, and Vincent/William Radzvill.  We remain in contact with our Lithuanian relatives after more than one hundred years because both my grandmothers corresponded for the rest of their lives by mail with their families back home, and established a connection from them to me. My second cousin Nijolė Krotkutė in Lithuania has reported to me research on our family history, through the Radzvill branch, to Lithuania in the 14th century. After the Nobel ceremony in December 2013, we went to Lithuania and were regaled at a celebration with a dozen of our relatives from there, who concluded by seating me in the center of a circle of them, singing to me old Lithuanian folksongs. But, still, after more than a century of separation, Lithuania now seems largely foreign to me, and our sense of identity contains no more than a glimmer of our memories of this past.  I think instead that the individual migration to America (as to other destination cities or areas around the world) selects for people with independent spirit, who invest in a new culture, and who may also convey this culture to their progeny. My grandfather Shiller, reacting to the Russo-Japanese war in 1904, left to avoid conscription into the Russian army, which he considered an occupying army. My grandmother Miller came in part to avoid an arranged marriage to a man she loathed. My grandfather Vincent Radzvill came to attend college at the Cleveland Institute of Art. My grandmother Rosalia Serys came by herself via London, just to make a good life for herself. They all became part of a new Lithuanian-American culture that produced me. **Elementary School** In my first few years of elementary school at the Edison School in Detroit, I did poorly. I remember worrying that I might fail the second grade and be held back. Perhaps I had a touch of attention deficit-hyperactivity disorder (my wife is convinced that I still do) but I grew up before this was regularly diagnosed. My second-grade teacher, Mrs. Ashdown, would say to me “Bobby, if you get up from your seat one more time I am going to tie you down.” It wasn’t just independence of spirit; I was very restless and talkative, uncontrollably so, which earned me a very low grade in “citizenship.”  Whatever it was, I was very distractible, but also could be highly focused if something caught my attention, particularly written material. My mother used to tell a story about when she had taken a book from the library entitled *Care of the Feet* because of a minor foot problem she then had. She never had time to read the book, but I as a child found it and read the whole thing and told her all about it.  Even today, I am easily distracted by reading material, and will pick up articles on virtually any factual material if I have the time. Fortunately, some of my traits were discovered by my elementary school science teacher, Mr. Keener, who took an interest in me as well as my brother John and helped both of us form strong identities with scientists.  As a psychologist, my wife Ginny argues that attentional differences are important, and don’t simply represent “deficits” but also can underlie creativity. She is amused at my interest in giving interviews to reporters; perhaps she is right that the desire to talk that got me in trouble in elementary school is wellchanneled in expounding on my viewpoints with the media. **General Interest In Science** As a child, I was fascinated by any branch of physical or biological science. Even today, I find great excitement in discovering the complexity and variability of the world we live in, getting a glimpse into the deeper reality that we mostly ignore in our everyday human activities. I want to know diverse facts about such things as galaxies or molecules or proteins or insect species.  I have an impulse to want to know the little details, which are usually of no significance to non-specialists. I own a dissection microscope, and if there is an insect in the house I sometimes catch it and look at it under the microscope. I find myself marveling at these tiny things, finding them most impressive when one really sees them, and I enjoy wondering about how they came to be. I have felt the greatest admiration for true scientists, leaving me often wondering why other people seem to have so much admiration for actors and singers, who sometimes seem to know little about the real workings of the world.  In some sense science became a sort of religion to me. I do not remember how or when I fi st discovered [Albert Einstein](https://www.nobelprize.org/nobel_prizes/physics/laureates/1921/einstein-facts.html)‘s article “Religion and Science” that he fi st published in *The New York Times Magazine* in 1930, long before I was born. Perhaps my father told me about it. But at some point I found it and it became an inspiration. Einstein described his own visceral spiritual longings and said in his life they were transformed into a quest to discover the true laws of nature. He concluded: “I maintain that the cosmic religious feeling is the strongest and noblest motive for scientific research … in this materialistic age of ours the serious scientific workers are the only profoundly religious people.” **Family Impulses to Entrepreneurship** My father Benjamin Shiller had an exceptionally entrepreneurial attitude, even for America. This attitude was revealed in many little things he did that I recall.  My father’s greatest achievement in his life was the founding of his firm The Sahara Corporation, which manufactured fluidized-sand industrial ovens according to a patent he obtained on his invention. The event ended badly, with difficulties getting the business established and with interruption by his disabling heart attack in 1973, when he was 62 years old.  Watching him must have colored my thinking. I have always thought that my own profession should pay more attention to invention. Journals should publish ideas about how things could be done differently, and not just ideas about manipulating the usual government policy tools, or about which bad practices which should be made illegal. There should be more articles offering trial-balloon ideas about how economic institutions and methods could be set on a completely different framework, even if the ideas are not fully developed.  But there isn’t enough of a tradition for such thinking in academia, certainly not in economics, which seems overly focused on quantification of the behavior of the world as it has existed in the past. Undeniably, it is difficult to keep the right balance between innovation and development of established ideas. Management schools and law schools sometimes seem more attuned to practical economic inventions, though they tend often to fail to appreciate economic theory. **High School And College** While I was just beginning high school at Southfield High School near Detroit in 1960, my brother John, who is four years older than I, came home on a holiday from college with his assigned textbook, *Economics* by [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1970/samuelson-facts.html). Samuelson, at M.I.T., was later to win the 1970 Nobel Prize in Economic Sciences. I managed to read much of the book on that holiday, and this launched my interest in economics. I felt that economics, as Samuelson practiced it at least, really is a science. I was intrigued that economic models can actually explain many important things that happen in our lives.  I went off to college from Southfield High first to Kalamazoo College, a small liberal arts college in Michigan. I had a good freshman year there, but I wanted to try also the big university, and so I transferred for fall 1964 into the University of Michigan, where my brother John had also been a student.  I started writing there for the *Michigan Daily*, the student newspaper, and that surely was an important experience for me. I found the fact-finding that a newspaper writer does appealing. The experience of writing for a broad newspaper readership may seem completely different from the work of a scientist, but it did not to me. I saw a parallel in both roles as getting to the real facts. I was not writing grass for general readers, and I imagined my actual readers, however few, were as sophisticated as real scientists. The main point of a newspaper seemed to me to be that there was great value to there being a place for certain kinds of inquiry, about topics of immediate importance, helping us to tie events already in our mind into our broader world view.  A couple of faculty members at Michigan had significant influences on me while I was an undergraduate there. Kenneth Boulding, in the economics department, advocated what he called “general systems,” meaning an approach to research that is respectful of the interconnections between the various sciences. I have held the conviction ever since that these interconnections are vitally important. He also conveyed a moral imperative for economists to work to make a better world.  Though I had only one lecture from George Katona, in the Michigan psychology department, he was the first person to impress me about the importance of psychology for economics. He was perhaps the real beginning of behavioral economics for me. I kept his ideas in the back of my mind for years, but they then seemed to belong to the psychology department. I felt then that I had to make a choice between economics and psychology, one or the other, but could not have both.  As I approached the end of my undergraduate career, I agonized about what career choice to make. In fact, I took so many long walks mulling over choices that I was eventually diagnosed with a stress fracture of a metatarsal, which the doctor told me then, was typical of soldiers on long forced marches.  The two most prominent alternatives, beyond economics, were physics and medicine. I was very attracted to medicine, but I did not think the life of a typical doctor would be attractive to me; having appointments booked back to back seemed onerous. Perhaps my hyperactive nature made me prefer the relatively unstructured life of an academic. However, I could have gone into either field, and it may be just a matter of chance that it ended up to be economics, the chance event of my thinking at the time in my life when I needed to make a decision. **Graduate School** From Michigan I went directly in 1967 to enter the Ph.D. in economics program at Massachusetts Institute of Technology. There I met Theodore Keeler and Jeremy Siegel, fellow graduate students, who have remained friends for life. We went our separate ways geographically after receiving our Ph.D.s, but have remained close.  At MIT, I felt honored to have the man I so admired in high school, Paul Samuelson, as a teacher. I felt that there was something different about him, when compared with many other academics, for he approached economics as a real scientist. Some of this feeling may have been superficial. He, more than any other economics professor I had ever had, would make frequent analogies to principles of the physical sciences. But I think that there indeed was something fundamentally different about him too, for he approached economics with the kind of creativity and respect for evidence that befits a real scientist. Samuelson was important to me also because of his warmth to his students. He called me up on some occasions long afterwards.  My dissertation adviser and first coauthor was [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1985/modigliani-facts.html), who later also won the Nobel Prize, in 1985. I was attracted to him as an adviser because he combined an interest in economic theory with a really lively interest in the real world. He had a sense of reality that appealed to my own inclinations, and my sense of what science should be about. He attracted others with the same inclinations at the time, notably my fellow graduate student Mario Draghi, now head of the European Central Bank.  I didn’t fully share all of Franco’s interests, however. At the time I was a graduate student, Franco was working with Albert Ando at the University of Pennsylvania on a gigantic simulation model of the U.S. economy called the MIT-Penn-SSRC Model. I felt that that model was too ambitious and too cumbersome, and felt skeptical about its likely effectiveness as a forecaster. It turns out that a lot of other people were skeptical too, and this skepticism seems to have led to the rational expectations revolution, which focused on one aspect of such models, their representation of expectations.  While I was first attracted to the field of econometrics in graduate school, I decided later not to make econometrics as my narrow field of specialization. I came to think that for me, I needed to stay focused on the real economic questions, not just on methodology. Econometrics remains of course very important, and I have continued to follow the field and to publish and do some work using new econometric methods, such as the index numbers, but I long ago decided that I wanted to my own driving more on the big elusive questions that cannot be addressed entirely with statistical methods. **Marriage and Family** I met Ginny, now my wife of 37 years, in 1974 at an M.I.T. folk-dancing party. This happened while I was back in Cambridge, Mass. visiting the National Bureau of Economic Research and Harvard University and then M.I.T. I found a kindred spirit in her, and I am sure that whatever successes I have had are attributable to our good marriage, her intellectual companionship, and her willingness to allow me considerable time to spend on my research while she shouldered domestic responsibilities.  In our early marriage, while she got a Ph.D. in clinical psychology from the University of Delaware, we lived in Newark, Delaware, and I commuted to my job at the University of Pennsylvania. While at Delaware, Ginny regularly brought home books and articles about many fields of psychology, and I continued with my habit of picking up interesting reading material. I also went to parties with psychology faculty and graduate students, and thus picked up ideas that I wasn’t exposed to within the field of economics.  More recently, when I have engaged in more popular writing, Ginny has consistently provided a sounding board for my ideas. She steers me away from ideas that may be too eccentric, and helps me frame ideas in ways which make them more attractive and accessible for popular audiences.  We had two sons together, and they followed in my footsteps of not being top-notch students at early ages. However, I am proud to say that our older son Benjamin Shiller is now an assistant professor of economics at Brandeis University in Waltham, Massachusetts, with a specialty in information economics and industrial organization. Our younger son Derek Shiller is currently in the philosophy Ph.D. program at Princeton University, and also a lecturer at the University of Nebraska in Omaha. He is interested in epistemology, meta-philosophy, and Bayesian inference, interests that in some important ways parallel my own interests. While Ginny largely works as a practitioner, she has an academic appointment at the Yale Child Study Center and has written and lectured throughout her career. **Academic Career, Colleagues and Co-Authors** My first academic position after my Ph.D. was at the University of Minnesota in 1972–74. I had close colleagues, [Thomas Sargent](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2011/sargent-facts.html) and [Christopher Sims](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2011/sims-facts.html), who themselves won the Nobel Prize together in 2011. I was a great admirer of their work, and found interaction with them stimulating. But I gave up my faith in strict rational expectations models more definitively than they did, or sooner. My tendency towards skepticism began to divide us a bit. Eventually I just didn’t believe that these rational expectations models, or their finance counterparts, efficient markets models, could possibly be basically right, except in certain special cases. Maybe I overreacted against these models, but the good result was that I began to get much more interested in other social sciences, and learned a great deal.  Irving Fisher (1867–1947), who taught at Yale for his entire career, was never my colleague, as our lives overlapped only by a year and I never met him. Yet his example has always stimulated my imagination, and I have pursued somewhat similar ideas in a similar style. Both he and I developed a theory of index numbers. We both advocated inflation-indexed bonds, and we also both tried to launch new securities. Both he and I were ready to propose inventions, in his case including an analogue computer for solving economic equilibrium, a new map projection, and a new folding chair. Fisher and I both wrote books for a broader public and also wrote regular newspaper columns. I think some of these similarities represent a common belief that one needs to take risks in research, risks of appearing undignified or even unprofessional to some who judge on superficial qualities, but that one must work to be sure these activities are sincere and based on the best interpretation of scientific method.  My actual colleague at Yale, [James Tobin](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1981/tobin-facts.html) (1918–2002), who was awarded the 1981 Sveriges Riksbank Prize in Economics, also was an inspiration; he shared my respect for fact-oriented economic science, as well as a commitment to moral causes. He overlapped with me at Yale for 20 years.  The skepticism I had developed in graduate school about large-scale econometric models led me to do some work with Ray Fair at Yale, comparing modeling techniques. We concluded that at least one large-scale simulation model, his FairModel, does indeed seem to carry useful information about the future beyond that of other simpler statistical models and judgmental forecasts. So, my skepticism about these large-scale models, like the one my advisor Modigliani had worked on and that I had been doubtful of, was reduced substantially.  I met Richard Thaler when he was at Cornell University and I gave a talk there in 1982. He and I took a walk around campus then, and talked about the scientific method and where economics was going. This was the beginning of a long collaboration with him, specifically to organize seminars on behavioral economics, starting at the National Bureau of Economic Research in 1991 and ever since. He and I have together watched the economics profession become less isolated from other social sciences as the years go by. Our behavioral economics community has now expanded dramatically beyond just our colleagues in psychology: it now includes other social sciences as well, and biological sciences – most significantly, in recent years, neuroscience.  I wrote over a dozen scholarly papers with my Yale graduate student John Campbell, now a professor of economics at Harvard, on expectations models in finance. John has a precise mind and the energy to complete the ideas that come to him. He has been a major influence on all my work. He brought my initial results on the excess volatility in financial markets into much clearer focus, so that the results could be seen to survive formidable criticism.  Karl Case, who eventually co-developed with me the home price indices that are still produced today, also worked with me on understanding the bubble in home prices that preceded the recent financial crisis. Our 2003 Brookings Paper had an analysis that showed some of the dangers ahead.  I have also worked extensively with [George Akerlof](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2001/akerlof-facts.html), who won the Nobel Prize in Economic Sciences in 2001, on a 2009 book *Animal Spirits* about the foundations of macroeconomics. This book is a statement about ultimate causes of macroeconomic fluctuations, pushing macroeconomics back to its inevitable origins in human behavior. We are working on yet another book together, for we find a lot that is similar, or complementary, in our patterns of thinking. We work well together, augmenting each other’s imaginations, often indulging even more in speculative thinking together than we would alone, but our joint interaction also provides some discipline to our thinking.  My latest co-authors are my Yale student advisee Oliver Bunn, now at Barclays Bank, and others on the research team at Barclays, who have helped lend another new dimension to my work with their different experience and focus.  In all, I calculated that I have written joint work with 46 co-authors in my career. The 46 count includes 14 co-authors I had on a single project, who were finance specialists who collaborated on *The Squam Lake Report: Fixing the Financial System*, 2010. Long lists of co-authors for a single work are not common in economics as they are in the physical sciences, and my other co-authored papers were usually the product of close collaboration with only one co-author.  I have had a similarly large number of research assistants, both graduate students and undergraduates, and dissertation and senior-essay advisees. I have enjoyed my relation with all of them.  Throughout my career I have been able to find others who complement my own thinking in many ways, and my own research under my own name reflects their contributions. One of the greatest joys of academic research is discovering this meeting of minds with all of these people. It is something like the joy I remember singing hymns with a community in church, or singing folk song duets with my fiancée Ginny forty years ago.  On that note, it is most important to mention that, after having had Ginny’s indirect input on my research and writing for so many years in our long marriage, I have finally written an economics article with my wife Ginny, entitled “Economists as Worldly Philosophers,” that is an appeal to economists to take abroad view and to incorporate evidence from other disciplines into their work. This reflects attitudes that were consolidated by our marriage 37 years ago, that follows from discussions she and I had from the very beginning of our relationship, and that in some sense continue to define our marriage today. **My Entrepreneurship and Other Forays into the Real World** My father’s memory was probably the influence for me to be an entrepreneur. Not many economics professors start companies. There was something from my father that gave me the impulse to venture into the business world, to go outside the ivory tower. My drive to be an entrepreneur didn’t really come from the desire to become wealthy, but more from the desire to have a genuine impact on the world.  I consider part of my entrepreneurship to be books with inventions in them, which include my 1993 book *Macro Markets*, my 2003 book *New Financial Order*, my 2008 book *Subprime Solution*, and my 2012 book *Finance and the Good Society*. These books contained specific, though incompletely worked out, proposals for the creation of new financial markets and institutions and different types of regulations of financial markets, as well as a broad vision for the future of our society in the financial capitalism that is sweeping the world.  Then too I became directly involved in establishing companies that would pursue some of these ideas, picking and choosing among them for some that we might realistically get started with the help of a team of people. I continued to work full time at Yale University, which tolerates, even encourages, such activities as long as time devoted to them is limited.  In 1991 Case Shiller Weiss, Inc. (CSW) was launched with my colleague Karl Case and my former student at Yale, Allan Weiss, to produce an array of home price indices. That company was a success, for it led to the production of the Case-Shiller home price indices as well as an automated valuation model (AVM) for home prices that our team developed, and which we called CASA. We were the first company to have automated home valuation available to the general public on the Internet. We sold CSW in 2002 to Fiserv, Inc., and in 2013 it was resold to CoreLogic, Inc.  When we first sold CSW we kept a patent that Allan and I wrote, for *MacroShares*, paired long and short securities tied to an index. We used that to launch a new company MacroMarkets LLC, named after my book *Macro Markets*. We hired Samuel Masucci to be CEO of MacroMarkets and this new company licensed the production of our home price indices to Standard & Poor’s in 2006, creating the S&P/Case-Shiller Home Price Indices. At the same time, our company worked with the Chicago Mercantile Exchange to launch home price futures in 2006 on each of ten U.S. cities and on the U.S. as a whole. MacroMarkets LLC is no longer active, and unfortunately did not manage to establish the MacroShares we hoped would importantly change the economy. I was quite disappointed that my dream of establishing new markets that might benefit many people did not succeed better, but the experience has only strengthened my belief that such new markets will become important eventually.  Another line of work outside of traditional economics that I have consistently done over the years has been questionnaire survey work about economic attitudes and opinions. I did a questionnaire survey of individual and institutional investors within days of the biggest one-day stock market crash ever, on October 19, 1987, asking people why they sold that day. Starting in 1989, and to this day with the help of the Yale School of Management, I have been doing regular surveys of stock market participants’ attitudes. With my colleague Yoshiro Tsutsui, we extended these surveys to Japan. Starting in 1988, working with Karl Case, I began regular surveys of home buyers, inquiring why they bought when they did. In 1990 I began working with Maxim Boycko in Russia and Vladimir Korobov in Ukraine, comparing attitudes to free markets across countries. Many of the questions on these surveys are open-ended, with space for write-in answers, which I think help me to understand what people were really thinking at economic turning points.  These surveys are motivated by sociological and cultural-anthropological literature, which I think are underappreciated by most economists. It pays to be a good listener (without taking answers at face value) when trying to understand human behavior. I particularly enjoy listening to a large sample of people. If we don’t listen to their views at important historical junctures, we will later never be able to understand the events.  Undertaking such surveys is difficult, requires an organization, and is in a way entrepreneurial. Indeed, one of my colleagues at MacroMarkets, Terry Loebs, I think partly out of our collaboration, has just started a new survey research company called Pulsenomics  After having first begun to write for newspapers in college, I have in recent years returned to writing newspaper columns, with regular columns at newspapers that are members of Project Syndicate since 2003 and at *The New York Times* as a regular *Economic View* columnist since 2007. Still today, I do not regard these columns as simply popularizations of economics, but as part of a dialogue that informs academic research as well. Academic economics needs this kind of research. Economics is less of an exact science than are the traditional sciences, for it is more in need of approximations and has less control of >circumstances, and must keep up with continuing fundamental changes in our economic world. Hence a broad looking-around at what is going on currently is especially important for economics. **Looking Back On A Long Career In Economics** I suspect that most people with a scientific proclivity sense a sort of personal tragedy that the best one can do with one’s interests is to specialize quite a bit. One cannot understand it all, cannot work through it all, so one will never know the final answers to all of one’s deepest questions. Economics became my specialty. But I have discovered after many years that the tragedy is not really so severe, as I find myself interacting with people in more and more branches of social and even physical and biological sciences, and with kindred spirits in management and business and legal professions as well, as we try to find the truth. For me the sense of tragedy has faded with all the rewarding experiences and friendships with people of diverse intellectual positions I have had in the course of my career.  Having been devoted to the field of economics now nearly a half century, I think that I certainly made a good decision to go into economics. As I have detailed here, my temperament was suited for such career, and to pursue the research directions I chose. Even if economics lacks some of the exact science qualities that had been my original interest as an adolescent, the field seems to offer interesting challenges to those who admire the essence of a scientific method. I haven’t been disappointed by the field.  My various co-authors were chosen by me (or me by them, sometimes with the help of matchmakers) to help look for evidence of the truth behind theories. I have not found it difficult within the economics profession to find congenial colleagues who can share in this quest to genuinely advance our understanding.  Working with other people, colleagues and students, has been rewarding as well because with them I have found more and more that our work has a moral basis, in finding ways to improve lives and our society. |
| Autobiographical |  |
| Podcast | **”Pursuing of expertise doggedly can’t be the goal for everyone because being specialised means losing some breadth of understanding. We need both kinds of people”** In this podcast episode recorded in 2014 economist Robert Shiller speaks about technology and the role he thinks it will have in the future. He also shares his best advice for young economists and what he thinks about teaching online courses to large audiences. Together with the Nobel Prize’s Adam Smith, they also discuss stage fright, and how to overcome it.  Listen as we take you back to this conversation with Shiller, recorded in 2014 as part of the series ‘Nobel Prize talks’. The host of this podcast is nobelprize.org’s Adam Smith, joined by Clare Brilliant.  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.  Clare Brilliant: Welcome to Nobel Prize Conversations. We’ve gone back to the archives for this episode. I’m Clare Brilliant. I’m here with our host Adam Smith. Who are we going to hear from today, Adam?  Adam Smith: Clare, today it’s 2013 laureate in economic sciences, Robert Shiller.  Brilliant: How long ago was this conversation with Robert recorded?  Smith: I spoke to him in 2014, just the year after he’d been awarded the prize.  Brilliant: Why have we picked this episode to come back to you now?  Smith: It’s particularly interesting to hear how he speaks about technology. He’s really worried by it and also excited by it. He keeps referring to it throughout the episode. It’s just fascinating to hear him speak about it, me ask about it, and to realise just how fast things have moved.  Brilliant: I completely agree. He mentioned so many different things, Google Glass being one of them…  Smith: …which nobody would have heard of that anymore. And also, he speaks about not having to be in the same place.  Brilliant: Teaching groups of students online, it all seems quite new the way it was talked about 10 years ago.  Smith: Yeah. We’ve all sadly got rather used to that, but it was all new then. A lot’s changed in that time. The episode starts with him just returned from Davos, where technology was very much on his mind. Let’s drop in there.  Robert Shiller: Hello.  Smith: You are just back from Davos, is that right?  Shiller: Yes. It’s a great. It’s my 12th. I’ve always enjoyed them.  Smith: So does it change it attending as a new laureate in the economic sciences?  Shiller: A lot of things have changed. What really amazes me is people wanting to take their picture with me. I never had that before and at Davos, I had people standing in line to do that. I couldn’t believe it.  Smith: What is it that you like so much about Davos if you’ve been 12 times in a row?  Shiller: It seems to me that I know Davos, the World Economic Forum, is controversial, but it seems that Klaus Schwab, who’s the genius behind this annual conference, brings together people in business and government and some academics who are socially involved, care about people. Believe it or not, there are even billionaires who care about people. He invites them, but he doesn’t particularly, or he or his organisation, doesn’t particularly emphasise them. But people with, often with power or people who’ve published ideas and or in introduced legislation. They seem to be from so many different walks of life that I like to interact with them. I learn from them.  Smith: What do you think the secret of its success has been? Because it’s become a very influential meeting.  Shiller: I think it’s run well. I think it’s been going for many years and influence rises if you consistently fulfill some objective. I don’t fully understand why things are, why certain institutions have the prestige they do, but it seems to be not unwarranted.  Smith: Yes. I suppose one could ask the same questions of the Nobel Foundation and its prizes. There must be a temptation at Davos to speak about so many things. There’s so much going on, and you are probably being asked right, left, and center for comment on things. How do you resist the temptation to comment on things that are, so to speak, beyond one’s sphere of competence?  Shiller: This is a tension in modern society. We admire people who have expertise and know what they’re talking about. Unfortunately, pursuing of expertise doggedly can’t be the goal for everyone because being specialised means losing some breadth of understanding. We need both kinds of people. Davos is for broad thinking. We need the researchers who will focus in. It’s just there’s a problem that we can’t, the human minds at this stage in history, are still separate, and they still can’t pool all of their knowledge effectively. We have to work around that as best we can.  Smith: I like the idea of, “at this stage in history” as separate. Do you foresee a time when that situation improves?  Shiller: One thing that struck me about Davos is that I thought there was greater urgency. Now, maybe this is just where I went and who I heard, but there’s greater urgency about the potential problems for our society and our economy of artificial intelligence broadly construed. I tried on Google Glass for the first time as someone said ‘have a beer and try them on’. What made me think, wow, I think we’ll be wearing these, maybe I’m wrong but I can see that they would be addictive. Your everyday activities are going to be affected by new technology in the most transforming way. It is frightening. It was that sense of fear that I thought I detected at the latest Davos, and I detected all over the place, not just at Davos. Everyone’s worried just in the last few years, we have phones that like Siri on iPhone or others. You can talk to your phone, there’s nobody there. It answers you, something is changing that is profound, and I think has profound implications for our society.  Smith: Just getting back to that theme of specialisation versus breadth. How do you individually cope with that? Because I suppose conferring Nobel Prize status on you adds to people’s desire to hear you comment on things, on all nature of things. How do you deal with it?  Shiller: I think to some extent it’s a lifecycle thing. Younger people specialise. Then as they get older, they get broader. It’s natural. Some highly specialised research that might’ve attracted me when I was in my twenties. I don’t think realistically I’m going to do them now.  Smith: Yes.  Shiller: But I think maybe I’m trying to be part of a society that and trying to contribute what I can. Maybe it is true with age comes wisdom. At least we want to hope so. I should perhaps be more broad, but on the other hand, you are right that there comes a risk that you end up talking fluff. Things that everybody already knows. That’s the criticism that some people make of Davos, by the way. Everyone has to deal with these issues. They’re fundamental issues in our society.  Smith: As you say, it’s a tension. One wants to get engaged in things. How far can you go?  Shiller: I see that in my students. I think when I teach a class, if I get too technical and narrow, I’ll lose some students and they’ll not care anymore. But if I go the other way and I become too generalised, they’ll think I already know this. This is too broad.  Smith: Have you found yourself increasingly wanting to get engaged with bigger and bigger audiences as you go through your career?  Shiller: It’s happened, I think I’ve overcome stage fright substantially.  Smith: Did you ever suffer from stage fright?  Shiller: I remember in high school, I had something memorised, and I got up in front of the whole class to recite it, and I just couldn’t remember it. It was a panicky feeling. It was just my class. I’m beyond that now. When I was a child, my mother told me not to value celebrities. That they’re mostly fake and they have publicity managers. I’ve had a certain contempt ever since for celebrity status. That means I don’t exactly value large audiences. At least I have mixed feelings about it.  Smith: Understood. But at the same time, you’d like to be speaking to a bigger audience. For instance, you have a column. You have a regular column.  Shiller: Yes, I have two of them.  Smith: So that indicates that you want to say something to an audience who are very much outside the academic sphere.  Shiller: Why do I do these things? I don’t know exactly. I was on the student newspaper in my college, so I have a journalistic side. I could go either way. I think the directions you take in life are somewhat random. I was invited to give these newspaper columns. I didn’t approach them with the idea. Then it may have reflected my mood at the time, or a frustration with the progress of macroeconomics, which I was teaching, and a sense that maybe I could be more productive with a broad picture. But I don’t know, I never really went one way or the other. Sorry, I do both of these things.  Smith: That’s…  Shiller: Maybe it’s a joint product, that’s a term economists use. Certain kinds of productive activities benefit by being done together, like the family farm. You have chickens, and you have cows, and you also have wheat growing in the fields. The theory was that those activities work well together because they fill up different available time slots for your work. Similarly there’s the idea of a modern university that combines teaching and research based on a theory of education that Humboldt gave, ‘Bildung durch Forschung’ I think in German, ‘Education through Research’. That works. I think for a research scientist, it’s actually productive not to just stay in the lab all the time, but to take some time out and talk with young people about what you do, and talk in much more general terms than your current lab experience. Now, I guess I thought writing a newspaper column is like that. It’s teaching, it’s another kind of teaching, I suppose, but it just makes me think more broadly. I thought I would be more productive doing that. I also do public speaking to different kinds of audiences. Every time I speak to a different kind of audience, I reassess things from their point of view as part of my preparation. I imagine, what are they going to think of this topic? Then I actually hear from them. It seems to me to be productive. You can narrowly focus and you can close yourself into a room and just think, and you’ll come up with something that may be very intricate. But the question is, will it be right?  Smith: Another sort of outreach you do is you’re involved in is new models of education. You’re teaching these online courses through Coursera to vast numbers of students who are not enrolled in the university, but are following the courses online. Tell me a bit about that.  Shiller: I’ve actually done this several times, not through Coursera, through Yale.  Smith: Yes.  Shiller: I’m now starting with Coursera. Why do I do this? I was invited. One thing about creativity I’ve learned over the years, when someone thinks that you’d be the perfect person for some task, take it seriously because they often know something. When they want to pair you up with someone else – matchmakers – I think that also matters. I think that often you end up paired with someone very good. Good in the sense that you mesh well. The other thing about it, to me, it was kind of an experiment, because I think it’s like a step into the future through the online learning. I’m just wondering where this is going, and like everyone else, I have worries about it.  Smith: Yes.  Shiller: Is it replacing people? My Coursera course currently has 101,000 students signed up. I’m thinking that’s a lot of classes.  Smith: Yes.  Shiller: Am I contributing to the unemployment of other teachers?  Smith: Where are these people coming from? Where are these 101,000 students coming from?  Shiller: It seems to be all over the world, judging from emails I get. I’ve read studies that unfortunately it’s not as tilted as you might think toward poor underdeveloped countries. It tends to be the developed countries, and it tends often to be older people who already have college degrees. I’m thinking that perhaps I should try to reach out a little bit more toward the developing world. I don’t know if I can get these people. Those are the people who need it the most.  Smith: But I suppose it starts somewhere. I suppose you are using the internet to make teaching available, and it will grow. Is this the future of education? Does it in some ways, talking of redundancy of teachers? Does it make in any way, do you think universities redundant in the future? Can it all be done online? How do you see it evolving?  Shiller: I wish we had answers to these things. I don’t know. I don’t see how anyone can know. I think that the artificial intelligence, and I’m using the grandiose term for it. But generally, computers are going to change our society in the coming centuries, just profoundly. It’s going to matter. It’s going to be life or death. I hope that we have a community spirit so that people who are left behind in this rush to modernity won’t be hurt too much. I’m afraid for the future but now it’s not obvious what online learning is going to do, because we don’t even know what the future computer configurations will be. I’m kind of thinking that one likely outcome is that it won’t be MOOC, massive open online courses. That’s what I’m doing with so many students. I’m thinking, and I don’t know this, but that people will still want to have a personal relation with a real human being. Therefore the form will be different. It won’t be massive. It will be it just like having a class by Skype or some kind of communications device. You still have a small class. It’s still a teacher in the class. We don’t have to be in the same room anymore. I don’t know if you call that online, but maybe that’s where it will go. I’m a little bit sceptical that MOOCs will really take over because it lacks the human interaction. It’s too one-sided.  Smith: Absolutely. If one thinks back to all the conversations I’ve had with Nobel Prize laureates, most of them will talk animatedly about individual interactions they’ve had with mentors and colleagues in the past that have meant a very great deal to the way they’ve developed as thinkers. That individual interaction with people who can guide and lead one along the right path, stimulate one, seems to be absolutely key. This sort of learning where it’s mass distribution of information to people without the personal contact completely avoids that sort of that chance interaction.  Shiller: Yes. I’m thinking that’s probably valid for I’m guessing for our lifetimes or the lifetimes of our children. But eventually, who knows? Do you know this book by Ray Kurtzweil called *The Age of Spiritual Machines*? When I first saw that book, I thought it was a little bit far out, but now I start to worry that he might be right. He’s claiming that our computers will get so good at talking to us, that we’ll start to think that they have a soul. We’ll start to think that they’re our best friends, and we don’t need people anymore. I worry about that. Though, I’m guessing it’s not in our lifetimes.  Smith: But what a thought. Asking someone who was the mentor in your past? I’ll say it was this computer I was talking to. Just one more question about teaching. How do you teach a 101,000 students? How do you prepare your material for such a diverse group that you know so little about?  Shiller: I prepare it as if they were my own students. I’m thinking that my students come from all over the world. They tend to come from privileged families. I think I try to avoid US-centric talking. It’s something that has always bothered me, that Americans think that we’re the only country that matters. It’s just not true. I still fall into that trap somewhat, though. It’s hard not to because I live here, but I try not to. I don’t know what else I can do. I don’t see the 101,000 students. I see my students in my class, and I know how they react.  Smith: Switching gear a bit. I wanted to ask about within economics, what is the appeal of the study of the financial markets?  Shiller: I’ve always been fascinated by financial markets, going back to my graduate school days. Why is that? Because I think they’re kind of a infrastructure of some complexity that guides our economic decisions. It affects our society, our civilisation in ways that are not always good, but on balance are probably good. People around the world are embracing financial capitalism, more so over the last 30 years and rejecting collectivist or extreme socialist solutions. Why is that? I think because modern financial capitalism, while it can be cruel and hurtful at times, seems to bring prosperity and great diversity of outcomes. When I say finance is about just changing the wording a little bit, finance is about financing activities. What does it mean to finance an activity? Most things that people want to do require groups of people working together as part of organisations. The organisations have to have resources that they need. People have to be incentivised to pursue the goals of the organisation beyond their personal goals. It has to be something that lasts for a long time to work effectively. We just mentioned the Nobel Foundation, which is a nonprofit financial entity. We mentioned the World Economic Forum, another nonprofit financial entity. Finance is not really about making money. It’s about achieving activities. They have to be financed. That’s what Alfred Nobel did. He just financed it.  Smith: That’s a very positive view of finance. It’s not perhaps the view that a large section of the world’s population hold of the financial institutions. How do you go about, if you like, painting the financial institutions in a different light?  Shiller: I’m not saying that Alfred Nobel is typical of founders of financial institutions. He is a bright light in the field. But it’s not perfect. I view our financial institutions as dealing with the imperfections of people. We’re not all saints. I like to point out that 3% of the population in the world are psychopaths. We have people of all sorts. We have a system that allows place for competition, allows people to express themselves without hurting anybody, hopefully, and as constructively as possible. We’d rather not put the psychopaths in the mental institution. We’d like to have something that channels their activities in a productive way. That’s what we have. So people who are in charge of financial institutions are not always nice guys. But they’re there.  Smith: Yes.  Shiller: They’re doing much better than they might have in a different system.  Smith: So basically working to improve financial institutions is a task that is not just desirable, but absolutely necessary in your view. For you, what sustains you in your work, would you say?  Shiller: It seems to me that when economists are most productive, it’s usually when they suggest improvements to our institutions. Unfortunately, it’s a frustrating task because there’s a lot of inertia in our institutions, and we have vested interests for the present institutions who don’t want them changed.  Smith: One thing that the mention of financial institutions often conjures up for people is income inequality. I know that’s something you think strongly about.  Shiller: Yes. People have a very basic sense of justice and fairness. My recent book was called *Finance and the Good Society*. What is the good society? I think that’s a term that has been used to define and describe a society of people who are basically caring. Maybe they’re mostly out for themselves, but they’ll pick up a sharp object on the street before it hurts somebody. We take it almost for granted that there’s a basic human feeling that we’re part of society that cares about other people. But the problem is that finance does seem selfish, especially when you have people trading against each other. We have a game that looks very unfriendly, but it still goes by certain rules. After the game, it’s like in any game, the players can all get together and be friends again, that’s the vision. But it doesn’t always look nice. Remember there are some people who are not motivated in a human way as we’d like.  Smith: But as we currently have it, we have increasing incoming inequality in many places. It’s presumably in some ways the financial market’s job to try and help alleviate that.  Shiller: Now that’s one point that I’ve been trying to stress that finance is substantially about risk management. If risk management is pursued correctly, it reduces inequality. We have institutions of insurance, for example. What they do is prevent inequality from occurring because of any of the insured events. This is a powerful force removing inequality. As time goes on, insurance can become more and more comprehensive in dealing with risk that people face. I think that it does help reduce inequality. It focuses on risks that people are concerned about and care about. It produces a plan to deal with those risks as they happen. That’s part of finance. Portfolio management is another example that variations in portfolios in people’s investments create inequality. As we improve our ability to manage those, we lower inequality. Now, that’s not the whole story, but I’ve been arguing that these financial techniques could be applied much more broadly and help reduce inequality even more. I don’t think we can get rid of it completely, because some level of inequality is necessary to provide incentive. There still have to be penalties for failure or for lack of enterprise, lack of work. So that there will be some inequality at all times.  Smith: Some, but the magnitude we have is extreme. Do you think there’s enough attention paid to that?  Shiller: My feeling is that there should be attention, especially for the future, that inequality has been getting worse. If you extrapolate those trends the next 10, 20, 30, 40, 50 years, as our children grow up, it’s going to get maybe awful. I think that we can at least have a plan. There is no plan to deal with this. I’ve been advocating that countries should legislate automatic changes in the tax structure in the future, that will kick in automatically if inequality gets much worse. That’s an idea. It would reframe the discussion if they could possibly do that.  Smith: Because presumably one can’t really be satisfied with a world where there are such disparities between rich and poor.  Shiller: I think even the rich don’t want it. Would you like to be a multi-billionaire amongst starving people? I wouldn’t be happy. See, the funny thing about inequality is that some people are thinking that most nice people don’t even try to get rich. Most people don’t try. They take some job, like something that helps people. In extreme cases, it would be idealistic jobs like teaching, school teachers or nurses. They do this because they naturally like people. They feel a little bit annoyed when some people who don’t seem to share their social feeling use some very aggressive things to get rich. I can see that we don’t want a society that rewards that kind of behaviour too heavily. We can allow some of that. But I think it’s okay to have billionaires, but let’s not make it too extreme. I was looking at a list of countries by their gini coefficients, which is a measure of inequality, and was struck that Sweden on the list that I looked at, was the most equal country in the world. I thought that’s where I just was with the Nobel thing. But it didn’t seem so equal because I had dinner with a King and his family. I’ve never experienced that anywhere else in the world. I thought, in a sense, what the Swedes are doing, I can’t speak for them, but my guess is that their king and queen are hardworking people who are providing a certain kind of entertainment and meaning. It’s not inequality, but it’s fun to look at rich people within limits. I had a great time. The dinner was very memorable. A society that was too equal would just get boring.  Smith: Yes. Although you were taught by your parents not to think too much of celebrities, yes. Everybody enjoys watching celebrities, sometimes.  Shiller: That’s right.  Smith: Now you’ve spoken about the need to take a behavioural view of finance. This of course was at the root of your much publicised disagreement, if you like, between you and [Eugene Fama](https://www.nobelprize.org/prizes/economic-sciences/2013/fama/biographical/) about the interpretation of some of the data surrounding financial markets, the rational versus the irrational analysis of what’s happening in the markets. Do you think it’s important that people have focused so heavily on that discussion between you and Fama?  Shiller: I think the truth has some subtle dimensions which elude many people. You have to recognise that Fama has a real point. The real point is: markets are substantially efficient in some sense, in the sense that it’s not easy to make money quickly, and that the opportunities to make money that work fast and effectively will quickly be discovered and over exploited and then they’ll disappear. That’s a basic truth. I think there’s another problem, though. Once you recognise this truth, you can easily be misled into assuming that the markets are perfect as they are, when in fact they’re not. The efficient markets lesson can lead you into wrong conclusions. It can lead you into conclusions, for example, that we don’t need to regulate markets at all or very little or that we don’t need government invention intervention at all. That conclusion can have real costs.  Smith: The focus of the attention has been on rational versus irrational. Is that what people should be thinking about? Or is that debate really not the main point?  Shiller: The term irrational suggests things that might be… the problem is with our words, what does irrational mean? I guess, in English, that you think of a hysterical person, who’s screaming and shouting or something like that. You would say, come on, you’re acting irrational. But I think the kind of not so rational behaviour that underlies bubbles in the stock market is not quite so extreme. It is more like when you’re crossing the street and there’s a crowd of people crossing, you don’t look both ways because you just fall in step with the other people, and you don’t realise that maybe nobody is looking, and you’re all going to get run down if you don’t get out of the way. That’s a different kind of irrationality. It’s not suggested by the word irrational. The other thing is the word bubble. I got into conflicts with Eugene Fama about this during Nobel Week, and as the week progressed, I got the idea that he had a different concept about what I mean by bubble. He thought that I mean that a bubble is the time when people are acting very crazy and insane and it should be obvious to any rational person. Secondly, that it’s all going to collapse suddenly and finally, like a bubble bursts, and then it’s going to be all over. When you blow a soap bubble and it collapses, it’s catastrophic. It sounds to him like one of those religious people who say the judgment day is at hand. That’s not what I mean by bubble.  Smith: Yes, the semantics are important, but they’re unimportant in a wider sense too, because the words that economists use when talking to each other strike the rest of us as being part of the language that we always use on a day-to-day basis. It gives the idea that we should understand what economists are talking about, and therefore there’s a sort of direct translation to the world at large, whereas actually, there’s a subtlety to the way you use words that most of us wouldn’t be able to pick up on, I assume.  Shiller: We have different cultures. We have an academic culture, and we have a news media culture, and we have a business culture. We use similar words, but they have somewhat different meanings. Also, we use words differently because there’s different incentives. I think one problem with the word bubble is that news media, people love that word because it suggests impending catastrophe. Like this thing is going to blow any day now, you better take action. You’re going to regret it. That detracts a lot of the reader’s interest. The term is overused by news media.  Smith: Must be hard for you because having predicted, being credited with predicting two bubbles, people must be a, constantly wanting you to predict more, and b, expecting you to predict them if they do happen.  Shiller: That is a problem. I don’t really know how to predict these markets generally. One problem with economics is that history doesn’t repeat itself exactly. It’s constantly transforming into different kind. You see parallels between events, but they’re not the same. How do you get scientific? How can you make a scientific forecast? I think weather forecasting is so much easier, although we still have problems that things are changing, like global warming that throws them. But ideally, weather is weather and it repeats itself. We have a difference that our economic institutions are constantly changing. When in the United States, they created the Federal Reserve in 1913, they thought that would be the end of banking panics and recessions. Turned out to be wrong in 1929. But then we came up with new things like Federal Deposit Insurance Corporation that was supposed to prevent financial crises. It worked for a long time. There’s always adjustments and changes in the system that makes our previous solutions irrelevant, or at least only partly relevant.  Smith: But it makes it very hard for somebody who gets set up a little bit like a Delphi oracle by the press who say, ‘when’s the next one going to be then?’ How much do you worry about that pressure?  Shiller: Yes, I’m trying not to make forecasts. I get quoted by the news media, they asked me on tv ‘do you think the home price increases that we’re seeing is another bubble?’ I try to say something sufficiently cautious, but there’s a headline writer who writes a headline. It says we’re in another bubble.  Smith: Then I suppose it could become a self-fulfilling prophecy, because I suppose if people are listening to you too closely.  Shiller: Yes. That’s one worry. Unfortunately, I’m not fed chairman or Central bank head, so what I say isn’t taken that seriously.  Smith: Which opens another topic for discussion. Do you ever entertain ideas of taking on that sort of role of moving into that sort of political arena?  Shiller: I don’t think I’d be a good politician. My wife tells me that too. I have too much an impulse to speak the truth. I sympathise with these people. You can’t just speak your mind as a politician. Everything has to be calculated. I kind of like being just an outsider who says things that sometimes sound a little strange that wouldn’t get me elected.  Smith: What would you like people to say about you when your career is sort of coming to a close? Because at the moment, people tend to say he’s the guy who predicted the two bubbles.  Shiller: Yes, I think these two bubbles will be forgotten. They’re already fading. People don’t even remember the 2000 bubble, that’s 14 years ago. The peak in the stock markets of the world that occurred in 2000. I find my students don’t even know about 1929. Some of them, what happened in 1929? These things only persist if storytellers want to keep it going. The 1929 stock market crash has been retold so many times, and it’s kind of a legend like Mickey Mouse or Sleeping Beauty or something like that. Everything else gets completely forgotten. Of course everyone has forgotten eventually, but I guess I’d like my legacy to be some improvement in our economic system. I’ve been trying to focus on that. Now, unfortunately, academia doesn’t seem to want to reward people who think about how we could change our economic institutions. Very rarely does an economics professor ever write draft legislation and send it to a politician asking them to introduce this as a bill. We just don’t seem to be on that wavelength very much.  Smith: Why not? Because obviously you can.  Shiller: I think it’s law school professors may tend to do that more. It’s a division of labour and economists are kind of abstract and they see their role as explaining things the way they are.  Smith: It’s for others to pick up that ball, and use the knowledge.  Shiller: Yes. At Yale University until sometime in the 1930s, they had a Department of Economics, sociology, and government. It was all in one department, because all those problems are interrelated. As in other universities, they’ve split them up into separate departments, and as a political science department and a law school, and they’re all going their separate ways. If somehow we could integrate our thinking better, I think we’d be more effective.  Smith: Last thing I wanted to ask you about was influences. You had [Franco Modigliani](https://www.nobelprize.org/prizes/economic-sciences/1985/modigliani/facts/) as your PhD supervisor who also received the prize in economic sciences. Who influenced you as a thinker, would you say?  Shiller: I’m actually struck in terms of economics. The first thing that comes to my mind is my brother, who is four years older than me, went off to college and took economics. He had Samuelson’s textbook, [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/), he’s another Nobel Prize winner, by the way. He brought it over Christmas vacation, he brought his book home and left it out. So I read it. That’s the way I was. Anything that was left out, I would read. I was so impressed with Samuelsson. Here I was like 14 years old, and I got off onto economics from him. Later, I had the privilege of studying with him at MIT. I was impressed by the application of careful analysis to some of our societies’ deep issues of resource allocation of as I was saying, the support of organisations and institutions that achieve goals that people really want. Maybe Samuelson is my most important mentor.  Smith: Again, despite the advice of your parents to avoid celebrities, do you have sort of heroes that you look to?  Shiller: I don’t know where to start. One of them is John Maynard Keynes, the economist. What I particularly like about him is his first book in 1919 called *Economic Consequences of the Peace*. He criticised the Versailles Treaty, which would’ve imposed heavy reparations on Germany and practically predicted World War II in 1919, from the kind of rancor and anger that the reparations that were imposed by the Versailles Treaty would’ve caused. He wrote another book in 1936 about stimulus policy for depressions. I never met this person, but I thought that he had a independence of thought and a sense of importance that I found inspiring.  Smith: Gosh, yes. The ability to see through the kind of haze, apply one’s intelligence and focus correctly amazing attributes.  Shiller: I have many heroes, they’re never movie stars, and they’re never singers.  Smith: Great. Okay. That’s been absolutely wonderful.  Shiller: Great. Okay.  Smith: Thanks very much for speaking to me.  Shiller: Bye.  This podcast was presented by Nobel Prize Conversations. If you’d like to know more about Robert Shiller, 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 | 0824=RS  [RS] Hello?  [AS] Hello, may I speak to Robert Shiller please?  [RS] Speaking.  [AS] Hello, my name’s Adam Smith. I’m calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm.  [RS] Oh okay.  [AS] We have a tradition of recording very short interviews with new Laureates – could we just speak for a few minutes?  [RS] Yeah.  [AS] Thank you very much indeed. Well first of all, many congratulations on the Prize.  [RS] Thank you.  [AS] How did you hear the news?  [RS] I was getting ready to leave on a trip to Phoenix, Arizona, getting dressed, and I got called.  [AS] (laughs) And, what was your first reaction?  [RS] Disbelief.  [AS] Do you think the trip to Phoenix is going to go ahead?  [RS] Well, it’s at least postponed and it may be cancelled.  [AS] How do you think the day is going to pan out? (laughs)  [RS] (laughs) I don’t know, the phone is ringing an awful lot.  [AS] (laughs) Have you imagined yourself in this position before? Have you imagined how you might react to it if the news came?  [RS] Well, I certainly imagined, because friends of mine repeatedly tell me they think that … they expect me to win it. But I’ve always reflected that there are so many worthy people, that it’s difficult to … I thought it was a very low probability.  [AS] Who was the first person you told, after the household?  [RS] I woke my wife up and told her. Then I told the limo driver to cancel my limo.  [AS] Very practical. (laughs)  [RS] Well, I had to do it!  [AS] But, I just wanted to ask you how, just one question, because this is going to expose your field of research to a very wide audience, who won’t previously have been aware of it. It’s all to do with the predictability of asset pricing. How good a handle do you think we have on that? Do you think it’s very much a work-in-progress or have we got things pretty well worked out?  [RS] It’s very much a work-in-progress, yeah. I could briefly say that it’s … we’ve learned a lot about asset pricing, but there’s a basic human element in it that is irreducible. So, predicting what asset prices will do is partly similar to trying to predict what one person will do, and so could there ever be a science in predicting what you, Adam, will be doing? No, because there’s an irreducible human element. And that’s part of the reason why the field of finance will never completely understand asset pricing movements.  [AS] That was a lovely, very understandable comment. Thank you very much indeed.  [RS] Alright. (laughs)  [AS] Okay, so I look forward to talking again, at length, later in the autumn ([RS] Good). But for now, let me wish you a wonderful day, and congratulations.  [RS] Alright.  [AS] Thank you.  [RS] Bye.  [AS] Bye. |
| Interview |  |
| Q18 | Could you please explain your Prize awarded work for 13-14-year olds? |
|  | I was part of a three-man award. It’s a little hard because I think that the descriptions they might give will be different than mine, so I have to speak for all three of us. I think it has something to do … it’s hard for a thirteen-year-old, well, a thirteen or a fourteen-year-old would know about something called the stock market or the housing market. We have done work on understanding what drives these markets and why do they move through time, what patterns can you expect them to show? Looking at from my perspective I was always among the three of us the one that was most sceptical about rationality of human behaviour. Basically, these markets can go crazy sometimes and that was my view and it coloured the kind of work that I did. I don’t know that I proved that markets go crazy, but I think that the evidence that I showed inclines really in that direction. |
| Q7 | What were you doing when you got the message of being awarded the Prize? |
|  | I was getting out of the shower at home, I was dripping and the phone called and I was thinking, maybe I just won’t answer that, but then I thought, I had a suspicion I did answer it and it was quite a surprise. Then I woke my wife up and I said, I apologized to her, I said: “I don’t know if I should wake you for this but I just won the Nobel Prize” and she said: You are kidding!”. I had people telling me that I might win it, but they are friends of mine, so I then decided I will ask other economists, well-known economists: “Do you have friends telling you that you are going to win the Nobel Prize?” and they always said: “Yes”, so I figured it meant nothing that I have friends telling me that. |
| Q1 | What brought you to Economic Sciences? |
|  | When I was a child, I thought I wanted to be a scientist, I could have been in chemistry or physics or medicine. At some point in my college years I decided to switch to economics when I was in a junior in college. Why did I make the switch? I don’t exactly know. What struck me is that the choices young people have make about careers, there is so many things you can do, but you are expected to commit at a very early age to one line or another. I think it is part of the tragedies of ordinary living that you have do that. I would love to be all of those things, a chemist, a physicist, the medical researcher and how about a writer as well, but in reality you can’t do all of those things and you never know, you make a choice in life, you never know at the end whether it was correct. |
| Q11 | Have you ever had an eureka moment? |
|  | There were times in my research that I thought that I might have something important and I don’t think that I was thinking I would win the Nobel Prize for it though.  Robert J. Shiller: Thinking about the importance of my work and especially in preparing for my [Nobel Lecture](https://www.nobelprize.org/prizes/economic-sciences/2013/shiller/lecture/) I am struck at how many different people did important work and often they are forgotten, not completely forgotten, but largely forgotten. I am inclined to reflect that the Nobel Prize is quite a rare event. It is not enough to do good work, it has to somehow be really highly regarded. I just feel that there are so many other people who are highly deserving, I feel humbled by the thought of that. |
| Q3 | Who is your role model, and why? |
|  | At various times in my upbringing and in my carrier I have had role models that inspired me. It goes back to my high-school math teacher who inspired me to think that I could do mathematics and then in college I had a professor who was combining psychology and economics. I thought that is really solid, it sounded very real to me. Then in graduate school I had an adviser who connected me, made my economic work connected to real problems in the world and then over the years I have had others, there are lots. My wife has been an inspiration to me too, my wife is a psychologist and that has helped mould my thinking. |
| Q26 | What is the importance of the Nobel Prize? |
|  | What I like about the Nobel Prize … I was thinking of all the prizes that are given out, a lot of them are related to sports, the arts and often like movies. What strikes me as unique about the Nobel Prize is that it’s really high-minded, that’s why I am feeling very honoured to get it, but it’s for people who are contributing to our intellectual capital in the world. I particularly like the idea that we have a prize in chemistry. I am not a chemist, so I have no reason to say this, but I think we don’t give enough attention to our chemists. They are really important, and I just so much admire what they do. I am glad that somebody has this award for them and for the other scientist as well. |
| ID | 0825 |
| Biographical | I was born on December 18, 1951 in the New York City borough of Queens. My parents, Ernest and Lillian, were both public high school teachers of a subject that is probably no longer taught, called Secretarial Studies, which focused on typing and taking dictation via two methods of *shorthand stenography*, Pitman and Gregg. Their students were young women planning to go directly to work as secretaries after high school.  Not long after I was born, science became very fashionable in the United States, following the launch of the Sputnik satellite by the Soviet Union in 1957. I have an early memory of sitting in class listening to a radio broadcast of the launch of an American satellite, maybe one of the Explorer satellites in early 1958, when I was in first grade. That, plus the fact that my older brother Ted thought we should be scientists, was enough to convince me that science could be a career.  I was fortunate to also follow Ted in attending the Science Honors Program at Columbia University, which offered classes to junior high school and high school students on Saturdays. I thrived there, and I entered Columbia’s engineering school in the Fall of 1968 when I was 16, without having graduated from high school.  At Columbia, I spent a lot of time practicing Shotokan karate, which was very satisfying and which taught me that I could work harder than I had thought. I also found time to study Operations Research, which seemed to me to offer the promise of bringing scientific methods to the organization of at least some parts of human activity. After graduating from Columbia I moved to Stanford University in 1971 to pursue my Ph.D. in what was then Stanford’s Department of Operations Research.  I decided to do research in game theory after taking a class from Michael Maschler, who was visiting Stanford from the Hebrew University of Jerusalem. Bob Wilson agreed to be my advisor and rescued me from having what looked to be a very short academic career after I failed one of my Ph.D. qualifying exams. He was on sabbatical that year, but met with me regularly once a week for an hour. In memory, our meetings followed a kind of script: I would spend a while explaining to him why I hadn’t made progress that week, and then he would spend a while telling me not to be discouraged. Then I would describe some roadblock to further progress, and he would, as we finished our meeting, recommend a paper for me to read. Because his recommendations had always been very much on target, I would go straight from his office to the library and start to read the paper. As I did, I would think, this time Bob made a mistake, this paper has nothing to do with my problem. But then, somewhere in the middle of the paper would be a lemma or remark that helped me get around that roadblock …  My dissertation concerned the then popular models of games with transferable utility, and explored a generalization of the stable sets of outcomes that von Neumann and Morgenstern had proposed as”solutions” of such games. The progress of game theory has included the thorough exploration of a number of dead ends, and this eventually turned out to be one of them. But it gave me some tools (I proved a fixed point theorem on lattices), and an enduring interest in game theory.  I applied for jobs in OR and math departments, and was about to take a job in a math department – I had already decided which one – when, at the last minute, I got a phone call from Bill Zangwill, who was putting together an operations research group in the business school at the University of Illinois at Urbana-Champaign. I was completely relaxed at the campus interview, since I figured I already knew what job I was going to take. While I was visiting Illinois, I suggested that, were I to come, I would like a joint appointment in Economics, since that seemed where the most interesting problems for game theory were to be found. I flew home and found a telegram on my front door, saying that a formal offer was on the way. And so I became at least a nominal part-time economist early in my career.  As I prepared to leave California for Illinois, I paid a visit to Lloyd Shapley at the Rand Corporation in Santa Monica to tell him about my work. I recall a pleasant visit, in which he correctly conjectured how the proof of my fixed point theorem worked, and (if I recall correctly) ended with him driving me to the airport in his station wagon. Around that time, on a visit home to New York, I also visited Oskar Morgenstern at New York University to tell him about my work on von Neumann-Morgenstern solutions. I visited him several more times over the next three years, at NYU and once at his home in Princeton. My discussions with Morgenstern were always non-technical: he would reminisce about people, and I would tell him what I was working on. I sometimes felt a little like Columbus reporting back to Queen Isabella about the new lands that had been discovered in her name; Morgenstern appeared to be entertained by the fact that young people were working on game theory, and pointed out more than once that it had not always been so.  My arrival at Illinois is memorable for two psychologists I met there in my first year. The first, in the first weeks after my arrival, was my colleague Keith Murnighan. We were both new assistant professors in 1974. He had just received his Ph.D. in social psychology from Purdue. One of our senior colleagues suggested we would enjoy talking to each other, and we did, so much so that we decided to do some experiments together, on the kinds of games I had studied in my dissertation. Experiments were newer to me than game theory was to him, but over the course of the next decade we taught each other how to do experiments that would say something useful about game theory. He and I remember our early interactions differently, but we both agree that our first papers took many drafts to converge. Eventually we wrote a dozen papers together, exploring various aspects of game theory including the game theoretic predictions made by theories such as [Nash](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1994/nash-facts.html)‘s (1950) “solution” to the problem of determining the outcome of two-person bargaining. (Game theory was young, and many things that today would be called models of behavior, or kinds of equilibrium, were optimistically called “solutions,” following von Neumann and Morgenstern.) Keith and I, together with my graduate student Mike Malouf and our colleague Francoise Schoumaker, developed some experimental designs (such as binary lottery games, see Roth and Malouf, 1979, or probabilistically terminated repeated games, see Roth and Murnighan, 1978) that remain in use today. In 1978 I also took a semester leave at the Economics Department at Stanford, where I taught a course whose lecture notes became my first book, *Axiomatic Models of Bargaining* (Roth, 1978). Axiomatic theories of the kind initiated by Nash were beautiful, and I enjoyed pushing the theory forward, but their failure to account for the kinds of behavior we observed so clearly in experiments convinced me that these too were a dead end for economics.  We published some of those early experiments in psychology journals, which in retrospect reflects my misunderstanding of how experimental economics was going to develop. Psychologists had already done some experiments motivated by game theory, but by and large they had not controlled for the kinds of things that game theorists felt were needed to provide clear tests. I imagined that if we could demonstrate experimental designs that controlled for the things that seemed important to game theorists, psychologists might then proceed to test game theoretic predictions in the laboratory in a way that would be convincing to game theorists. But psychologists have their own agendas, and it quickly became clear that economists could not rely on a division of labor with psychologists, we would have to do for ourselves the experiments whose results we were curious to know.  I had the early opportunity at Illinois to teach a graduate course in game theory, and as I prepared my lectures I realized I didn’t understand as well as I would like many of the things I had been taught. My attempts to understand these things better led not only to my theoretical papers about Nash’s solution, but also to a set of papers about the Shapley value, which had been given an axiomatic foundation when introduced by Shapley (1953). I found that I could not convincingly explain some of the axioms to my students. Instead, I ended up writing a short series of papers in which I explored the idea of formalizing the Shapley value as a utility function for playing a game, so that a decision maker who was a utility maximizer could compare the option of playing a particular position in a given game with other opportunities. The tools for doing this, initiated by Shapley, have not gracefully generalized to the models of games we now mostly consider, but the usefulness of being able to evaluate the prospect of playing a game remains, and how to do that remains an open question. (I also eventually edited a volume of papers on the Shapley value, which we presented to Lloyd at a conference in honor of his 65th birthday and which came out as Roth, 1988.)  Two of my other explorations of models formulated by Shapley began streams of work which have continued throughout my career. In 1974, in the first issue of the first volume of the new *Journal of Mathematical Economics*, Shapley and Herb Scarf (Shapley and Scarf, 1974) explored a simple abstract model of exchange without money, in which each trader was endowed with a single unit of an indivisible good, which they called a “house,” and each player had preferences over all the houses. They also introduced the “top trading cycles algorithm,” which they attributed to David Gale, for using the preferences of the players to find a set of trades that would result in an allocation in the core: i.e. a set of trades among all the players such that no coalition of players could have done better by going off and trading among themselves. Andy Postlewaite had also arrived as a new assistant professor at Illinois in 1974, and he and I explored the model further (Roth and Postlewaite, 1977), from the point of view of understanding the structure of the core, given the known preferences of the players, a point of view that characterized what was then called cooperative game theory. But I had already begun to think that if one wanted to necessary to elicit the preferences of the players. In a subsequent paper (Roth, 1982a) I showed that the top trading cycles algorithm had the potential to do that in a reliable way, since it would make it safe for players to reveal their true preferences and would never reward a player who mis-stated his preferences.  I had already begun to think about the problem of making it safe for participants to reveal their preferences in connection with the very different kind of clearinghouse that was already in use for organizing the labor market for new American medical doctors. I don’t recall how I first heard about that clearinghouse, but I do remember calling the medical librarian (the medical school was in Chicago) and asking her to send me copies of the documents sent to participants in the clearinghouse, which in those days was called the National Intern and Resident Matching Program (NIRMP). Those documents described briefly how the clearinghouse worked: how residency programs advertised their job descriptions, doctors applied for interviews, residency programs interviewed applicants and rank order lists of residency programs were then collected from doctors, and rank order lists of doctors from residency programs, and combined into a match. The documents made what seemed to me to be a surprising claim, namely that the clearinghouse was designed in such a way that everyone – employers and applicants – did best if they submitted rank order lists that corresponded to their true preferences. As I began to explore the history of this medical match, I found that an early algorithm for turning the rank order lists into a matching of applicants to positions had been replaced after it was noticed that it didn’t have this property. It had been replaced in the early 1950s by an algorithm that I was able to show was essentially equivalent to the ‘deferred acceptance algorithm’ proposed and studied by Gale and Shapley (1962), in their seminal paper “College Admissions and the Stability of Marriage.” Gale and Shapley showed that such an algorithm could always produce a matching that was stable in the sense that no applicant and position would both prefer one another to [one of those to] whom they were matched.  In a paper called “The Economics of Matching: Stability and Incentives,” I showed that there were not any mechanisms that would always both produce a stable matching and make it completely safe for all firms and workers to reveal their true preferences. But the deferred acceptance algorithm could make it completely safe – a “dominant strategy” – for *applicants*to submit a rank order list that ranked jobs according to the applicant’s true preferences. Some of these ideas were ‘in the air’ and were also discovered by others: Dubins and Freedman (1981) also showed that the deferred acceptance algorithm could make it safe for applicants to reveal their true preferences and Bergstrom and Manning (1983, unpublished) also showed that no stable matching mechanism could do this for both sides of the market.  Some measure of how much economics has changed in the last decades can be glimpsed from the reaction my paper received when I submitted it to the *Journal of Political Economy* sometime around 1980. The editor, [George Stigler](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1982/stigler-facts.html), wrote me a polite letter in which he said that he could see that the paper was interesting and important, but that, except for the title, it wasn’t about economics at all. He didn’t think that a clearinghouse was a marketplace if it didn’t use adjustments in prices/wages to clear the market.  That paper was eventually published in *Mathematics of Operations Research* (Roth, 1982b), and it marked the beginning of my broader interest in “matching markets” − which are precisely the many important markets, labor markets among them, in which price adjustment alone doesn’t clear the market. Loosely speaking, these are markets that have application procedures, or selection criteria or other institutions, and in which you cannot simply choose what you want (even if you can afford it), but also have to be chosen.  Meanwhile, my relationship with another psychologist was also developing: I met and courted my wife Emilie, who was pursuing her Ph.D. in cognitive psychology at the University of Illinois’ famous psychology department. We met while folk dancing and were married in 1977. When she finished her Ph.D. in 1980 we moved about 25 miles West of Champaign-Urbana to the metropolis of Farmer City, Illinois (population about 2000), which was half way between the University of Illinois and Illinois State University, where she briefly taught. But we were only there for two years, after which the search for two jobs led us to Pittsburgh in 1982.  I began a long and happy sojourn in the Economics Department at the University of Pittsburgh (game theory was beginning to become firmly established in economics, and it had not yet thrived in Operations Research). Emilie began work at the Research and Development Center of the Westinghouse Corporation. The work that she and her colleagues did there gave rise to the new discipline that is today called Cognitive Engineering, which focused on understanding the kinds of information that people need to solve the problems they face, and making sure that this information is available to them, for example in control panels designed to help operate power plants, and deal with emergencies as they arise. Her work has since branched out to the design of many other things, and she likes to point out that I followed her into design.  “Design” is a noun as well as a verb, and in Pittsburgh I continued to look in detail at the design of the medical match, and the conditions in the market for new doctors that had given rise to it in the early 1950s. One of the problems that had plagued the labor market for new doctors in the first half of the 20th century was that competition for good students and for good jobs gradually caused positions to be offered earlier and earlier, until eventually medical students and hospitals were arranging the first post-graduation jobs of medical students two years before they graduated from medical school. Not only did this mean that jobs were being arranged so early that important information on the quality of the match was missing, but job offers were made at different times, and decisions about whether to accept offers were demanded very quickly, so that medical students often had little opportunity to compare different potential positions. The labor market clearinghouse that was organized in the early 1950s solved both these market failures, and my 1984 paper analyzed its design, and pointed to some difficulties it was then facing as a consequence of the growing number of medical students who were married to each other and who wished to find two positions in the same vicinity. This history of that market made clear how a clearinghouse that produced stable matchings had played a critical role in making the market thick, so that lots of potential matches could be considered together, and in dealing with the congestion that had arisen in the less centralized marketplaces that preceded it.  As I began to study the history of other labor markets I found that the “unraveling” of annual markets, in which each year offers are made earlier, and in which there is little opportunity to compare offers, is not so rare. My student Xiaolin Xing and I wrote a paper documenting this in over a dozen markets (Roth and Xing, 1994). It became clear that the timing of transactions was also important in understanding how markets and marketplaces work. Timing is important not just in terms of the date at which transactions take place, but also in terms of how much time they can take, and Xiaolin and I studied the congestion that can result when a marketplace doesn’t give participants enough time to evaluate as many of the transactions as they would like, in connection with difficulties that were being experienced in the market for clinical psychologists which was then run by telephone on a specified day (Roth and Xing, 1997).  While at Pittsburgh I also continued my theoretical and empirical investigation of stable matchings and clearinghouses. I also began to hire postdoctoral fellows, and with one of them, Marilda Sotomayor (who was introduced to me by David Gale) I wrote a monograph (Roth and Sotomayor, 1990) that helped introduce much of what was then known on the subject to a wider audience of economists. On the empirical side, I studied a set of medical clearinghouses that had been organized in different regions of Britain’s National Health Service to deal with the unraveling of their markets for newly graduated doctors (Roth, 1990 and 1991). Those clearinghouses, some of which succeeded and some of which failed, helped provide a kind of natural experiment to clarify how the design of a marketplace could influence the operation of a market.  Pittsburgh became an important center of experimental economics, and my colleague John Kagel (with whom I had earlier edited the *Handbook of Experimental Economics* (Kagel and Roth, 1995) and I were able to use a laboratory experiment to further illuminate the importance of how the clearinghouses were designed. We compared the performance of the failed clearinghouse design used in Newcastle and Birmingham with the successful design used in Edinburgh, under laboratory conditions that controlled away all the many other differences that exist between those three cities and their medical labor markets (Kagel and Roth, 2000).  It was becoming increasingly clear that how participants learned about the strategic environment would be an important part of market design, and my own understanding of some of the issues was helped by a series of papers that Ido Erev and I wrote about the learning behavior that we observed in experiments (e.g. Erev and Roth, 1995 and 1998). At Pittsburgh I was also able to continue studying bargaining behavior in the laboratory, with Jack Ochs and Bob Slonim (Ochs and Roth, 1989; Slonim and Roth, 1998).  My first chance to put market design into practice came in a phone call from Bob Beran, the director of the medical clearinghouse that was by then called the National Resident Matching Program (reflecting changes in the structure of medical practice that had led “internships” to give way to “residencies” even in the first year of postgraduate employment). The Match was already receiving very able technical assistance from Elliott Peranson, whom I had met previously, and Beran was now calling to ask if I would agree to direct the redesign of the underlying match algorithm, to address an emerging crisis of confidence in a way that would be consistent with changes that had taken place in the underlying conditions in the market for new doctors.  I still recall vividly that my gut reaction was “why me?” as I took that first call. I knew of course why I was a natural for him to call; I had written papers on the medical labor market and the history of the match, and a book on matching. But I also knew that the only things in the book that applied directly to the task I was being asked to undertake were the counterexamples. The book was about simple models of matching, and was full of theorems framed in terms of things that would always happen or could never be achieved: e.g. “the set of stable outcomes is always nonempty,” or “there doesn’t exist any stable matching mechanism that always makes it a dominant strategy for everyone to state their true preferences.” Complications were addressed by counterexamples, such as showing that when there are two-career households in the market, the set of stable matchings could be empty. In my role as a theorist, it had been enough to note that couples therefore presented a hard problem. In my new role as a market designer, they would become my problem.  I worked closely with Peranson, and we together developed the algorithm that continues today to run the “main match” for new residents (Roth and Peranson, 1999). Peranson is an entrepreneurial matchmaker, and the Roth-Peranson algorithm has since been adopted by a number of clearinghouses for more senior medical positions, and for other labor markets in (mostly) health care professions, including the market for clinical psychologists that I had studied as a congested decentralized market. In the course of that collaboration, and in my subsequent collaborations on market design, I have found that my tastes in theory have changed accordingly. I no longer just want to know that some features of the market might cause problems: I am also interested in learning more about how big those problems might be, how frequently they might be encountered, and how to work around them.  In Pittsburgh I taught courses both in experimental economics and in game theory. The 1974 Shapley and Scarf paper was among those I taught regularly in my game theory class, and students always expressed unease that the indivisible goods that were being traded, without the use of money, were called “houses.” It happens that the University of Pittsburgh Medical Center was then one of a small number of very active transplant centers, due in large part to the pioneering efforts of Pitt surgeon Thomas Starzl to overcome immunological barriers to transplantation. And in 1990 kidney transplantation was in the news as another surgeon, Joseph Murray, shared the Nobel Prize in medicine, for having performed the first successful kidney transplant (between identical twins) and for his subsequent work in overcoming immunological barriers between donors and patients. In my class notes from around that time show that I started referring not just to “houses” but to “kidneys” as indivisible goods that could be traded among patient-donor pairs to obtain better matches. I didn’t yet imagine that this would one day be a practical design problem (the first actual kidney exchange was still in the future), but kidney exchange gave me an easy answer to students’ questions about why money could not be used: it had been against American law since 1984 to buy or sell organs for transplantation.  Pittsburgh is also where our sons Aaron and Ben were born, in 1984 and 1991, and the ease of raising a family there made it hard to leave. (A word of advice for parents: pay attention! Childhood moves fast. As I write this in early 2013, our younger son is in graduate school, and our older son is a professor of computer science …) It was hard to leave Pittsburgh, but as our older son was preparing to enter high school (and hence change schools in any event) we moved to Boston in 1998, where I took a position at Harvard, dividing my time equally between the Department of Economics and the Harvard Business School.  At Harvard I occupied two offices and crossed the Charles River twice almost every day, as I would walk from HBS to Economics and then back to get on my bike or in my car for the trip home. It was a short walk, but it sometimes felt like a big change in perspective. As a market designer I was glad to be able to work on both sides of what sometimes seemed like a wide river, between theory and practice, and simple abstraction and messy detail.  One of the first markets I began to study in earnest after arriving at Harvard was the market for top law school graduates who seek prestigious work for a year or two as law clerks to appellate court judges. This is a market that has frequently unraveled (Xiaolin Xing and I had written about it) and just as frequently has tried new rules in an effort to have hiring commence only after law students had completed at least two years of law school. Together with my colleagues Chris Avery and Christine Jolls, and Judge Richard Posner, we surveyed judges and applicants and considered possible solutions (Avery, Jolls, Posner and Roth, 2001 and 2007), as the market struggled and failed to find a home-grown design that would work.  Around that time I also began to study, with Axel Ockenfels, the very different way that timing played a crucial role in the auctions run on eBay. The design of their auctions included an ending rule that elicited a lot of bidding in the final minutes and even seconds of many auctions. We studied, in the field and later (with Dan Ariely) in the lab, how small changes in auction design elicited big changes in bidder behavior (Roth and Ockenfels, 2002; Ariely, Ockenfels and Roth, 2005).  Not long after, [Paul Milgrom](https://www.nobelprize.org/prizes/economic-sciences/2012/roth/biographical/) came for a two year visit, on leave from Stanford, and he and I together taught what may have been the first class in Market Design, in 2000 and again in 2001. Teaching with Paul was a thrilling experience, and helped shaped my views of how the field might develop. Some of those views are in two “manifestos” that I wrote, “The Economist as Engineer …” (Roth, 2002) and “What Have We Learned from Market Design” (Roth, 2008).  My first Ph.D. student at Harvard was Muriel Niederle, who arrived at Harvard the year before I did, and together with fellow student Stefano DellaVigna peppered me with emails urging me to accept Harvard’s offer and assuring me that I’d have no trouble attracting students there. (Indeed, one motivation for moving was the attraction of being able to help educate Harvard students, who would in turn help establish experimental economics and market design more firmly in economics.) She and I began to study the market for gastroenterologists, which was experiencing unraveling like the law clerk market. After studying the gastro market in a series of papers in both economics and medical journals, we were able to help the Yale gastroenterologist Debbie Proctor convince her colleagues to adopt some novel guidelines giving applicants the ability to change their minds if they accepted very early offers, and this helped moderate the pressure to unravel sufficiently so that a successful clearinghouse could operate (see e.g. Niederle and Roth, 2003a,b, 2004 and 2009; Niederle Proctor and Roth, 2006 and 2008). The gastro market underlines what in my experience has been a general rule in market design: for a design to successfully pass from conception to adoption and implementation, it is necessary to have a talented insider who can help us (the economists) understand in detail the market and its problems, and who can take a leading role in explaining and persuading and in implementing institutional changes.  My opportunity to design school choice systems began in 2003 with a phone call from Jeremy Lack at the New York City Department of Education. He knew of my work on the medical match, and wondered if similar efforts might help reorganize the dysfunctional, congested system then used to match students to high schools. Together with Parag Pathak, then a graduate student at Harvard but now a Professor at MIT, and Atila Abdulkadiroğlu, who was then a professor at Columbia, we studied the problem and found ways to adapt a deferred-acceptance clearinghouse that solved the congestion problem and others. It fell to Neil Dorosin, who was director of high school operations at NYC-DOE to implement the new system. He went on to found the nonprofit Institute for Innovation in Public School Choice, which Atila, Parag and I have supported in spreading this technology to a growing number of American cities. But the first city to which we turned our attention after New York was Boston. Atila and Tayfun Sönmez had written a paper about the school choice system used in Boston, and it opened the door for us all to be invited to study the system from the inside, and eventually to redesign the choice algorithm used to implement Boston’s school choice goals (see Abdulkadiroğlu and Sönmez, 2003; Abdulkadiroğlu et al. 2005a, b and 2009).  In 2000, the first kidney exchange in the United States took place at Rhode Island Hospital, and my class notes on top trading cycles started to seem to have practical potential to organize such exchanges. When Utku Ünver (who had been my Ph.D. student at Pitt) visited Harvard in 2002, I suggested that we teach a class on kidney exchange. We posted our notes on the web and Tayfun Sönmez, who was Utku’s colleague at Koç, read them and suggested that we collaborate. The time zone difference between Istanbul and Boston made it seem as if we were working around the clock. When we finished, we had designed an algorithm both for kidney exchange among patient-donor pairs and for integrating these exchanges with non-directed donors, such as deceased donors (and a growing number of living donors) who aren’t part of a pair with a particular intended recipient. We sent a draft of our first paper (Roth, Sönmez and Ünver, 2004) to many surgeons, but initially only one, Frank Delmonico responded. We helped Frank form the New England Program for Kidney Exchange. My colleagues and I (especially, more recently, Itai Ashlagi) have since collaborated with a number other kidney exchange networks, including the Alliance for Paired Donation, founded by an innovative surgeon, Mike Rees, whom we helped to develop new ways of arranging exchanges, including long non-simultaneous chains (Roth et al., 2006 and Rees et al, 2009). While kidney exchange has now helped thousands of patients with incompatible donors receive transplants of life saving compatible kidneys, many other avenues will still need to be explored, as the number of people waiting for kidney transplants has continued to grow. With my student Judd Kessler I have started to explore ways of increasing deceased donation (Kessler and Roth, 2012 and 13a,b)  In thinking about these issues, I have become eager to understand the quite widespread reluctance to consider monetary markets for kidneys, which are illegal almost everywhere in the world. Economists need to better understand popular attitudes towards all sorts of economic transactions. My 2007 paper “Repugnance as a Constraint on Markets” was my first effort in this direction, but it remains a subject well worth exploring, and of importance well beyond medical issues.  At Harvard I was involved in other market design efforts, both of clearinghouses (for Teach for America, and for field assignments for HBS MBA students with Clayton Featherstone, and for freshman and junior seminars for undergraduates with Steve Leider), and for decentralized markets (such as the signaling mechanism for the market for new Ph.D. economists; see Coles et al., 2010). Decentralized marketplaces face design issues just as much as centralized ones do, and I anticipate that this will be a next frontier in market design.  In the summer of 2012 Emilie and I sold our house and moved to California, where I took up a position at Stanford, after 14 years at Harvard. It was a difficult decision, but we were empty-nesters, and we felt ready for a new adventure. I was 60, and to retire gracefully from Harvard my age plus years of service needed to add up to 75, and so it was that when the call came from the Economics Prize Committee it found us in California, at 3:30 in the morning. I was on leave from Harvard but already teaching at Stanford, as a visitor until I turned 61 in December and became the Gund Professor of Economics and Business Administration Emeritus at Harvard and the McCaw Professor of Economics at Stanford. One of the big attractions of coming to Stanford is that I get to be the colleague of several of my students, including Muriel Niederle, Mike Ostrovsky and Fuhito Kojima, whose dissertations I advised at Harvard, and of my advisor Bob Wilson, who remains active as an emeritus professor. Having students, and being one, form some of the important relationships in an academic life.  The call from the Economics Committee was followed by several hectic months, with some difficult decisions involving the allocation of scarce, indivisible resources, since laureates are only allowed to invite fourteen guests to the main festivities. (Allocation of scarce goods is hard; someone should study it …). In addition to family, our guests included both economists and non-economists who had played important roles in some of the markets I had helped design. Assembling the group made it even clearer that market design is an outwardfacing part of economics.  Now, still in the near-aftermath of all that excitement, I am working on getting back to work. The Nobel is a very famous prize indeed, widely covered in the press, and the nicest part of the avalanche of emails and other contacts that result from that has been that many old friends have made contact, some of whom I hadn’t been in touch with since childhood. It turns out that a Nobel is also followed by other recognitions, and perhaps the most unexpected of these is that the Japan Karate Association in Tokyo has now made me an honorary 7th-degree black belt, something that, given my athletic abilities, is even more unimaginable than being an Economic Sciences Laureate. **References**  1. Abdulkadiroğlu, Atila, Parag A. 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| Autobiographical |  |
| Podcast | Should you be able to buy a kidney? Economic sciences laureate Alvin Roth would call that, and other taboo exchanges, repugnant transactions. Roth pioneered ways of describing outlier markets where prices don’t work, explaining why you can’t buy a job at Google, acceptance at Yale – or a human organ.  This Nobel Prize conversation was conducted in March 2020. The host of the podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0825=AR  [Allegra Grevelius] Hello, is that Alvin Roth?  [Alvin Roth] It is.  [AG] My name is Allegra Grevelius and I’m calling from Nobelprize.org, the Nobel Prize website.  [AR] Uh, huh.  [AG] I’m sorry to bother you so early in the morning.  [AR] We are already up.  [AG] I expect so. Nobelprize.org has a tradition of recording short interviews with new Laureates and I was hoping that we could have a quick chat now.  [AR] OK, I’m having a little trouble hearing you. Can you speak up?  [AG] Ah yes, right. Of course, I’ll speak up. First of all, congratulations.  [AR] Thank you.  [AG] How do you feel today? How did you feel when you received the news?  [AR] Well, I’m surprised but it is still too early in the morning for me to have more articulated feelings than that. I imagine that it’s going to be a busy day. My phone has already started to ring.  [AG] I understand. Many of Nobelprize.org’s visitors are high school students. How would you explain your prize awarded work in layman’s terms?  [AR] Well, my prize is about matching and matching is the work that the economy does when deciding for instance which students go to which schools. If they have a choice – so high school students in some cities get matched through a choice system where they submit preferences and the schools have requirements or perhaps preferences also. And some decisions are made about who goes where. And that’s what matching is about. It’s about who gets what. And we try to, in the school choice, we try to make it happen in a way that is sufficient, but doesn’t send people to schools they would rather swap with other people if the schools would allow them. And if your students are in high school, they are going to go through many matching markets in their lives. They’re going to get married, they’re going to get jobs, and so, they can think about us then.  [AG] So your work has a lot of practical applications in our lives, school applications, maybe matching kidney donors and receivers. Are you driven as an economist by these questions by applying your theories to real life?  [AR] Yes, economics is about real life, so I’m very interested in that.  [AG] When you recruit post docs and students, do you apply the same theories?  [AR] Well, my post docs are all different, but they are all interested in economics and how it shapes our lives and how we can learn more about it. We want to try to make things better but we also want to learn to understand them. Those are related tasks. Different post docs, you know, have different points of view.  [AG] Yeah, indeed. As a young person, what inspired you to become an economist?  [AR] Well, I didn’t become an economist until rather late in life. My PhD is inoperations research. I was interested in making things work better, and using mathematics to help do that. So operations research is what I studied as an undergraduate and graduate student. The kinds of things that I found myself interested in, trying to understand and trying to make work better were things that involved people and that meant economics.  [AG] And today you teach a course called experimental economics, is that what you are talking about now? Can you tell us more about that?  [AR] Well, I teach two courses today. One is called market design and one is called experimental economics. It’s on Monday. Experimental economics is about conducting experiments, bringing economics into the laboratory or creating controlled conditions in the field, that allow us to understand better what we are seeing in less controlled circumstances. Market design is about understanding the details of markets in sufficient detail so that we can help fix them when they are broken.  [AG] In 1988 you wrote a book called “The Shapley Value, essays in honour of Lloyd S. Shapley”. How did his work influence you?  [AR] Well, so … Lloyd Shapley and David Gale, developed the first clear theory of what’s called stable matching and they thought about algorithms. They really wrote a, they didn’t think about it that way at the time, but they really wrote a very early market design paper and I’ve been a big follower of theirs in the sense that I wrote a book with Marilda Oliveira Sotomayor in 1990 that was called “Two Sided Matching” it sort of followed up the developments in the intervening 30 years in the theory of those kinds of markets. So I’m a, you know, I’m a big follower of Lloyd’s and it’s going to be a great honour to get the Prize together with him.  [AG] It is. I read that you have a blog and …  [AR] Yes, I do, on market design.  [AG] In your latest post you wrote about the correlation between a country’s chocolate consumption to the number of Nobel Prizes they get. What will you post today?  [AR] Ah, well, I don’t know. I may not have time to blog today. It’s shaping up to be a busy day. As I speak to you, I’m hearing beeps that suggest that other people are calling on the telephone.  [AG] I understand. Last question, how are you going to celebrate today, if you get a chance?  [AR] Well, pretty soon I’m going to have a cup of coffee.  [AG] That sounds good. That sounds excellent. Well congratulations and we look forward to meeting you in Stockholm in December.  [AR] I’m looking forward already.  [AG] OK, thank you.  [AR] Thank you.  [AG] Bye bye. |
| Interview |  |
|  |  |
| ID | 0826 |
| Biographical | My grandfather, Harlow Shapley, was a noted astronomer. He worked at the 100-inch telescope on Mt. Wilson in Pasadena, California, where he did some important work, most notably determining that the sun was not located at the center of the galaxy, but rather, out on the fringes. His first three children were born in Pasadena, but then in 1920, he was appointed director of the Harvard Observatory, and moved back east.  My father was born on June 2, 1923, in Cambridge, Massachusetts, into a scientific family, the fourth of five children. He grew up in the director’s residence at the Harvard Observatory, where he showed an early talent in mathematics.  *I had two brilliant, straight-A students for brothers, and I guess my sister … was rather bright, too. Nevertheless, we would play mathematical games sometimes around the house, play with cards and multiply them, do things like that. So I had this kind of boost from trying to out-excel my brothers, who were four and six years older than I was, and I did fairly well. So I had a family reputation of being the math whiz.*  He attended Philips Exeter Academy, then attended Harvard as an undergraduate, where he did well in mathematics and less well in almost every other subject. Then in the middle of his junior year, in 1943, he was drafted, and spent nearly three years in the army.  My father has told me many times of his experience after getting drafted. He was shipped down to North Carolina for boot camp. They also sent him to weather school, and trained him as a weather observer, plotting weather on maps and interpreting them.  Then he and his group were put on a train, which ran them across the country. They spent some time at a base near San Diego, then were put on a troop transport (a converted ocean liner), which sailed out of San Diego towards an unknown destination.  Using his watch and crude home-made navigational instruments, my father plotted the ship’s position on a world map posted in the troops’ quarters. The ship went due south for days, past the equator, to about 50 degrees south, then turned west, staying well clear of the Japanese presence in the Pacific.  At one point an officer reprimanded him for plotting and publicizing the “secret” position, so after that he took the map down, but continued plotting the position, and some weeks later, correctly named their first stop as Hobart, Tasmania. They finally disembarked in India, then after a long train ride, they flew over the Himalayas on DC-3s, finally arriving in central China.  He also told me of a letter he wrote home. Everything was, of course, censored, so he couldn’t even let his family know where he was. To let them know he composed a realistic sounding letter mentioning things like his “Uncle Charlie” – things which wouldn’t attract the censors’ attention, but would signal to the family that the meaning of the letter wasn’t in the words (there was never an “Uncle Charlie” in that family). Back home, his brother Willis figured it out, reading the first letter of each line: “C-H-I-N-A.”  My father was in the Army Air Corps, assigned to a secret air base in western China, a weather station which not only made weather observations, it also intercepted broadcasts. This work also involved a certain amount of cryptoanalysis, breaking the codes used in intercepted broadcasts. He was put onto that because he had a high score on the army’s mathematical aptitude test.  The bombers had to shuttle bombs and fuel over from India, and about every two weeks they had enough fuel ready and could make a raid on Japan. The planes couldn’t wait on the ground in China, since they were in range of the Japanese bombers. Because of this, they needed to know if the weather would be good three days in advance over the target area. The fronts moved down from Siberia, across China and to Japan. The weather center intercepted broadcasts from the Soviets and the Japanese, and even the US Navy in the Pacific. They needed masses of data because the quality was often very poor. So they accumulated all the data they could, plotted it out on the maps, and made long-range forecasts, and then sent it out to all the air fields within range on a particular day.  My father managed to break the Soviet weather code, which provided them with a lot more reliable data. For this he received a Bronze Star, promotion to corporal, and a raise of $4 a month. I remember him mentioning that the pay increase seemed to him at the time to be the most important part.  He was discharged right after VJ Day, and within a few months he returned to Harvard for the spring semester. He finished up in June of 1947, though he was class of 1944 for alumni purposes. (We received a bulletin from his class a few months ago – my father is the sixth member of the Harvard class of 1944 to receive a Nobel Prize.) But he didn’t graduate at that point:  *I didn’t get my degree in ’47. I finished all the requirements for the degree, but the last semester I also failed two courses … I took four courses over the load you take, and I failed two, and maybe got a couple of A’s in the other courses. The math courses I was generally good at, and music courses sometimes. So they said, “No, you’re on probation. We can’t give a degree to someone on probation. If you survive for a year, you’ll get the degree next year” … So I wasn’t all that gung-ho about Harvard. My performance at Harvard hadn’t been that much. Of course, a great deal of education takes place in a person hanging around – of course, I’d hung around Harvard beforehand.*  He had been a math major at Harvard, and done well (he was twice a member of the Putnam team). But he didn’t have any focus on any particular area of mathematics at that point. Out of the Army, out of college, he didn’t really know what he was going to do.  *I ended up not poised to go to graduate school. I didn’t know what I was going to do. I was a big music lover, but I had no technical skills there. So I even went to a course at the Union Conservatory one summer before I went off.*  He did send out some job applications, one of which was to RAND. And without a formal interview, he was hired. And it turned out RAND was just right for him.  *I’m not all that disciplined in getting places on time or going to bed when I should so that I can get up when I should. And RAND is sort of nice – it’s open all the time, twenty-four hours. People work at night, late, and you can just go in there.*  At that time, the mission of RAND was very open. It was set up by the Air Force to keep in contact with the scientific community, to get scientists to see and solve problems before a war made the solutions urgent.  *So the military felt that, yes, we should keep in touch with the scientists after the war … ” So let’s just not give them an assignment. Give them some money and say, ‘You think of some problems and tell us about it.'” It was a kind of wide-open contract with the Air Force. This led to people like [John] Williams putting together a rather motley crew of people … And he hired crazy students from the math department, me.*  At RAND, a group decided to look at game theory. They set up a seminar, meeting weekly, and they would work through a chapter of *Theory of Games and Economic Behavior*. This book had been originally published in 1944, creating a branch of mathematics to use for economics – that is, to study and analyze situations with multiple actors.  *… this was reasonably soon after the publication of* Theory of Games and Economic Behavior*, John von Neumann and Oskar Morgenstern’s opus, a big thing. I think it was published in ’47, a so-called second edition, which simply has an appendix added, which they didn’t have finished in time before. So it had appeared and had not made much of a splash, got big reviews and von Neumann, anyway, was well-known in mathematics already. But nothing had happened beyond that.*  As a result of these seminars, my father spent some time working with Roger Snow, another young mathematician, on a problem raised by the book, how to find all the solutions to a matrix game.  *We made some progress, and finally I guess I broke it, but Roger was also working on it. So it turned out to be Shapley-Snow … This was a work of mathematics where I had not really even read very many math papers as published and didn’t have any clear concept that I was doing anything special except solving a problem.*  Von Neumann read it and became very enthusiastic, because there had been very few papers at that point which had responded to *Theory of Games*.  *So, von Neumann, partly, I guess, for his own ego, said, “I want to encourage this work,” even though he was really not working on game theory anymore. He was working on computer ideas mostly. So he wants to encourage it. So it came back a big rave review or maybe a letter …. von Neumann was all excited about this, and he’ll publish it, and he’ll sponsor it in any journal you name, and so on.*  *So at that point, my stock went up, and Roger’s went up. At least these two kids – and there weren’t all that many of us around – had something enough to get a real pro like von Neumann interested. So, of course, stop everything else while we write this paper and send it off. This is my first contribution, Shapley-Snow. I call it my piece, really, though I mean it’s helpful to have Roger in the thing, but he kept saying, “You write it, and I’ll read it.”*  So that was the real start of my father’s career in game theory.  My father was at RAND less than two years before he headed off to graduate school at Princeton, though during the summers he worked at RAND as a consultant. The time at Princeton was vital to his development as a mathematician.  *I had a lot of mathematical – I learned an awful lot of math. I just learned a lot of math at Princeton and I really got educated mathematically.*  At Princeton, his dissertation was “*Additive and Nonadditive Set Functions*,” done with Albert Tucker. While there he published several other papers, including one called “A Value for *n*-person Games,” which introduced what is known as the “Shapley Value,” a solution concept in game theory, which has become a major part of the field.  *One of the early works was this thing which led to the Shapley Value. It was called the Shapley Value, so my name is an adjective for that solution concept. In that case, I can sort of trace the ancestry of how did I think of it. But more specifically, one can look at the – what’s the word? Not paradigm. But anyway, the layout of the field as defined by von Neumann and Morgenstern exactly where, when they decided to do this, you could have done something else, and this would lead to the Shapley Value. So I can sort of provide a foundation for my work by following their arguments up to a certain point and then saying, “No, this is more important.” Sometimes you can use a unique answer. If you insist on uniqueness, you have to give up something else and then a different way. So it was a kind of rank and file. Sometimes it was rather neat that way.*  Very soon after he developed the Shapley value, in considering applications, he worked with Martin Shubik on applying it to the measurement of power in voting situations. This led to an item that became known as the Shapley-Shubik Power Index. They, as two unknown graduate students, one in mathematics and the other in economics, had the temerity to submit this paper to the leading journal in Political Science, and much to the surprise of all concerned it was accepted in a few weeks.  My father found he enjoyed (and was very good at) doing research – coming up with problems, solving them, proving theorems, and writing and publishing papers. Once he had his degree, he was set for doing academic work – however, he was not really interested in teaching. Largely because of that, he returned to RAND, rather than taking a job as a mathematics professor at a university. He kept doing mathematics, proving theorems, and publishing for years.  Though his specialty was game theory, the so-called mathematics of economics, my father has never done economics. He has told me his only real exposure to the field was his collaborations with his good friend Martin Shubik, a classmate from Princeton and a long-time professor of economics at Yale. He told me last year, “Shubik would talk to me for a while, then I would go and explain to him what he had just said.”  My father also kept in contact with others of his classmates from Princeton. One of these was David Gale, who was at Brown University in 1961. They communicated by mail – long distance telephone calls were expensive and rare.  One day, a letter arrived from Gale framing a problem of choosing roommates. If you have two groups, with each individual having different preferences, is there a way to come up with a set of stable pairings of one from each group? Gale suspected there wasn’t a way to make a stable solution, but couldn’t prove it.  The way my father describes it, he received the letter around noon, spent some time thinking, wrote up the solution (There *was* a stable solution, and this is how you come up with it), and mailed it back to Gale later that afternoon.  This solution, the deferred matching algorithm, became the paper “College Admissions and the Stability of Marriage,” published in 1962. My father has described how two reviewers rejected it before it was finally published, probably for being too simple – it’s a mathematical paper which contains no equations. As stated in the paper:  *The argument is carried out not in mathematical symbols but in ordinary English; there are no obscure or technical terms. Knowledge of calculus is not presupposed. In fact, one hardly needs to know how to count.*  *Yet any mathematician will immediately recognize the argument as mathematical …*  Yet the deferred matching algorithm has turned out to be a major tool in game theory and economics, notably for how Alvin Roth applied it to school admissions.  Even though the mission of RAND was basically military, my father was seldom pressured to work in that direction.  *RAND was never really trying to pull me into … war-gaming things or that sort of problem, but the great thing about RAND for me is they let me do what I wanted. I succeeded in doing what I wanted … I eventually was getting National Science Foundation (NSF) grants, which were supporting me at RAND and not really drawing on RAND … I stayed at RAND for maybe ten years longer than I would have otherwise, because almost all my support came from there. NSF grants which helped me to do research on whatever it was, that attracted me at that time.*  From 1952 until around the late 1970s he had a considerable collaboration with Shubik on the applications of game theory to economics beginning with a note on Solutions on N-Person Games with Ordinal Utilities. His basic work on the core of a game prompted Shubik to consider the convergence of the core to the competitive equilibrium in an exchange economy modeled as market game with side-payments. They generalized the convergence of both the core and the Shapley value to the competitive equilibrium and further work led to a generalization in the form of “Market Games” utilizing the concept of balanced sets of coalitions. This led to a deep understanding of how cooperative game theory solutions related to the competitive equilibria of exchange economies. They also applied the core concept to an economy with externalities. Their investigation of a classical economic example illustrating the emergence of competition led to The Assignment Game and the role of marginal pairs of traders.  Having completed their investigation of the relationship between cooperative game solutions with many agents and the competitive equilibria of an exchange economy, they observed that although Cournot in the 19th century had suggested the relationship between the noncooperative equilibrium and the competitive equilibrium in an open economy, this result had not been generalized to account for a closed economy. A basic economic model was suggested by Shubik, but it took much work and care to tighten this to a clean mathematical model suitable for generalization. The key observation was that the use of money arose naturally in the model in the strategic form of a game. They called the resulting general class of games “Strategic Market Games.”  My father was interested in several aspects of voting throughout his career. With Irwin Mann in 1962, he determined the voting power of the several states in the 51-party voting game known as the Electoral College (used to elect American presidents). He also suggested an approach to evaluating a presidential election, treating it as a game among several million voters. This eventually led to results which showed that the voters of medium-sized states have the lowest voting power in the country.  Another interesting question dealt with the optimal assignments of “weights” to voters – optimal in the sense that a “correct” result is most likely to be obtained. In this case, my father and Bernard Grofman in 1984 determined a formula to assign voters weights based on their likelihood of making a correct decision.  My father also worked with Guillermo Owen on analyzing voting in a “political” situation, using an approach called “spatial games.” In this case, voters are assigned ideal positions in a multidimensional space, as well as weights, and the outcomes of the game are also points in this space. These games require players to form coalitions to come up with more favorable outcomes. The value of an outcome to a voter is based on the distance between the voter’s ideal position and the location of the outcome – the closer the two points, the more the outcome is worth to that voter.  Working with Yale economist Herbert Scarf, my father described a system where large, indivisible items (such as houses) could be exchanged in an optimal way, building on earlier work by David Gale on top trading cycles. This was published as “On Cores and Indivisibility” in 1974, and the system described led directly to Alvin Roth’s work on kidney exchanges.  In 1974, he and [Robert Aumann](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2005/aumann-facts.html) coauthored the book *Values of Non-Atomic Games*; it concerns games with many players, who impinge significantly on the outcome only when they form large coalitions, but not as individuals. Examples are national elections or large economies or markets.  Another collaboration with Aumann concerned perfect equilibria in repeated games; it is a sharpening of the Folk Theorem, which was cited by the Nobel Committee when presenting Aumann with the 2005 prize.  Over the years, my father also did important work in the development of the core, the development of stochastic games, abstract side-payment games, potential games, oceanic games, convex games, and other fields. Most of these, and the above, involve some extremely complex mathematics, a world apart from “College Admissions and the Stability of Marriage,” with its simple logical reasoning. But the common thread is that he was solving mathematical problems.  By the late 1970s, my father realized that he was the only one left doing game theory at RAND. His colleagues had retired, or gone to various universities. So he quietly put the word out that he would be open to offers. He really only had two institutions on his list – UCLA and Stanford – but he got offers from all over the world. When the offer finally came from UCLA, he accepted it, and moved there in the fall of 1981.  *Teaching didn’t appeal to me at all, but academic research did … there was a big reentry on the teaching basis when I came to UCLA … The plus was I had some graduate students who I could really train and imbue with my knowledge, whatever it is. Most of my colleagues in game theory, they have generations of students of students. I had nothing like that. But the other side is I have to teach students, which I’ve learned to do, but I don’t know how to do it very well. So I’m not a big teacher, no. Research is my thing.*  It was a joint appointment – in the math department to teach math to math students, and in the economics department to teach math to economics students. So despite being a professor of economics, he has never taught economics.  At a conference in June of 1987, my father included in his remarks,”Yesterday was my birthday. I feel like I’m a million years old.” Then he added, “Base two, of course.” The mathematicians got the joke. A year later, Alvin Roth published a book, *The Shapley Value: Essays in Honor of Lloyd S. Shapley* which begins by saying it is “… in honor of the 1000001st (binary) birthday of Lloyd S. Shapley.”  In his 20 years at UCLA, my father had a number of outstanding graduate students, several of whom are now established professors and have their own students.  Though he retired from UCLA in 2001, he has tried to remain active in the field since then, collaborating with former students and attending conferences.  *Game theory, I think, was made for me, because I was always messing around with great big game-like models, the sort of thing that now they call “Dungeons and Dragons.” I used to do that with my friends in school and so on. So I maybe had that mentality, too, but also the mathematical push, because this was a kind of mathematics that I had generated, quite apart from the way it’s applied. I’ve always enjoyed the mathematics of it …*  *The problem I get on, from whatever source, if it starts out mathematical interest, I’ll follow through to the end. If there’s simply an interesting application, well, maybe someone else can do it. I’m right in there in that close equation. The mathematical discovery is the really exciting part. You create conjectures, but you discover results. I think that’s the way. I don’t consider mathematics – you don’t create facts. Of course, the facts are always there, but you discover them.*  *Game theory … spreads out so quickly into so many different fields. Once you get multiple decision-makers in the same model, all kinds of things can happen. So there’s almost never been a time, after the very beginning of that seminar at RAND that first couple of months, where there was not a game theory problem ahead of me, in front of me somewhere to see. It’s also not only unexplored but unstructured, because I think my main contributions here, the big thing – someone wrote a preface in my book with papers on my sixty-fifth birthday – and he, I think correctly, said that for many years I set the agenda for the field by my work. In other words, I was sort of trying to cover the problems that I could see, so I’m building the theoretical structure as well as working on it.*  Note \* This is a biography of my father, Lloyd Shapley, based on various sources and discussions with him over the years. Most of the quotes (in italics) are from an interview he did about 20 years ago concerning the RAND Corporation\*\*, where he spent much of his career. I also received assistance from Professors Martin Shubik, Robert Aumann, and Guillermo Owen, and my brother Christopher Shapley. *— Peter Shapley*  \*\* Interview, February 9, 1994. Joint Oral History Project on the RAND Corporation. Archives Division, National Air and Space Museum, Smithsonian Institution, Washington, DC. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0826 |
| Interview |  |
|  |  |
| ID | 0827 |
| Biographical | *Thomas J. Sargent did not submit an autobiography.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0827=TS  [Thomas Sargent] Hello?  [Adam Smith] Hello. May I speak to Thomas Sargent, please?  [TS] Yeah, this is Sargent.  [AS] Oh, hello, my name is Adam Smith, and I’m calling from Nobelprize.org, the official website of the Nobel Prize in Stockholm. Congratulations on the news of your award.  [TS] Yeah, thank you.  [AS] We have a tradition of recording extremely brief telephone interviews with new Laureates. Would you be able to speak for just a few minutes?  [TS] Yeah, sure, okay.  [AS] May I just first ask where you were when you heard the news? Did you get the call from Stockholm?  [TS] Yes, I did. I was in my study. I get up early so I was in my study.  [AS] Right, so you already … because it’s only … it’s still early where you are, yes. And, you’ve been awarded with Christopher Sims, and you were PhD students together in Harvard, and got your PhDs together in 1968. But, you haven’t really worked together?  [TS] Oh, no, we were colleagues at Minnesota.  [AS] Okay.  [TS] And, we had many students together. I learned a tremendous amount from him. And, so, we’ve had wonderful students together and we interacted a lot through our students. So, we were at Minnesota for fifteen years together.  [AS] Right. And, indeed, you’ve had a tremendous number of students. I think one of your great contributions has been your mentorship, I gather?  [TS] I’ve had great students. I’ve been very lucky. Yeah.  [AS] It’s quite a time to be chosen to be a Nobel Laureate, with so much of the world’s attention focused on the economy. Do you find it a daunting prospect to be the subject of so much media attention?  [TS] Well, I … sorry, I don’t know what’s involved in that. You know, we’re just … yeah, we’re just bookish types that look at numbers and try to figure out what’s going on. So, I don’t know what to say to that!  [AS] It’s a nice description. I mean, broadly, would it be true to say that among your contributions has been the demonstration that – the demonstration of the importance of people’s expectations in macroeconomic modelling?  [TS] You know that’s an insight that economists have had for a long time. I mean Marshall had that and Keynes had it. And what I try to do – figure out – and I wouldn’t even say I did it by myself, I wouldn’t say I mainly did it – but I tried to use methods of John Muth and [Robert Lucas](https://www.nobelprize.org/nobel_prizes/economics/laureates/1995/) … pointed out how to discipline – you know, the thing, expectations matter is, it’s true, but it’s loose. You have to have … You know, to make progress on that, you have to have some idea about how expectations are formed. And, that was a loose end in econometrics and quantitative analysis and we tried to discipline and tighten that. And, under certain circumstances that works, under others it doesn’t work. There’s a lot of people who have worked on that just, you know, scores who made … And, people now when they’re thinking about the crisis, are thinking about new ways of extending theories of expectations. In panics and crises, are all about – you know what’s going on in Europe right now with the euro – that’s all about expectations about what other people are going to do. So, that’s what many economists work on and I do too.  [AS] And, the work basically allows us to better predict the roles and limits of policy intervention?  [TS] Yes. Well, we try to [lines cuts out] the limits and possibilities of policy, yeah.  [AS] Broadly, your models provide, if you like, a laboratory where you can experiment with what the effect of a policy intervention might be?  [TS] Absolutely, that’s what we try to do. We try to experiment in our models before we wreck the world.  [AS] [laughter]  [TS] … so that we don’t wreck the world, right?  [AS] And how close do you think those laboratories are to equating to the real situation?  [TS] Some are quite good. Some need improvements. Some are extremely influential in terms of influencing policies for good or bad. I mean, there are … Models guide what the central banks do now to a very large extent. And, competing models do. So like models of bank runs and models of moral hazard, those are all models about expectations and about how expectations and incentives interact. So, there’s some really close calls. But, I know for a fact that, now at central banks, and at some treasuries, very sophisticated … people who are very knowledgeable about economics are worried about exactly the forces that our models are about. Many of the practical problems are ahead of where the models are. But, that’s why …  [AS] Yeah, I guess one always needs to refine the models. But, yes, yes. Okay, well I promised to take up only five minutes and you’ve been extremely kind in talking to me. When you come to Stockholm in December we have a chance to speak at greater length.  [TS] Okay, I’d like to, I’d like to.  [AS] I look forward to it very much. Okay, congratulations again and enjoy your day. Thank you.  [TS] Okay, okay, thanks, bye. |
| Interview |  |
|  |  |
| ID | 0828 |
| Biographical | My grandfathers were both immigrants to the US, one from Estonia, then part of the Russian empire, and the other from England. The Estonian, William Morris Leiserson, was Jewish. He ﬂed Estonia in 1890 at the age of seven, through a forest in the dark of night, with his mother and two brothers. The family is not sure why they had to ﬂee. It could have been because of a pogrom, or it could have been because of political activities of my great grandfather. Many Jews were ﬂeeing Russia at the time. William’s father was meant to join them after they reached the US, but was not seen again. One of William’s older brothers, Louis, owned one of the shirtwaist companies in New York that were the site of the famous 1909 strike by the ILGWU, and William worked there as a youth. His formal education in public schools ended at the age of 14, but he read widely and attended public lectures in New York. At the age of 21 he went to Wisconsin, where he persuaded John R. Commons, the institutional economist, to take him on as a student despite his having no high school diploma. He later attended Columbia University in New York for his PhD work. He went on to teach at Antioch College and to become one of the first members of the US National Labor Relations Board, and then served on the National Mediation Service. He married Emily Bodman, whose family had many generations of roots in New England, and had seven children, one of them my mother, Ruth. Ruth, besides raising three children, headed the Connecticut League of Women Voters for a time and served two terms as First Selectman (similar to a mayor) of the town of Greenwich, Connecticut. She was the first woman, and the first Democrat in many decades, to be elected to that position.  My father Albert’s father James was an English immigrant who worked as an itinerant manager of textile factories in New England, brought in to turn around poorly performing factories (at a time when the industry was declining). He died at a relatively young age. His wife Minnie Love was also an immigrant, from Northern Ireland, and she, too, died at a young age, leaving seven children to manage their own household, which they did successfully. Albert went to Michigan State University and received a masters degree in public administration from Syracuse University. He worked in the US State Department, then as vice president of the College Board in New York.  Two of my uncles, Mark and Avery Leiserson, were professors. Mark was a labor economist, first at Yale, then at the World Bank. Avery spent most of his career at Vanderbilt University and served a term as president of the American Political Science Association. Both of them, and my grandfather “Billy” Leiserson, loved intellectual argument. I remember being greeted by grandpa, when I was about seven years old, with a twinkly eye and “Well, Chris, what do you think of the present situation of the country?” This was not an isolated incident. This was his standard greeting for grandchildren. Mark prodded me regularly, from about age 13 onward, to study economics. He gave me von Neumann and Morgenstern’s *Theory of Games* for Christmas when I was in high school. When I took my first course in economics, I remember arguing with him over whether it was possible for the inﬂation rate to explode upward if the money supply were held constant. I took the monetarist position. He questioned whether I had a sound argument to support it. For years I thought he was having the opposite of his intended effect, and I studied no economics until my junior year of college. But as I began to doubt that I wanted to be immersed for my whole career in the abstractions of pure mathematics, Mark’s efforts had left me with a pretty clear idea of an alternative.  While I was born in Washington DC (in 1942), from the ages of about five to seven I lived in Germany. My father had been part of the military government while in the Army, then switched to the State Department and brought the family over. We lived in Berlin and in Kronberg, near Frankfurt. At the time, I spoke some German, though I subsequently almost entirely forgot it. After returning to the US, we lived in Hollin Hills, Virginia for a few years, then moved to Greenwich, Connecticut when I was 11. I finished my schooling there and graduated from Greenwich High School in 1959. In high school I played football (third string linebacker, most of the time) and trombone in the band. I had a memorable math teacher in high school, Steven S. Willoughby, who later became president of the National Council of Teachers of Mathematics.  My undergraduate education was at Harvard, where I majored in mathematics, worked in a political group opposing nuclear testing and proliferation, played club rugby and weekend soccer, and played trombone in the band. My honors thesis generalized Khinchin’s version of the coding theorem of information theory to infinite-memory channels. Engineers who were roped into reading the thesis tried to convince me to enter graduate school in electrical engineering. In some of my recent research I have found a way to connect information theory to economics.  My first year of graduate study was at UC Berkeley, where I took Microeconomic Theory from [Dan McFadden](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2000/mcfadden-facts.html), econometrics from Dale Jorgenson, and Monetary Economics from Richard Lipsey, inspiring teachers. McFadden posed problems that were only *nearly* impossible, from which I learned a lot. He taught micro as largely a set of corollaries of convex analysis. He later told me that this was in the period before he learned how to teach, for which I think I am grateful. Lipsey taught economics as a set of open questions and gave students the sense that they might be finding answers. Jorgenson gave off an almost electric sense of energy and confidence in the power of the rapidly developing field of econometrics.  I moved back to Harvard in my second year of PhD study. My adviser there was Hendrik Houthakker, who largely gave me free rein. I decided to study, empirically, embodied technological change. As I thought I was nearing completion of the work on my model, Henk insisted that I should be able to formulate it in continuous time, then derive the discrete time version from the continuous time version. This was harder than I think Henk realized, and led me to learn a great deal of real analysis in a short time. John Chipman, then visiting Harvard, was the only person I found who not only understood the issues I was dealing with, but could point me to relevant mathematical literature. John urged me to consider the Minnesota department, but Harvard was about to hire Zvi Griliches and Dale Jorgenson, so I decided to stay at Harvard as an assistant professor for two years.  In February 1967, a few months before completing my dissertation work, I married Catherine Sears. We postponed our honeymoon trip to the summer because of dissertation work, and then had to repostpone it because the work kept not finishing. We ended up with a rapid road trip to the Rocky Mountains in September. We arrived and pitched our tent in the dark, in the rain, and awoke to find our tent door ﬂap frozen shut. Cathie, who had not camped before, has remained skeptical of camping trips ever since – even though the weather in the Rockies was better after that morning.  Our son Ben arrived two years later, shortly before we moved on to Minneapolis.  Tom Sargent and I knew each other slightly as graduate students at Harvard. We had both come across the issue of interpreting two-sided distributed lag regressions, and we talked about it on the phone once or twice. I had accepted a position for the next year at Minnesota, and my recollection is that I had heard that Neil Wallace and Jack Kareken at Minnesota were enthusiastic about hiring Tom, and that I was encouraging Tom to go to Minnesota during our telephone conversations. Tom, on the other hand, has said he is sure I proposed his name to Minnesota. In any case it was for me tremendous good fortune that he and I arrived at Minnesota at about the same time. He was and is a gifted teacher, whose students emerged both well trained and enthusiastic and confident about research. I was at Minnesota for 20 years, most of them with Tom as a colleague. Though Neil Wallace, Tom, [Ed Prescott](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2004/prescott-facts.html) and I were seen by many outside Minnesota as a package of “fresh-water” innovators, from inside the Minnesota department we seemed to ourselves to represent sharply distinct viewpoints. We argued, and stimulated each other and our students. Actually much of the “arguing” was implicit, in the conﬂicting advice we sometimes gave our shared PhD students.  Our two younger children, Jody and Nancy, were born in Minneapolis and both live there now. We spent the entire 20 years in the same house in South Minneapolis, in a neighborhood where the single block-length of our street housed dozens of children. Summer evenings regularly involved “duck-duck grey-duck” and kickball games with street corners as bases.  My first econometrics and statistics courses (a statistics course from Dempster and econometrics courses from Hendrik Houthakker and Lester Taylor, all while an undergraduate at Harvard) did not devote much attention to Bayesian approaches. I had looked at, but not read through Raiffa and Shlaifer’s book on decision theory and read some of Pratt’s articles that took a Bayesian perspective. As I started graduate school I think I had the impression that the difference between Bayesian and frequentist approaches was mainly semantics, with little implication for practical data analysis. Another student at some point showed me the standard “rare disease” example, in which a 99% confidence interval, while having 99% pre-sample coverage probability, fails to contain the truth in 99% of the cases where the test is positive for the disease. I still remember puzzling over the example. It changed my thinking. It was not until I started analyzing the discrepancy between Bayesian and frequentist approaches to possibly non-stationary time series models that I became convinced that the distinction between the two approaches was important enough in a wide enough class of applications that teaching should always start from the Bayesian perspective. Since then, my teaching at every level has started from a Bayesian perspective, teaching non-Bayesian approaches as side topics.  In macroeconomics, my thinking was of course inﬂuenced by my two colleagues Sargent and Wallace, who were part of the core group fomenting the “rational expectations revolution” in macroeconomics. My own research made little use of rational expectations theory at first, focusing instead on using simpler, minimal theory to find the effects of policy by analyzing time series data. This is not because I was opposed to rational expectations ideas – I thought they represented an advance. I just thought the “revolution” aspect of it, in particular the deprecation of the ideas and efforts of the Keynesian econometricians, was overdone. My own work involved criticizing the statistical underpinnings of the Keynesian econometric models, but I viewed them as ﬂawed but important, not worthless. I found the story that those Keynesian models had led to the inﬂation of 1970s implausible, unsupported by evidence, and recent research seems to confirm that.  While at Minnesota I heard Michael Woodford give a talk, one part of which laid out how the government’s intertemporal budget constraint can be regarded as determining the price level. This set me to thinking and writing about the theory of price determination in models that treated both monetary and fiscal policy explicitly, a major part of my research from then on. In this theoretical work I made regular use of a rational expectations framework, which was by that time standard.  After Sargent left Minnesota, the department began to have internal disagreements about whether econometrics was an essential part of graduate training in economics. Many of the most interesting dissertations I helped supervised had been joint projects with Sargent, and his teaching had instilled an interest in careful quantitative research in Minnesota students. I decided it might be more interesting to work elsewhere, and settled on Yale. There I took a relatively bigger role in macro teaching and a relatively smaller one in econometrics teaching. Yale also had internal divisions, in this case over hiring macroeconomists who worked in the rational expectations framework. After nine years there, Princeton, which had a stellar collection of macro and monetary economists in Michael Woodford, [Ben Bernanke](https://www.nobelprize.org/prizes/economic-sciences/2022/bernanke/facts/), and Alan Blinder, and one of the best time series econometricians in Mark Watson, looked attractive, and I moved there. Soon Lars Svensson joined the Princeton faculty. For a few years, the research environment there, with constant interaction and discussion of theory and policy and shared responsibility for advising students, was as good or better than the early days at Minnesota.  Ten years ago, my wife Cathie, who had ridden horses extensively when she was younger, decided to take up riding again, and eventually bought a horse. I thought I should learn to ride to keep up with her. Riding turned out to be a great pleasure for me, and eventually I, too, bought a horse.  Cathie and I now have four grandchildren, aged two to ten, two living in Minneapolis, two in Los Alamos, New Mexico. We visit them regularly, and were happy that all four, with their parents, joined us in Stockholm for the Nobel ceremony. |
| Autobiographical |  |
| Podcast | How important is the money in your pocket? Try buying a sandwich with an IOU and a promise to come back and pay, and you’ll soon understand. Christopher Sims’ research explores topics from the meaning of money to his prize-awarded work on cause and effect in the macroeconomy. In this conversation, conducted in April 2020, Sims touches on sandwich shops, terrific teachers and a horse with a name that’s almost impossible to pronounce.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0828=CS  [Christopher Sims] Hello?  [Adam Smith] Hello. May I speak to Christopher Sims, please?  [CS] This is he.  [AS] Oh, hello, this is Adam Smith calling from Nobelprize.org.  [CS] I’m sorry I didn’t answer your previous calls but there were so many that I just turned everything off.  [AS] How very wise. It’s probably the last quiet time you’ll have for some days I’d imagine.  [CS] Mmhmmm.  [AS] What were you doing when the call came?  [CS] I was sleeping. We actually got the call twice because the first time it came my wife couldn’t find the talk button on the phone.  [AS] Oh, well, nice of them to call back. That’s good. You’ve been awarded with Thomas Sargent and you were graduate students together so you’ve known each other a long time?  [CS] Yes. We’ve known each other a long time.  [AS] Yes, and do you still work together or do you work independently but in the same area?  [CS] Recently – we haven’t done joint research in recent years but we are jointly teaching a graduate course in macroeconomics, at Princeton actually, this semester.  [AS] Ah, okay.  [CS] And we were colleagues at University of Minnesota for many years.  [AS] Yes, indeed, yes, indeed. And, you’ve referred to the media attention already. With so much focus on the world economy at the moment, I suppose it’s perhaps an even greater response than usual to the creation of new Laureates in Economic Sciences. Do you welcome the sort of attention that will follow this award?  [CS] Do I welcome the attention? Yes, I think it’s good to have people thinking about macroeconomics and about careful, quantitative, scientific macroeconomics.  [AS] Hmm, and that’s broadly the contribution that you and Thomas Sargent have made – that you’ve helped to put people’s expectations back in the models and made them more precise?  [CS] Uhm, yes, using and applying rigorous statistical methods also, as we do that.  [AS] I used the analogy with Thomas Sargent, but you’re basically creating laboratories into which you can put data and see how things would turn out if particular policies were followed. Is that right, broadly?  [CS] That’s the way they described Tom’s research. It’s not really very close to what they awarded the prize to me for. His approach is to start with a model economy for the most part, though not all of his research is like that, but he usually starts with a model economy and then tries to fit it to data and run experiments in it. I usually start with a statistical model of the data and then add economic assumptions sparingly until I can begin to get answers.  [AS] Right, right. And, the Academy as made the distinction between the fact that he works on effects of long term policy and you work more on economic shocks. Would that also be broadly true?  [CS] Well, that’s the way a lot of people think about the difference between our research, but I think it’s not as clear, or sharp, as that. I probably do put more emphasis on being sure the short run implications of a model are accurate. But, we both have worked on both long and short term effects. And, in fact, my more recent research is very long term oriented. I have recent research that’s not really in the Nobel citation that’s oriented towards thinking about long run consequences.  [AS] Mmhmm. And will you, do you think, be able to get back to work straight away or do think this is going to eat up the next little while?  [CS] I’m hoping to get back to work pretty straight away. I have a class to meet at ten o’clock!  [Laughter]  [CS] … And, I still have a few papers to grade before I go.  [AS] Then I should get off the phone and let you continue!  [CS] But there will be a press conference in the middle of the day which will break up the routine.  [AS] Yeah, I imagine Princeton has been preparing for this moment for some time and has some plans for you  [CS] Yup.  [AS] Okay, well, splendid. Thank you very much for talking to us. When you come to Stockholm we have a chance to speak at greater length, but for now congratulations and thank you very much indeed.  [CS] Okay. Thank you.  [AS] Okay, bye, bye. |
| Interview |  |
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| ID | 0829 |
| Biographical | My grandparents immigrated to the U.S. around the turn of the last century. My mother’s parents and six older siblings came from Poland. My father’s parents met in New York, she having come from Russia and he from Romania. My parents, both born in 1908, grew up in New York and never lived outside the metropolitan area. Both finished high school and went to work, my father studying at Brooklyn Law School at night while selling shoes during the day. When they married in 1929, my mother was earning $15 a week as a bookkeeper and my father, $5 a week as a novice lawyer.  My brother, Richard, was born in 1934 and I in 1940. My father continued to practice law until his late 80s, but my mother had left the paid labor force by the time of my birth. She volunteered in a number of organizations after that, often serving as treasurer, drawing on her bookkeeper background. In more recent times, with better opportunities for women, she would have had a good career.  I started public school in the Bronx, and switched to suburban public schools in second grade when the family moved to Woodmere, on Long Island. Our house faced the Long Island Rail Road and was so close to the tracks that the family thought the first train at 5 AM was coming through the bedrooms.  After high school, I went to Yale. I have joked that I loved college and just never left. After considering a major in engineering, I chose math instead. I learned how to do a rigorous mathematical proof from Shizuo Kakutani, who taught the class in real analysis. He had proved a fixed-point theorem that plays a key role in some economic analyses.  In my second year at Yale, I took a year-long introductory economics course from Charles Berry. He interested me in economics and became a friend. In the following year, at Berry’s suggestion, I took the year-long intermediate honors theory class, taught by Ed Budd. This increased my interest in economics enough so that I broke off my study of French to take the graduate mathematical economics course taught by [Gerard Debreu](https://www.nobelprize.org/nobel_prizes/economics/laureates/1983/), based on his newly published *Theory of Value*. Debreu was an outstanding teacher, and my early and thorough grounding in general equilibrium theory has stood me in good stead ever since, shaping my thinking about economics. At the same time I was studying game theory in the seminar for senior honors math majors and graduate math classes in Algebra and Topology.  My first job as an economist (sort of) was as a research assistant for [Tjalling Koopmans](https://www.nobelprize.org/nobel_prizes/economics/laureates/1975/) for the summer of 1960. I shared an office with T.N. Srinivasan and got to hang out in the Cowles Foundation, with its memorable coffee time discussions. I was hired to help with the mathematics. When asked to produce an example of a function with certain properties, I found it easier to produce a class of functions with the desired properties, rather than cranking out a single example. That led Koopmans to reorient his research plan, giving this class of functions a central role, and to promote me to co-author for my first publication (appearing in 1964). I had had no idea that the class of functions would be interesting, and have come to recognize that an important part of education is learning to recognize the value of what you stumble over, as well as choosing research topics with a sense of how valuable possible findings might be. I have always viewed this splendid economist and splendid person as a role model for how to do economics and how to relate to people.  In light of my interest in both math and economics, I applied to graduate schools in both subjects, but settled on the MIT math department. My plan was to take micro and macroeconomics and complex and real variables, deciding at the end of the year which was the better career route. But the complex variables class conflicted with both micro and macro. I decided to drop complex variables, figuring I could pick it up in the summer if economics didn’t hold me. The math graduate registration officer (GRO), George Thomas, who had written the calculus book I had learned from, thought I should be advised by the economics department. He simply transferred me (along with my fellowship) to the economics department and GRO [Bob Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/). Bob promptly added statistics and economic history to my class load.  I enjoyed economic theory and didn’t enjoy real variables (and I was better at economics). It seemed to me that the real variables class was about proving the same theorem about integration over and over, in more and more general settings. I found the generalizations of no interest, perhaps because I was ignorant of what could be done with more general versions that couldn’t be done otherwise. As an economist, I have always been more interested in working on models generating new insights, and not much in generalizations, as important as they sometimes are. So my experimentation ended after one semester.  The public finance class, taught by E. Cary Brown, was, for me, a key part of the normal two-year course load. We worked through everything in the newly published *The Theory of Public Finance* by Richard Musgrave, an outstanding scholar who had written an exceptional book. Musgrave’s drive to put public finance on a general equilibrium basis was important for my development and fitted well with the general equilibrium orientation that I had acquired from the class with Debreu. This background played a central role in my later work on optimal taxation.  My thesis, supervised by Bob Solow, had one essay that took a different approach to the same questions I had worked on with Koopmans, and two essays in growth theory. One of them built on work of Srinivasan and one on work of Solow and of W.E.G. Salter, whose book Solow brought to my attention at a time when I was failing to find a topic for the needed third essay. Solow was and is an outstanding economist, a splendid person and a good friend. He has supervised a large fraction of MIT dissertations. He and Koopmans have been my role models. More generally, MIT was (and is) a terrific place to be a graduate student, to get an outstanding and broad education, while having a good time. And it has been a terrific place to be a faculty member. The students, both undergraduate and graduate, have provided stimulating teaching opportunities, both pleasurable and educational for me, and a series of outstanding research assistants. My colleagues, for whom I have always had the greatest affection and admiration, have functioned as a team, with open doors and profound interest in encouraging the students. They have kept me interested and informed over a wide range of economics topics, have helped with my research, including joint authorship, have joined in the smoothest running of a department one could imagine, and have been wonderful friends.  I landed a job at Berkeley as one of four new assistant professors who started in September 1963, having been recruited by Andy Papandreou, shortly before he returned to Greece. Among the four of us are three Nobel Prizes. In 1963–64, Berkeley was a perfect place to be a young theorist. The junior faculty interacted all the time, and became the best of friends. Particularly important for me were [Dan McFadden](https://www.nobelprize.org/nobel_prizes/economics/laureates/2000/), Bernie Saffran, and Sid Winter. The senior faculty were supportive. Together with Tibor Scitovsky, I taught the graduate micro-macro sequence (one semester of each). I also taught a year-long undergraduate public finance class, which had a prerequisite of intermediate theory. I taught public finance again the following year, along with money and banking. The theory prerequisite and the length of the public finance class allowed for a thorough exploration of topics, and introduced me to the experience of discovering results leading to good papers as part of developing material for a class of sharp, attentive students. My first such experience led to my 1965 paper on the public debt. While many are concerned about the tension between teaching and research, my experience is that they reinforce each other. Indeed, the times when I had the most difficulty in finding good research topics have occurred when I did little teaching.[1](https://www.nobelprize.org/prizes/economic-sciences/2010/diamond/biographical/#not1)  Berkeley was a great place for me at the time for more reasons than good teaching and research opportunities. Top of the list was meeting my wife Kate (real name Priscilla Myrick), a student in law school at the time I started teaching economics. Despite the fact that I was teaching public finance and she found taxation to be her least favorite class, there was enough attraction that we married in 1966, shortly after I returned to MIT.  For more on how valuable teaching has been for my research, and for the diverse ways I approached finding and choosing topics to work on, see Moscarini and Wright, 2007 and Diamond, forthcoming.  Berkeley also gave me the opportunity to witness the start of the student uprisings, with the Free Speech Movement of 1964. I watched the repeated mistakes of the Berkeley administration, mistakes repeated again and again by university administrations across the country. Being part of a faculty trying to help, but not succeeding much, was quite an education.  One of the pleasures of an academic career is the opportunity to visit places, for short times and, especially valuable, for long ones. My first year of leave was 1964-5, after just two years of teaching. I was an Overseas Fellow at Churchill College, Cambridge, an opportunity organized for me by Frank Hahn. Having Frank as a colleague was one of the prime attractions for going to Cambridge. Talking economics with him lived up to expectations, and we became good friends. Living in college, meeting scholars from an array of fields, spending lots of time in the economics faculty, and tutoring a few outstanding students was terrific.  In the early spring there arrived one of those blue air letters, which one rarely sees any more, from Bob Solow, asking if I would have any interest in returning to MIT. Expecting to accept, I promptly altered my plan for the rest of my leave and returned to Berkeley to see Kate, before heading for MIT. I proposed in October, and we were married 10 days later. Our marriage, and our sons, Matt (born 1972) and Andy (born 1979), and the love we all share, have shaped, enlivened, and enriched my life in ways I could not anticipate as a young single man. They are three great people.  Family and economics have been the two poles around which I function, one a source of great joy and the other of great pleasure. As a 70th birthday gift, Kate commissioned a piece of music from John Harbison in my honor, titled “Diamond Watch: Double Play for Two Pianos.” And she arranged for me to throw out the ceremonial first pitch at Fenway Park, home of the Boston Red Sox, as I had dreamed of doing for many years. A photo of that pitch, and one of the shirts worn by the graduate students at that game, were donated to the Nobel Museum. And my family has put up with my heavy commitment to work. Apart from following professional sports, economics is my only hobby – reading and writing, talking and listening. I do read a fair number of mysteries and enjoy looking at paintings with Kate, an art historian who was a Curatorial Associate at the Boston Museum of Fine Arts.  Having cut short my time at Churchill, and not having had a proper honeymoon, Kate and I returned to Churchill for the summer of 1967, the start of much wonderful traveling we have done together. Shortly after we arrived, I gave a seminar on optimal taxation, based on results that had come from teaching a graduate public finance class for the first time that year. [Jim Mirrlees](https://www.nobelprize.org/nobel_prizes/economics/laureates/1996/) approached me after the seminar to point out that my model of a one-consumer economy was a good base for analyzing a many-person economy since I had set up the problem in price space instead of quantity space. That prices were the same for all households was key to that extension. We started joint research on optimal taxation that summer, and had essentially finished our first paper by the end of the summer, although it was not published until 1971.  Jim and I have written a dozen papers together over the years, having something in progress (sometimes very slowly) at all times. I think that the success of this collaboration has come from an ideal distance between the ways our minds work. Similar enough that communication is quick and thorough, but different enough that we did things that I suspect neither of us would have accomplished alone. (Even our rare miscommunications were sometimes fruitful, opening up an issue neither of us had recognized.) When collaborations leave you with that feeling it is very good, much much better than merely having shared the work. I have had positive experiences with a long list of collaborators, with very positive memories of all of my collaborations but one.  My next leave was 1968–69 and we each chose a place for an extended stay – Kate chose Nairobi and I chose Jerusalem. A hike up Mount Longonot, a dormant volcano in the Rift Valley shortly after we arrived in Kenya was practically the first time I had my feet off pavement. Both places were eye-opening, given how limited was my range of previous experiences. And our route home included two months with the Mirrlees family – a month in Oxford and a month with the two families sharing a place on the Mull of Kintyre. In the time before the Internet, finding good ways to be together played a key part in our collaboration and in our friendship.  Since 1974, while continuing to do basic research, I have also been involved in policy analysis, primarily about pensions. Key to my work in this realm has been a series of enjoyable collaborations, with Bill Hsiao, Jerry Hausman, Jon Gruber, Peter Orszag, and Nick Barr. This started serendipitously when, at the recommendation of [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/), Bill invited me to join the Panel on Social Security Financing consulting to the U.S. Senate Finance Committee, and I accepted. Pensions have been a perfect topic for me. They fit well in my public finance theory interests and with my social concerns. I have given many talks on Social Security, and ended some of them with a quote from Franklin Roosevelt, which I saw at his memorial in D.C.: “The test of our progress is not whether we add more to the abundance of those who have much; it is whether we provide enough for those who have too little.” (The Second Inaugural Address, January 20, 1937.)  For me, policy analysis and basic research are mutually supportive. Policy discussions have alerted me to interesting research questions that had not received adequate analysis. And my policy analysis draws heavily on my understanding of economic theory and reading in the empirical literature. Indeed, without being based on real understanding of how policies could accomplish good ends, making policy recommendations seems very hit or miss.  In 1992, I became the first holder of the Paul A. Samuelson chair. This pleased me greatly, and seemed to please him. Beyond being a great economist, as everyone knows, and, together with Bob Solow, the creator and shaper of the MIT Economics Department, Paul has always been a true friend. In 1997, I gave up the chair and became an Institute Professor.  I have worked in a large number of different areas. The kind of theoretical work I most enjoy is sorting out how to approach a problem to get insights, more so than refining models to shed further light on it. Thus, it was natural for me to explore new areas once I felt I had hit diminishing returns in one area. However, revisiting a topic after years away from it has been fruitful as well. Of the different areas in which I have worked, I want to discuss only how I came to do what was eventually recognized for this prize.  My deep grounding in general equilibrium ([Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/)-Debreu) theory includes a keen awareness of its limitations. The limitation that particularly interested me was the completeness of the coordination of agents that happens with complete competitive markets. For my first foray into changing the theory, I simply limited the set of available markets in a 1967 paper on the role of the stock market in resource allocation. Arrow-Debreu theory does not contain a mechanism or process for an economy to achieve its equilibrium allocation. In the 1960s there was ongoing work to find a hypothetical process that would converge to this equilibrium, with a focus on equations for price adjustment based on excess demands or supplies at tentative prices (referred to as tâtonnement). It struck me that the wrong question was being asked.  For my second foray into changing the theory, rather than asking whether a process could be found that would converge to a standard competitive equilibrium, I decided to look for the allocation to which a plausible process would converge. This led first to my 1971 paper that applied search theory to a retail market. I then took thinking in terms of a process in real time to the law-and-economics question of the effects of alternative rules for breach of contract (jointly with [Eric Maskin](https://www.nobelprize.org/nobel_prizes/economics/laureates/2007/)), and then to the labor market, and then to the entire economy. Dissatisfaction with (well-understood) analysis was part of the drive that led to this success; trying to get a more satisfactory perspective that would open up the ability to better answer economic questions was another part. And greatly enjoying the work itself mattered too.  This is still what I love doing and hope to continue.   |  | | --- | | References | | 1. Debreu, Gerard. 1959. *Theory of Value: An Axiomatic Analysis of Economic Equilibrium*. New Haven, CT and London UK: Yale University Press. | | 2. Diamond, Peter. 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| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0829=PD  [Peter Diamond] Hello?  [Adam Smith] Hello, Professor Diamond, this is Adam Smith calling from the Nobel Prize website in Stockholm.  [PD] Mm hmm.  [AS] My congratulations on the award of the Prize.  [PD] Thank you.  [AS] So, may I start by asking where you were when the news broke?  [PD] I was on a red-eye from San Francisco to Boston, the last leg of a trip that started in New Zealand. So, when I got off the plane at seven in the morning, I knew nothing about it. My wife picked me up, wife and son picked me up, at the airport. And, they hadn’t yet mentioned it when my cell phone rang. And, it was my good friend Nick Stern calling from London to congratulate me! And, that was the first way I learned about it.  [AS] Well, these first days of Laureate-hood are notoriously busy, so what an extraordinarily sort of tiring start to such a busy day.  [PD] Yes! [Laughs] I’m certainly feeling it already. And, there’s quite a bit of the day stretching out ahead of me.  [AS] I bet! Are you the sort of person who enjoys the publicity this garners?  [PD] Um, yes and no. It’s fun. It is an opportunity to get out some messages. It does block doing the kinds of things I normally do, which I will of course need to get back to at some point.  [AS] At some point, yes. You mentioned messages. Is there any message in particular that you’ve been using the opportunity to promote?  [PD] Well, the press conference that happened here included questions about the fiscal stimulus in the US, the bail out of the big banks, and I think it’s important – and that’s not tightly connected with what the Prize recognized, but it’s in the general ball park – and I think it’s important that the American public realize that if we hadn’t bailed out the banks, unemployment would be a great deal higher than it is now. If we hadn’t had fiscal stimulus, unemployment would be a lot higher than it is now. And so [phone rings in the background], when I was asked those questions, I was glad to have the opportunity to say that.  [AS] Yes, indeed. Now, you’ve worked previously with other Economics Laureates, for instance [James Mirrlees](https://www.nobelprize.org/nobel_prizes/economics/laureates/1996/).  [PD] Yes, we first met at an Econometrics Society meeting in Zurich in 1964. And we became friends – good friends – when I spent 1965/6 in Cambridge at Churchill College. And then we started collaborating in the summer of 1967, when I returned to Cambridge with my wife, having just married over that year. And we had both been thinking about optimal taxation and we started working together. We liked working together. We’re still working together! There hasn’t been a time from then to now where there wasn’t a paper in process.  [AS] [Laughs] That’s a good collaboration indeed.  [PD] Yes.  [AS] The work for which you’ve been awarded is essentially the development of models, which incorporate real-world inefficiencies into the study of market transactions.  [PD] Real-world frictions is what’s being incorporated, and they can lead to inefficiencies. I mean, I think that the slight difference in my wording from yours, I think matters for the sense of the underlying methodology.  [AS] Right.  [PD] The target isn’t ‘let’s stick in an inefficiency’. The target is ‘let’s stick in some realistic frictions and find out what that implies for how the markets work’.  [AS] Mm hmm. And, can you give me just one example of the sort of insight that such models provide?  [PD] The work I did with Olivier Blanchard on the relationship – the Beveridge curve – the relationship between vacancies and unemployment, and the fact that it is really a process that has a dynamic to it. And so one doesn’t jump to instant conclusions from a current snap shot. One has to recognize that this is a process that has hysteresis in it, has significant legs. And one has to think through stimulating the economy around the idea that you’re working through a process. And a process where expectations matter a great deal.  While the bulk of the work, and what the citation referred to, was labour market work, there was also reference made to a related paper I did on the role of aggregate demand. And the point here is that you can have – just looking at rational expectations – you can have multiple equilibria. So, you have to look beyond that to get a determinant of what would actually be happening. And doing that is an underpinning for thinking about how the economy can be, at times, fragile. Because there are two different ways where it’s sensible for people to go if they believe other people are doing something. A bank run is the obvious example, but that’s primarily the work of [Douglas Diamond](https://www.nobelprize.org/prizes/economic-sciences/2022/diamond/facts/), no relative!  [AS] And have you worked closely with Christopher Pissarides and Dale Mortensen as well?  [PD] Ah, no, I have not. I mean, I’ve certainly known them for a long time and admired their work. And, as I’ve mentioned in the press conference here at MIT, one of the key steps for me in developing my work was reading one of Dale Mortensen’s papers, back before he was collaborating with Chris, where he used Poisson processes in a way that I realized was an enormously valuable tool for exploring the kind of dynamics I had in mind. So, I have frequently cited that paper of his as a very stimulating one for me.  [AS] Ok, well thank you very much indeed. Have you been able to make any plans for celebrations for later in the day?  [PD] [Laughs] Sleep is, I think, the prime celebration I’ll be having! At some point the brain will shut down and the body will collapse and tomorrow we’ll think of actually having enough energy to celebrate.  [AS] Well, I wish you luck in finding sleep with lots of people like me trying to get you.  [PD] Thank you.  [AS] Thank you for speaking to us. Thank you, bye bye. |
| Interview |  |
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| ID | 0830 |
| Biographical | As the children of immigrants, my parents were raised in Scandinavian Minnesota. My mother, Verna Ecklund, was a university student for only one year but my father, Thomas Peter Mortensen, graduated from the School of Forestry at the University of Minnesota in 1936. They were married shortly after and moved to Enterprise, Oregon, where I was born in 1939. Enterprise, located in the far northeastern corner of the state, was in cattle ranching country surrounded by one of the most beautiful mountain ranges in the U.S. In these mountains, my father began his career as a lookout officer for the U.S. Forest Service. In the war years, they migrated further west to the Portland area where Dad help build Liberty ships in Mr. Kaiser’s ship yards and Mom provided day care for the children of Rosy the Riveter. After the war, the family, which now included my brother Arne born in 1941, moved to the Hood River Valley 60 miles east of Portland where again my father returned to the practice of forestry. There my brothers and I, who included Irving born in 1947, were raised.  The valley of the Hood River runs north-south between the majestic volcanic peak of Mt. Hood and the stunning Columbia River Gorge. Nestled as it is in the beauty of the Cascade Mountains, it divides the wet western part of the state of Oregon from the dry highlands of the eastern two thirds. As such it contains all of the commercial forest species of the Northwest on the slopes of the mountains: the Douglas fir, the hemlock, the true firs such as noble and white on the west and several pines, including white and yellow, on the eastern slopes. The valley itself is a garden of fruit trees that include bartlett, anjou, and bosc pears as well a variety of apples and cherries. Since I grew up there, the valley has also developed many vineyards and the town of Hood River on the Columbia has become a major destination for wind surfers.  My parents, as one of a few with any advanced education among the fruit growers and loggers of the valley, entertained the intellectuals of their community, their colleagues of like mind, the local school teachers, and a few eccentrics. I grew up listening to my father argue politics into the night and taking trips every Saturday to the Hood River library where my mother maintained her interest in reading and encouraged the same from her sons. This library was one of those small town institutions that benefited from the largess of Andrew Carnegie, the founder of U.S. Steel and the great “robber baron” of Pittsburgh. In the days of my childhood in the 40s and 50s, long before the internet, it was these institutions and the occasional inspirational teacher that provided windows to the world of ideas to those living in the small towns and rural countryside of America. I benefited greatly from having been exposed to excellent examples of both sources of enlightenment.  I was a good student with mathematical ability and interests. As such, I took the usual college preparatory program in high school for one looking to become an engineer, all the available courses in mathematics and science. In my last two years of high school and all through college, I also used these skills in summers as an assistant to my father who managed the forest holding of a local timber company, a job that included assisting in surveying the company’s land, cruising its timber, and designing the roads used in the logging operations. I was also “well rounded” in the sense of that cliché in the 1950s. I had participated in boy scouts and 4H while in elementary school, had a bass baritone voice which I used in school and church choirs until 15 years ago, dabbled with acting and musical theater in both high school and college, and lettered three years as a member of the varsity football team of Wy’east High School, the new combined high school that served the valley students. I was even named all-league in my senior year high school, an achievement that I attribute to my cleverness more than my athletic ability.  In my last year at Wy’east, I became interested in American social history of the late 19th and early 20th centuries and the related literature, much of which concerned the industrialization of America. My grandfathers had been immigrants in the period; one had done reasonably well but the other lost his wife to tuberculosis in the twenties and his farm to the Depression of the thirties. From [Sinclair Lewis](https://www.nobelprize.org/nobel_prizes/literature/laureates/1930/), [Hemingway](https://www.nobelprize.org/nobel_prizes/literature/laureates/1954/), [Faulkner](https://www.nobelprize.org/nobel_prizes/literature/laureates/1949/) and Upton Sinclair as well as the stories that my father’s friends told about their experiences, I expanded my knowledge of the human condition during this period. From the progressive historians of the early twentieth century, I formed my own picture of the industrialization that had transformed the country at that time and developed an understanding of how it had affected the lives of my elders for both better and worse. By the end of my senior year in high school, I faced a dilemma … how could I combine my interest in analytical problem solving with a concern for social issues?  A high school friend, Rusty Beaton, who had graduated the year before my last year, told me about his economics professor at Willamette University, Richard Gillis, who had inspired him in his first year at university. By chance, I had also found at the Hood River Library *The Theory of Games and Economic Behavior* by von Neumann and Morgenstern, the seminal text on what was to become game theory as well as a major application of mathematics to social analysis. I realized that economics was a possible way to combine my two principal interests. The next year I enrolled in Willamette as the beneficiary of a full tuition scholarship, majored in economics and mathematics, and ended up as one of two of the senior assistants to Professor Richard Gillis in my senior year. After benefiting from the solid liberal arts tradition of the oldest university west of the Mississippi, I went on to do graduate work at what was then the Carnegie Institute of Technology. In addition to the fact that I was offered a more generous fellowship than either Stanford or Harvard, it was an innovative program that emphasized an analytic as well as interdisciplinary approach to the study of economics and management. I must admit, I was also attracted by the fact that it was located in Pittsburgh, which had played central role in the industrialization of America.  As it turned out, Pittsburgh was already on its way to becoming the beautiful city that it is today when I arrived in 1961, not the smoky and dirty place it was still reputed to be. Although [Herbert Simon](https://www.nobelprize.org/nobel_prizes/economics/laureates/1978/) had turned his attention to the application of the computer to artificial intelligence by the time I had arrived in 1961, he still taught those of us in the Ph.D. program how to think about social phenomena and how to represent those thoughts in a mathematical model. We were also introduced to the wonders (and frustrations in these days of punch cards and slow turnaround) of the digital computer and taught how to apply it to real computational problems that arose in management using the recently developed methods of linear, non-linear, and dynamic programming. We were instructed in the new behavioral approach to the theory of organizations in general by Jim March and Herb Simon and the application of that theory to the analysis of the firm in particular by Richard Cyert and Jim March. Although these ideas interested me, eventually I turned to more traditional economics and began a thesis in growth theory directed by Michael Lovell, who by chance happened to be the son of my favorite history professor at Willamette. Along the way, I also found the love of my life, Beverly Patton. We were married in 1963 and soon became the proud parents of three children, Karl, Lia and Julie, between the time I started and completed my thesis.  Carnegie Tech was an amazing place at the time. New ideas of all kinds were in the air. They were not always consistent with each other, as in the case of the conflict between John Muth’s suggestion about how to model expectations as “rational” and Simon’s notion of “bounded rationality.” We students benefited from the lively debates among the faculty. We were also encouraged by the faculty to get involved in the research process even before we had mastered the details of the literature. In my last two years, I took courses from [Robert Lucas](https://www.nobelprize.org/nobel_prizes/economics/laureates/1995/) and [Oliver Williamson](https://www.nobelprize.org/nobel_prizes/economics/laureates/2009/) with fellow student [Ed Prescott](https://www.nobelprize.org/nobel_prizes/economics/laureates/2004/), all of whom are now Laureates. In retrospect, it was obviously a very special educational experience.  I became a member of the faculty at Northwestern University in 1965 but did not complete my thesis until two years later at a graduate ceremony at which Carnegie Institute of Technology became Carnegie-Mellon University. At Northwestern I was mentored by the “three Bobs,” Robert Eisner, Robert Strotz and Robert Clower. In my first couple of years, I finally learned the foundations of economic analysis by teaching [Hicks](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/) and [Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/) to first year Ph.D. students. Those first three years were a struggle trying to develop teaching skills, finish a thesis that was taking too long, and helping care for three babies. Without the patience and perseverance of a loving wife, I would not have succeeded.  My interests in economics changed in this new environment. Two new and important colleagues arrived, Frank Brechling and Art Treadway. Frank’s research was focused on the dynamics of the labor market and Art, a student from the University of Chicago, was developing costs of adjustment models and applying them to investment theory. Together with colleague Bernt Stigum who was doing work on both time series analysis and dynamic general equilibrium, we taught ourselves dynamic control theory from the pages of Pontriagin’s book. Finally, I was fully proficient in control theory as well as dynamic programming, the tools that dominate macroeconomic theory to this day.  In the mid 1960s a debate raged over the Phillips curve and its implications for economic policy. Some interpreted the curve, a negative statistical association between the inflation rate and the unemployment rate, as an unpleasant trade-off that policy makers had to face. According to this view, one could use monetary policy to lower unemployment but only at the expense of a higher permanent rate of inflation. However, the theory behind the curve was suspect. [Milton Friedman](https://www.nobelprize.org/nobel_prizes/economics/laureates/1976/) in his 1968 AEA Presidential Address declared that the economy tends toward a ‘natural’ rate of unemployment determined in market equilibrium which is invariant to the inflation rate, at least in the long run. Monetary policy can reduce the unemployment rate over the relative short run by lowering real wages. However, any attempt to maintain the level below its natural or equilibrium rate though monetary policy will be frustrated by ever increasing inflation as workers and employers realize what is happening.  [Edmund Phelps](https://www.nobelprize.org/nobel_prizes/economics/laureates/2006/) (1968) argued along similar lines. Moreover, he complemented his assertion with a new view of how the labor market works. Speficially, he suggested that some positive unemployment level was a natural outcome of the matching process in the labor market. Any attempt to lower unemployment below that determined by the rational agent behavior of individual employers and workers by inflating the economy would simply induce adjustment back to its natural level at a higher rate of inflation. Underlying the arguments of both Friedman and Phelps was the long standing proposition that there is no money illusion. Hence, only real wages matter, at least in the “long run”. Any attempt to artificially manipulate the price level through monetary policy would induce agents in the labor market to agree to offset the effect by revising the nominal wage.  At Northwestern we created an informal reading group focused on the macro economic implications of the new ideas about the labor market circulating in the profession. Included in the group were my colleagues Frank Brechling and Art Treadway as well as a visitor to the department, the British economist Chris Archibald. Both Frank and Chris had known Phillips at the London School of Economics, knew his work well, and had done research that was stimulated by it. With their help, I began to think about how to capture the essential features of a decentralized market with search friction in a formal but simple economic model. Using the labor market as my focus, I came up with the idea of modeling the consequence of search and matching friction as the outcome of a sequence of random meetings between potentially interested parties.  After I had written a very long working paper designed to formulate and work out some of these ideas, Edmund Phelps came to Northwestern to give a seminar in the fall of 1968. During his stay, he informed me that he had read my paper with considerable interest. Furthermore, my ideas were closely related to those in a working paper of his co-authored with Sid Winter as well as several other working papers by others that tackled the issues that arose in decentralized exchange among rational agents that possessed only imperfect information about trading opportunities. He then revealed that he was planning a conference on the topic at the University of Pennsylvania, his academic home at the time, in which these papers would be presented and discussed. I don’t recall whether he invited me to participate in the conference on the spot or whether that invitation came later. In any case, I was fortunate to have my first major paper, “A Theory of Wage and Employment Dynamics” published in the collection of papers presented at his conference that became universally known as the “Phelps volume”.  The Phelps volume published in 1970 bore a rather pretentious title: *Microeconomic Foundations of Employment and Inflation Theory.* It became a classic anyway. The basic message of the collection was that one could and should consider the dual macroeconomic problems of employment and inflation as the outcomes of market behavior of individual agents who act in their own interests as best they can in a market environment characterized by uncertainties and incomplete information. As a corollary, the papers argued that macroeconomics should be founded on microeconomic principles. Included among the authors who published in the volume was Robert Lucas. Although the three of us, now all Nobel laureates, have not always agreed on the details and taken different direction in the pursuit of the goal, we have shared a common view that macroeconomics needs a foundation in equilibrium market analysis based on the principle that agents in such markets act in their own self interest. In the intervening forty years this view has come to dominate macro-economics.  My companion paper, “Job Search, the Duration of Unemployment and the Phillips Curve,” published in the *American Economic Review* in the same year, was an attempt to use my ideas about decentralized exchange in the labor market to provide an interpretation of the Phillips curve. I refer to it here, not because I accomplished that goal. In fact, I regard the paper as a failure in that dimension for several reasons. One of these was the inability to close the model with a convincing theory of agent expectations. Arguably this could have been done by invoking “rational expectations,” a concept that had already been introduced by one of my then Carnegie Tech professors, John Muth (1961). Unfortunately, I did not see its relevance for my work at the time even though I had participated in a seminar on the subject briefly as a student. The issue was subsequently resolved by later adopting this approach in spite of its drawbacks.  The actual contribution of the AER paper, along with those of John McCall (1970), published at about the same time, was the formal model of sequential wage search that it embodied. In the model, an unemployed worker samples sequentially from a known distribution of wage offers until one is found that exceeded an optimally chosen reservation wage. The optimal reservation wage is simply that which compensated the worker for forgoing the option of continuing to search for an even better wage. Although this formulation was simply an application of the well-known optimal stopping problem in statistical decision theory, it was a new dynamic approach to the understanding of unemployment that incorporated the need of workers to gather information. Since this need suggested that the time spent searching was productive, the model offered an entirely new view of unemployment.  At the time, Keynesian thought dominated the profession’s view of unemployment. Viewed through the lens of classical supply and demand analysis, Keynesians argued that unemployment arose because the real wage was too high. Unemployment in this view was totally involuntary; unemployed workers were simply those that could not find a job at the prevailing wage. The idea that workers might rationally choose to be unemployed was beyond their imagination. Indeed, when I presented a working paper version of what became my AER article at the annual meeting of the American Economic Association, some who attended walked out in protest. Nevertheless, my colleagues at Northwestern chose to promote me to Associate Professor with tenure in the fall of 1970 with only two forthcoming papers to my credit.  My family and I spent the academic year 1970–71 on sabbatical visiting at the University of Essex in England with my colleague Frank Brechling. At the time Frank and I were working together on a project designed to formulate and estimate a model of employment dynamics based on the idea that the process of recruiting and hiring workers was a time and resource consuming process. As part of that project, I wrote a draft of my theory paper “Generalized Costs of Adjustment and Dynamic Factor Demand Theory” (Mortensen, 1973) which provided a mathematical foundation for investment in an arbitrary number of capital goods couched in terms of mathematical control theory.  Several of the Essex faculty had visited Northwestern previously, so we had a congenial group of colleagues that also included John Kennan, a Ph.D. student at the time, who served as our research assistant. Christopher Pissarides was a student at Essex finishing his undergraduate course that year. He tells me we discussed his plan to pursue search theory as a graduate student at the London School of Economics although I don’t recall the meeting. During the year I was invited to Oxford, London School of Economics, and Durham University to present my current work on search in the labor market. These were opportunities to meet English economists, opportunities that were rare in the expensive early days of the jet age.  The 1970s was the decade of developments in the new area of information economics. Search theory, which emphasized the need to gather information, was joined by models that featured asymmetric information, the case in which information differed across individual agents. Signaling, screening, moral hazard and adverse selection, terms never mentioned when I was a graduate student, became the new vocabulary of the analysis of market performance during these years. Search theory, particularly as it related to the phenomena of unemployment, continues to develop as well.  The debate over whether unemployment was “voluntary” or “involuntary,” “natural” or not, reflected “disequilibrium” or was an “equilibrium” phenomenon continued. Although thankfully this vocabulary has disappeared for the most part, the basic issue has not. Are workers unemployed because the real wage fails to clear the labor market or does unemployment reflect the trade-off between the value of time spent searching and its value in production given the search frictions identified in the new approach to unemployment and inflation expressed in the Phelps volume? [James Tobin](https://www.nobelprize.org/nobel_prizes/economics/laureates/1981/) (1972) in his AEA presidential address, “Inflation and Unemployment,” was perhaps the first to ask the question in precisely this form.  Tobin was particularly critical of the prevailing job search model which assumed, as many still do, that only unemployed workers search and that workers might quit to unemployment in order to find better jobs. He cited sketchy evidence for the hypothesis that large numbers of workers move from one job to another without an intervening period of unemployment, a hypothesis which has since been verified. We now know that about half of hires in the U.S. and an even larger fraction in other countries consist of workers who already have jobs. To the extent that search-on-the job was feasible, productive search unemployment as an alternative explanation for unemployment was questionable.  Stimulated by Tobin’s critique, my student Ken Burdett developed the first formal model of search on-the-job in his Ph.D. thesis, which was published as Burdett (1978). Although the analysis clearly illustrated the fact that the worker criteria for an acceptable job are less stringent when there when the option to search while employed is available, the idea that the unemployed were engaged in job search, as standard measures require, was fully captured by the theory. Of course, this fact did not rule out the possibility that the real wage may be too high as well, a point, which seemed to escape some Keynesian critics of the new theory.  Challenges to the idea that workers search because wages differ across employment opportunities came from the other contributors to the theory. Peter Diamond (1971), in his attempt to understand how prices might be set in a world of search friction, found that wage-setting employers would offer a single wage when workers located employment opportunities sequentially. Surprisingly, that wage was the monopsony wage, one which lies below the value of a worker’s marginal product. This result led Rothschild (1973) to ask, “What is the source of wage dispersion that is supposed to motivate search unemployment?” Later Burdett and Judd (1983) answered the question with a paper that provided that foundation for the modern theory of wage dispersion.  Partly in response to these developments, the theory of unemployment shifted its emphasis from the problem of a worker seeking a high paying job to the formation of good job-worker matches. The matching problem, whether found in the labor, housing, or the marriage market, is one of forming complementary pairs in a world in which individual workers and jobs are heterogenous. It takes time and resources to accomplish this task, and the duration of unemployment experienced by individual workers as well as the length of time that an existing job is vacant reflect this fact. Empirical labor economists found this idea persuasive and used the models developed in the 70s, such as that presented in Mortensen (1976) and Burdett and Mortensen (1978) together with the statistical tools of duration analysis to interpret both unemployment and job spell data.  In 1980, Ken Burdett and I taught an intensive two-week short course on search and matching theory in Oslo. Our students were young Ph.D. candidates from all of the Scandinavian countries. At the time, Ken and I together with George Neumann and Nicholas Kiefer were estimating three state models of worker flows using longitudinal panel date generated as part of the Seattle/Denver income maintenance experiment (Burdett *et al,* 1984). One of our students, Lars Muus, informed us that a group of economists at Aarhus University were creating employment and earning history spell data from Danish administrative records. These data had the advantage that they were collected continuously, covered the entire population, and could be linked to detailed information about each individual’s education, age, and family demographics. In August 1982 all four of us were invited to attend the first conference featuring these data held at Sandbjerg Manor, a convention center in the south of Jutland, owned by Aarhus University and built around an 18th century manor house. My visit to Denmark with my wife and father, who had been born in Jutland and emigrated to the U.S. at age 10, initiated a fruitful association with Aarhus University as well as a rewarding interaction with my Danish cousins.  In the early 80s, Peter Diamond (1982a, 1982b) and I (1982a, 1982b) were both creating models of two sided search and matching equilibrium. These papers incorporated the concept of a matching function and assumed that wages were determined through bilateral bargaining. They were two sided in the sense that agents on both sides of the market made search investments in the process of creating a match.  The matching function is a postulated relationship between search and recruiting effort and the aggregate rate at which workers and job meet. Once a particular pair meets and finds that the match offered a surplus relative to continued search by both, it is formed. The division of the surplus, which determined the expected future wages and profits that employer and employee enjoy once matched, is regarded as the outcome of a bilateral bargain. Shortly after the appearance of these papers, Christopher Pissarides (1985) completed what became known at the Diamond-Mortensen-Pissarides or DMP model of the labor market by adding a job creation condition which determines the number of vacant jobs as that which equates the cost of creating a job to the expected present value of profit from an acceptable match.  The DMP model is an equilibrium theory of unemployment determination. It has important implications for unemployment dynamics and for the effects of government policies on labor market outcomes. For example, the model implies that procyclical movements in vacancies will induce countercyclical movements in unemployment as Beveridge (1944) documented for the U.K. in the early 20th century and has been established for every other developed economy with data on job openings. However, it also provides insights into the effects of unemployment insurance and taxes as well as active labor market policies. As more generous unemployment insurance adds to the option value of continued search as an unemployed worker, one expects higher wages and longer unemployment spells in countries with high benefits and long unemployment benefit periods. The theory also implies that job search training will lower the time required to find acceptable employment. Generally empirical studies have confirmed these predictions as well as the existence of a relatively stable matching function.  In the original version of the DMP model, job separations were regarded as exogenous. The research of Davis and Haltiwanger demonstrated that layoffs were an important determinant of movements in unemployment (Davis *et al.,* 1998). In a review of the first edition of Pissarides’ book, Equilibrium Unemployment, published in 1990, I suggested that this fact should be incorporated into the model. Later, Chris and I collaborated on the task in Mortensen and Pissarides (1994) and Chris incorporated the extended model into the second edition of his book in 2000.  The extended version of the DMP model supposes that the productivity of a job is subject to an idiosyncratic shock process as well as an aggregate shock. In this setting, a job is destroyed when its productivity falls below the value to both the employer and employee of seeking an alternative match. This formulation explained the basic asymmetry which characterized the typical effects of a business cycle. Namely, layoffs which occur early in the downturn are typically large and take place over a relatively short period of time, while recovery in employment once started is spread over a much longer period of time.  In a subsequent series of co-authored papers, Chris and I also investigated the implications of the model for a wide variety of government policies including employment protection. We found that our model did a good job of capturing the principal effects of firing restrictions, a reduction in both the typical flow of workers from employment to unemployment and from unemployment to employment, that characterize economies with strong employment protection. According to the model, economies with labor market inflexibilities of this kind have problems adjusting to the changes in technology experienced in the last thirty years. This argument has induced reforms in a number of countries, which take the form of allowing fixed contracts with no layoff penalty. Most of these findings are summarized in Mortensen and Pissarides (1998a, 1998b).  In another line of research, Ken Burdett and I pursued the implications of Diamond’s original model of dynamic monopoly in the late 80s and 90s. As noted earlier, Diamond’s analysis when applied in a labor market context implied that wages are set so that employers capture the entire surplus of a match if employers have the power to set the wages of their employees and workers search for jobs sequentially. Although the assumption that employers can post wages seemed consistent with how wages are determined in many labor markets, Diamond’s conclusion that all employers set the same wage in an environment where many employers compete seemed inconsistent with empirical observation.  Ken Burdett, together with his then student Ken Judd, generalized the Diamond result by showing that equilibria exist characterized by price dispersion, different prices offered for the same good, if some buyers have the option of choosing between two offers while others must accept or reject offers sequentially. Shortly after the publication of their paper, Burdett and Judd (1983), Burdett invited me to spend a quarter visiting at Cornell, his employer at the time. One day, I pointed out to Ken that conditions that he and Judd had derived for the existence of price dispersion were automatically satisfied in a labor market model in which unemployed workers search sequentially but employed workers also search and can choose between continued employment and any alternative wage offer generated. In 1990, we both published papers that worked out implications of this idea and submitted a jointly authored paper that provided the basic logic of our model to a number of top rated journals. Apparently referees and editors were not ready for our idea. After a series of rejections, each following a long review period, our paper entitled “Wage Differentials, Employer Size and Unemployment” finally appeared in the *International Economic Review* in 1998. By this publication date, the paper was already well known and had become the theoretical foundation for a new empirical literature on wage dispersion, which I reviewed in my Zeuthen lectures published in 2003 under the title “Wage Dispersion: Why Are Similar Worker Paid Differently?” The paper remains the classic reference in the new labor literature on monopsony. (See Ashenfelter *et al.,* 2010 for a review of recent work.)  I was again attracted in 1998 to Aarhus, where I spent 6 months at the Center for Labor Studies associated with Aarhus University learning about a new data source. My friends and colleagues there had integrated their longitudinal data on individual labor market histories with data that included firm identifiers and some information about firm characteristics. Later these data were augmented with accounting information from income statements and balance sheets. Along with the well-known French data set, it has become a major source of information about job-worker matches. Although there is now a similar U.S. data set created by combining business census data on firms with employment spell histories of worker qualified for unemployment insurance, the Danish data dominate the other two in the sense that it includes more detailed information about individual workers.  Detailed longitudinal data about firms had recently been exploited for labor market studies by Davis and Haltiwanger for their purpose of creating so-called job-creation and job-destruction time series, work, which is summarized in their book with Scott Schuh, *Job Creation and Destruction*, published in 1998. Their evidence clearly reflected the heterogeneity of firm growth rates in employment at the firm level. Subsequent research based on these data illustrated several interesting patterns relating firm employment, wages, and productivity. Namely, larger firms pay more and are more productive. Trade theorists added the fact those firms that engage in international trade are larger, more productive, and pay wage premiums. To my colleagues and me in both Denmark and the U.S., these facts suggested that workers and other resources should be moving from smaller less productive and lower paying firms to larger more productive firm and that this reallocation process might be an important source of economic growth as well as an explanation for why wages differed so much across firms.  In Lentz and Mortensen (2008), Rasmus Lentz and I developed and estimated a model in which productivity differences are the outcome of differences in the ability of firms to develop new products. These differences induce differential firm growth, which can explain the positive association found between firm productivity, average wage paid, and size in the data. Furthermore, an empirical version of the model estimated using the Danish data implies that over 50 percent of the growth rate in the value of goods and services of the private sector over the sample period studied can be attributed to this reallocation process. In our original model, the friction that sustains differences in productivity is a combination of the fact that every product is eventually displaced and that growth is costly. Although we abstracted from search and matching friction we are now working on estimating a model which also includes this possibility.  Obviously, the focus of this narrative is on my research and the development of some of its principal themes over the years. At the same time I was leading a life that was enriched by teaching and family relationships. I taught both undergraduates and graduate students while still at Carnegie Tech in the early 1960s. Although most of my teaching took place at Northwestern University, my home base since leaving graduate school in 1965, I have also taught at Essex University, Cornell University, the California Institute of Technology, New York University, and more recently at Aarhus University during visits of various length. Indeed, during the last five years I have split my academic year equally between Northwestern University as the Ida C. Cook Professor of Economics and Aarhus University as the Niels Bohr Visiting Professor of Economics.  With undergraduate class sizes that varied from 30 to 300, simple calculations suggest that there was a large number of students that listened to my voice if not to my wisdom. I have been fortunate to have excellent students and I hope that my influence on them has been for the good. Some of my most satisfying undergraduate teaching was in the program of Mathematical Methods for Social Sciences, an honors curriculum at Northwestern designed to train undergraduates in the analytic techniques and models in the social sciences that are usually taught only to graduate students. I also had the privilege of directing that program at two different intervals of time. The program continues to thrive as one of the most popular available at Northwestern.  My many Ph.D. students have been at least as important in disseminating my research ideas and results as my published papers. Many of them are professors themselves, some at top universities, but a number are also contributing their skill within the Federal Reserve Bank system and agencies of the federal government such as the Bureau of Labor Statistics. These individuals are not only former students but continue to be supportive friends that I value highly, along with colleagues at both Northwestern and Aarhus, as members of my close academic family. Finally, the continued support for my work by Northwestern University must be acknowledged. During my 45 years in the institution it has progressed from what was a party school that catered to the children of the upper income professional classes of middle America to a first rate international center of learning and research. Although I may be the first Economics Prize Laureate on the faculty, I expect fellow company soon.  Let me finish with a brief tribute to my life partner. ‘Behind every great man is a great women’ is an old cliché that fits my case well, as any of our friends and colleagues will attest. As I mentioned in the beginning, Beverly and I started our lives together while I was still a student at Carnegie in 1963. Four years later, we had three children and my Ph.D. degree. That period established the relationship.  But as an undergraduate, Beverly trained as a musician and teacher, a profession that she practiced for the first year of our marriage. After five years of diapers, she ventured out into the world as a guitar player and folk singer. Later she directed the choir for 25 years at our local Catholic church and composed numerous hymns, psalms, and two oratorios for her favorite instrument, the choir. She then used her composing talents to collaborate with others in the writing several original musicals, all of which were performed by either high school or community groups. She also conducted a community chorus for many years that performed both pop music concerts and madrigal extravaganzas. Finally, as the three children were finishing their college studies, Beverly returned to school herself to complete a Ph.D. in Religious Studies. Since 1994 she has taught at Northwestern and was named last year by her students to the faculty honor roll. During these same years, she and I have managed to raise three children who have now provided us with eight grandchildren. The cousins, who range in age from 9 to 17, form a cohesive group that has enhanced the lives of their elders without measure. We see in them a bright future for our family.  During all this activity over these many years Beverly has been and continues to be my strongest supporter and best friend. My colleagues have long noted that my research on search and matching deals with the formation and dissolution of relationships. How is it, they ask, that you have been had the same job for 45 years and the same wife for 47? My response is simple: Both are excellent matches!   |  | | --- | | References | | 1. Ashenfelter, O.C., H. Farber, and M.R. Ransom (2010). “Labor Market Monopsony,” *Journal of Labor Economics*, 28: 203–210. | | 2. Beveridge, W.H. (1944), *Full Employment in a Free Society*, George Allen and Unwin. | | 3. Burdett, K. (1978), “A Theory of Employee Search and Quit Rates”, *American Economic Review*, 68, 212–220. | | 4. Burdett, K. and D.T. Mortensen (1978), “Labor Supply Under Uncertainty,” *Research in Labor Economics*, 2: 109–158. | | 5. Burdett, K. and K. Judd (1983), “Equilibrium Price Distributions,” *Econometrica*, 51: 955–970. | | 6. Burdett, K., N. Kiefer, G. Neumann, and D.T. Mortensen (1984), “Earnings, Unemployment, and the Allocation of Time over Time”, *Review of Economic Studies*, 51: 559–78. | | 7. Davis, S.J., J.C. Haltiwanger, and S. 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| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0830=DM  [Adam Smith] Professor Mortensen, hello. This is Adam Smith, calling from the Nobel Prize website in Stockholm.  [Dale T. Mortensen] Ok, you do call! I had just heard from [Ed Phelps](https://www.nobelprize.org/nobel_prizes/economics/laureates/2006/) what happens …  [AS] Right, yeah! I’m glad it’s all working as it should. Grand, well, my congratulations on the award of the Prize.  [DM] Thank you.  [AS] We catch you in Denmark now, is that right?  [DM] Yeah, I’m in Denmark for another week. I’m half-time here and half-time in the US.  [AS] And, what were you doing when you received the call that told you?  [DM] What was I doing? I was having lunch with some of my colleagues. We have a seminar this afternoon.  [AS] And, I think I detect in your voice that it’s slightly disconcerting news?  [DM] It’s what?  [AS] It’s slightly disconcerting news?  [DM] [Laughs] No, I know that there’s less than a positive side as well as a positive side. No, it’s very good news, thank you.  [AS] It is indeed. So, the models for which you’re being awarded the Prize, that you’ve developed, have provided a framework for studying real-world transactions. Is that correct?  [DM] In the labour market.  [AS] In the labour market, yes. And applicable to other markets as well?  [DM] It has been applied to other markets as well, although mostly by other people.  [AS] And, the models in particular incorporate this concept of economic friction. Can you tell me what’s special about the models?  [DM] Well, economic friction is a large term. It covers a lot of scope. This particular model covers friction having to do with, having to gather price information and job location information. Any sort of deviations from so-called ‘perfect markets’ – which is just an ideal, right? – is called a friction. And, one of the assumptions of perfect markets is perfect information. So, one form of perfect information is information about what all the prices are and where you can make transactions. So, our model, really simply, relaxes that assumption. And that’s the sense in which it deals with friction. There are a lot of other frictions, like [line cuts out], that you can talk about, ok? Asymmetric information is another one.  [AS] Right, right, yes, where one side knows and the other doesn’t, yes. In particular for the labour market then, what insights have these models provided?  [DM] One of the early insights was to focus on the point that, with friction of this kind … You know this is obvious to everybody; that’s the problem with economics, right? It takes time to match workers and employers. All right, so you would expect people to spend some time unemployed before they find a job. And, even after they find a job, to look for maybe something better, alright? So, it focuses on those – on that sort of time-consuming aspect of information gathering. And, as a consequence, you can apply it to data, such as we have here in Denmark and in Sweden, which records the length of time people spend in unemployment, the length of time they spend in a particular job before they transfer to another – or move to another job, etcetera.  [AS] Mm hmm.  [DM] Is that clear?  [AS] Yeah, no, that’s clear! I wanted to ask you, just, you mentioned there that it’s, you know, economics is obvious to everyone. When one describes what these models have incorporated, it is what we all know. It takes time to find a job, for instance.  [DM] Yes, that’s the bottom line.  [AS] So it … I guess it must be a difficulty to sell to those who don’t, who aren’t engaged in economic theory, what one is doing?  [DM] Yes, it is. It’s hard to express it particularly when, of course, you go beyond that to try to talk about how do prices and quantities get determined, when it takes time.  [AS] And, so how do you normally do it? If one knows nothing at all, what do you say?  [DM] Ah, one knows nothing, well I try to start where I am now! And [Laughs] usually the people’s eyes glass over quickly and I don’t have to go too far. But, you know, where one would go from there is one of the implications of the setting is that there’s room to negotiate over wages, something else that people know, right! [Laughs] And so, when you talk about how wages are determined, you have to also deal with the bargaining aspect, and the part of the bargaining aspect that has to do with having to wait. For example, in a bargaining problem, what is your outside option, what is your alternative to make a deal, right? – is important. And, in the labour market, what your outside option is, if you’re unemployed, is finding another job opportunity, alright? You know, so there’s some wage that you wouldn’t accept because you’d rather go find another opportunity.  [AS] And, so part of the importance of having these models is that it helps people incorporate this sort of thinking into policy?  [DM] That’s right. That’s the point of the models, is to allow you to focus on aspects maybe that even you have a sense of, alright, but it allows you to focus on policy. Then the issue of how wages are determined is related to this information problem.  [AS] Hmm, ok.  [DM] And how many workers you want to hire is related to this information problem.  [AS] Hmm. Thank you, thank you. So, just lastly, you know Christopher Pissarides very well, I imagine. You’ve worked together …  [DM] Yeah, we’re co-authors and have known each other for a long time. I’ve known Peter Diamond also for a long time.  [AS] Grand.  [DM] We’re more or less contemporaries.  [AS] But you haven’t spoken yet? [Laughs]  [DM] No, I haven’t! [Laughs]  [AS] Ok, well, any idea what’s going to happen right now?  [DM] What, right this … Oh you know, for me?  [AS] Yes.  [DM] I know I’ve been warned a bit by some of my friends who this has happened to …  [AS] What, that you’re …  [DM] But, I don’t know the details, no!  [AS] But, you’re just about to lose control of the day, I imagine?  [DM] Yeah, I imagine. I’ve already told my secretary to worry about it for a while! [Laughs]  [AS] Good, it pays to have some watch dogs, I suppose. Good.  [DM] Yeah.  [AS] Ok, well I wish you a very pleasant day …  [DM] Alright, thank you. |
| Interview |  |
|  |  |
| ID | 0831 |
| Biographical | I was born in Nicosia, Cyprus, on 20 February 1948. My father Antonios was born in the village of Agros, in the Troodos mountains, a village in a valley surrounded by mountains on three sides and with an opening to the south overlooking in the distance the bay of Limassol. He was one of seven children, and at the age of ten he was taken out of school and sent to Nicosia to work as a shop assistant for his uncle. He lived and worked with his uncle’s family until his twenties, when he was able to open his own shop, selling materials for making clothes and other items for the home. His business flourished when we were growing up but late in his life economic development and cheap imports made his trade obsolete.  My mother Evdokia Georgiades was born in Nicosia to a better-off family, and she went to French school, learning to speak fluent French and English. Her family also claimed their origins from the village of Agros. They got married in 1943 and had three children: my sister Maro, myself and my younger sister Anna. Despite her education, my mother stayed home to look after us. She also had home help from my father’s nieces from the village, who followed his steps and moved to town to ﬁnd work. When my sisters and I left home she got a job as a shop assistant for one of her cousins, who had a thriving clothing business.  We were a happy family and I had a good upbringing. I have particularly fond memories of our family evenings at home before television, and our summers on the coast of Kyrenia and the mountains and valleys of Agros. I used to spend the time with my cousins, fishing in Kyrenia (mostly unsuccessfully) or playing in the riverbeds and springs of Agros. Cultivation in the village was still at subsistence level, in small plots, and occasionally we would stray into some vegetable patch or tomato bed, to be chased away by hard-working men and women in their traditional village clothes. Work on the fields would begin at dawn with mules and donkeys and end at sunset. On Sunday everything changed, as the whole village gathered at the church to pray, gossip and hold memorial services for their dead, distributing to everyone the traditional home made “kolliva” (boiled wheat with dried fruit, almonds and pomegranate seeds). Service in the Greek Orthodox Church was in the original Byzantine Greek of the bible, so I doubt whether anyone in the village, except perhaps the priest and the teacher, understood much. But it was certainly a great social occasion that I enjoyed very much. Watching how my relations who stayed behind in the village worked and how generous they were with their crops when we visited taught me a lot about family and inner well-being.  My elementary school education in Cyprus, behind the sandstone walls of the elegant Eleneion school, was badly disrupted by the independence movement against British colonial rule. During colonial rule, Greek and Turkish schools were given a lot of local autonomy. When the independence movement started in 1955, schools became the focus of some anti-colonial activity, and whenever a Greek ﬂag was raised on the school mast, or when the army suspected something “subversive” going on, the British administration would close the school down for extended periods of time. My parents, who valued education greatly, made me take lessons from tutors outside school, and I got on. Then, when I moved on to high school, and after a brief peaceful period in a new independent state, violent clashes between the local Greek and Turkish communities broke out. This was another big disruption to my education. Of the eleven years that I spent in schools in Nicosia, only three were not disrupted by the sound of marching soldiers, exploding bombs or ﬂying bullets. We learned to identify the location of the “troubles”, and if it was perceived to be sufficiently far, we got on with our lessons.  When I was in high school military conscription for young men started but I finished school at 17 instead of the usual 18, and I was considered too young to serve in the army. I was allowed to leave to go abroad for degree studies, with the expectation that the “problems” between the local Greeks and Turks would end before my return, and military conscription would be abolished (it is still mandatory).  Like many well-off Cypriots, I went to London to study for a degree in economics. I wanted to study architecture, but because of my father’s business my parents persuaded me to try economics or accounting. My love at school was mathematics, but it was not considered to be a good profession for a young man, with which I agreed. When I tried economics I liked it, so I decided to pursue my studies in it.  I entered a college of further education to study for my A levels (university entrance exams), speaking little English and not well prepared. It was a particularly poor college, and when I said I wanted to apply to university to study economics, I was advised to give up mathematics and study the principles of British constitution in its place. Fortunately, I had the good sense to study mathematics on my own and enter the A level exams as a private student. Needless to say, it proved to be the most useful preparation for my economics degree.  I applied to the maximum six universities that we were allowed by the central office of university admissions, but with my school background I was probably lucky to be accepted by just one of them, the University of Essex. It turned out to be a blessing, because being a campus university helped me integrate more easily into British student life, and the standard of work was very high. Like the university, the economics department was new and it was set up by young successful academics from the London School of Economics. It was a very friendly place, and it opposed separate facilities for faculty and students (a practice that I understand was given up a few years later). Richard Lipsey, Michael Parkin and Chris Archibald were particularly influential in my early studies.  In the late 1960s, when I was an undergraduate student, Essex became active in the student uprisings that characterised the times. In 1968 student political activity became particularly intense, and although I was never very active, the liberal attitudes of the time had a lasting inﬂuence on me. I remember the Vietnam war and the assassinations of Robert Kennedy and [Martin Luther King](https://www.nobelprize.org/nobel_prizes/peace/laureates/1964/) as particularly dramatic events that affected our political outlook.  Soon after, the decision whether to end my studies with the first degree and return to Cyprus, as planned, or go on to graduate school, had to be made. My teachers at Essex pressed me to go to one of the top American schools and I got an offer from Harvard. I also met Dale Mortensen around that time and he invited me to go to Northwestern to do a PhD with him. I decided to move to the London School of Economics, partly because I considered America to be “too far”, partly because my experience at Essex, which I vastly enjoyed, made me feel that I knew what to expect at LSE. Unlike Harvard, British universities were particularly bad at making up their minds about funding in timely fashion but I was fortunate to have my father’s promise that he would pay for all the costs of my education if I needed it. Eventually all my higher degree education was ﬁnanced by scholarships, so I did not have to draw on that promise, but without it I couldn’t have stayed in Britain.  LSE had a completely hands-off and disorganised approach to doctoral research, leaving students alone to find a topic and write a thesis. Of course, I had a supervisor who was very helpful, Michio Morishima, whom I had met at Essex before he moved to LSE. But he was not interested in the topics that I got interested in at Essex, search theory and unemployment, which I wanted to research. He made me appreciate the classics and I read Keynes and [Hicks](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/) under his guidance, but I spent most of my time reading articles and books on my own in LSE’s dusty old library rooms.  I read an enormous amount of literature, diligently taking notes, just because it happened to be there. On one occasion, and quite by chance, I stumbled on a paper by Samuel Karlin in one of the Stanford collections in mathematical economics, describing mathematical models for selling an asset when outcomes are uncertain (“Stochastic Models and Optimal Policy for Selling an Asset”, in *Studies in Applied Probability and Management Science*, edited by [K. J. Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/), S. Karlin and H. Scarf and published in 1962). I would take down from the shelves anything with Arrow’s name on it, because of Frank Hahn, who taught us general equilibrium theory from his book with him. “Ken Arrow strides our profession like a Colossus,” he used to tell us. Reading Karlin’s paper made me reason, let’s think of a worker’s human capital as an asset that he sells in the labour market, where job outcomes are uncertain. I tried to work on the idea and it delivered. Within six months I had a thesis in search theory that was approved for the degree of PhD. Soon after it was published as a book by Cambridge University Press, largely under the influence of Michio, who thought it was better to keep it as a single unit than break it into articles. He was himself writing only books at the time, feeling frustrated by the journal refereeing process – a feeling that I often reflected on with sympathy!  With the PhD in hand, another big decision had to be made, go back to Cyprus or apply for a university job. I had the urge to return to Cyprus but my teachers, mainly those that I got to know well at Essex, were putting a lot of pressure on me to stay in Britain, or try America. I returned to Cyprus in the autumn of 1973 to tidy up some loose ends with the thesis, intending to stay until the new year and in the meantime try life in Nicosia as a young man. The political situation at that time was getting really bad, with a military dictatorship in Greece, skirmishes between different Greek factions on the island, a Greek army that was hostile to the popular government of Archbishop Makarios, and a Turkish army to the north getting ready for action if the opportunity arose. Despite that, job opportunities started coming my way, and one of those looked particularly interesting – in the research department of the central bank. I took it, and started work in February 1974. With money that I borrowed from my father I also bought myself a plot of land covered with pine and carob trees, nestled under the cathedral rocks of Bellapais, my favourite spot on the Kyrenia mountains. I loved the combination of pine trees and views of the deep blue sea below, and I was intending to build a weekend retreat there when I made some money. With the political situation that I could see around me, it was probably not a very clever ﬁrst investment!  But the urge to continue academic research was getting more intense, and I was also missing my girlfriend, whom I met at LSE but who stayed behind to complete her studies. She was from Athens and could not bear the thought of living under the dictators, so we agreed that when she finished her studies in the summer of 1974 she would move to Cyprus to live with me, get married and after a year try for academic jobs in Britain. July came and she was back in Athens with her degree, but when the time came for her to fly to Cyprus, she was too shy to fly on her own and be greeted by the extended Pissarides family at the airport. After some gentle persuasion I agreed to take a few days off work to go to Athens to fly back with her.  That decision turned out to be a pivotal event in my life. I took a flight out of Nicosia airport on Sunday night, 14 July 1974, that turned out to be one of the very last civilian flights out of that airport. At dawn on Monday, when America was preoccupied with President Nixon’s future and was not looking, the Greek army overthrew the government of Makarios, replacing it with Greek military rule. Airports closed and young men were immediately ordered to report for military service. Within days the Turkish army invaded and divided the island, causing untold misery to thousands of people. My parents escaped to the village and I lost all communication with them. Members of my family were made refugees in their own land and friends of mine went missing. The places that I loved on the north coast were lost, and my pine retreat over the blue sea had gone forever. I lost personal possessions, mostly family things of sentimental value, and even the manuscript of my book looked like it had gone up in flames at one point (although I later managed to retrieve it). My life had changed forever.  I was in Athens unable to get news of any kind other than military propaganda from the Greek colonels. Then, a few days later, news came that the dictatorship had collapsed and the exiled former Greek prime minister was returning to form a government. The whole of Athens seemed to break out into the streets in a massive party. News from Cyprus arrived that the island was destroyed by war. Even the ineffective British foreign minister, a guarantor of the peace of the island, was desperate about the situation and did not know what to do. The contrast between the celebrations in Athens and the destruction of Cyprus could not have been more stark. When I got news of my family I went with my fiancée to a resort near Athens to relax for a few hours but I ended up in the local hospital close to a breakdown. I called my teachers in Britain in desperation and Michael Parkin, whom I got to know well at Essex, found that two universities had one-year positions open, to replace faculty that took leave of absence later than usual. I borrowed a jacket and flew to England to be interviewed for them, with no money and only summer clothes for a five-day break in Athens in my bag. I was offered both jobs on the spot, and I took a one-year lectureship at the University of Southampton, teaching business economics to second year students.  Life in Southampton was tough, having to borrow both money and clothes to get by, and worrying about events back home. Teaching was boring, being in an area that I knew nothing about and was not interested in. But I threw myself into work, finishing the book on search theory and writing papers whenever I could spare a few minutes. My lectureship was renewed for a second year, but in the meantime a position had opened at LSE and I applied for it. I was offered it and I moved to London in 1976. I have not moved since.  As a young lecturer, I found LSE about as intimidating as I did when I was a PhD student. I was left alone to do my work and submit my papers, and I doubted (incorrectly, I suspect) whether many of my famous senior colleagues knew that I had made the transition and I was now their new colleague. On one occasion two or three years after I went to LSE, one of my senior colleagues called to say that he wanted to recommend me for a winter retreat in Germany organized by the Econometric Society for junior faculty. I was astonished that he knew me and I wondered, why me? Because he knew I would do well, he said.  At the retreat I was given the first chance to present to other young economists from Europe my new way of doing search – a very early version of one of the papers cited by the Economics Prize committee. They did not like it very much, saying that it assumed away too many things. I thought this was the advantage of the approach, in contrast to my PhD work, which had too many things in it, so I carried on. Unfortunately, journal referees of the early work also thought it had assumed away too many things and I had difficulty publishing my first papers. In contrast, during my brief forays into other branches of macroeconomics, like consumption theory, I found it easy to publish my papers in good journals.  During this time, the only faculty member at LSE that I remember trying to mix with junior lecturers and graduate students, and organise a research group of some kind, was Richard Layard. He talked to me about joining the informal group that he was putting together to work on applied labour economics topics, like social security, pensions and education. Although I did not consider myself to be a labour economist, and I had never taught labour economics, I agreed, and some papers were published. But my true passion was my theoretical work in unemployment theory, with which I quietly carried on in the background, without talking much to anyone about it. Still, the association with the Layard group, as it used to be known, brought several good results. It taught me to think of the empirical implications of what I was doing, something that was never impressed on me during my PhD work. It also taught me to think first of the “big problem” that we needed to understand, and then write models about it. The big problem in the late seventies and early eighties was unemployment, and that’s what I wanted to do. It also financed my ﬁrst visit to the United States, a one-year Ford Foundation fellowship at Harvard, to work on the economics of education.  The visit took place in the academic year 1979–80. In the previous year, [George Akerlof](https://www.nobelprize.org/nobel_prizes/economics/laureates/2001/) and Janet Yellen came to LSE and for the first time someone with more experience of the profession than I had, explained to me how to make my work better known. I was still too shy to do much about it, but at least now I knew how. They did not know my work but liked it when they saw it, and not surprisingly George advised me to focus on my unemployment work. That encouraged me and that’s what I did at Harvard, although I also wrote the two papers on education that I got the funding for.  I found Harvard a difficult place to visit. The facilities offered to visitors were basic and there was no effort to integrate visitors within the department. A few weeks after arriving, I despaired and started seriously thinking about leaving and returning to London. The lowest point coincided with the get-together party organised by the department for new and old faculty. I half-heartedly went along, and by chance I met Martin Feldstein, who asked me how I found Harvard as a visitor. When I tactfully described some of my experiences, he immediately suggested that I leave the department and I come to the NBER in Massachusetts Avenue, where I could have access to all the facilities and work in my own office. I loved it. It rescued my visit, I immediately got down to work, and I have been going back to the NBER practically every year since then. At one of their seminars I met Peter Diamond for the first time, giving a presentation of his early work on search theory. I saw the similarities with my work, but decided that I could carry on regardless, focusing, as I did, on labour markets, policy and explanations of the rise in unemployment, rather than on the more theoretical issues that preoccupied him. I think I met Peter very briefly once again several years later, but apart from these two brief meetings, I did not see him or communicated with him about our work until we both went to Stockholm for the award ceremonies.  On my return from Harvard I was deep into search and matching theory. I knew then that I was on to something good, although the work was still completely unknown outside my very small circle at LSE. This time also marked the beginning of my most productive and innovative period, which lasted for about ﬁve years. Half way through this period I went for my second long visit to the United States, six months at the Industrial Relations Section at Princeton University, under Orley Ashenfelter. I wrote my best papers during that time, even though the theoretical work that I was doing did not fit in well with the empirical focus of the Section. This period was also a very happy one (the two always seemed to go together), despite the breakdown of my marriage.  In London, Richard Layard had secured funding for a research centre in labour economics, and when the Centre for Labour Economics was created at LSE, I became one of its first members. I channelled my entire research output through the Centre and my focus on unemployment was undoubtedly reinforced by the Centre’s research objectives, which were to understand what was happening to European unemployment in the 1980s. Many distinguished economists visited the Centre and I got to speak to them, among them [Bob Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/), Jacques Drèze, Edmond Malinvaud, Orley Ashenfelter, Olivier Blanchard and Rudi Dornbusch. My association with the Centre helped my work, especially in its empirical and policy implications, and it at last made LSE feel like an outward-looking, progressive place concerned with important questions. The annual meetings of the Centre were the highlight of the research year for us and equal to the best I have ever experienced. It taught me what made a successful research group: one that has people thinking along similar lines, with a common objective, and with a strong belief that the objective is important and they can make a difference. All the intellectual activity of the Centre was directed to one issue, the big question in Europe, unemployment.  But it is also fair to say that my approach to unemployment was not the preferred approach among the Centre’s members, and I did not have any papers in the Centre’s main publications, like the special issues of journals on unemployment that it sponsored. Instead, I worked on my models in a fairly isolated way and wrote my own book on unemployment, at about the same time that the Centre’s other three senior members were collaborating on the Centre’s opus magnum, *Unemployment* (by R. Layard, S. Nickel and R. Jackman, published by Oxford University Press in 1991). When I participated in Centre meetings, I restricted my activities to discussions about the macroeconomics of unemployment and labour market policy, some of which later featured in my work. Olivier Blanchard recently recalled my role in the early life of the Centre as a “vox clamantis in deserto,” and it reflects well what I was thinking back then.  Like other British universities, LSE went through bad times in the late 1980s, as funding was cut and salaries fell behind. I got married again and had two lovely children in quick succession, Antony and Miranda. In order to support the family, I started looking for consultancy work outside, as our salaries were not enough to give a reasonable standard of living in London to a family of four. I also thought that once the children went to school, I should not go away for long, to avoid disrupting their education and their social circle. I heard horrible stories of how badly young children of British academics on leave coped with the American environment. So in 1990, when the children were too young for school, I took the young family and went to the University of California at Berkeley for the year, knowing that it was the last chance to spend a year abroad. George Akerlof was instrumental in arranging it and making us feel welcome.  The visit was enjoyable, although with the demands of the family and the attractions of California and the West, not very productive. But it did have one big highlight. During a short visit to Northwestern for a seminar I got to know Dale Mortensen better, and we started our collaboration. It lasted ten years, we became close friends, and produced our best known paper, which sealed what was already known to some as the Diamond-Mortensen-Pissarides model. We met frequently, both in London and Chicago, and on one recent occasion at the Rockefeller study centre in Bellagio, Lake Como, the nearest to heaven that one can find in an academic research environment.  Back in London in the 1990s economic conditions improved, and London became a vibrant cosmopolitan centre with a European feel. Of course, most of this prosperity was driven by the ﬁnancial sector, but at least for the fifteen years following the exit of Britain from the European exchange rate mechanism in 1992, London prospered. We could enjoy the benefits, both in university life and in my personal life. As my book and my work with Dale were becoming better known, I thought it was time to devote more time to other activities, including spending more time with my young children. I travelled frequently to Cyprus to serve on the Interim Governing Board of its first university, which planned its statutes and made the first appointments, and I later joined the Monetary Policy Committee as an external member. I also got more involved with administration at LSE. The three years that I was head of department, from 1996 to 1999, coincided with the arrival of Tony Giddens, a progressive Director whose objectives matched those of ours in the Economics department: to make LSE the pre-eminent social sciences research centre in Europe. The time was ripe for reform and with his help I got deeply involved into modernising the department, by updating our salary structure, improving our office accommodation and helping junior faculty fund their early research and integrate into the life of the department. My early difficult experiences at LSE left a big mark on me, and I tried to make sure that things had changed for the better. The Director and Department heads that followed pushed further down the modernisation lines, establishing LSE as a stimulating place to work, equal to the best in the world.  By the time I completed my three years as head of department my collaboration with Dale gradually came to an end, and the second edition of my book was in print. It took some time to get back to serious research, slowed down by some health problems and by family pressures. I collaborated in several projects with colleagues, the main one of which was with Rachel Ngai on the employment implications of structural change. As my work became better known, and especially after winning the IZA prize in labour economics with Dale, invitations to deliver lectures abroad were coming at higher frequency. Coupled with increased funding for collaborative research in Europe and with my frequent trips to Cyprus, travel for short periods became fairly frequent and quiet research in my office more rare.  In 2008 my marriage abruptly broke down. Once the worst was over I realised that it was time to put it all behind me and turn a new leaf. Life returned to a new normal, with a new partner in Rachel and more involvement with the University of Cyprus, where I started spending more time pursuing new research interests. The affection shown to me by my colleagues and several generations of students, the family in Britain and back in Cyprus, and by both official Cyprus and its people, are moving memories that will always remain with me. |
| Autobiographical |  |
| Podcast | In this digital conversation podcast we meet Christopher Pissarides, a humble London School of Economics professor who finished his PhD in two years and was awarded the 2010 prize in economic sciences. He and his co-laureates Peter Diamond and Dale Mortensen were awarded for finding ways to incorporate real-world frictions into the mathematical models that describe market behaviour. Their Diamond-Mortensen-Pissarides (DMP) model is one of the most widely-used analytical tools for labour markets.  Besides discussing labour markets, Christopher Pissarides speaks about educational systems, how life has been affected by covid-19 and how he experienced moving from Cyprus to the UK.  The host of this podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0831=CP  [Adam Smith] Hello, this is Adam Smith.  [Christopher A. Pissarides] Oh, yes, hello. This is Chris Pissarides.  [AS] Thank you so much for calling. So, must have been quite a day so far?  [CP] Yeah! [Laughs] Amazing! I could never believe that change could be so sudden.  [AS] [Laughs] What were you doing when the call actually came?  [CP] I’ve been trying to recover from a really bad cold. So the weekend, I was in bed. And then I started again this morning in bed. And, about ten thirty in the morning my time, I thought, you know, I have to get out and I have to do some work, I’m falling behind. And then, the phone call arrived and of course I haven’t done a stroke of work since then! I could hardly speak, my voice, and so I thought you know, is this real?  [AS] You seem to be holding up well. And, do you enjoy this sort of thing? Do you enjoy the publicity?  [CP] No one could say that they are not enjoying winning a Nobel Prize. [Laughs] Yeah, of course, I mean it’s a great joy and honor and everything. It’s what we’re all dreaming for when we’re allowed to have dreams!  [AS] You’re both a British and a Cypriote citizen, and I think I’m right in saying you’re the first Greek speaking Laureate since [Odysseus Elytis](https://www.nobelprize.org/nobel_prizes/literature/laureates/1979/) in 1979.  [CP] Probably, yeah. I was born and brought up in Cyprus, my family are there. I came over here to do a PhD and, when I finished, the political situation in Cyprus wasn’t exactly welcoming for young men finishing their studies so I stayed on here to work. I mean I was also interested in academic research, but these two things combined made me stay in Britain.  [AS] But, I imagine the news will be welcomed in the Greek-speaking world as well.  [CP] Yes, I had such tremendous support. I’m really pleased actually by the support that I’m getting from both sides within Greece.  [AS] So, just turning briefly to the models for which you’re being awarded the Prize, so these incorporate real-world situations, the frictions that real-world transactions suffer, into the mathematical models that are used to study the markets. What does that incorporation allow you to do? What do these models now help us understand?  [CP] It allows you to understand unemployment better, the foundations of unemployment, you know, sort of what goes to make unemployment, to tell me why the unemployment rate is what it is. Let’s state it very simply: it tells you why people might lose their jobs – what might increase the probability that someone will lose his or her job, what might make it less, then how long they will remain unemployed. And then, what incentives there are to take a job again, for firms to create jobs and for workers to take those jobs. So it enables you to study all the processes involved and, especially, to study the impact of policy at each individual level.  [AS] It helps you choose the correct policy?  [CP] Exactly, yeah, that’s the idea behind it, that it will help you make a considered choice in the sense that it will give you all the alternatives and it will give you all the implications of choosing any of the alternatives.  [AS] Are there already well-documented examples where the employment of, as they are called I think, the DMP models have allowed one to actually improve unemployment situations?  [CP] In fact the best examples actually are probably from, well, your own country, if I should say Sweden … [phone rings in the background] … I haven’t switched it off … or Britain.  It’s mainly, you know, for example, if you take the duration of unemployment, how long people remain unemployed after they lost their jobs, these models have really helped in the design of unemployment support policy, in the form of income support, and the incentive to take a job after unemployment. You know, the reason we have programs now that, after six months of unemployment, we have active policy measures, and they help in moving the people from the state of unemployment into employment, all those policies have really been influenced by what we’re doing in the last two or three decades, really. You know, showing how these programs would have an impact on the incentives that workers have to take a job.  [AS] Is this work very much within the realm of economics pure and simple or does it extend into lots of other disciplines as well? Is it basically a collaborative exercise at this point?  [CP] No, the work is economics; at least it started as being economics. But, what it does though mainly, is to tell us how people come together, you know, like how a firm and the worker will come together, when there are some information imperfections about the quality of any job match that will take place. So, it’s been applied to other economic situations like housing, for example, like house by a country. But it’s also been applied to marriage, a man and a woman who come together, given that they don’t know what the outcome will be once they move in together.  [AS] Yes, it’s any situation where a long-term sort of bond is formed.  [CP] Exactly, yes.  [AS] Ok, and what are you hopes for the work in the future? How do you see this developing? Are the models just subject to constant refinement as one goes forward?  [CP] The models have been refined quite a lot. I mean people still cite the original works, but nowadays there’s been so much work on that – and both work with numbers, with statistics, but also theoretical work. I expect I will continue in that direction. There is also work being done on sort of broader ways of looking at jobs. You know, as part of the structural change in an economy, you know the change from industrialization, manufacturing jobs to the service sector. Those are related to what we’re doing because again there is a sort of friction, market friction in going from one equilibrium, where you have a lot of manufacturing jobs, to another equilibrium where the manufacturing goods have been produced in low wage countries abroad, and at home you’re mainly employing people in the service sector. There are many applications of the field taking place, and also many theoretical refinements taking place. This is an active area of research.  [AS] Ok, thank you. Last question. You’ve had a few hours now, not to think for yourself, I’m sure, but to talk to people. Have you managed to make any plans for how you might celebrate this evening?  [CP] No, in fact I haven’t had time with saying earlier, “Oh, maybe we should go and have a good dinner,” and all that. And, suddenly another phone call arrives, “Are you free later this evening for talks?” I don’t know! But, I expect that there will be a nice meal in that time somewhere, yes!  [AS] I’m sorry, you should be drinking champagne, not talking to me. So, thank you very much.  [CP] Some champagne, yes! Plenty of time. [Laughs]  [AS] When you come to Stockholm we have the chance to interview at greater length, and I look forward to it very much indeed.  [CP] Yes, of course, yes.  [AS] Thank you. We look forward to seeing you in December. But, thank you for speaking to us now.  [CP] Thank you.  [AS] And, congratulations again.  [CP] Thank you, bye.  [AS] Bye, bye. |
| Interview |  |
|  |  |
| ID | 0832 |
| Biographical | I was born in Los Angeles, California, on August 7, 1933, and grew up during the Great Depression. Fortunately, our house had a large backyard that we filled with a vegetable garden and fruit trees. I learned how to grow vegetables and how to can apricots and peaches during the heat of summer. During World War II, I learned how to knit scarves for the “boys overseas.” My childhood was spent learning and doing the traditional activities of a girl during the last century. My major recreational activity was swimming, and I eventually joined a swimming team and swam competitively until I started teaching swimming to earn funds that I could save to help put me through college.  Since our home in Los Angeles was located at the lower edge of Beverly Hills, my mother arranged for me to attend Beverly Hills High School. Fortunately, I was encouraged to join the debate team in my junior year of high school and participated actively in speech competitions around the state. Learning debate was an important early impact on my ways of thinking. You are taught that there are always at least two sides to public policy questions, and you have to learn a good argument for both sides as well as knowing how to critique both sides. Participating in team efforts including my debating experience and being on a swimming team was also important.  While it was a challenge being a poor kid in a rich kid’s school, it did give me a different perspective on the future. Since 90 percent of the students in Beverly Hills High School went to college, it appeared going to college was the “normal” thing to do after high school. Even though no one in my immediate family had any college experience, I decided that I should go on to college. My mother saw no reason to support me during my college years since she had been supported only through high school. Fortunately, the semester fees at UCLA at that time were extremely low. I worked in the library, at a dime store, and at the bookstore. I was able to complete my undergraduate degree without going into debt. I took courses across the social sciences and graduated after three years by attending multiple summer sessions and by taking extra courses throughout. In my last year as an undergraduate, I graded Freshmen Economics.  When I started to look for a position after graduation, it was somewhat of a shock to me to have future employers immediately ask whether I had typing and shorthand skills. The presumption in those days was that the appropriate job for a woman was as a secretary or as a teacher in a grade school or high school. I began a correspondence course on shorthand, which I have never used to take dictation, but have found to be very useful when taking notes in face-to-face interviews on research projects. Fortunately, after a year of working as an Export Clerk in a large clerical pool, I did obtain a position as Assistant Personnel Manager for a business firm that had never hired a woman in anything but a secretarial position. I think my experience of obtaining a very good job in my early twenties helped me later when I decided to think about attending graduate-level courses and eventually applying for a research assistantship and admission to a Ph.D. program. I learned not to take initial rejections as being permanent obstacles to moving ahead.  My initial discussions with the Economics Department at UCLA about obtaining a Ph.D. in Economics were, however, pretty discouraging. I had not taken mathematics as an undergraduate primarily because I had been advised as a girl against taking any courses beyond algebra and geometry in high school. While the Economics Department encouraged me to take an outside minor in economics for my Ph.D., they discouraged any further thinking about doing a Ph.D. in economics. Political Science at that time was also skeptical about admitting any women to their Ph.D. program as they feared that only a city college would employ a woman with a Ph.D. That was not a good placement for building the reputation of the UCLA department. I was, however, admitted in a class of 40 students with three other women. We were told after we began our program that the faculty had a very heated meeting in which they criticized the Departmental Committee for admitting any women and offering them assistantships. Fortunately, our fellow male graduate students were friendly and encouraged us all to continue in our program.  In my graduate work, I participated in a research team studying the water industry in Southern California based on some of the initial framing by Vincent Ostrom, Charles Tiebout, and Robert Warren (1961). Several of the graduate students working on this project undertook efforts to analyze the political economy of a group of groundwater basins in Southern California. I was assigned to study the West Basin, which underlay multiple cities along the coast of the Pacific Ocean. The city of Los Angeles partially overlapped the basin as well as a good portion of Los Angeles County. Without knowing I was studying a common-pool resource problem, I became very familiar with the kinds of problems that users of a common-pool resource face in trying to manage such a resource.  It was only after I defended my dissertation in 1965 that Garrett Hardin’s article on “The Tragedy of the Commons” was published in *Science*, and Mancur Olson’s book on *The Logic of Collective Action* was published. And, in 1965, Vincent was offered an attractive position as full professor at Indiana University, Bloomington. I tagged along as it was very hard for any department to hire a woman in those days. Fortunately, the Department of Political Science later needed someone to teach Introduction to American Government on Tuesdays, Thursdays, and Saturday mornings at 7:30 a.m. They appointed me as a Visiting Assistant Professor to do that. After a year of teaching freshmen, they asked me if I would be Graduate Advisor and moved me to a regular appointment at that point.  The first 15 years of my research career at Indiana University were focused on studying police industries across the United States. Unfortunately, many scholars had confused multiplicity of units serving the same metropolitan area with a chaotic distribution of services. Not that all multiunit servicedelivery arrangements are effective, but the arrangements we studied in metropolitan areas across the United States were far more effective than the scholarly criticism. We never found a large police department with over 100 officers able to outperform a small- to medium-size department (25–50 officers) in producing direct services including patrol, traffic control, response services, and criminal investigation.  After fifteen years of extensive research on police industry structure and performance, I returned to studying the commons, but this time with the recognition of what I was studying. The National Research Council created a special committee in the mid-1980s to review the empirical research written about common-pool resources. Scholars began to recognize that much research on this topic had been conducted but was divided by discipline, sector, and region. Consequently, scholars who studied inshore fisheries in Africa did not know about other studies of resources in Africa. If they were sociologists, they did not know any of the work done by economists and vice versa. Participating in the NRC committee, and seeing the immense amount of research that had been done but not synthesized, taught me a major lesson. The way we organize the modern American university fragments our knowledge badly. Not only are we divided by discipline, but we are divided by the methods that scholars use. Economists using nationwide statistical data are critical of economists using the experimental lab to test theory. Scholars who do field research are critical of the use of any other method.  I have been very fortunate that Vincent Ostrom and I were able to establish an effective research center with a different philosophical foundation during the early 1970s. Vincent named it the Workshop in Political Theory and Policy Analysis. He thought the term “workshop” conveyed a sense closer to his philosophical view of science as a form of artisanship (V. Ostrom 1980). The logic of our Workshop has always been that there would be a variety of scholars across economics, political science, and other disciplines who worked together trying to understand how institutional arrangements in a diverse set of ecological and social economic political settings affected behavior and outcomes. We have delved deeply into how institutional arrangements affected performance of urban police agencies, irrigation systems, and forest resources.  The Workshop philosophy has permeated a very large international program started in the early 1990s. The FAO wanted information collected in a systematic way on the conditions in forests and their governance arrangements and how this affected livelihoods as well as forest conditions. We did not think that this was something that was an appropriate thing for scholars in the United States to do by flying out and collecting data in multiple countries. We developed a collaborative network where scholars who lived in a particular country and had very strong interest in forest conditions and forest policy conducted the studies. We very carefully trained a core staff in each center to use the data collection techniques that took many years to work out. We have encouraged regional meetings of scholars in a particular region to discuss method findings and strategies. We meet as a big network every two years and find that we all continue to learn from one another.  If the research that one wants to pursue can all be done sitting in a library carrel somewhere in one’s home institution, then one does not need to develop the equivalent of a Workshop. However, if one is trying to understand and test theory in the field and in the experimental lab and to really pursue in-depth studies of diverse institutional arrangements around the world, then working with colleagues located in diverse settings at various stages of their careers is crucial for making scientific progress. The general strategy that we have used is now described in considerable depth in Poteete, Janssen, and Ostrom (2010).   |  | | --- | | References | | 1. Ostrom, Vincent (1980), “Artisanship and artifact,” *Public Administration Review* 40(4): 309–17. | | 2. Ostrom, Vincent, Charles M. Tiebout, and Robert L. Warren (1961), “The organization of government in metropolitan areas: A theoretical inquiry,” *American Political Science Review* 55(4): 831–42. | | 3. Poteete, Amy, Marco Janssen, and Elinor Ostrom (2010), *Working Together: Collective Action, the Commons, and Multiple Methods in Practice.* Princeton, NJ: Princeton University Press. | |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0832=EO  [Elinor Ostrom] Hello  [Adam Smith] Oh, good morning. May I speak to Elinor Ostrom please?  [EO] Yes.  [AS] Hello, my name is Adam Smith. I’m calling from the Nobel Foundation’s official website, in Stockholm.  [EO] Yes. Adam Smith, what a name! I’m sorry, you’re kidded a lot, I’m sure.  [AS] Exactly, and I think sometimes the new Economics Laureates think I’m a hoax caller when I do this. We have a tradition of recording very short telephone interviews for the Nobel Foundation website, with the new Laureates, so would you mind speaking for a few minutes?  [EO] Ah, yes, fine.  [AS] Thank you very much indeed. Of course, congratulations on the award.  [EO] Well, it’s an unbelievable honor, yes.  [AS] You, as I know has just been pointed out on the press conference, are the first woman in the forty year history of the Sveriges Riksbank Prize in Economic Sciences to be awarded. Does that make it a greater honor?  [EO] Yes. Having lived through an era, where I was thinking of going to graduate school and was strongly discouraged because I would never be able to do anything but teach in a city college … Ah ha ha, life has changed!  [AS] Do you think that the ratio of Laureates in Economic Sciences – the gender ratio – is it in any way representative of the ratio of people working in the subject now or has it really changed?  [EO] It’s slowly changed. I’ve attended economic sessions where I’ve been the only woman in the room, but that is slowly changing and I think there’s a greater respect now that women can make a major contribution. And I would hope that the recognition here is helping that along.  [AS] I imagine it sends a strong signal, yes. Now, you work on the management of common property by common ownership contrasting it with the effects …  [EO] Among other things, yes.  [AS] Would it be true to say that, broadly, you’ve found that common ownership can be more effective than people thought it might be?  [EO] Yes! It’s not a panacea but much more effective than our common understanding.  [AS] And, is there one example you’d like to give of where this is so?  [EO] Well, let me use the example of lobster fisherman in the state of Maine. In the 1920s, they almost destroyed the lobster fishery. They regrouped and thought hard about what to do and over time developed a series of ingenious rules and ways of monitoring that have meant that the lobster fishery in Maine is among the most successful in the world. The big threat that comes now is that the other fisheries around it have so over-fished fish, that the lobster is a little bit of an extreme example of … If there were an illness or something that came, a bug, that infected them, they would be very exposed. But they have been incredibly effective through the years. There are many other small to medium sized groups that have taken on the responsibility for organizing resource governance. We’ve studied several hundred irrigation systems in Nepal. And, farmer-managed irrigation systems are more effective in terms of getting water to the tail end, higher productivity, lower cost, than the fancy irrigation systems built with the help of Asian Development Bank, World Bank, USAID, etc. So, what we have is many local groups are very effective, but that it’s not universal. So we can’t just now be naïve and think, ‘Oh, well, just leave it to the people, they will always organize.’ There are many settings that discourage self-organization  [AS] Right.  [EO] And, thus, we must understand both the conditions – that they can, but the conditions under which they will.  [AS] Well, I was going to ask you whether your research has also shed light on the conditions that lead to good self-organization. Are there particular features that have to be in place, for instance enough time for participants to work out what their set of regulations should be?  [EO] Yeah, and I have an article in Science in July of this past year which lays out a broad diagnostic framework and identifies a number of variables that are associated with self-organization.  [AS] Would you say broadly though that self-organization should be used and trusted more than it is now? That society should move towards trying to implement self-organizing structures?  [EO] Yes, but not with a formula. So there are many, many efforts now to decentralize and they create a rigid formula and give people rules from on top and say, ‘Now it’s yours.’ And that isn’t worked very well either.  [AS] So, again, there’s a great deal of subtlety to it …  [EO] It’s this … yes. And, you think about the variety. If you look at a countryside, think of the variety out there in terms of the ecological variety. Well, if people are going to manage ecological variety, no single set of rules will work in a semi-arid versus a tropical-wet region. They have to be different.  [AS] Indeed. Another thing you’ve done is to conduct lab experiments …  [EO] Oh, yeah!  [AS] Which I believe have shown that people appear to be more willing to enforce mutually agreed rules than had been expected, again.  [EO] Yes, we’re showing that. But we’re also showing a very important role for face-to-face or even written communication. So, the prediction was that nobody would self-monitor because that’s a second level social dilemma, if you go to Game Theory. But, what we’ve found is that people will, but it can be … people can escalate into, ‘I’ll punish you, you punish him, mamamamamma,’ and it gets worse and worse. So, with communication, where there’s an agreement on what is going to be the … what *we* are going to do. The ‘we’ then being well-defined, then people can follow rules, be cooperative and, occasionally, sanction one another to help that continue.  [AS] You mentioned Game Theory, there. How much of this is actually an extension of Game Theory and what we’re looking are repeated games in developing these structures?  [EO] The … Game Theory has been very, very important in our work in that we’ve been able to take game-theoretic models and put them in the lab and test them. And thus my early exposure in the 1980s to the work of Reinhardt Selten, who is himself a Nobel Laureate, was a very, very important step in my training. We still … Classical Game Theory is very predictive in some environments but not fully predictive, by any manner/means, in an environment which is a social dilemma. But very helpful for us in analyzing and as we develop a behavioral theory of humans and of other formal mechanisms we can explain why people do cooperate in some settings and not others.  [AS] Right, yes. I’d like to finish just by asking whether you consider that your work is economics or political science or social science, or maybe it doesn’t matter what it is, what it’s branded?  [EO] I consider it to be political economy or the study of social dilemmas. I was trained heavily in economics as an undergraduate. I studied with Armen Alchian and others, and then worked with Reinhardt Selten in the 80s. I work with two colleagues, economists, here in Bloomington that have been very, very important in my work. My husband worked with Charlie Tiebout and they developed a theory of metropolitan organization that was an economic/political science overview, so the … I’ve crossed disciplines, there’s just no question about it!  [AS] I suppose this award has the potential to catch the public imagination, because the citation brands it as economic governance and you’re talking about people getting involved in their own governance.  [EO] Yes!  [AS] It’s likely to be … it’s likely to spark people’s imaginations and they’re going to …  [EO] I hope! Ha ha! That’s what I’ve been working on for all my life! Humans have great capabilities and somehow we’ve had some sense that the officials had genetic capabilities that the rest of us didn’t have.  [AS] Uh hum.  [EO] I hope we can change that.  [AS] Excellent. Well that’s a lovely note to stop on, thank you. When you come to Stockholm in December to receive your Prize we have a chance to speak at greater length, so …  [EO] Wonderful, I’ll look forward to that.  [AS] I look forward to it too. I hope you have a splendid rest of day and once again congratulations.  [EO] Thank you very, very much.  [AS] Thank you, bye, bye. |
| Interview |  |
| Q6 | I think when I phoned you both in October you were both somewhat surprised by the name, it’s a bit of a liability on that day of the announcement. How have the couple of months since the announcements been for you? |
|  | Oliver Williamson: In a word – hectic.  Elinor Ostrom: And two words – very hectic.  You must be catapulted into a spotlight and a world of activity.  Oliver Williamson: It is, one of the things that is really gratifying however is how much goodwill you soak out from hearing from friends, former students, colleagues and family. It’s unreal but it’s there and so genuine that I couldn’t have imagine it.  That is nice.  Elinor Ostrom: I double that, some parts of the e-mail load are not friends and family and trying to cope with that has been a bit of a challenge. For our former students and colleagues all over the world, they have been sending joyous messages and that is very gratifying. |
| Q2 | And since you are the first woman to have been awarded the Prize in Economic Sciences in its 40-year history I imagine that is slightly added to the load you have been under. |
|  | Elinor Ostrom: Yes, I think that there have been many people who have been particularly asking about gender problems in American universities. Universities more general, it’s not just US.  I am sorry, you must be terrible bored of discussing this, but the fact that there has been one in 40 years, do you think that is in any way representative of the female contribution to economics over the last 40 years?  Elinor Ostrom: Not the contribution, but the capability to be in academia.  Do you see things improving?  Elinor Ostrom: Yes, I think that many departments now have new graduate students coming in at a rate, new women graduate students that they did not have previously, and our departments are moving toward hiring more. I don’t think I will be the last.  Do you see a change in the Nobel women coming to the subject?  Oliver Williamson: Oh yes, yes, and as Lin suggests, many are really excellent, and they get good placements and they are doing good work and we are going to hear from them.  Elinor Ostrom: Yes! |
| Q18 | So the prize this year is divided into two parts, but you are united in your work by the phrase the committee used such as ‘economic governance’ and broadly you both study ways of regulating transactions in the broader sense. If I could start with you, you study complex economic organizations and complex transactions and one part of your work that has been particularly sort of commented on by the committee is the fact that that allows you to say something about what sort of transactions should be conducted within firms and what sort of transactions should be conducted within a market. |
|  | Oliver Williamson: Yes!  Can you give examples of the sort of transaction that we are talking about when we say that?  Oliver Williamson: Yes, but I wonder if I could back off for a minute and talk about governance, because there is a compact two-sentence statement that was made by John R. Commons who was an older stylist, institutional economist, and I think undervalued by a lot of people then and since but never the less profound in many of his insights. And the two-sentence statement runs roughly as follows: “The principal unit of analysis should contain in itself, the three principles of conflict, mutuality and order. This unit is a transaction”. The part of it that I initially related to, because I was working at this more microanalytic level of transactions rather than completed goods and services, was that the transactions /- – -/ analysis. But as I got deeper into the study of organization, I started to ask what is the value added for some of these more complex forms of organization.  This triple of conflict, mutuality and order goes right to the essence and specifically I would describe governance as the means by which to infuse order thereby a medicate conflict and realize mutual games and that was contemplated, I would say, by Commons, even though he didn’t have much success in implementing it. That’s a profound sense of what I think was a missing arena for economics for a long time. A specific example would be the first project that I undertook along these lines and that’s the make-or-buy decision that firms was confronted with, it needs a specialized component let us say. It can either outsource to a independent contractor and mediate that transaction with a written contract that stipulates what’s to be delivered when, why and how they are going to handle difficulties in the like or you can take that transaction out of the market and organize it internally which is the make decision and vertical integrations issues had been around for a long time, addressed mainly as a technological issue addressing it in contractual terms with reference to two alternatives that one might have an advantage over the other or not. But the challenge was to first look at it contractually actually compartive contractually because you always want to think of two or more alternative ways of getting it done and then thirdly one other fact is that to drive it around in one way or another. And working that through was the transaction cost’s economics challenge. |
| Q70 | I can see that you can analyze a situation in which a firm is saying should we have this component made outside, buy it in or should we make it ourselves? But you have been able to turn that into theories which suggest in which circumstances the make decision is correct, which circumstances the buy decision is correct and then those theories have been put out to empirical testing and they presumably refine the theory. Is it now, are you able to suggest principles by which firms can govern their decision to the make-or-buy decision? |
|  | Oliver Williamson: What you need to do is you need to identify the critical attributes that define transactions so that operationalizing that the concept of transaction by naming what you take to be the critical factors across which transactions cost will vary. Then secondly you need to do the same thing with reference to governance structures, you need to identity the critical dimensions with respect to which governance structures differ. Then you ask the question given this cluster of attributes of transactions. Do they pose easy or complicated kinds of contractual issues? If they are easy, there are lots of easy transactions, I mean this is what spot markets and so are all about, neither party has any kind of dependency relation on the other. It’s sort of the classical problem of exchanging nuts for berries at the edge of the forest, you know. We both benefit from the exchange, but we don’t have any continuing relationship. Where you have to have a continuing relationship, where one party needs to make specialized investments in support of the other and where this contract may break down if it’s done in an intra-firm fashion, that’s where the concept of taking transactions out of the market and organizing them internally is important.  And actually, there is two other sort of major figures that I would say that enter into this and one is, both of them talk about adaptation as being the central problem of economic organization. One of these people is an organization theorist also from the 1930s, namely Chester Barnard, and he was interested in what it was distinctive that was going on within firms and his fear was that coordinated adaptation was the central kind of a contribution that internal organization had to bring and during that in a conscious deliberate purposeful way. This was, I would say, the marvel of internal organization, all was bureaucracy then and since it has been widely scorned. Sometimes for good reasons, but nevertheless there are things that internal organizations are especially good at. The other person was [Friedrich Hayek](https://www.nobelprize.org/prizes/economic-sciences/1974/hayek/facts/), former Nobel Prize winner, that emphasized autonomous adaptations accomplished to the market in response to changes of relative prizes. This was the marvel of the market as economists had long regarded it but as Hayek specifically nailed it. Well, we have two marvels actually, the marvel of the market and the marvel of hierarchy and we had to appreciate both of them, and we had to know the strength and weaknesses of both of them and that’s part of the exercise. |
| Q70 | Is this very much an area of on-going study? Do we know the strength and weaknesses of the firm versus the market or is this just something that is going to continue to evolve as firms evolve and the world becomes more global and all of this? |
|  | Oliver Williamson: Well,I don’t think this exercise is over, but I do think that we have made headway and an appreciation for firms and it goes beyond firms and markets, I mean there is also a place for bureaus, theres’s a place for cooperatives. There are lots of challenges that haven’t yet been uncovered and some that have that need to be more fully refined. But one other thing that I would also say, and that is that beyond intermediate product market transactions or close approximations theretoo, any issue that you can, that either arises as or can be reformulated in contracting terms is an issue that you can get some deeper insights by addressing it in transaction cost economizing terms. That isn’t to say that’s the only ones to bring to bear, but it is an instructive ones and that caught me by surprise because I really work with solving this early problem and go out and do other things.  There is more to be uncovered.  Oliver Williamson: Yes! |
| Q33 | The mention of cooperatives in particular, leads into your work. You have studied the use of common-pool resources and the different ways that shared resources can be controlled. And your work comes against a back-drop I understand of a body of thought which was suggesting that outside control was the right way to manage, outside control was the right way to manage shared resources that outside control tended to stop people just overexploiting the resources they had access to. But you suggested that actually it was a little different and that common control of one’s own resources was often a more productive way, a more successful way of controlling their exploitation. |
|  | Elinor Ostrom: A lot of people presumed that it was impossible for those who used a fishery or groundwater basin or a lake or river to self-organize. So self-organization was considered to be impossible and that was why they recommended either the market or the state not well defined not well worked out, but at least an idealized form. A great deal of research had been undertaken by people describing these efforts, but it was people in multiple disciplines about multiple sectors in multiple parts of the world, no cumulation. The presumption was the news headlines would always headline would give sardines were the story in California was a very great tragedy, but that did not mean that resources everywhere were being destroyed, but that’s what people payed attention to. What we have been trying to do is a systematic understanding of when will people engage in the transaction of self-organizing and then sustaining that organization over time. And they use a variety of forms, so sometimes they create a small government, but that’s not ‘the state’. The group of ground water harvesters I looked at in southern California created, there were 11 cities, all sorts of diversity, but they created something called a special district private water association, a variety of other things and did an incredible job without a single external authority doing it for them. It was tough, but they did self-organize and among the things they did was to develop their property right system.  That’s what I did for my dissertation and I did not know I was studying the commons. I was studying, I thought, Schumpeter’s problem of how do entrepreneurs develop and what did they do etc. I titled my dissertation “Public entrepreneurship” because that was the sort of thing that there were several leaders that were incredible in their effort that they put to getting people to meetings, getting them to discuss, getting them to sit down and this problem of conflict was just immense. If you can’t find a forum in which people can get the facts so that they aren’t arguing about facts at the same time they are arguing about what we should do and that makes a huge difference. In any case we now studied this in the experimental lab where we take the simple mathematical theory and have eight people in a lab making decisions that are the ones that come from the theory and indeed, if in a lab no communication is allowed so we have them sitting independently at a computer terminal around and can’t see each other, can’t communicate, they do overharvest. In fact, they overharvest worse than predicted. |
| Q33 | What do you give them to harvest in the lab? |
|  | Elinor Ostrom: We give them a certain set of tokens that they can invest in two options, one of which is the same, mathematically, as the common pool resource. The other would be that they go out and work on job market 8 hours for a set wage and there is an equilibrium concept called ‘the Nash equilibrium’ that you can take with a mathematical formulation and they should, according to the Nash equilibrium, pull out more resources from, because they are not cooperating. If we are all going in there and being aggressive you then harm each other and you, in the long run, are actually harmed because you are doing more than you should. Doing worse than Nash means they were even much more aggressive. We allowed one change in the lab, that people could engage in face to face communication, which in game theory was called ‘cheap talk’ because that process, if one person says ‘Why don’t we do x?’ and the others agreed, there was no external enforcer and without a third person enforcer the court or the state it was considered mere cheap talk. Well, mere cheap talk allowed them to greatly increase, they are a joint pay-off, and to preserve the mathematical resource and we tried a number of other things that we are seeing in the field.  We then studied irrigation systems around the world and compared farmer managed systems where the farmers have got to organize everything and figure out who is going to build and how they are going to build and who is going to do the labor and how they are going to allocate water, but they don’t have much, they do not have very many resources so they build very primitive systems. We have compared those with government systems with fancy concrete and beautiful gates and just fantastic amount of money put in them and they can get more water to the tail-end, they can produce more food and their efficiency is higher than the amount of the cost of the resources going in as opposed to the value of the what they obtained. So, to be more efficient, more equitable and do so with extremely primitive tools, a log that is what diverts the water, mud channels etc., is right amazing and now we are studying forests around the world. |
| Q33 | As the study of these variety of systems has led you, in 1990, to produce a set of guiding principles so to say which suggest how common pool resources should be used? |
|  | Elinor Ostrom: That wasn’t quite my view, I was not doing the ‘should’. I was trying to understand a series of systems that had existed for two hundred or so years and we had good data about them and good information and where there any … I thought we quoted the specific rules they had used in many of these and I could not find a specific rule or set of rules that were always used, so I tried to move up a level and ask what were the generalities across the long-lasting robust systems, I called them design principles, not from the perspective that they were what you should do but if you wanted to be robust you should probably take this seriously but how you would actually apply that would differ from system to system. So, I didn’t mean that the farmers or fishermen or anyone had actually had the principles in mind but they were, maybe we could call it best practices, they worked, they worked over time and then we looked at the failures and they not have the same characteristics. |
| Q49 | But given that one must be able to look around the world and just see innumerable examples of places where it is not working as well as it should be. There must be quite a temptation to start saying ‘should’ because if you have got a set of desired principles which you see working broadly around the place. It must be quite tempting to think. |
|  | Elinor Ostrom: Some people have applied it and I have a little bit of a tension with some of the ways it’s been applied because USAID and all sorts of – SIDA, others – have gone in and said: Now do this. When one of the principles is that they have developed a way to have conflict resolution locally, well sometimes that’s they have a court, in Spain, in eastern Spain there is a court that has been maintained once a week for back to 1500, Valencia. It’s got a very stylized way of handling things that works for them because they have multiple systems, the water master on each of the systems comes in its under court and if there is a conflict on their system they leave the court and testify in front of it, but that’s a particular design that works very well, that when you get experts who know the problem and face with it every day and then you bring a conflict between two neighbors before and you got a decision now. That is conflict resolution within a week of the conflict emerging. Now that won’t work everywhere as a conflict mechanism, so why I am nervous about people who want to just impose it, and that has been happening from time to time, is: What is it you are imposing? Are you running training sessions with people to give them an idea of the array of conflict mechanisms that people have used and then ask them what are some of their traditions, their ways of doing things, then they may be able to develop a conflict mechanism that works for them, but sometimes that’s a traditional chief. Well, that won’t work in LA.  Or it might!  Elinor Ostrom: Well, not usually, might work in Chicago, not my /—/.  Oliver Williamson: If I could make a couple of remarks and relate to, I think both of our work, and one of them is that I think that we have in different ways explained that organization matters and is susceptible to analysis. A lot of people are persuaded that organization matters, but for a economists it’s important to show that its susceptible to analysis and by doing this kind of microanalytic research and establishing the pre-conditions for some of the stuff to go through was really vital. But also, they were both affiliated with new institutional economics and in a way institutional economics operates at a couple of levels, one of which is kind of this organizational governance level, but there is a higher level. It relates to this inability to have a ‘cookie cutter’ that you can just go in and stamp, that is different nation states, they are differently organized and different traditions and customs as Lin says and those are important to how you organize and that should be factored in rather than simple ignored or glossed over. The last thing I would suggest in this, suggest the ambition of this line of research is that right now we all of I think major states, have something that’s akin to a council of economic advisors and I think that they’re well advised is to have such. I think that organizations are sufficiently important and as we make progressive headway with our understanding of it I think that all nation states ought to begin thinking about having a counsel of organizational advisors so there is lots of stuff that’s going on in Washington DC and around the world right now that has huge organizational applications and isn’t being factored in the same systematic way that basic economics is and sometimes that comes back to serious regret. |
| Q14 | I was going to ask about the frustration there must be in, when you study these things and, see people not thinking about it enough and whether that leads you ever to think or maybe having spent all this time studying it, I ought to be in more of a policy position myself and trying to make people listen from a different perspective. Is that a conflict you feel? Do you wish people would listen to you in a different way? |
|  | Oliver Williamson: I think we make headway, and it takes time but actually one of the access points for me was I served as special economic assistant to the head of the antitrust division in 1966–67. These were marvelously capable people who had been running the antitrust division but they were doing in a one sided way, there are essentially using text book economics, micro economics supplemented by a little bit of industrial organization but there was almost no attention given to the possibility that organization was important and that instead if you saw a non-standard practice or organizational form, the immediate suspicion was this has anti-social, anti-competitive intent and it’s easy to run with that ball and get the courts to buy into it. And they did. But the other possibility was that some of these have benefits when they are associated with it and one of the things, they are trying to do is infuse confidence into contractual relations that otherwise would break down and that this is an efficient thing to do. And that point of view has made progressive headway. I don’t say it’s the mainstream now but there is much more interest in being symmetrical rather than one sided in our way of doing anti-trust enforcement, and doing regulation and it should be the case that as we design bureaus that this is actively taken into account and take the problem of homeland security that was put together in wrapped fashion in a way which I think many of us that are closer to the organizational side of things are say this really should have been vetted by people with organizational backgrounds and interest and capabilities.  Elinor Ostrom: I did series of studies that looked at a slightly different question now and public goods and how do you organize local metropolitan areas, because we started with water, but then I turned to policing. The presumption was that in a lot of the literature that fragmentation was evil and that what you needed was a single center. Hierarchy was the model that lot of reform efforts had in their mind, that hierarchy would solve all of the problems of metropolitan areas, and I was deeply suspicious of this having done work on a polycentric industry where there were public and private firms working together at multiple levels. It did not appear to me chaotic but many of my colleagues would put a map in a textbook and it would show some individual cities and then show some other units and the heading would be “The chaotic structure of x metropolitan area” because there were multiple units on the map that was chaos.  We studied policing in a number of smaller studies comparing very similar neighborhoods served by independent small departments versus large. We never found a large department that could out preform a small, partly because of knowledge and understanding the … I rode in a lot of patrol cars and if you were in a small or medium size patrol car the officer would tell you a lot about the neighborhood he was patrolling and really knew it. In large departments to cope with the problem of corruption, which happened in large departments, they rotated the officers and so they might have 90 districts and you rotated every 28 days. Well, you didn’t know much about that district so the knowledge level of the officer and the knowledge that the citizens had of the officers serving them was very low. We also then did a study of 80 metropolitan areas looking at the areas in terms of direct services like patrol being provided by small or medium size versus the problems of crime lab for which there were economies of scale, and the argument had been: the economy had scale for crime labs so consolidate everything. We found that in field in 80 metropolitan areas we found only 84 crime labs, so in the field officers and departments and majors had figured it out and they did all try to have their own crime lab, they frequently contracted with the local hospital that had all the equipment and just had to have some personnel that understood the problems of criminal investigation. In 80 metropolitan areas we found the most efficient and effective had some units at the very top and then had a large number of small to medium size direct producers.  This is an entirely different vision but its consistent with the idea that there isn’t just one way of organizing a corporation and this was there is one way of organizing a metropolitan area and I think our work, it complements each other’s because you can’t make those kinds of presumptions although the transaction costs involved in an interaction between a officer and a citizen who don’t know one and other and the interaction between an officer and a citizen who have seen each other in different times, their faces are familiar, you know a little bit about the background, is entirely different. |
| Q70 | I am getting into hot water by using such terms. People are better able to, if you like, to look after themselves, they either use water in a common way more sensibly than the government might think they might or if they’re local police forces they organize themselves more sensibly again than the government might think they might. |
|  | Elinor Ostrom: But there are settings in which they would just grab like that so you can’t just assume that people under any circumstance will always take into account others and always be good. Humans are neither all angels or all devils and so it is the context in the institutional context in which they find themselves that enable them to have more willingness to use reciprocity to trust one and other and to be in a situation that ‘I can trust you because I think you trust me, and I won’t be sucker’. And one of the problems with the commons is that if I trust everyone else is going to be a good guy and I am cooperative and there are not, I am a sucker and people are worried about being a sucker. |
| Q18 | Are you surprised to find yourself studying what you study? Because you mention that you started out studying entrepreneurship as a graduate student and now you study the commons, when you look back, is it odd that you ended up here? |
|  | Elinor Ostrom: It’s a great honor to be in here, but I didn’t, I was studying the commons from the beginning, but I didn’t know it. I was studying a tough problem that people were trying to solve and entrepreneurship in the public or private sphere has to do with people who are able to understand some of the complexity of a setting and how to organize, so the capacity to organize is crucial and yes, the presumption has been the only way that people organize is inside the market or through a state and that’s what I think has to be, we have to move on to understanding that organization can occur at multiple scales and multiple ways and not always the best, so the mafia is organized. That is not always good. |
| Q2 | But I asked the question as a prelude to asking who you think ought to come into the area now, what are you looking for in terms of new blood to come in and advance the theory further. Is it possible to say? Is there something lacking? |
|  | Oliver Williamson: It is a little bit idiosyncratic but I do think that there is a movement in economics to be more interdisciplinary and in pulling related disciplines together, whereas previously there is and I think there always will be a sense that economics is the prince of the social sciences but that there is a greater appreciation, I think, that there are complex phenomena that if you look at them exclusively through the lens of economics not assisted by any of any of contiguous sciences that you are going to be missing things. One of the things, one of the reasons why I think that I was able to look at some of the issues in anti-trust enforcement differently then was the sort of standard view was that I was part of a really unusual interdisciplinary program in social sciences at Carnegie Tech, then Carnegie Tech, now Carnegie Mellon, in which organization theory was thought to have co-equal status with economics and should both inform and be informed by economics.  There was sort of traffic across this boundary and I do think that having more of our students exposed to the contiguous social sciences and you know, your principal discipline could be political science and you will reach out to economics and that’s been going alright and could be economics and you reach out to political sciences or sociology and that’s been going on. So I think there is, I don’t think everybody by any means should be sort of multi-disciplinary in their work, but I would hope that there will be growing agreements that these sciences have a lot of common ground and that they should work on it in a collaborative way and then often it’s going to be a better more productive. It’s going to be more interesting as a matter research and it’s going to be more productive for the common well.  Elinor Ostrom: I also think interdisciplinarity is a very important. Vincent Ostrom and I established a center called a workshop in political theory and policy analysis back in 1973–74 and we have always had multiple disciplines around the table and that’s just been my life. We now are reaching out more to ecologists because of, if you’re studying forest, you have to learn a little bit more about the ecology of forest because again rules that work well in a deciduous forest don’t work well in a tropical wet forest and trying to understand the ecological foundations is very very tricky and I am having learned a lot of new concepts and new terms.  Are you specializing in forest studies?  Elinor Ostrom: No, it happens to be the applied area I am working in now heavily. I have a couple of articles I am working on, irrigation, so we have a large database and we have a book coming out in another 6 months or so assuming all of this. But I have just finished a book that I am very very pleased about with the title “Working together – collective choice, the commons and multiple methods in practice” and the thesis of it is besides interdisciplinary is multiple methods. Sometimes we have people only do a mathematical model or only do only case studies or only do large N and they criticize everything else and the problem is that we need to be able, either if we can’t ourselves do the model, that’s foolish but we need to be aware of the advantages and know enough about some of the methods that we can work with an expert in a method and then bring some of our knowledge from a different set of methods together. I love experimental work, but it doesn’t have the richness that I have when I study forests in the field. I love both of them as ways of different ways of getting insights and sometimes being in the field leads back to the lab. In particular I kept seeing people monitoring and sanctioning each other in the field, amazing times and so I came back from a particular vivid occasion in Nepal and said to my colleague Roy Gardner who is a game theorist and Jimmy Walker who is an experimentalist: Let’s please, let’s do a formal game involving people paying their own cost to sanction others, because in the field if they go this was an instance where someone had dug into an irrigation and so several of them went running down the hill and yelling and screaming and others starting patching it immediately and I mean the energy they put in, they didn’t not think about it. There was no rational calculation about this, they just did it.  Well again and the game theory prediction is they wouldn’t, then we could put it in the lab with a simple game we used earlier, but now changing one attribute and giving them a chance to pay a fee to find someone and they did it even though it’s predicted they won’t and if we gave them eventually the choice of what kind of sanctioning mechanism they had and then allowed them to use it, they got to about 94% of optimal. And here is having the game theory model great because we knew what optimal was and we could put it in the lab and figure out what happened, but that was field lab and now on our forest studies because of our earlier findings we asked quite about monitoring and sanctioning. We are finding that whether people who use a forest actually monitor each other to be more important then who owns. Government ownership, private ownership, community organization – all of those are important but if they, the formal rules are there, and the users don’t see a future and don’t see that they are involved and don’t take an interest and don’t monitor – forest goes down. |
| Q75 | Forgive this last question, but since you both work on organization theory and the right use of common resources, I wonder whether you are organized people yourselves or whether you are good at organizing things like a complex family which are five children I believe. |
|  | Oliver Williamson: That’s true! Well, I think of myself as being disciplined and I guess because I enjoy my work so much I spend a lot of time struggling with new and different issues trying to understand them and trying to see if I can find ways to fold them in and whether or not they have public policy ramifications and the like. It’s been a joyness of work and see things make headways and have good students come through. And its obvious from what Lin says that she’s just really wrapped up in this stuff, its easy to. I wouldn’t call myself an excellent organizer but then the less that it does require keeping a lot of disparate ideas in mind and finding ways to pull them together. There is a certain amount of background organization that goes on. Sometimes actually to my surprise, this has been true more recently than it was when I was a younger fellow, but I will start dreaming about some of this stuff and thinking about what I should be doing and if the ideas are good enough. It doesn’t always turn out that they are, but I get up and jot little notes down and then get back to sleep rather than struggle in the middle of the night and so I have got some subconscious work in front of me, too.  That quite an advantage.  Elinor Ostrom: We have an unusual center that Vincent and I organized long ago, and we had an opportunity both to be in Germany at Bielefeld at the interdisciplinary center and saw the advantage of working with young postdocs as opposed to just graduate students and so we innovated and developed a postdoctoral program, modest in size. That has been a very successful way of both organizing the research side and the teaching side because you have graduate students who are, some of them state of the art on their new tools, but then they are able to talk to young to middle faculty who are not their supervisors but are in the same building and going to the same seminars and discussing when they get and sometimes you know its two-way learning. Then to have interdisciplinary teams and what we have learned is we have had to learn new rules and ways of organizing as we have run into things over time and so adjustability and new circumstances lead you to have to do things slightly differently. We have had the good fortune of a wonderful set of colleagues from all over the world and I think one of the things that will be happening, as far as new entrance, is that we will have more scholars from the developing world and from Asia and that will be contributing to our joint knowledge because they have different experiences and are bringing in new ideas and new ways of thinking about it so I see that as part of the future.  That seems like a marvelous way.  Oliver Williamson: Do you mind if I come back to the organizational issue. One of the things that I was wise about doing is I never took seriously and followed up on any opportunities on to be a Dean.  Elinor Ostrom: Yes, same.  Oliver Williamson: And my experience as department chair is that this was mainly discouraging. There are several things that I did do that did have organizational ramifications and one was when I was at Penn then at Yale and then at University of California Berkeley. I helped to organize workshop and obviously the workshop has been really important to Lin’s work and it’s been important to mine and having colleagues around it interacting and in a constructive way. There is another thing I did, got into it a sort of a backdoor of being a member of the editorial board of the then *Bell Journal of Economics*, now the *RAND Journal of Economics* and so I was going to become editor of it and as editor you have a lot of opportunity to move the field around and I have a bunch of really exceptional associate editors who shared a lot of my views in this, probably because I recruited them, but I think that was very satisfying experience. Then when I went to Yale, Yale Law School was interested in having a journal that was edited by the faculty rather than by students, as most law journals are, and we organized a journal of law, economics and organization and I am proud of the accomplishments of that journal. If I put my heart into it I guess I can organize, but taking on jobs like Deans which are important requires you to be a lot more tolerant than I am. |
| ID | 0833 |
| Biographical | I was born in Superior, Wisconsin on September 27, 1932 as the second child of Scott and Lucille Williamson. Both of my parents had been high school teachers but my father left teaching when he married my mother and joined my grandfather, Oliver E. Dunn, in the family real estate business. My father was a successful small businessman and active participant in the life of the community, culminating with nine years of service as President of the Superior City Council. My mother had been the principal of the small high school in Minnesota where my parents met. The prevailing rule on married women required that my mother retire from teaching, but her knowledge of the subject matter was undiminished. Her recall of Latin after 20 years was such that she could have resumed teaching where she left off on a moment’s notice.  I attended the public schools in Superior. This was a very egalitarian experience. Superior is the most democratic community I have ever lived in. Talent took many forms and was respected in all.  I was a good student, a mediocre athlete, and had many good high school friends I hung out with – attended Saturday night dances, went bowling, played basketball and pool, and, especially, played Friday night poker. Many of them remain close friends to this day.  My university teacher and mentor [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/1972/arrow-facts.html) remembers me as a student who asked good questions. Although I had not previously thought of myself in that way, on reflection I think that Arrow was right. I was forever curious about how things worked (or didn’t work), which led me to identify lapses or anomalies and/or to push the logic to completion. Such an orientation would serve me well throughout my academic career.  My initial thoughts of becoming a lawyer changed in high school as I became more attracted to math and science and began talking about being an engineer. My mother declared that M.I.T. was the place to go and, with the advice of the physics teacher at the local college, I enrolled in Ripon College, which had a combined plan with M.I.T.  That combination worked out well. My first jobs after graduation in 1955 were as a project engineer for G.E. and later with the U.S. government in Washington, D.C., where I met and married my wife, Dolores Celini. I applied to and was accepted into the Ph.D. program at the Graduate School of Business at Stanford, where I enrolled in 1958. To my surprise and delight, I discovered that much of my engineering training in mathematics and statistics and model building carried over. But there was more to it than that. My engineering training gave me a much more grounded foundation than would most undergraduate programs in any of the social sciences.  Although I would not come to appreciate this last until later, there was a major difference between engineering and economics with respect to hypothetical ideals. Thus whereas assumptions of weightlessness or perfect gas laws or frictionlessness etc. served the purpose of simplification in engineering, these assumptions would give way to realities (in the form of friction, resistance, turbulence, and the like) as engineering applications were attempted. In economics, however, assumptions of frictionlessness (of which the standard assumption of zero transaction costs was one) often went unquestioned or, even worse, were invoked asymmetrically. Thus whereas markets were subject to “failures” for which corrective public policy measures were prescribed, there was no corresponding provision for failures in the public sector. A more symmetrical approach would be to recognize that positive transaction costs were the economic counterpart of friction and that all forms of organization experience such costs – albeit in variable degree (depending on the attributes of the transaction to be organized). I credit my engineering background with giving me a receptive attitude toward transaction costs, to include an interest in pinning down and working out the organizational ramifications of such costs.  My move from business into economics was accomplished with the aid of four events: (1) I first became intrigued with economics when I took an economics class from James Howell, who had just joined the business school faculty and recognized (before I did) that I had economic interests and intuitions; (2) on Howell’s advice I took a class with [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/), whose class confirmed Howell’s perception and greatly whetted my appetite; (3) on the advice of another new appointment to the business school, Charles Bonini, I came to learn about and became intrigued with the interdisciplinary program in economics and organization theory at Carnegie; and (4) I was awarded a portable three-year fellowship to support my graduate studies by the Ford Foundation.  Dolores and I and our first two children, Scott and Tamara, moved to Pittsburgh, Pennsylvania in the fall of 1960, where I found my niche: the combination of economics and organization theory, as taught by the small but remarkable faculty of the Graduate School of Industrial Administration to classes with a small but remarkable group of students. Seven Nobel Prizes in Economics have since been awarded to faculty and students at GSIA in its halcyon years. Jacques Drèze, who was a visitor, speaks for me and many others in his observation that “Never since have I experienced such intellectual excitement.”[1](https://www.nobelprize.org/prizes/economic-sciences/2009/williamson/biographical/#not1)  My dissertation on “The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm” was completed in 1963 and, as a winner in the Ford Foundation dissertation competition, was published by Prentice-Hall the following year. I joined the faculty of the Economics Department at the University of California, Berkeley in the fall of 1963.  The field in economics that most closely related to my training at Carnegie was that of industrial organization, which is the field into which I was hired. I had never, however, taken a course in industrial organization – which was both an advantage and a disadvantage, more the former than the latter. With the benefit of hindsight, the field of industrial organization, especially its public policy applications to antitrust and regulation, had fallen on hard times in the 1960s. As Victor Fuchs put it in his introduction to National Bureau of Economic Research Colloquium on *Policy Issues and Research* *Opportunities in Industrial Organization*, “Whither industrial organization?… All is not well with this once flourishing field” (1972, p. xv). Never having been “indoctrinated,” I had no hesitation in taking exception with the prevailing orthodoxy.  The basis for the hard times within IO to which I refer can be variously described. Mainly I would ascribe them as due to three related misconceptions. First, there was undue reliance within the field of industrial organization on the neoclassical theory of the firm according to which the firm, to include the modern corporation, was described as a “black box” for transforming inputs into outputs according to the laws of technology. This was fine for some purposes but not for all and led to the second misconception: the internal organization of the firm could be ignored because it was inconsequential. Third, as mentioned above, frictions were ignored or suppressed by selectively invoking the assumption of zero transaction costs.  The propositions that (1) organization is often important and (2) economics and organization theory need to be joined if we are to develop a deeper understanding of complex contract and internal organization are ones that I introduced into the classrooms in the industrial organization and public policy courses that I taught at Berkeley (from 1963–65) and thereafter at the University of Pennsylvania. But my experience in 1966–67 as Special Economic Assistant to the Head of the Antitrust Division of the U.S. Department of Justice was in many ways the defining event.  Although the leadership and staff of the Antitrust Division in the late 1960s were both superlative, the prevailing attitude toward nonstandard and unfamiliar contractual practices and organization structures was that such “abnormalities could be presumed to have anticompetitive purpose and effect.” Indeed, given that the prevailing price theoretic orientation effectively disallowed economies of a non-technological kind, it could hardly have been otherwise. That economies could result from organizational and contractual design was simply outside the canon.  Vertical integration and vertical market restrictions were regarded as especially problematic. Absent “technical or physical aspects,” the unified ownership of successive stages of production or the application of vertical market restrictions by a manufacturer on a distributor were routinely taken to be anticompetitive (as in the *Schwinn* case, which was argued before and decided by the U.S. Supreme Court in 1967). I therefore resolved to re-examine the antitrust ramifications of vertical integration when I returned from Washington, D.C. to resume teaching and research at the University of Pennsylvania.  Teaching can be learning, especially if student curiosity with the question “What’s going on here?” can be elicited. This was a recurrent question in the graduate seminar that I organized to examine the literature on vertical integration and vertical market restraints. Although parts of this were instructive, almost none of this literature recognized that markets and hierarchies differed in kind, much less that such differences had efficiency ramifications. Given this lapse, I decided to reformulate vertical integration from a combined economics and organization theory perspective in which express provision for positive transaction costs were made.  My 1971 paper on “The Vertical Integration of Production: Market Failure Considerations” differed from orthodoxy in the following respects: (1) the orthodox lens of choice is supplanted by the lens of contract/governance (which leads into an examination of more microanalytic detail and treats markets and hierarchies as alternative modes for mediating contracts); (2) the assumption of hyperrationality is supplanted by bounded rationality (on which account all complex contracts are incomplete); (3) the conveniently narrow view of simple self-interest seeking is expanded to make provision for strategic behavior, to include defection from the spirit of interfirm cooperation when the stakes are great; (4) the standard assumption of zero transaction costs is supplanted by a focus on positive transaction cost differences (conditional on the attributes of transactions, as between markets and hierarchies); (5) adaptation (of autonomous and coordinated kinds, which correspond, roughly, to market and hierarchy) is taken to be the main problem of organization; and (6) much of the comparative institutional action is shown to reside in the condition of bilateral dependency (which could vary from slight to great, depending on whether the investments made in supporting assets were generic or specific) in combination with disturbances to which adaptations were needed.  This reformulation revealed that the contractual hazards and maladaptation costs that would be incurred under market procurement (outsourcing) of a good or service for which the aforementioned condition of bilateral dependency was significant could be mitigated if the buyer were to produce to its own needs (integrate) – although, to be sure, bureaucratic cost differences attributable to a sacrifice of incentive intensity would also need to be taken into account. By contrast with the ideal transaction in law and economics, where each party could go its own way at little cost to the other if the requisite supporting investments were generic, a fundamental transformation sets in if the requisite investments are transaction specific (hence are nonredeployable to alternative uses and users except at the sacrifice of productive value) and the supporting long-term contracts are incomplete.  The seeds of a predictive theory of firm and market organization resided therein, but this was unfamiliar territory and would require more research before it would take hold. As, however, extensions to and refinements upon the basic framework were made, as additional contractual phenomena were interpreted as variations on a theme, and, especially, as empirical tests were corroborative of the predictions of the theory, “transaction cost economics” gained momentum.  Publications of mine that contributed to the progressive development of transaction cost economics include: *Markets and Hierarchies* (1975), which received a surprisingly favorable reception for a book that was difficult to write and put a heavy burden on its readers; “Franchise Bidding for Natural Monopoly” (1976), which demonstrates that doing public policy on regulation heedless of the contract relevant details is parlous; “Transaction Cost Economics: The Governance of Contractual Relations” (1979), which gave more structure to the theory and invited empirical work on transaction cost economics; “Credible Commitments: Using Hostages to Support Exchange” (1983), which develops the importance of credible contracting to support interfirm exchange and would serve as an introduction to “Comparative Economic Organization” (1991), which paper explicates the hybrid mode of contracting (located between markets and hierarchy) and makes the case for supplanting the all-purpose contract law of “legal rules” by making provision for “contract as framework” and introducing “forbearance law”; *The Economic Institutions of Capitalism* (1985) pulled the foregoing together and expressly addressed the burdens of bureaucracy by showing that limits to firm size arise because of the impossibility of implementing replication and selective intervention. Also, “Corporate Finance and Corporate Governance” (1988) and “Corporate Boards of Directors” (2008) interpret debt and equity not merely as modes of finance but also as modes of governance to which corporate governance ramifications accrue; and my 2000, 2002, and 2005 articles on “The New Institutional Economics,” “The Theory of the Firm as Governance Structure,” and “The Governance of Contractual Relations” make a place for differences in the rules of the game (the institutional environment) as well as the play of the game (the institutions of governance) and broaden the reach and perspective.  My appointment in the Economics Department at the University of Pennsylvania was expanded to include appointments to the faculty of the Law School and the School of Public and Urban Policy in the late 1960s, which turned out to be a productive mixture with many good colleagues and students in all three places. It was with reluctance, therefore, that I accepted an offer for what I thought would be an even more productive appointment to the School of Organization and Management, the Law School, and the Economics Department at Yale in 1983. Organizing workshops on law and organization in the Yale Law School, on economics and organization in the School of Organization and Management, and serving as founding editor of the *Journal of the Law, Economics, and Organization* were all gratifying. The complexities of the Yale appointment turned out to be considerable, however, and Dolores and I returned to the University of California, Berkeley in 1988 where we have been since.  Unmentioned in the foregoing, but nonetheless pertinent, is that our five children – Scott, Tamara, Karen, Oliver, Jr., and Dean – were troopers in all of the moves that we made between 1958 and 1983. (All had “left the nest” before our last move in 1988.) A factor that mitigated the dislocation costs of these moves is that we purchased my parents’ summer home at Lake Nebagamon, Wisconsin in 1972 after my father died. We found that I could both work and play at the Lake and that the entire family enjoyed the change of pace – which would include tennis, golf, swimming, sailing, water skiing, and bridge, as well as canoeing in the Boundary Waters and flat out relaxation and periodic repairs to the property. Northern Wisconsin would become a fixture in the lives of all of us – parents, kids, grandchildren – over the years.  Life at Berkeley has been good. We have a home near the campus that looks out on San Francisco and the Bay with many marvelous sunsets. My main appointments in the University have been in the Haas School of Business and the Economics Department, to which the Law School generously extended an appointment as well. Creating a new field in the Economics of Institutions (in the Economics Department) and reshaping the Business and Public Policy curriculum (in the Haas School) have both been rewarding experiences. I am surrounded by outstanding colleagues in both places and have supervised the dissertations of a large number of excellent students who have gone on to have successful research, teaching, and administrative careers. They have been a joy to work with. Dolores has always adapted quickly to new surroundings. She worked with the League of Women Voters during our first two years at Berkeley and continued her League work to become President of the League when we lived in Lower Marion Township, where she also was President of the neighborhood library and took art classes at the Barnes Foundation. These classes would serve her well when we moved to Guilford, where she took a job with the Greene Gallery, which sold fine art by contemporary local artists. She adapted again when we moved back to Berkeley in 1988, where she has been active in the Section Club (serving as President and as a member of the Italian Section, and working with SOS and University Village), as Chair of the Parks and Recreation Committee for the City of Berkeley, as an active member of Town and Gown, and as Judge of elections at the polls.  My main administrative stint was to serve as Chair of the Academic Senate at Berkeley in 1995–96, which was a demanding but fulfilling experience. Working with Chancellor Chang-Lin Tien and with faculty across the entire campus gave me a deeper appreciation for the unique qualities of the University of California, Berkeley. The “Berkeley Triple,” as I have come to understand it, is this: excellence, energy, and joy – in that the commitment to excellence applies across the length and breadth of the campus, merely to step onto the campus is energizing, and a sense of joy and satisfaction go with the job.  I retired from teaching in 2004 but continue to remain active in research and I attend and sometimes present workshops (my colleagues renamed a workshop after me, to my surprise and delight). I also selectively participate in recruiting and fund raising. And, weather permitting, I play doubles tennis with Dolores as my partner every Sunday and golf with my high school friend John Salmela every Tuesday. Travel, nearby and abroad, for both pleasure and business, gets worked in.   |  | | --- | | References | | *Books* | | *The Economics of Discretionary Behavior: Managerial Objectives in a Theory of the Firm*, Prentice Hall, Englewood Cliffs, N.J., 1964. | | *Markets and Hierarchies: Analysis and Antitrust Implications*, The Free Press, New York, 1975. | | *The Economic Institutions of Capitalism: Firms, Markets, Relational Contracting*, The Free Press, New York, 1985. | |  | | *Articles* | | “The Vertical Integration of Production: Market Failure Considerations,” American Economic Review, May 1971, 61, 112–23. | | “Franchise Bidding for Natural Monopolies – in General and with Respect to CATV,” *The Bell Journal of Economics*, Spring 1976, 7, 73–104. | | “Transaction Cost Economics: The Governance of Contractual Relations,” *Journal of Law and Economics*, October 1979, 22, 233–261. | | “Credible Commitments: Using Hostages to Support Exchange,” *American Economic Review*, September 1983, 73, 519–40. | | “Corporate Finance and Corporate Governance,” *Journal of Finance*, July 1988, 43, 567–91. | | “Comparative Economic Organization: The Analysis of Discrete Structural Alternatives,” *Administrative Science Quarterly*, June 1991, 36, 269–296. | | “The New Institutional Economics: Taking Stock, Looking Ahead,” *Journal of Economic Literature*, 38 (September), 2000, pp. 595–613. | | “The Theory of the Firm as Governance Structure: From Choice to Contract,” *Journal of Economic Perspectives*, 16 (Summer), 2002, pp.171–195. | | “The Economics of Governance,” *American Economic Review*, 95 (May 2005), 1–18. | | “Corporate Boards of Directors: In Principle and in Practice,” *Journal of Law, Economics, and Organization*, 24 (October 2008), 247–272. |   1. 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| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0833=OW  [Oliver Williamson] Hello?  [Adam Smith] Oh, good morning. Professor Williamson, my name is Adam Smith. I’m calling from the Nobel Foundation website in Stockholm, Sweden.  [OW] Oh yes.  [AS] Where, of course, it has just been announced that you have been awarded this year’s Sveriges Riksbank Prize in Economic Sciences.  [OW] Yes.  [AS] Congratulations, of course.  [OW] Oh, thank you very much.  [AS] We have a tradition of recording extremely short telephone interviews with all new Laureates immediately following the announcement so, despite the early hour, would you mind if we spoke for just a very few minutes?  [OW] Uh, fine.  [AS] Thank you. So, first of all, I imagine that the phone call has woken you up because it’s 4am there?  [OW] That’s correct.  [AS] Was it a tremendous surprise?  [OW] Well, you know, you’re sort of aware that you’re on these lists but it was a very pleasant surprise and I’m deeply grateful.  [AS] The citation for the award reads “for his analysis of economic governance, especially the boundaries of the firm,” and, broadly, you work on the question of why firms exist. Would that be correct to say?  [OW] Well, I work on that. But, more generally, what my work is, and this is in a large measure a result of the graduate training I had when I was a student at Carnegie Tech in the early 1960s, is it’s an interdisciplinary effort to draw economics and organization theory together, to try to understand the boundaries of the firm and a whole set of practices that firms engage in, and more generally to understand complex economic organizations, of both the private and public sector kinds.  [AS] An analysis of why firms can accomplish some transactions better than the markets can accomplish them?  [OW] Well, that’s true. But it also has a lot of public policy ramifications for antitrusts and regulations and for trying to advance some of the ideas of people that I was a student of, or deeply influenced by, who included Kenneth Arrow and Herbert Simon and Ronald Coase and others who brought these issues to the fore. And I saw that there was a research opportunity to push them ahead and give them more operational content. And, as they say, it does have a lot of ramifications for public policy.  [AS] And, one of the things that stressed about your work is that it provides for empirical testing of the theories you produce that …  [OW] That’s exactly right. And, that’s been an objective of mine right along, but I owe enormous debts to my students who were persuaded on the merits of this approach and many of whom who undertook to engage in empirical testing and broaden the reach of it. And, it’s … I have no doubt that the economics of governance is influential in significant measure because it does speak to real world phenomena and invites empirical testing and much of it has been corroborated.  [AS] And is there any particular example of a recommendation that has come out of your work that, in the public policy field for instance, that you would like to cite?  [OW] To try to understand that all feasible forms of organization are flawed, and that we need to understand the trade-offs that are going on, the factors that are responsible for using one form of governance rather than another, the strengths and weaknesses that are associated with each of them, and to fashion public policy, as they say, with respect to antitrust. And that project, one thing leads to another, it relates to aspects of finance with reference to the uses of debt and equity, it relates to corporate governance. It is … It adopts a different orientation than the standard one which is to look at the firm and market organization in significant measure from a technological point of view. I look at firm and market organization from a contractual point of view and …  [AS] You’ve used this phrase in the past; the ‘lens of contract’ …  [OW] That’s correct.  [AS] So you view the organization through the contracts that the organizations are generating?  [OW] That’s correct. The market contracts are one way of accomplishing … Let’s say the make-or-buy decision: you decide to buy. Contracting within the firm and with the … [inaudible] … the governance structures that firms provide is another alternative. And, it has as they say … [inaudible] … for coordination purposes, but it has limitations in incentive respects. And there’s a series of, as they say, strengths and weaknesses that are associated with firms, with markets, with intermediate forms of the hybrid kind that fall between them. It actually, and I mentioned to you earlier that it was an interdisciplinary project that combines economics and organizational theory, … it also appeals to aspects of the law, mainly contract law, and tries to draw these together and demonstrate the, well, a new way of trying to interpret complex economic organization.  [AS] Thank you. Thank you very much indeed.  [OW] OK.  [AS] It was a great pleasure to speak to you, congratulations and thank you very much.  [OW] Bye.  [AS] Thank you, bye, bye.  [OW] Bye, bye. |
| Interview |  |
| Q6 | I think when I phoned you both in October you were both somewhat surprised by the name, it’s a bit of a liability on that day of the announcement. How have the couple of months since the announcements been for you? |
|  | Oliver Williamson: In a word – hectic.  Elinor Ostrom: And two words – very hectic.  You must be catapulted into a spotlight and a world of activity.  Oliver Williamson: It is, one of the things that is really gratifying however is how much goodwill you soak out from hearing from friends, former students, colleagues and family. It’s unreal but it’s there and so genuine that I couldn’t have imagine it.  That is nice.  Elinor Ostrom: I double that, some parts of the e-mail load are not friends and family and trying to cope with that has been a bit of a challenge. For our former students and colleagues all over the world, they have been sending joyous messages and that is very gratifying. |
| Q2 | And since you are the first woman to have been awarded the Prize in Economic Sciences in its 40-year history I imagine that is slightly added to the load you have been under. |
|  | Elinor Ostrom: Yes, I think that there have been many people who have been particularly asking about gender problems in American universities. Universities more general, it’s not just US.  I am sorry, you must be terrible bored of discussing this, but the fact that there has been one in 40 years, do you think that is in any way representative of the female contribution to economics over the last 40 years?  Elinor Ostrom: Not the contribution, but the capability to be in academia.  Do you see things improving?  Elinor Ostrom: Yes, I think that many departments now have new graduate students coming in at a rate, new women graduate students that they did not have previously, and our departments are moving toward hiring more. I don’t think I will be the last.  Do you see a change in the Nobel women coming to the subject?  Oliver Williamson: Oh yes, yes, and as Lin suggests, many are really excellent, and they get good placements and they are doing good work and we are going to hear from them.  Elinor Ostrom: Yes! |
| Q18 | So the prize this year is divided into two parts, but you are united in your work by the phrase the committee used such as ‘economic governance’ and broadly you both study ways of regulating transactions in the broader sense. If I could start with you, you study complex economic organizations and complex transactions and one part of your work that has been particularly sort of commented on by the committee is the fact that that allows you to say something about what sort of transactions should be conducted within firms and what sort of transactions should be conducted within a market. |
|  | Oliver Williamson: Yes!  Can you give examples of the sort of transaction that we are talking about when we say that?  Oliver Williamson: Yes, but I wonder if I could back off for a minute and talk about governance, because there is a compact two-sentence statement that was made by John R. Commons who was an older stylist, institutional economist, and I think undervalued by a lot of people then and since but never the less profound in many of his insights. And the two-sentence statement runs roughly as follows: “The principal unit of analysis should contain in itself, the three principles of conflict, mutuality and order. This unit is a transaction”. The part of it that I initially related to, because I was working at this more microanalytic level of transactions rather than completed goods and services, was that the transactions /- – -/ analysis. But as I got deeper into the study of organization, I started to ask what is the value added for some of these more complex forms of organization.  This triple of conflict, mutuality and order goes right to the essence and specifically I would describe governance as the means by which to infuse order thereby a medicate conflict and realize mutual games and that was contemplated, I would say, by Commons, even though he didn’t have much success in implementing it. That’s a profound sense of what I think was a missing arena for economics for a long time. A specific example would be the first project that I undertook along these lines and that’s the make-or-buy decision that firms was confronted with, it needs a specialized component let us say. It can either outsource to a independent contractor and mediate that transaction with a written contract that stipulates what’s to be delivered when, why and how they are going to handle difficulties in the like or you can take that transaction out of the market and organize it internally which is the make decision and vertical integrations issues had been around for a long time, addressed mainly as a technological issue addressing it in contractual terms with reference to two alternatives that one might have an advantage over the other or not. But the challenge was to first look at it contractually actually compartive contractually because you always want to think of two or more alternative ways of getting it done and then thirdly one other fact is that to drive it around in one way or another. And working that through was the transaction cost’s economics challenge. |
| Q70 | I can see that you can analyze a situation in which a firm is saying should we have this component made outside, buy it in or should we make it ourselves? But you have been able to turn that into theories which suggest in which circumstances the make decision is correct, which circumstances the buy decision is correct and then those theories have been put out to empirical testing and they presumably refine the theory. Is it now, are you able to suggest principles by which firms can govern their decision to the make-or-buy decision? |
|  | Oliver Williamson: What you need to do is you need to identify the critical attributes that define transactions so that operationalizing that the concept of transaction by naming what you take to be the critical factors across which transactions cost will vary. Then secondly you need to do the same thing with reference to governance structures, you need to identity the critical dimensions with respect to which governance structures differ. Then you ask the question given this cluster of attributes of transactions. Do they pose easy or complicated kinds of contractual issues? If they are easy, there are lots of easy transactions, I mean this is what spot markets and so are all about, neither party has any kind of dependency relation on the other. It’s sort of the classical problem of exchanging nuts for berries at the edge of the forest, you know. We both benefit from the exchange, but we don’t have any continuing relationship. Where you have to have a continuing relationship, where one party needs to make specialized investments in support of the other and where this contract may break down if it’s done in an intra-firm fashion, that’s where the concept of taking transactions out of the market and organizing them internally is important.  And actually, there is two other sort of major figures that I would say that enter into this and one is, both of them talk about adaptation as being the central problem of economic organization. One of these people is an organization theorist also from the 1930s, namely Chester Barnard, and he was interested in what it was distinctive that was going on within firms and his fear was that coordinated adaptation was the central kind of a contribution that internal organization had to bring and during that in a conscious deliberate purposeful way. This was, I would say, the marvel of internal organization, all was bureaucracy then and since it has been widely scorned. Sometimes for good reasons, but nevertheless there are things that internal organizations are especially good at. The other person was [Friedrich Hayek](https://www.nobelprize.org/prizes/economic-sciences/1974/hayek/facts/), former Nobel Prize winner, that emphasized autonomous adaptations accomplished to the market in response to changes of relative prizes. This was the marvel of the market as economists had long regarded it but as Hayek specifically nailed it. Well, we have two marvels actually, the marvel of the market and the marvel of hierarchy and we had to appreciate both of them, and we had to know the strength and weaknesses of both of them and that’s part of the exercise. |
| Q70 | Is this very much an area of on-going study? Do we know the strength and weaknesses of the firm versus the market or is this just something that is going to continue to evolve as firms evolve and the world becomes more global and all of this? |
|  | Oliver Williamson: Well,I don’t think this exercise is over, but I do think that we have made headway and an appreciation for firms and it goes beyond firms and markets, I mean there is also a place for bureaus, theres’s a place for cooperatives. There are lots of challenges that haven’t yet been uncovered and some that have that need to be more fully refined. But one other thing that I would also say, and that is that beyond intermediate product market transactions or close approximations theretoo, any issue that you can, that either arises as or can be reformulated in contracting terms is an issue that you can get some deeper insights by addressing it in transaction cost economizing terms. That isn’t to say that’s the only ones to bring to bear, but it is an instructive ones and that caught me by surprise because I really work with solving this early problem and go out and do other things.  There is more to be uncovered.  Oliver Williamson: Yes! |
| Q33 | The mention of cooperatives in particular, leads into your work. You have studied the use of common-pool resources and the different ways that shared resources can be controlled. And your work comes against a back-drop I understand of a body of thought which was suggesting that outside control was the right way to manage, outside control was the right way to manage shared resources that outside control tended to stop people just overexploiting the resources they had access to. But you suggested that actually it was a little different and that common control of one’s own resources was often a more productive way, a more successful way of controlling their exploitation. |
|  | Elinor Ostrom: A lot of people presumed that it was impossible for those who used a fishery or groundwater basin or a lake or river to self-organize. So self-organization was considered to be impossible and that was why they recommended either the market or the state not well defined not well worked out, but at least an idealized form. A great deal of research had been undertaken by people describing these efforts, but it was people in multiple disciplines about multiple sectors in multiple parts of the world, no cumulation. The presumption was the news headlines would always headline would give sardines were the story in California was a very great tragedy, but that did not mean that resources everywhere were being destroyed, but that’s what people payed attention to. What we have been trying to do is a systematic understanding of when will people engage in the transaction of self-organizing and then sustaining that organization over time. And they use a variety of forms, so sometimes they create a small government, but that’s not ‘the state’. The group of ground water harvesters I looked at in southern California created, there were 11 cities, all sorts of diversity, but they created something called a special district private water association, a variety of other things and did an incredible job without a single external authority doing it for them. It was tough, but they did self-organize and among the things they did was to develop their property right system.  That’s what I did for my dissertation and I did not know I was studying the commons. I was studying, I thought, Schumpeter’s problem of how do entrepreneurs develop and what did they do etc. I titled my dissertation “Public entrepreneurship” because that was the sort of thing that there were several leaders that were incredible in their effort that they put to getting people to meetings, getting them to discuss, getting them to sit down and this problem of conflict was just immense. If you can’t find a forum in which people can get the facts so that they aren’t arguing about facts at the same time they are arguing about what we should do and that makes a huge difference. In any case we now studied this in the experimental lab where we take the simple mathematical theory and have eight people in a lab making decisions that are the ones that come from the theory and indeed, if in a lab no communication is allowed so we have them sitting independently at a computer terminal around and can’t see each other, can’t communicate, they do overharvest. In fact, they overharvest worse than predicted. |
| Q33 | What do you give them to harvest in the lab? |
|  | Elinor Ostrom: We give them a certain set of tokens that they can invest in two options, one of which is the same, mathematically, as the common pool resource. The other would be that they go out and work on job market 8 hours for a set wage and there is an equilibrium concept called ‘the Nash equilibrium’ that you can take with a mathematical formulation and they should, according to the Nash equilibrium, pull out more resources from, because they are not cooperating. If we are all going in there and being aggressive you then harm each other and you, in the long run, are actually harmed because you are doing more than you should. Doing worse than Nash means they were even much more aggressive. We allowed one change in the lab, that people could engage in face to face communication, which in game theory was called ‘cheap talk’ because that process, if one person says ‘Why don’t we do x?’ and the others agreed, there was no external enforcer and without a third person enforcer the court or the state it was considered mere cheap talk. Well, mere cheap talk allowed them to greatly increase, they are a joint pay-off, and to preserve the mathematical resource and we tried a number of other things that we are seeing in the field.  We then studied irrigation systems around the world and compared farmer managed systems where the farmers have got to organize everything and figure out who is going to build and how they are going to build and who is going to do the labor and how they are going to allocate water, but they don’t have much, they do not have very many resources so they build very primitive systems. We have compared those with government systems with fancy concrete and beautiful gates and just fantastic amount of money put in them and they can get more water to the tail-end, they can produce more food and their efficiency is higher than the amount of the cost of the resources going in as opposed to the value of the what they obtained. So, to be more efficient, more equitable and do so with extremely primitive tools, a log that is what diverts the water, mud channels etc., is right amazing and now we are studying forests around the world. |
| Q33 | As the study of these variety of systems has led you, in 1990, to produce a set of guiding principles so to say which suggest how common pool resources should be used? |
|  | Elinor Ostrom: That wasn’t quite my view, I was not doing the ‘should’. I was trying to understand a series of systems that had existed for two hundred or so years and we had good data about them and good information and where there any … I thought we quoted the specific rules they had used in many of these and I could not find a specific rule or set of rules that were always used, so I tried to move up a level and ask what were the generalities across the long-lasting robust systems, I called them design principles, not from the perspective that they were what you should do but if you wanted to be robust you should probably take this seriously but how you would actually apply that would differ from system to system. So, I didn’t mean that the farmers or fishermen or anyone had actually had the principles in mind but they were, maybe we could call it best practices, they worked, they worked over time and then we looked at the failures and they not have the same characteristics. |
| Q49 | But given that one must be able to look around the world and just see innumerable examples of places where it is not working as well as it should be. There must be quite a temptation to start saying ‘should’ because if you have got a set of desired principles which you see working broadly around the place. It must be quite tempting to think. |
|  | Elinor Ostrom: Some people have applied it and I have a little bit of a tension with some of the ways it’s been applied because USAID and all sorts of – SIDA, others – have gone in and said: Now do this. When one of the principles is that they have developed a way to have conflict resolution locally, well sometimes that’s they have a court, in Spain, in eastern Spain there is a court that has been maintained once a week for back to 1500, Valencia. It’s got a very stylized way of handling things that works for them because they have multiple systems, the water master on each of the systems comes in its under court and if there is a conflict on their system they leave the court and testify in front of it, but that’s a particular design that works very well, that when you get experts who know the problem and face with it every day and then you bring a conflict between two neighbors before and you got a decision now. That is conflict resolution within a week of the conflict emerging. Now that won’t work everywhere as a conflict mechanism, so why I am nervous about people who want to just impose it, and that has been happening from time to time, is: What is it you are imposing? Are you running training sessions with people to give them an idea of the array of conflict mechanisms that people have used and then ask them what are some of their traditions, their ways of doing things, then they may be able to develop a conflict mechanism that works for them, but sometimes that’s a traditional chief. Well, that won’t work in LA.  Or it might!  Elinor Ostrom: Well, not usually, might work in Chicago, not my /—/.  Oliver Williamson: If I could make a couple of remarks and relate to, I think both of our work, and one of them is that I think that we have in different ways explained that organization matters and is susceptible to analysis. A lot of people are persuaded that organization matters, but for a economists it’s important to show that its susceptible to analysis and by doing this kind of microanalytic research and establishing the pre-conditions for some of the stuff to go through was really vital. But also, they were both affiliated with new institutional economics and in a way institutional economics operates at a couple of levels, one of which is kind of this organizational governance level, but there is a higher level. It relates to this inability to have a ‘cookie cutter’ that you can just go in and stamp, that is different nation states, they are differently organized and different traditions and customs as Lin says and those are important to how you organize and that should be factored in rather than simple ignored or glossed over. The last thing I would suggest in this, suggest the ambition of this line of research is that right now we all of I think major states, have something that’s akin to a council of economic advisors and I think that they’re well advised is to have such. I think that organizations are sufficiently important and as we make progressive headway with our understanding of it I think that all nation states ought to begin thinking about having a counsel of organizational advisors so there is lots of stuff that’s going on in Washington DC and around the world right now that has huge organizational applications and isn’t being factored in the same systematic way that basic economics is and sometimes that comes back to serious regret. |
| Q14 | I was going to ask about the frustration there must be in, when you study these things and, see people not thinking about it enough and whether that leads you ever to think or maybe having spent all this time studying it, I ought to be in more of a policy position myself and trying to make people listen from a different perspective. Is that a conflict you feel? Do you wish people would listen to you in a different way? |
|  | Oliver Williamson: I think we make headway, and it takes time but actually one of the access points for me was I served as special economic assistant to the head of the antitrust division in 1966–67. These were marvelously capable people who had been running the antitrust division but they were doing in a one sided way, there are essentially using text book economics, micro economics supplemented by a little bit of industrial organization but there was almost no attention given to the possibility that organization was important and that instead if you saw a non-standard practice or organizational form, the immediate suspicion was this has anti-social, anti-competitive intent and it’s easy to run with that ball and get the courts to buy into it. And they did. But the other possibility was that some of these have benefits when they are associated with it and one of the things, they are trying to do is infuse confidence into contractual relations that otherwise would break down and that this is an efficient thing to do. And that point of view has made progressive headway. I don’t say it’s the mainstream now but there is much more interest in being symmetrical rather than one sided in our way of doing anti-trust enforcement, and doing regulation and it should be the case that as we design bureaus that this is actively taken into account and take the problem of homeland security that was put together in wrapped fashion in a way which I think many of us that are closer to the organizational side of things are say this really should have been vetted by people with organizational backgrounds and interest and capabilities.  Elinor Ostrom: I did series of studies that looked at a slightly different question now and public goods and how do you organize local metropolitan areas, because we started with water, but then I turned to policing. The presumption was that in a lot of the literature that fragmentation was evil and that what you needed was a single center. Hierarchy was the model that lot of reform efforts had in their mind, that hierarchy would solve all of the problems of metropolitan areas, and I was deeply suspicious of this having done work on a polycentric industry where there were public and private firms working together at multiple levels. It did not appear to me chaotic but many of my colleagues would put a map in a textbook and it would show some individual cities and then show some other units and the heading would be “The chaotic structure of x metropolitan area” because there were multiple units on the map that was chaos.  We studied policing in a number of smaller studies comparing very similar neighborhoods served by independent small departments versus large. We never found a large department that could out preform a small, partly because of knowledge and understanding the … I rode in a lot of patrol cars and if you were in a small or medium size patrol car the officer would tell you a lot about the neighborhood he was patrolling and really knew it. In large departments to cope with the problem of corruption, which happened in large departments, they rotated the officers and so they might have 90 districts and you rotated every 28 days. Well, you didn’t know much about that district so the knowledge level of the officer and the knowledge that the citizens had of the officers serving them was very low. We also then did a study of 80 metropolitan areas looking at the areas in terms of direct services like patrol being provided by small or medium size versus the problems of crime lab for which there were economies of scale, and the argument had been: the economy had scale for crime labs so consolidate everything. We found that in field in 80 metropolitan areas we found only 84 crime labs, so in the field officers and departments and majors had figured it out and they did all try to have their own crime lab, they frequently contracted with the local hospital that had all the equipment and just had to have some personnel that understood the problems of criminal investigation. In 80 metropolitan areas we found the most efficient and effective had some units at the very top and then had a large number of small to medium size direct producers.  This is an entirely different vision but its consistent with the idea that there isn’t just one way of organizing a corporation and this was there is one way of organizing a metropolitan area and I think our work, it complements each other’s because you can’t make those kinds of presumptions although the transaction costs involved in an interaction between a officer and a citizen who don’t know one and other and the interaction between an officer and a citizen who have seen each other in different times, their faces are familiar, you know a little bit about the background, is entirely different. |
| Q70 | I am getting into hot water by using such terms. People are better able to, if you like, to look after themselves, they either use water in a common way more sensibly than the government might think they might or if they’re local police forces they organize themselves more sensibly again than the government might think they might. |
|  | Elinor Ostrom: But there are settings in which they would just grab like that so you can’t just assume that people under any circumstance will always take into account others and always be good. Humans are neither all angels or all devils and so it is the context in the institutional context in which they find themselves that enable them to have more willingness to use reciprocity to trust one and other and to be in a situation that ‘I can trust you because I think you trust me, and I won’t be sucker’. And one of the problems with the commons is that if I trust everyone else is going to be a good guy and I am cooperative and there are not, I am a sucker and people are worried about being a sucker. |
| Q18 | Are you surprised to find yourself studying what you study? Because you mention that you started out studying entrepreneurship as a graduate student and now you study the commons, when you look back, is it odd that you ended up here? |
|  | Elinor Ostrom: It’s a great honor to be in here, but I didn’t, I was studying the commons from the beginning, but I didn’t know it. I was studying a tough problem that people were trying to solve and entrepreneurship in the public or private sphere has to do with people who are able to understand some of the complexity of a setting and how to organize, so the capacity to organize is crucial and yes, the presumption has been the only way that people organize is inside the market or through a state and that’s what I think has to be, we have to move on to understanding that organization can occur at multiple scales and multiple ways and not always the best, so the mafia is organized. That is not always good. |
| Q2 | But I asked the question as a prelude to asking who you think ought to come into the area now, what are you looking for in terms of new blood to come in and advance the theory further. Is it possible to say? Is there something lacking? |
|  | Oliver Williamson: It is a little bit idiosyncratic but I do think that there is a movement in economics to be more interdisciplinary and in pulling related disciplines together, whereas previously there is and I think there always will be a sense that economics is the prince of the social sciences but that there is a greater appreciation, I think, that there are complex phenomena that if you look at them exclusively through the lens of economics not assisted by any of any of contiguous sciences that you are going to be missing things. One of the things, one of the reasons why I think that I was able to look at some of the issues in anti-trust enforcement differently then was the sort of standard view was that I was part of a really unusual interdisciplinary program in social sciences at Carnegie Tech, then Carnegie Tech, now Carnegie Mellon, in which organization theory was thought to have co-equal status with economics and should both inform and be informed by economics.  There was sort of traffic across this boundary and I do think that having more of our students exposed to the contiguous social sciences and you know, your principal discipline could be political science and you will reach out to economics and that’s been going alright and could be economics and you reach out to political sciences or sociology and that’s been going on. So I think there is, I don’t think everybody by any means should be sort of multi-disciplinary in their work, but I would hope that there will be growing agreements that these sciences have a lot of common ground and that they should work on it in a collaborative way and then often it’s going to be a better more productive. It’s going to be more interesting as a matter research and it’s going to be more productive for the common well.  Elinor Ostrom: I also think interdisciplinarity is a very important. Vincent Ostrom and I established a center called a workshop in political theory and policy analysis back in 1973–74 and we have always had multiple disciplines around the table and that’s just been my life. We now are reaching out more to ecologists because of, if you’re studying forest, you have to learn a little bit more about the ecology of forest because again rules that work well in a deciduous forest don’t work well in a tropical wet forest and trying to understand the ecological foundations is very very tricky and I am having learned a lot of new concepts and new terms.  Are you specializing in forest studies?  Elinor Ostrom: No, it happens to be the applied area I am working in now heavily. I have a couple of articles I am working on, irrigation, so we have a large database and we have a book coming out in another 6 months or so assuming all of this. But I have just finished a book that I am very very pleased about with the title “Working together – collective choice, the commons and multiple methods in practice” and the thesis of it is besides interdisciplinary is multiple methods. Sometimes we have people only do a mathematical model or only do only case studies or only do large N and they criticize everything else and the problem is that we need to be able, either if we can’t ourselves do the model, that’s foolish but we need to be aware of the advantages and know enough about some of the methods that we can work with an expert in a method and then bring some of our knowledge from a different set of methods together. I love experimental work, but it doesn’t have the richness that I have when I study forests in the field. I love both of them as ways of different ways of getting insights and sometimes being in the field leads back to the lab. In particular I kept seeing people monitoring and sanctioning each other in the field, amazing times and so I came back from a particular vivid occasion in Nepal and said to my colleague Roy Gardner who is a game theorist and Jimmy Walker who is an experimentalist: Let’s please, let’s do a formal game involving people paying their own cost to sanction others, because in the field if they go this was an instance where someone had dug into an irrigation and so several of them went running down the hill and yelling and screaming and others starting patching it immediately and I mean the energy they put in, they didn’t not think about it. There was no rational calculation about this, they just did it.  Well again and the game theory prediction is they wouldn’t, then we could put it in the lab with a simple game we used earlier, but now changing one attribute and giving them a chance to pay a fee to find someone and they did it even though it’s predicted they won’t and if we gave them eventually the choice of what kind of sanctioning mechanism they had and then allowed them to use it, they got to about 94% of optimal. And here is having the game theory model great because we knew what optimal was and we could put it in the lab and figure out what happened, but that was field lab and now on our forest studies because of our earlier findings we asked quite about monitoring and sanctioning. We are finding that whether people who use a forest actually monitor each other to be more important then who owns. Government ownership, private ownership, community organization – all of those are important but if they, the formal rules are there, and the users don’t see a future and don’t see that they are involved and don’t take an interest and don’t monitor – forest goes down. |
| Q75 | Forgive this last question, but since you both work on organization theory and the right use of common resources, I wonder whether you are organized people yourselves or whether you are good at organizing things like a complex family which are five children I believe. |
|  | Oliver Williamson: That’s true! Well, I think of myself as being disciplined and I guess because I enjoy my work so much I spend a lot of time struggling with new and different issues trying to understand them and trying to see if I can find ways to fold them in and whether or not they have public policy ramifications and the like. It’s been a joyness of work and see things make headways and have good students come through. And its obvious from what Lin says that she’s just really wrapped up in this stuff, its easy to. I wouldn’t call myself an excellent organizer but then the less that it does require keeping a lot of disparate ideas in mind and finding ways to pull them together. There is a certain amount of background organization that goes on. Sometimes actually to my surprise, this has been true more recently than it was when I was a younger fellow, but I will start dreaming about some of this stuff and thinking about what I should be doing and if the ideas are good enough. It doesn’t always turn out that they are, but I get up and jot little notes down and then get back to sleep rather than struggle in the middle of the night and so I have got some subconscious work in front of me, too.  That quite an advantage.  Elinor Ostrom: We have an unusual center that Vincent and I organized long ago, and we had an opportunity both to be in Germany at Bielefeld at the interdisciplinary center and saw the advantage of working with young postdocs as opposed to just graduate students and so we innovated and developed a postdoctoral program, modest in size. That has been a very successful way of both organizing the research side and the teaching side because you have graduate students who are, some of them state of the art on their new tools, but then they are able to talk to young to middle faculty who are not their supervisors but are in the same building and going to the same seminars and discussing when they get and sometimes you know its two-way learning. Then to have interdisciplinary teams and what we have learned is we have had to learn new rules and ways of organizing as we have run into things over time and so adjustability and new circumstances lead you to have to do things slightly differently. We have had the good fortune of a wonderful set of colleagues from all over the world and I think one of the things that will be happening, as far as new entrance, is that we will have more scholars from the developing world and from Asia and that will be contributing to our joint knowledge because they have different experiences and are bringing in new ideas and new ways of thinking about it so I see that as part of the future.  That seems like a marvelous way.  Oliver Williamson: Do you mind if I come back to the organizational issue. One of the things that I was wise about doing is I never took seriously and followed up on any opportunities on to be a Dean.  Elinor Ostrom: Yes, same.  Oliver Williamson: And my experience as department chair is that this was mainly discouraging. There are several things that I did do that did have organizational ramifications and one was when I was at Penn then at Yale and then at University of California Berkeley. I helped to organize workshop and obviously the workshop has been really important to Lin’s work and it’s been important to mine and having colleagues around it interacting and in a constructive way. There is another thing I did, got into it a sort of a backdoor of being a member of the editorial board of the then *Bell Journal of Economics*, now the *RAND Journal of Economics* and so I was going to become editor of it and as editor you have a lot of opportunity to move the field around and I have a bunch of really exceptional associate editors who shared a lot of my views in this, probably because I recruited them, but I think that was very satisfying experience. Then when I went to Yale, Yale Law School was interested in having a journal that was edited by the faculty rather than by students, as most law journals are, and we organized a journal of law, economics and organization and I am proud of the accomplishments of that journal. If I put my heart into it I guess I can organize, but taking on jobs like Deans which are important requires you to be a lot more tolerant than I am. |
| ID | 0834 |
| Biographical | Paul Krugman’s is a lone voice, telling things as they are and debunking Washington policies that are neither compassionate nor conservative.” – Paul Samuelson  Paul Krugman has at least three jobs: he is professor of economics and international affairs at Princeton University, Centenary Professor at the London School of Economics, and perhaps his best-known job an op-ed columnist for *The New York Times*. In recognition of his influence, *The Washington Monthly* called him the most important political columnist in America.”  In addition, Krugman’s reputation extends well beyond the U.S. *The Asia Times* recently called him the Mick Jagger of political/economic punditry.” *The Economist* said he is the most celebrated economist of his generation.” And, recently Krugman received what is often called the European Pulitzer Prize, the Asturias Award given by the King of Spain.  Krugman is the author or editor of 20 books and more than 200 professional journal articles, many of them on international trade and finance. In recognition of his work, he received the John Bates Clark Medal from the American Economic Association, an award given every two years to the top economist under the age of 40.  For the past 20 years, Krugman has written extensively for non-economists, including a monthly column, “The Dismal Science,” for the on-line magazine *Slate*. He has also been a columnist for *Fortune* and has published articles in *The New Republic*, *Foreign Policy*, *Newsweek* and *The New York Times Magazine*, before joining *The New York Times*.  Prior to his appointment at Princeton, Krugman served on the faculty of MIT; his last post was Ford International Professor of Economics. He also taught at Yale and Stanford Universities, and prior to that he was the senior international economist for the President’s Council of Economic Advisers, under Ronald Reagan. (Yes, he served under a conservative President.)  He is a Fellow of the Econometric Society, a Research Associate of the National Bureau of Economic Research and a member of the Group of Thirty. He has served as a consultant to the Federal Reserve Bank of New York, the World Bank, the International Monetary Fund, the United Nations, as well as to a number of countries including Portugal and the Philippines.  His most recent book is *The Conscience of a Liberal*. His previous work, *The Great Unraveling*, was highly praised and became a *New York Times* bestseller in both hardcover and paperback. Professor Krugman and his wife, Robin Wells, have recently collaborated on two college textbooks – *Microeconomics* published in October 2004, and *Macroeconomics* published in the September 2005. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0834=PK  [Paul Krugman] Hello.  [Adam Smith] Hello. This is Adam Smith from the Nobel Foundation web site in Stockholm. Is that Paul Krugman?  [PK] Yes it is.  [AS] Congratulations on the award. Quite a time to be awarded the Prize for Economic Sciences.  [PK] Yes. I’m a little, in a way I’m feeling that I don’t have time for this. It’s rather bizarre.  [AS] You’re in Washington at the moment for meetings, is that correct?  [PK] Yes, I am. For a Group of Thirty Meeting, which is a group of people I belong to, central bankers and so forth, and we’re having a meeting associated with the World Bank/IMF meetings right now. And we’re supposed to be hearing from [Ben Bernanke](https://www.nobelprize.org/prizes/economic-sciences/2022/bernanke/facts/) and Jean-Claude Trichet this morning, although I suspect that Trichet, who was back in Europe yesterday, won’t be there. And I thought that was going to be the exciting thing today.  [AS] A critical time, so, yes. You’re very well-known, certainly in the US, as a columnist and a blogger, and a journalist, and yet the award has been made for work that you did in previous decades.  [PK] Yeah.  [AS] I wanted to focus on that initially and then talk about the more recent things if I may. So, the Committee cites your new trade theory, which you developed in your papers in 1979 and 1980, and that’s essentially describing how trade functions in a world where countries produce and use the same goods. Is that correct?  [PK] Countries produce similar goods. I mean, it’s really a story about countries that are not very different in terms of their technology, in terms of their resources, but that nonetheless end up specializing on different goods that may be related but are not quite the same, and do that to take advantage of the advantages of large-scale production. So the crucial thing really is the similarity of the countries. It’s an explanation of why countries might trade even if they have the same climate, and the same resources, and the same technology.  [AS] And what does a theoretical underpinning such as the one you developed enable you to do?  [PK] Well, it actually – first and foremost it allows you to think clearly. One can describe – it’s kind of an odd thing, but right now I can explain in what sounds like plain English the essentials of the theory. But I could not, in fact, do that before having done the models. It required the math to get to the plain English. So there’s first of all that. There’s an enormous clarification that takes place. And secondly this kind of thing can be, and has been used, as a basis for empirical work. Once you have the clear statement of how the pieces fit together you can apply it to numbers, you can use it to try to assess the welfare impact of different trade policies, so all of this is necessary. But in the first stage the issue is that of, how do we think about this thing clearly?  [AS] And the second piece of work that was cited was your *core-periphery model*, which seeks to explain why production is becoming increasingly concentrated in certain places. Do you think that increased urbanization is a necessary consequence of increasing globalization?  [PK] Not necessarily, although it does work in some circumstances. The geography work all has the implication that there are forces both pulling things together and pushing things apart, which you can put some analytics and ultimately some numbers to. So, unfortunately they sound the same: centripetal and centrifugal forces. So there’s always a tension. And actually changes in the world tend to affect both of those. But what you do get is some understanding of how we can have gotten to this extremely unequal distribution of population across the surface of the world. That the reason why 80 million people live in a fairly narrow corridor along the East coast of the United States is not that there’s something especially favourable about the geography, but it’s simply the agglomeration force. It’s essentially each of those 80 million people is there because the other 80 million people are also there.  [AS] Yes, yes. Nicely put. Turning to your journalism, do you see that as a natural consequence of your work in academia, this move towards a more, sort of, public outreach of what you do?  [PK] To some extent it was. I mean, I do believe that the task of boiling down an intellectual problem to its essence, which is a lot of what’s involved in modelling, and the task of figuring out how to talk about some fairly complex problem in fairly simple natural language, are related. I always felt that what I do when I try to explain, let’s say explain the financial crisis in 800 words, and what I do when I try to model the financial crisis in a half-dozen equations, are very much the same kind of effort. That said, I was doing a fair bit of that kind of translation before I went to work for *The New York Times*, and that continues to be one of the things I do at *The Times*. I had not anticipated that I would end up in such a politically charged environment, where I feel I need to do more than explain, but it did seem natural. I went from writing little models to writing little articles. It seems like a quite natural transition.  [AS] The physicist Ernest Rutherford I think always said that you shouldn’t be doing it if you couldn’t explain what you were doing to your tobacconist, so it’s the same sort of idea I suppose.  [PK] Yeah, I mean there’s Alfred Marshall’s thing about, he was arguing that even for professional work, after you’d figured the thing out you should burn the equations, which I think don’t believe, but you should be able to explain what’s going on if at all possible without the apparatus.  [AS] Yes. You are very politically involved now, or at least you take a political stance on things. Is that also a necessary consequence of trying to explain things?  [PK] I don’t know. To some extent. Let’s put it this way, I was a very early critic of the Bush administration because I believed they were being dishonest, and the reason I reached that conclusion long before many other people was actually because it seemed obvious to me that they were lying about budget arithmetic. So in some sense my economics training was playing a role there. But, obviously, I’ve gone somewhat beyond my role as an economist in the column but, hey, economists are people too, and are citizens too, and have political opinions.  [AS] Yes. Okay. And then my last question. The media interest that will surround this award will no doubt focus to a large extent on your work as a columnist and your political views. Any thoughts on that?  [PK] Hard to say. My views as a columnist are less controversial than they were a few years ago. Not because I’ve changed, but because a lot of, certainly of the United States, has come around to my way of thinking. So, when I was being critical of Bush and he had an 80% approval rating, there might have been an enormous firestorm about all of this, but now that, I’m still critical of Bush but now he has a 22% approval rating, I don’t think it’s going to be as much of an issue. I think it may surprise some people who know me only as a columnist. But I don’t think it’s … I’m curious, I don’t know how this will play out.  [AS] It will be interesting to watch it over the days ahead. When you come to Stockholm in December to receive your award we have the chance to interview you at greater length, so I look forward to that greatly.  [PK] Great. Okay.  [AS] Thank you very much and congratulations again.  [PK] Thank you.  [AS] Bye bye. |
| Interview |  |
| Q7 | Yes and how big a crisis do you feel it’s going to be? |
|  | Oh, it’s awesome, I mean without an active policy response, it would be another great depression, it really is a financial crisis rivalling in scale, the bank runs that were really at the core of what made the Great Depression so great and so. No, this is like nothing that has happened in my lifetime. |
| Q6 | Do you find yourself being talked to more because of the award of the Nobel Prize in October, has it increased the focus on you, do you think? |
|  | Some, but I was pretty busy anyway. I’m probably getting, you might say, a higher quality of TV show, but this was one of the lines of work I have had is in fact financial crises. I write for the New York Times so I’m something of a public figure anyway and this crisis is very much up, you know we can say it’s up my alley, my kind of thing. The kind of thing that I used to have to fly off to Jakarta or Buenos Aires to see but now it’s in New York, so I’m in the middle of it anyway. |
| Q27 | Does it make it more difficult to report on it because you have this journalistic side to you as well as at the same time trying to suggest solutions to it? |
|  | I mean, it’s a special role. Actually I’m an opinion journalist so I’m expected to do stuff and in fact, I’m basically trying to work on multiple …, I write the 800 words layman’s pieces for the New York Times, I write longer pieces, some are posted on my blogs, various sorts of things that are more addressed to the commons and then there’s an enormous amount of off the record discussion of various types going on, this crisis isn’t going to … if we fail to deal with it, it’s not going to be because of lack of intelligent and very animated discussion. |
| Q27 | We’ll come back to the op-ed piece and the journalism later and of course you’re known to many as an economics and political commentator, especially through the New York Times pieces, but this prize is actually for your development of economic models, to explain international trade and economic geography. |
|  | That’s right, until the age of 40 I was very much a pure academic and if anyone remembers 50 years from now, what they’ll remember is the work on trade and geography about some big questions, enduring questions of economics, but you know academics in their mid 50’s in economics quite often are doing other things, you know, some of my my cohorts in graduate school included people like Jeff Sachs and Larry Summers who seem to have done a few things beyond their economic modelling so it’s not that unusual to be doing something. But yes I tell people the prize is not about the columns, the prize is about papers that you can’t read.  Yes, you refer to them as Greek letter papers.  Paul Krugman: That’s right, you know I’ve spent a good part of my life using you know … of substitution, utility functions and iceberg transportation costs and true price into season, all the various tools that one uses to simplify these issues on trade and geography down to something where you can actually think about them clearly and I’m not going to put that in the daily newspaper but that’s where you start. |
| Q1 | What turned you on to be an economist in the first place? |
|  | Oh, it’s an embarrassing story but I’ve told it publicly a couple of times. I was an avid Science Fiction reader when I was a teenager and there is the classic set of novels by Isaac Asimov, the Foundation novels, which are about how a group of social scientists save galactic civilisation through their understanding of the laws that determine the behaviour of societies and I wanted to be one of those guys and the closest you can get at this point I’m afraid is being an economist.  Do you still believe that economists have that role?  Paul Krugman: In the novels these people are able to predict with high accuracy what’s going to happen and find the precise intervention that saves civilisation. Economics doesn’t work that good, but no, there’s a tremendous amount of understanding that comes from economic models and sometimes that understanding can be the salvation of the economy. I believe that we’re living in one of those times right now. If we had only the level of economic knowledge that the world had in 1929, I believe we would have another Great Depression. The reason to believe that that won’t happen is that we think we understand this thing, at least somewhat better than our grandfathers did. |
| Q18 | This is a perfect segway because I wanted to ask you about the development of models. It was in 1979 that you published your new trade theory and that is a model explaining why similar countries trade in similar goods. Now one can express the model in the theory in fairly sort of self-evident terms, simple language and it all sounds as if it makes sense and yet one needs an atheoretical underpinning based on proper economic science in order to really understand. |
|  | Yes, you know it’s an interesting thing because the plain English came later, it’s not as if there was an intuitive story that was widely understood and what I did was find a way to mathematically model it. What actually happened was that I and a number of other people started using these models to try and model something, it was a little bit unclear exactly what it was we were trying to get at and as the models became clearer, they crystallised an intuition which you could then say, of course. It’s a little bit like the old joke about the Professor at the blackboard saying, obviously then … then he stops and pauses for 20 minutes and says I’m right, it was obvious, and these simple intuitive stories that we can now tell, I can now explain in a few hundred words in plain English what drives intra-industry trade among advanced countries. But no-one was doing that before, it really took the modelling. As it happens, having been there, I remember how hard it was to try and figure out which way the strategic simplifications needed to go to make this thing coherent and then afterwards it seems, well of course it’s obvious but there’s very much an after the fact interpretation. |
| Q22 | Do you think that that’s perhaps a sort of peculiarity of economic sciences that in other sciences, I don’t know, in biology or chemistry, even when you try and explain the theory in simple language, the language we’re using is a strange language, a language that’s particular to the subject whereas economics, when one explains that you use the English or whatever language, that everybody understands and so everybody feels they ought to be able to sort of get in there and massage it? |
|  | It’s not even true of all economics, I mean and it’s not necessarily that we’re talking about the highly technical fields. I think, as it happens, the areas in which I worked, trade, economic geography, the concepts are for the most part pretty intuitive although you know there’s quite a lot of stuff that pops up when you work through it that is not part of the intuition. I’m sacrificing quite a lot to explain it just in plain English but there are other things, Keynesian macroeconomics which is critical right now. Although it’s quite simple in a way, also there’s a subtlety about the concept that really does require, if not math, at least some very, very close logic and what reveals that fact is that so many people to this day just don’t get it or actively deny it, say this can’t be true and I think that, well there’s some of that in a lot of economics but it’s not as if all of it can be reduced to plain English. |
| Q18 | No, but I’m probably doing it clumsily, but what I was trying to get to was the idea that, because there is some plain English about it, people feel they ought to be able to sort of get to it themselves even if they haven’t got economics training, whereas they just don’t touch chemistry because it’s too far outside and that lays perhaps Economists open to people feeling, oh I could have understood that, I could have done that. |
|  | Yes and also of course economics … and one of the great definitions by Alfred Marshall was that economics is about the ordinary business of life. Economics is about getting and spending and we’re engaged in getting and spending so we all think we know about it and you know there’s a constant belief that you go to a great businessman for wisdom about the economy and that often doesn’t work, that what you need to know to run a business and what you need to understand to make good economic policy are not all the same thing, but people have a sense that they know what economics is about, people have strong prejudices and you know it’s not that easy. My great ideal among the Economists is John Maynard Keynes and he had a spectacularly accurate essay at the beginning of the Great Depression, *The Great Slump of 1930* and a little bit of temper shows, that something he says, “economics, don’t know when we’ll believe it, it’s a difficult and technical subject” and yes, you know if you very carefully you can manage to write to make it seem clear, but it is actually fairly difficult and technical when all is said and done. |
| Q23 | What makes a good modeller in economics? |
|  | Oh, you know there are many different ways. One of the things you learn, I think this is true of physical sciences as well, is that there are many different personality types who work in distinct ways, so there are people in my style and I’m a ruthless simplifier, I you know pair away everything, I try to make the math disappear and it never quite does but I’m a little model guy, I say here’s this huge complex subject, there’s got to be some little model that we get to the essence of it. Sometimes there isn’t, there are also people who are generalisers, who will look for some general theorems, general ways that you can think about a large subject. There are people who are magnificently good at sifting through large amounts of data, finding ways to process that data, to extract conclusions. You know there are very many different personalities, there’s a certain style kind of identified actually with MIT which is where I did my graduate work which is the little model that cuts through to the essence of a complex problem but there are many different ways you can do that. I think what it does take though is there’s some requirement that you be able to step back and see things differently, say that you know the way that everyone is talking about something is not actually maybe the way we should be thinking about it. |
| Q23 | Yes and that ability to take a sideways look at things is something that you very much used in your journalistic career. |
|  | Yes, there is certainly some continuity, being able to say hey, the conventional wisdom about, well you name it, but certainly about economics, but the ability to say that, hey, you know maybe these electricity shortages in California, there’s something funny here, there seem to be a lot of idle capacity, let’s think this through and then saying, you know circumstantial evidence strongly suggests that this market has been rigged and then later we actually found the tapes but that’s very much the same sort of thing, saying you know trade between industrial countries is not about comparative advantage, there is an attitude of mind. |
| Q18 | So the second model that the Prize Committee have quoted is your core periphery model which was also mentioned in 1979 and then further developed in 1991. |
|  | There is just a hint of it in 1979. Sometimes I look back at the 1979 paper and say, you know there seemed to be premonitions of lots of later stuff in there. The core periphery model, that’s the love of my life in terms of academics because being able to say that increasing returns makes stuff clump together, that’s not so hard, but being able to get this tension between the forces that pull things together and the forces that pull them apart and that made me very happy when it fell together because it was another one of those things where it sounds very simple but it actually took years of false starts before getting to the point where it actually suddenly cohered. |
| Q18 | Yes because it sounds as if you were just describing the process of urbanisation which we’re sort of witnessing around us. |
|  | That’s right, but no in fact one of the slanders against Economists, people used to say you know this stuff is just circular, you say that things agglomerate because of the agglomeration economies which was kind of the way that people talked about it and say no, actually we can derive those agglomeration economies, they’re about market size but to do it in such a way that you can say, but there are also centripetal forces that gives you some advantage to moving away from where … and then some kind of boundary condition that determines which of those prevails. That was not the way anybody was thinking about it until this new economic geography came along. |
| Q70 | Right and what sort of predictive capability does the core periphery model give you? |
|  | A lot of it is more sort of retro-casting. I mean the model has a story about what would happen and there are three factors that would push you across that boundary to where the centripetal outweigh the centrifugal forces which are reduces transport costs, increasing economies of scale, growing share of non-resource based industries and then you look back and you say, we can see that phase transition in the United States about 1855 when all of a sudden industry stops following the frontier west, instead sticks in the manufacturing belt, so you get that. Initially the core periphery model per se but models derived from it have I think had some successful predictions and one of the things early in the process of European integration, just after I come out with the initial model, began to say, you know the deep integration that’s taking place in Europe, if we believe these sorts of models should actually lead European economies to become more different from each other, in a lot of ways increasing specialisation and low and behold that has happened. So I think there are some successful predictions out of the approach. |
| Q70 | Right and interventions presumably also that one could think of? |
|  | That’s actually less clear. I mean the very nature of the model with the tension between reasons to disperse and reasons to agglomerate also suggest that in principle this is not an invisible hand situation where the market gets it right but it can get it wrong in more ways than one and trying to figure out which way you want to push the stuff is not so easy. So actually, yes I mean there are implications about policy, it certainly means that you should be looking into it but it’s not a simple, you know let’s push everybody into the cities or let’s put everybody into rural development programmes, it’s actually turned out to be quite hard to come up with what the policy implications are. |
| Q71 | Thank you. Let’s turn from model building to journalism. During the 1990s you became active in writing for a number of different organs and then in 1999 you signed a deal with New York Times for a twice weekly paper column. |
|  | Yes. My theory about the time commitment was *The Times* would be very newly and exclusive commitment that no business consulting, none of the things that middle aged academics do because those all pose conflicts of interest issues, so actually *The Times* would in effect give me an external backbone to say no to people who wanted me to you know fly off to Singapore and talk to a bunch of Investment Bankers and it’s largely worked out that way, I’m not sure that the drain on my time has been that much more than the other stuff that I might have been doing. But it was different, I think I’m not the first Economist to write for newspapers but the first to take on that kind of regular gig, I think is new. |
| Q18 | Absolutely, and your approach to this production is that you base it around the numbers, your reports are basically based on analysis of the numbers that other people aren’t doing. |
|  | That’s right, I mean obviously there’s been a political component and there have been issues like the War in Iraq where I you know took a non mainstream point of view but in much of it, it’s looking at the numbers, looking at the logic. It gets you a long way. Most Journalists, even in the business area, are not really that comfortable with thinking these things through, so I think at least different enough to be a useful voice on the scene. |
| Q18 | But I mean there must be quite a lot of people who are qualified to look at the numbers but as you say, not that many people who are in the position to have a wide audience to look at what they’re saying. |
|  | That’s right and so to some extent I think there are probably other Economists who could have done this job as well and … service translator. I mean on many issues there is in fact some academic literature and some academic research going on but it’s not known, it’s not getting reported and I can read the stuff and then translate it into English which is not that easy a job actually.  It is a huge commitment of time and also you need to produce, you need to produce quality material at such a pace. A lot of academics like to talk about how even you know the three year grant deadlines and the five year grant deadlines they work on are quite limiting to their ability to think. So having to produce every few days something must be …  Paul Krugman: My running explanation has been that you know your twice weekly column doesn’t have to be Nobel quality research and in fact the Nobel Committee didn’t consider that right. No but it doesn’t have to be something that would startle a trained Economist, it has to be something that is news to an intelligent but non-technical audience, so it’s not that hard and you know if there was a shortage of stuff happening to write about, then I suppose it might be quite difficult to keep the flow going but in these past eight years there has never been a shortage of stuff happening.  But it needs quite a lot of background, you need to be sure of yourself. One of the things that the analysis of the numbers led you to do was to criticise the Bush Administration earlier than a lot of people were doing and so you need to be pretty sure of where you are.  Paul Krugman: Right, but you work on it for a while, I mean it took me about four months during the 2000 campaign before I was willing to come out and say these people are being dishonest, they’re lying about their own numbers. So you get familiar with it and then you work forward and also of course it’s journalism so if you should happen to be wrong, it’s going to happen, if you’re never wrong on these things then you’re not taking enough changes. It’s not like academic research. |
| Q44 | Yes. In 2007 you introduced a blog to go alongside your columns. I imagine you’re the first Nobel Laureate to have a regular blog? |
|  | I wonder if that’s true, it might be. But you know in some ways I was a proto blogger. Back in the late 1990s I was posting many pieces about the financial crises on my personal web page so in some ways I was a blogger before there were blogs, but then I went away from that because I was writing a regular piece for *The Times* but in 2007 it became clear to me that there were two kinds of things I wanted to do, fast reaction pieces and things that were more technical than belonged in the newspaper proper, so the blog is a perfect answer to that, a fair number of my posts on the blog I actually just include in parentheses (Wonkish) as a warning that you know my general readers, you really don’t want to read this.  Right and does the blog have a very large audience?  Paul Krugman: I think it does, I haven’t checked lately but I believe I’m you know in the top 5 most read economics blogs or something like that but mostly you know people are aware of it, people in academics but also in the policy world say well you know you have this blog post and what does that mean for what we ought to be doing in this legislation? |
| Q27 | I suppose one further step might be, now that you’re writing so much about politics, to take political office. I know that during the Clinton Administration you were approached about a job in the Administration. |
|  | Not really exactly. But I was actually in the Reagan Administration which is hard to believe but I was, you know non-political level, I was on the …, the Council of Economic Advisors where I was the Senior International Economist, the Senior Domestic Economist was Larry Summers, don’t know what happened to him. Actually, partly because of that I know what the policy world is like and I don’t think I belong there. I think actually I’m a terrible administrator first of all, so you don’t want me in anything that requires administration and I am not a very good you know negotiator. I like to think I’m a good analyst but I don’t think I’m a good bureaucrat of any kind and you know I might think differently if I wasn’t at *The Times* but as it is, I have a mouthpiece, people are listening, I probably can have as much influence let’s say on the shape of this coming economic stimulus package from where I am as I could if I were you know the third ranking member of the Obama economics team, something like that. So I think it’s probably as good a position as any.  Okay and so that sort of makes null and void my next question which was going to be, you intend to continue writing for *The Times* for the foreseeable future?  Paul Krugman: I don’t know about forever but yes. I have to say it’s become in recent months more of an economics column and less political than it had been, partly because we won’t have George Bush to kick around anymore and partly because the economic situation is so dire and in a strange way it doesn’t make the actual workload less but it makes the emotional wear and tear less, so I’m actually finding that I’m enjoying the column more than probably I have at any point in the past eight years.  Because you’ve sort of stood a lot of comment from outside.  Paul Krugman: Well no, certainly there was a pretty fair number of accusations of treason and so on back when I was being critical of the President but now I really feel that you know there was a long period when I felt like I was the voice crying in the wilderness and you know how can you believe these people, they’re trying to sell you on a War and the evidence isn’t there. Now I’m in a position of saying that we have this crisis, here are some things we ought to do and people are listening. Whether they’ll do what I say I don’t know but it’s much less of a feeling of the sort of panic that no-one was willing to look at what was obvious. We’re now having a real discussion, even if my view doesn’t prevail, it’s a real discussion. |
| Q17 | There’s just one other side of your writing I wanted to ask about which are your books because you produce quite a lot of books, some of them are reprints of the columns but others are written as standalone things. What part do the books play in your sort of outreach? |
|  | I’ve had two kinds of books, I’ve had what amount to professional monographs which are useful because sometimes you have a longer story to tell than you can do in articles and so *Market Structure and Foreign Trade* with Elhanan Helpman back was sort of putting together, integrating the new trade theory work, or geography of trade which was the economicgeography in a longer format, but then the … is you have a longer argument. You know *The Times* is a wonderful place, lots of people read it but it’s 800 words, you know and those 800 words have to be comprehensible, can’t be too dense, has to start by telling people what you’re going to tell them, then you tell them, then you tell them what you told them and with all of that there’s room for only a core of an argument but no sustained development of a logic and even longer pieces don’t do that, so there’s nothing quite like being able to have a 250-300 page book which actually lays out some longer case.  But obviously very readable because they get to the top of the bestseller lists, people like them, yes.  Paul Krugman: Well yes. Again, I think I write these in English. That didn’t come automatically, I actually started writing newspaper columns way back in 1987 I think for *The Los Angeles Times* and they were terrible and I’ve gradually learned how to do this.  For the last question I want to take you just a little further back. In 1978 you wrote a paper called “The Theory of Interstellar Trade”.  Paul Krugman: Yes, which is now finally going to be published in *Economic Inquiry*.  Is it? … and to just encapsulate the argument there.  Paul Krugman: Oh yes. You know I was having those Assistant Professor woes, felling neglected, whatever, so for therapy I wrote this and the idea was that, if you’re shipping goods for long periods over very long distances or certainly in the 18th Century we shipped goods, the time in transit was an important part of the cost, it’s even true now to some extent. The interest costs on stuff on its way on a slow boat from China is an important part of the transport costs. So well, this will certainly be true for interstellar trade where the voyages are very long but the time in transit depends upon the velocity of the observer, so once we take relativistic effects into account, which time shall we be using for the interest costs?  So this is assuming that we achieve speeds of travel that are near the speed of light?  Paul Krugman: That’s right and so you know obviously it was just fun, so I got to put a diagram in Minkowski space time which has an imaginary time axis and so the diagram is blank because if an axis is imaginary, the whole diagram must be imaginary, that sort of thing, so I was having a good time. |
| ID | 0835 |
| Biographical | This text is adapted from “Leonid Hurwicz’s Game”, by Ann Bauer. Reprinted with permission from the author and Twin Cities Business magazine.  Professor Hurwicz says that he is simply a product of his history. He knows from experience how circumstances change when people don’t abide by the rules of the game.  “I cannot tell you my life story and what I did without telling you about politics as well,” Hurwicz says.  Leonid Hurwicz was born in Moscow on August 21, 1917 after the Kirensky revolution but before the October Bolshevik revolution. In early 1919, he and his family returned to Warsaw − his father’s home; his mother was also Polish − after the Communists came to power in Russia. “My father was convinced, I think rightly, that if he stayed in Russia, he would have trouble with Lenin,” Hurwicz says. “Of course, that’s not my memory; I was 14 months old. But I know we traveled in various imperfect ways, such as horse-drawn wagon, to arrive in Poland.”  His father was a lawyer, with a degree from the Sorbonne in Paris. In Warsaw a five-year internship was required to practice law, so he taught history while completing that. Hurwicz’s mother was a teacher as well, but after the move she stayed home with him and his younger brother, giving them lessons in reading, writing, and arithmetic. Hurwicz didn’t begin formal schooling until the age of nine, when he started at a private institution attended and staffed mostly by Jews.  It was a time of pogroms throughout Europe. Though he and his family were not swept up in anything so frightening, Hurwicz does recall strange, random attacks. One year, the university in Warsaw decided the Jewish community had not contributed its share of corpses to the medical school. When a group tried to force Jewish students to sit in a segregated section of their classrooms, the university rector failed to intervene. The Jewish students, including Leo, stood in the back of the room for the entire year in protest. The protest succeeded and the following year Jewish students were seated again.  “I experienced harassment during this period.” Hurwicz pauses, as if remembering. “But I never was beaten or attacked personally. And with my professors, I felt no discrimination.”  He graduated from Warsaw University in 1938 with a degree in law, originally intending to follow in his father’s footsteps. However, beginning in his second year of law school, he’d taken some obligatory economics courses and became more interested in this discipline than any other.  “I had the belief that many troubles you could observe on the European continent were due to politicians not understanding economic phenomena,” Hurwicz says. “Even if they had good intentions, they didn’t have the skills to solve problems.”  Hitler was in power in Germany and Jews all across Eastern Europe were on guard − made to feel like intruders in their own countries, hearing hideous rumors about persecution they could not fathom but nevertheless feared. Hurwicz’s father, sensing the changing tides, suggested that his older son apply to the London School of Economics rather than set up a law practice at home.  Leo went to London in the fall of ’38, but in the Spring of ’39 the British refused to extend his visa. So Hurwicz went to France and then Switzerland on a transit visa. He arrived in Bern in August of 1939 − less than a week before Germany attacked and occupied western Poland, including Warsaw. Hurwicz heard the news, but didn’t know that his parents and brother had managed to escape to Russian occupied eastern Poland. By the spring of 1940, they had been arrested and taken to a labor camp in Arctic European Russia. He spent months in Geneva, enrolled part time as a student, living off what little money he had left, and hoping for news of his family.  Hurwicz contacted his cousins in Chicago, whom his parents had told him would help if he needed to leave. In 1940, 23 years old and all alone in the world but for some cousins he’d never met who wired money and an address, Hurwicz booked passage on an Italian boat.  “In a month − no less − Mussolini joined Hitler,” Hurwicz recalls. His ticket was now worthless because no Italian ship could enter American ports. “That was bad luck for me, because there was the question of how to get my money back. Also, even if I had money, how did I travel from Switzerland to some city which was not on Hitler’s side and could get me to America?”  He found a way. An airline opened briefly between Switzerland and Barcelona, both neutral cities. Hurwicz was able to fly to Spain and take a train to Madrid, then Lisbon. He spent almost two months in the Portuguese city of Estoril which was “like Monte Carlo,” tutoring the children of wealthy Spanish and French families vacationing there. Then Hurwicz got word of a Greek boat, the Nea Hellas, sailing for New Jersey.  Leo needed help getting his money back from the Italian company, so he went to the harbormaster who said “What can I do?”. Leo suggested that the harbormaster could threaten to take the company’s license away, and that if he didn’t succeed, he would have Hurwicz on his hands for the duration of the war. The harbormaster followed Leo’s suggestion. With a refund and the little he had earned tutoring, Hurwicz booked passage on the Greek boat.  In Chicago, he lived with his cousins deep in the Polish section of town − sleeping on their couch and auditing courses at the University of Chicago with the famous economist Ludwig von Mises. Finally, he got an offer of a job at the Massachusetts Institute of Technology “from someone who is very well known: [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/). The job was as a teaching and research assistant for only one semester − a term no self-respecting graduate student would accept,” Hurwicz says. “But I had no other offers. In fact, it was a miracle I had this one.”  He moved to Massachusetts and began working under Samuelson, who would win the Memorial Prize for economics in 1970. At MIT, Hurwicz tested a hypothesis about how businesses arrive at prices for their goods and services. He returned to Chicago in mid-June 1941, by now a more desirable candidate for academic appointment.  Again, world events dictated his course.  After Pearl Harbor was bombed in December 1941, Hurwicz went to his statistics professor and asked how he could help the war effort. The professor, engaged in research on a brand new invention called radar, brought him onto the project. This led to an appointment with the Institute of Meteorology at the University of Chicago, where Hurwicz taught prospective Army and Navy inductees statistics, mathematics and physics needed to analyze weather data.  At Chicago, he hired, sight unseen, a young meteorology assistant from Wisconsin named Evelyn Jensen. They were married in July of 1944; the first of their four children was born two years later.  Hurwicz had a short academic stint at Iowa State University then at the University of Illinois. But in early 1951, with McCarthyism rampant on college campuses, his politically liberal colleagues in the economics department were targeted, and Hurwicz resigned in protest. Soon afterward, he heard from an Iowa friend who had moved north and thought Hurwicz should consider joining the economics department at the University of Minnesota. Within a couple of months, Hurwicz moved his family to Minneapolis, where he would develop the idea that would win him the Economy Prize.  “When you talk about the economic process of a society, sometimes we separate it into two stories,” Hurwicz says. “One is about monetary variables. But then, we very often assume the underlying arrangement, the other variable, is ‘perfect competition,’ which means people do whatever they are supposed to do. But actually, there is usually some chapter, not too long, which tells you there are different mechanisms that operate in a particular economy… My question was, What other systems or mechanisms or variables are possible?”  Hurwicz not only pondered the question, but used mathematics to create working economic models. He developed mechanism design to help businesses and other organizations arrive at solutions that combine truthfulness, individual rationality, and social welfare.  “The way I describe it is, there are two kinds of games in economics,” Hurwicz says. “One is the game where people use only legal moves. Then there is the true game, the one like real life, where the strategies and moves people make, some of them contain illegal gains. So you take into account when you write the rules of the game that the players will try to cheat.”  That’s the basis of “mechanism design,” for which Hurwicz (pronounced “HER-witch”), a University of Minnesota regents professor emeritus of economics, won the 2007 Memorial Prize in economics. Too frail to make the trip to Sweden, Hurwicz received the award from the Swedish ambassador to the United States in the university’s Ted Mann Concert Hall on December 10, 2007.  The basic premise underlying Hurwicz’s theory may seem obvious now. But when he began working on it in the 1960s, the idea of factoring in the self-interest of players in business transactions was considered cutting edge. He and his colleagues believed that incentives could encourage players to arrive at the best possible outcome not only for themselves but for the other players in the game.  Here’s a simplified example from Hurwicz’s work. An old coal-burning energy plant is spewing pollutants. In the not-so-distant past, the response would be for a government regulator to simply demand that the utility reduce emissions or be shut down. But there are problems with this approach. It might not be a simple matter to cut emissions − the cost may be prohibitively high. Or the owner may try to finesse the “game” by hiring an attorney who knows ways to keep the issue bottled up in court. He may also have influence with local legislators, who don’t want to lose the jobs (and electricity) the plant produces.  Mechanism design shows how to attain more efficient outcomes while taking into account the two sides’ different motivations. One possible scheme: A “cap-and-trade” system that creates financial incentives to reduce emissions by assigning a cost to polluting. The government establishes a cap to limit emissions. The emissions allowed under the new cap are then divided up into individual permits that give producers the right to emit that amount. Companies are free to buy and sell these permits. Those who can reduce emissions at a low cost do so, then sell their permits at a profit to companies that continue to pollute. Those high polluters thus have an incentive to reduce their own emissions.  In this case, mechanism design uses government regulation to create a market solution when the market, on its own, doesn’t work very well. Such Hurwicz-inspired mechanisms may be useful in creating innovative solutions to some of the thorniest problems in public policy − reducing the inequities in the U.S. health care system, or regulating the ever-expanding telecom industry.  Mechanism design builds on game theory, which arose in the 1940s as a mathematical means of studying various interactions, including business negotiations and transactions. According to game theory, each “player” in an exchange will choose a strategy to better his own position based on the strategies that he believes others will employ. (Think of a blind auction: How do you bid on something so that you win the item but don’t pay too much for it?)  In business, there are many examples of “games” in which one player can increase his odds of winning by being less than up front. What Hurwicz did − followed up by Eric Maskin at Princeton and Roger Myerson at the University of Chicago, who both share the Prize with him − was invent a mathematical way to assess the effects of dishonesty or other imperfections and prevent a zero-sum game in which dishonest winners turn other players into undeserving losers. Since imperfect results can occur in the free market − monopoly power, hidden information (Enron’s off-the-books funds, for instance), pollution, disincentives to make product improvements − Hurwicz and his colleagues wondered, how can regulators (or other players) create incentives so that everyone wins and the efficiencies of a market economy aren’t lost?  “Leo’s research provided a way to categorize all the outcomes that are achievable in different economic settings,” says Narayana Kocherlakota, chair of the Department of Economics at the University of Minnesota. “He understood that people might not abide by the rules and thought about all the possible games and arrangements and policies that could result. The way economists worldwide think through problems is heavily influenced by Leo’s thinking even today.”  Mechanism design is commonly used now to set up transactional strategies. For individual consumers, one of the most familiar and recognizable examples is the open auction, where potential buyers and sellers determine how valuable an item is and set the best price based on all of the players knowing what’s being bid. But the same principles are at work in other situations − the federal government’s auction approach to selling cell phone bandwidth, for instance.  Hurwicz translated the lessons of oppression into a now global belief system that is equal parts economics and philosophy, and that helps solve specific problems in the real world. Among the many things he’s learned: People can rise above their self-interest.  Hurwicz’s drew his 2007 Prize lecture from his 1988 paper entitled “But Who Will Guard the Guardians?,” where he wrote: “Somewhere at a finite end in the chain of guardians, there may be guardians (individual or collective) who are in sympathy with the rule (game-form) that makes certain behavior illegal, e.g., whose ethical standards rule out corrupt behavior, and who have the ability (through power, financial assets, personal charisma or status, combined with the population’s respect for it), as well as the inclination to act so as to discourage improper behavior of the guardians of lower order.”  He describes how, in a truly democratic society, corrupt politicians can be halted by concerned and selfless individuals − he calls them “intervenors” − who act to right the system. The same is true, Hurwicz postulates, in business, where a handful of righteous individuals can rebalance an equation thrown off kilter by dishonest peers.  “Just as despair can come to one only from other human beings,” proclaimed [Elie Wiesel](https://www.nobelprize.org/nobel_prizes/peace/laureates/1986/), who was interned at Auschwitz during World War II and won the Nobel Peace Prize in 1986, “hope, too, can be given to one only by other human beings.”  Both Hurwicz and Wiesel, in their own ways, are saying something similar.  From [*Les Prix Nobel*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)*. The Nobel Prizes 2007*, Editor Karl Grandin, [Nobel Foundation], Stockholm, 2008  This autobiography/biography was written at the time of the award and later published in the book series [*Les Prix Nobel/*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lesprix.html)[*Nobel Lectures*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/lectures/index.html)*/*[*The Nobel Prizes*](https://www.nobelprize.org/nobel_organizations/nobelfoundation/publications/nobel-prizes.html). The information is sometimes updated with an addendum submitted by the Laureate.  *Leonid Hurwicz died on June 24, 2008.* |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0835=EH  [Evelyn Hurwicz] – Hello.  [Adam Smith] – Good morning, I’m sorry to call so early. This is Adam Smith from the Nobel Foundation’s website in Stockholm.  [EH] – Yes.  [AS] – Would it be possible to speak to Leonid Hurwicz please?  [EH] – Yes, just a moment.  [Leonid Hurwicz] – Hello.  [AS] – Good morning, Professor Hurwicz, this is Adam Smith …  [LH] – Hello?  [EH] – He’s hard of hearing.  [AS] – Oh, I am sorry, I’ll speak louder. Can you hear me now OK?  [EH] – Yes.  [AS] – Thank you. My name is Adam Smith, which I know is a strange name for …  [EH laughs]  [AS] – …and I am calling from the Nobel Foundation website in Stockholm.  [EH] – Yes.  [AS] – And we have a tradition of recording very brief interviews with new laureates as soon as the news is announced. So I’m ringing to congratulate Professor Hurwicz, and just to ask a couple of quick questions, if I may.  [EH] – All right.  [AS] – Thank you. So we understand that you are now the oldest ever Laureate.  [EH] – The oldest winner?  [AS] – The oldest ever winner, yes.  [EH] – Oh.  [AS] – In the entire history of the Prize.  [EH] – Leo …  [LH] – Yeah.  [EH] – …you are the oldest winner of the Prize, Nobel Prize in Economics.  [LH] – The oldest, ah-ha. OK, you better listen Evelyn.  [EH] – No, he wants your remark about it. Are you glad you lived that long?  [EH and LH laugh]  [AS] – And the mechanism design theory that you developed, Professor Hurwicz, has been used in so many applications now. Is there one particular application that you are most pleased about?  [LH] – Well, I’m particularly pleased about welfare economics applications.  [AS] – It has brought good to many, many people.  [EH] – Right.  [AS] – OK, it’s been delightful to speak to you. Thank you very much indeed.  [EH] – Thank you, we’re very pleased.  [AS] – And again, my congratulations.  [LH] – Yeah, I’m very pleased and I hope that others who deserve it also got it.  [AS] – Bye, bye.  [EH] – Bye, bye. |
| Interview |  |
|  |  |
| ID | 0836 |
| Biographical | I was born in New York City, but grew up across the Hudson River in Alpine, New Jersey. With fewer than a thousand residents, Alpine was too small to have its own secondary schools, so I attended junior high and high school in the town of Tenafly, three miles down the road.  At Tenafly High, I was lucky to have some dedicated teachers; I’m especially indebted to my calculus instructor, Francis Piersa, who opened my eyes to the striking beauty of mathematics. Thanks to him, I became a math major at Harvard College, where I studied algebra with Pierre Samuel and Richard Brauer and analysis with George Mackey and Lars Ahlfors − all of them inspiring, some of them amusingly eccentric. Almost by accident, I wandered at one point into a course on “information economics” taught by [Kenneth Arrow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1972/), later my Ph.D. advisor. The course was a hodgepodge of topics from the frontier of economic theory, but a good part of it was devoted to Leonid Hurwicz’s work in the nascent field of mechanism design. This work was a revelation to me: it had the precision, rigor, and sometimes the beauty of pure mathematics and also addressed problems of real social importance − an irresistible combination.  In fact, I ended up essentially doing an economics Ph.D. The degree was nominally in applied mathematics. But the applied math program at Harvard in those days was remarkably flexible, and allowed students to study whatever they wanted, as long as they wrote a thesis with “significant mathematical content.” I took quite a few economics courses (although none, I regretted later, in macroeconomics or economic history), including Truman Bewley’s general equilibrium course, where I first got to work with my classmate and later co-Laureate Roger Myerson (we sometimes tackled the rather demanding problem sets together) and Jerry Green’s analytic seminar, whose student participants included Elhanan Helpman, Bob Cooter, and Jean-Jacques Laffont.  As an advisor, Ken Arrow was amazingly generous with his time; and I learned an immense amount from our many one-on-one discussions in his office. Under his supervision, I wrote a dissertation showing that, on any domain of preferences, the problem of finding a social welfare function satisfying the Arrow axioms[1](https://www.nobelprize.org/prizes/economic-sciences/2007/maskin/biographical/#not1) is solvable if and only if the problem of finding an unmanipulable mechanism is also solvable.  Ken thought I’d benefit from sitting at the feet of his friend and collaborator Frank Hahn, who helped arrange a post-doctoral research fellowship for me at Jesus College in Cambridge University. That year was a marvelous experience in every way − from Cambridge college life, to London theater, to touring around Europe. But the true highlights were weekly “tutorial” sessions with Frank, and the opportunity to start research projects and life-long friendships with Jean-Jacques Laffont (then in Paris) and Partha Dasgupta (then at the LSE).  While in England, I got caught up in a problem inspired by the work of another new friend, Leo Hurwicz (Ken had introduced me to him at Stanford): under what circumstance is it possible to design a mechanism (that is, a procedure or game) that implements a given social goal (formally, a social choice rule)? After struggling with that question for most of the year, I finally realized that monotonicity (now sometimes called “Maskin monotonicity”) was the key: if a social choice rule doesn’t satisfy monotonicity, then it is not implementable; and if it does satisfy this property it is implementable provided no veto power, a weak requirement, also holds. The proof of the latter finding was constructive, that is, I showed how one can explicitly design an implementing mechanism. But the mechanism was fairly cumbersome, and so I was most grateful to Karl Vind, my discussant at that summer’s Econometric Society meeting in Paris, who suggested a nice simplification. I wrote up the full details of my results in the paper “Nash Equilibrium and Welfare Optimality” during my first term as an assistant professor at MIT that fall. But I didn’t actually publish the paper for another twenty years: there seemed little point, since it was already well known in its working paper form.  The MIT economics department of that time was still small enough so most of the faculty would fit around a single large table in the faculty club each day for lunch. That occasion was always a treat − on any given day, [Paul Samuelson](https://www.nobelprize.org/nobel_prizes/economics/laureates/1970/) or [Franco Modigliani](https://www.nobelprize.org/nobel_prizes/economics/laureates/1985/) might hold forth, with frequent interjections by [Bob Solow](https://www.nobelprize.org/nobel_prizes/economics/laureates/1987/), to the rapt audience of junior faculty like me. But I probably learned most at MIT by teaching and working with Peter Diamond, who acted like a big brother to me during my time in the department. I also greatly enjoyed talking with visiting professor John Riley, who became a very close friend and a frequent collaborator.  Of course, MIT was notable not just for its faculty but also for its students. And, facing such extremely bright kids as a rookie teacher was something like being thrown to the wolves. Fortunately, some of them were able to overlook my faults as a lecturer and went on to write their dissertations with me. And a couple − Drew Fudenberg and [Jean Tirole](https://www.nobelprize.org/prizes/economic-sciences/2014/tirole/facts/) − soon became co-authors (and great friends).  Although MIT was undoubtedly the best place for my first teaching job, Harvard seemed a more natural long-term fit for the sort of work I was doing, and so after seven years I moved up the river to Littauer Center. The senior theory group that evolved at Harvard − Andreu Mas-Colell, Jerry Green, [Oliver Hart](https://www.nobelprize.org/nobel_prizes/economic-sciences/laureates/2016/hart-facts.html), Drew Fudenberg, Mike Whinston, Marty Weitzman − was rivaled by few others in the profession. And besides the theorists, Janos Kornai and [Amartya Sen](https://www.nobelprize.org/nobel_prizes/economics/laureates/1998/) proved to be extraordinary friends and colleagues. From Janos, I learned about central planning, and we ended up jointly supervising a cohort of unusually talented Ph.D. students from China, two of whom − Yingyi Qian and Chenggang Xu − later became co-authors. From Amartya, I learned the subtleties of social choice theory, and we twice taught a course on this subject together − once with Robert Nozick, once with Tomas Sjöström.  As at MIT, I was lucky to have a succession of terrific students at Harvard. Four of them (in addition to Yingyi and Chenggang) became long-term collaborators and friends: [Abhijit Banerjee](https://www.nobelprize.org/prizes/economic-sciences/2019/banerjee/facts/), Mathias Dewatripont, Sandeep Baliga, and [Michael Kremer](https://www.nobelprize.org/prizes/economic-sciences/2019/kremer/facts/). But after fifteen years at Harvard, I was feeling the pressure of over-scheduling: too many courses, too many students, and − worst by far − too many committee meetings. So, when an offer arrived for a position at the Institute for Advanced Study (entailing very few duties), I took it.  As of this writing, I’ve been at the Institute for seven years, and it has provided everything I had hoped for: first-class work conditions, stimulating colleagues, interaction across disciplines, blessed freedom. I do some teaching for the Princeton economics department and continue to supervise Ph.D. students − but at a more modest rate than before.  The word “luck” appears repeatedly in this autobiographical sketch − and that is no accident. I was exceptionally lucky to have discovered economics in the first place, to have entered the field at a time when mechanism design was just beginning to bloom, and, most crucially, to have had a succession of remarkable teachers, students, colleagues, and friends in the profession. Finally, in a world where so many people dislike their jobs, I am lucky to be spending my days working hard at something I love.  1. As laid out in K. Arrow, *Social Choice and Individual Values*, 1951. |
| Autobiographical |  |
| Podcast |  |
| Telephone  interview | 0836=EM  [Eric Maskin] – Hello.  [Adam Smith] – Oh, hello, may I speak to Professor Maskin please?  [EM] – Speaking.  [AS] – This is Adam Smith, from the Nobel Foundation’s website in Stockholm.  [EM] – Hi.  [AS] – Hi, congratulations of course on the news.  [EM] – Thank you.  [AS] – And we have a tradition of recording extremely brief telephone interviews with new Laureates, for our archives, and I wondered if you’d mind just speaking for a couple of minutes.  [EM] – That’s fine.  [AS] – Thank you. Well, you’ve been awarded the Prize, together with Leonid Hurwicz and Roger Myerson, for laying the foundations of mechanism design theory.  [EM] – Uh-huh.  [AS] – Now, as I understand it that’s a way of studying the design of institutions implementing collective decision-making.  [EM] – That’s right.  [AS] – Can you give us some examples of where it has been most effectively applied?  [EM] – Well, probably the most dramatic example in recent years was the various auctions that have been used for decentralization. In various countries around the world, assets that had previously been in the hands of governments were sold off to the private sector in the hope that this would lead to a more efficient allocation, that these assets would be put to better use. And the way that they were sold off was via auctions, in the hope that the auction mechanism would help promote a better application. So auctions are a particular example of a mechanism that has been used very effectively.  [AS] – And these are serious auctions, where social good matters. It’s not like an Ebay auction where it doesn’t really matter too much?  [EM] – Right, right. I mean to take an example in my country, the FCC, the Federal Communications Commission, had some auctions designed for selling off radio spectrum. And these auction sales made possible the telecommunications revolution in the US. So now cell phones and Blackberrys, and the like, are all using radio waves that had previously been in purely public hands.  [AS] – Goodness. So it really enabled a technological revolution. That connection between economic theory and, sort of, everyday use probably isn’t seen by many people.  [EM] – That’s right.  [AS] – And the list of places where mechanism design theory has been applied just goes on and on, doesn’t it. It extends into social systems, social policy.  [EM] – Yes. Well for that matter you can think of a tax system as a mechanism. Indeed James Mirrlees, who was a Laureate from about 10 years ago, did his work on the design of optimal income taxes. Again, there you have particular social goals, you are interested in using taxes to improve the income distribution, to help those at the lower end. But you don’t want to choke off the incentives of individuals to earn income, that is you don’t want to stifle initiative. And so a properly designed tax system can strike a balance between helping the poor and, at the same time, giving people the incentive to work.  [AS] – Right, so your work, and the work of your fellow Laureates, is a further step on the road to designing institutions that align individual incentives with overall social goals?  [EM] – That’s right. That’s what we *try* to do.  [AS] – Are there further roads to go down? Do you think that there’s …  [EM] – Oh, gosh, yes. I mean this is a vast subject which has only begun to be explored. Leo Hurwicz, the founder of the subject, started work on this, well, close to 50 years ago. And it remains a field which is extremely active. Mechanism design is one of the most active areas in economic theory still, and should continue to be.  [AS] – And I imagine it’s a special delight to you to be awarded the Prize together with Leonid Hurwicz?  [EM] – Yes, well, Leo and Roger both. They’re both friends, and collaborators for that matter. I’ve written papers with both of them. But Leo is the father of the field, and I can acknowledge now that for many years I put his name forward, I nominated him, for this Prize. I’d begun to worry that it was too late. He’s 90 years old.  [AS] – Yes, indeed.  [EM] – So, I was enormously relieved when the news came this morning that he’d won and yes, it’s a tremendous thrill to be able to share it with him, and with Roger.  [AS] – How nice. In fact we just spoke to him and I pointed out that he was the oldest *ever* Laureate, at 90 years old …  [EM] – Is that right? In all fields?  [AS] – In all fields, in all time.  [EM] – That’s remarkable. Well, I’m awfully glad that it wasn’t too late.  [AS] – His wife’s comment to him, which we overheard on the telephone, was “Well, are you pleased to have lived that long?”  [EM] – [Laughs]. There was a very nice celebration, which actually Roger and I both attended in Minneapolis, last Spring, in honour of the 90th birthday. And I’m happy to say that, although Leo has been in somewhat fragile health in recent years, his mind was as sharp as ever. And although he found it somewhat difficult to speak, on those occasions, when he said a few words his rather devilish sense of humour was still intact.  [AS] – Good. Well hopefully they’ll be another celebration come December when all three of you gather in Stockholm. And at that point we conduct rather longer interviews with Laureates, so I’ll look forward to speaking again then.  [EM] – I’ll look forward to that too.  [AS] – OK, thank you, and once again, my congratulations.  [EM] – Many, many thanks. Bye, bye. |
| Interview |  |
|  |  |
| ID | 0837 |
| Biographical | A scholar’s greatest asset is his or her intuition about what questions to study and with what methodology. A scientific autobiography should shed some light on how this intuition grew and developed over time.  The interests that shape one’s adult life generally have deep roots in childhood. I grew up in a comfortable suburb of Boston with a fine public school system, in a family that greatly valued reading and scientific learning. My father did research and engineering for a family business of manufacturing artificial teeth, and each of my parents returned to school to earn advanced degrees at different times in my youth.  Concern about the new risks of nuclear war was widespread in the 1950s, and, like many of my generation, I was aware of this terrible threat from a young age. I have early memories of telling my father that I was worried about political cartoons that depicted global dangers of the 1956 Suez crisis. My father reassured me that the leaders of the world were bringing all their wisdom and understanding to the task of managing the crisis peacefully. This perspective suggested, however, that perhaps it might be better if our leaders could have even more wisdom and understanding, to provide guidance for a safer and more peaceful world in the future.  When I was twelve, I read a classic science-fiction novel that depicted a future where advanced mathematical social science provided the guidance for a new utopian civilization. Ideas from this and other readings grew in long discussions with my friends. It was natural, perhaps, to hope that fundamental advances in the social sciences might help find better ways of managing the world’s problems, as fundamental advances in the physical sciences had so dangerously raised the stakes of social conflict in our time.  I have always loved to read about history and found a fascinating beauty in historical maps. But I hoped for something more analytical, and so I was naturally intrigued when I first heard about economics. I began reading [Paul Samuelson](https://www.nobelprize.org/prizes/economic-sciences/1970/samuelson/facts/)‘s basic economics textbook in a high school vacation. When I got to college, I chose to concentrate in economics and applied mathematics, but my high-school chemistry teacher predicted that I would switch to physical science before the end. I was not sure whether he might have been right until I discovered game theory in 1972.  In the spring of 1972, as a third-year student in Harvard College, I took a beautiful course on decision analysis from Howard Raiffa. He taught us to see personal utility functions and subjective probability distributions as measurable aspects of real decision-making that are expressed (however imperfectly) in our daily lives. At the end of the course, he told us that the analysis of interactions among two or more rational utility-maximizing decision-makers is called game theory, and he described game theory as a field in which only limited progress had been made. This negative assessment provided a positive focus for my studies thereafter. I felt that, if I do not know how to analyze such obviously fundamental models of social decision-making, then how could I pretend to understand anything in social science? I started reading a book on game theory that summer.  There were no regular courses on game theory then at Harvard, and so I began to do independent reading on the subject, searching through the libraries for books and articles about game theory. My primary form of intellectual dialogue was scribbling notes into the margins of photocopied journal articles, which were written by the distant leaders of the field: [Robert Aumann](https://www.nobelprize.org/prizes/economic-sciences/2005/aumann/facts/), [John Harsanyi](https://www.nobelprize.org/prizes/economic-sciences/1994/harsanyi/facts/), [John Nash](https://www.nobelprize.org/prizes/economic-sciences/1994/nash/facts/), [Thomas Schelling](https://www.nobelprize.org/prizes/economic-sciences/2005/schelling/facts/), [Reinhard Selten](https://www.nobelprize.org/prizes/economic-sciences/1994/selten/facts/), [Lloyd Shapley](https://www.nobelprize.org/prizes/economic-sciences/2012/shapley/facts/), and others. Their published writings gave me good guidance into the field. In particular, when I discovered the work of John Harsanyi, some time in the fall of 1972, I really knew that I had found the research program that I wanted to join.  I was first attracted to Harsanyi’s work by his (1963) paper that defined a general cooperative solution concept which included both the two-player Nash bargaining solution and the multi-player Shapley value as special cases. These two solution concepts had elegant axiomatic derivations, and their single-point predictions were much more appealing to me than the multiple sets of solutions that other cooperative theories identified. I worked for three days in the library to understand and reconstruct the derivation of Harsanyi’s cooperative theory, simplifying it until I found that everything could be reduced to a simple balanced-contributions assumption. This was my first result in game theory.  But Harsanyi also wrote a series of papers in 1967-8 about how to model games with *incomplete information*, in which the players have different information at the beginning of the game. In 1972, Harsanyi and Selten published a new paper that suggested a generalized Nash bargaining solution for two-person games with incomplete information. So I saw an important question about game theory that had not yet been addressed in the literature: How can we extend these cooperative solution concepts to games with more than two players who have incomplete information about each other? Nobody had any general theory for predicting what might happen when cooperative agreements are negotiated among rational individuals who have different information. This was the problem that I set out to solve in my dissertation research.  I did not solve this problem in college or in graduate school, but it was a very good problem to work on. To try to build a theory of cooperation under uncertainty, I first needed to rethink many of the fundamental ideas of cooperative game theory and noncooperative game theory, and along the way I got a reasonable dissertation’s-worth of results. My advisor [Kenneth Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/) patiently read and critiqued a series of drafts that gradually lurched toward readability.  In 1976, I had the good fortune to be hired as an assistant professor by the Managerial Economics and Decision Sciences (MEDS) department in the (soon-to-be Kellogg) School of Management at Northwestern University. In the 1970s, game theory was a small field, and few schools would consider having more than one game theorist on their faculty, but Northwestern was actively building on strength in mathematical economic theory. The MEDS department was probably the only academic department in the world where game theory and information economics were not viewed as peripheral topics but as central strengths of the department. I had great colleagues, and every year we went out to hire more.  My first papers were largely about cooperative game theory, including results from my doctoral dissertation. Cooperative game theory generally begins with the assumption that people will agree on some feasible outcome that is *efficient*, in the sense that there is no other feasible outcome that all of the cooperating individuals would prefer. But in most situations we can find a wide range of such efficient allocations, with different alternatives being better for different individuals. An *equitable* bargaining solution should identify an efficient outcome in which each individual gets a payoff that is in some sense commensurate with his or her contribution to the collective agreement. In several early papers that were based on my work in graduate school, I showed how simple principles of equity between pairs of individuals could be consistently extended to situations where many individuals cooperate in coalitions.  Everything that I did in game theory was ultimately motivated by the long-run goal of developing a coherent general methodology for game-theoretic analysis. What kinds of game models should we use to describe situations of conflict and cooperation among rational decision-makers, and what solution concepts should we use to predict rational behavior in these game models? In this quest, I was greatly influenced by three classic ideas of game theory: von Neumann’s principle of strategic normalization for reducing dynamic games, Nash’s program for subsuming cooperative games into noncooperative game theory, and Schelling’s focal-point effect for understanding games with multiple equilibria. These ideas are so important that I cannot describe my work on game theory without explaining something about them.  Game theory could not have developed without a basic understanding that some conceptually simple class of models could be general enough to describe all the complicated game situations that we would want to study. John von Neumann (1928) argued that one-stage games where players choose their strategies independently can be recognized as such a general class of models, even if we want to study dynamic games where play may extend over time through many stages. The key to this argument is to define a *strategy* for a player to be a complete plan that specifies what the player should do at each stage in every possible contingency, so that each player could choose his strategy independently before the game begins. By this principle of strategic normalization, for any dynamic extensive game, we can construct an equivalent one-stage game in *strategic form*, where the players choose strategies independently, and these strategy choices then determine everyone’s expected payoffs.  After presenting this argument for the generality of the strategic form, however, von Neumann actually studied cooperative games in a different nonstrategic *coalitional-form* model of games. In response, John Nash (1951) argued that the process of bargaining should itself be recognized as a dynamic game where players make bargaining moves at different stages of time, and so it should be similarly reducible to a game in strategic form. To analyze games in strategic form, Nash defined a general concept of equilibrium. In a *Nash equilibrium*, the predicted behavior of each player must be his or her best response to the predicted behavior of all other players.  When we follow this Nash program and write down simple models of bargaining games, however, we regularly find that these games can have many equilibria, and so a predictive theory cannot be determined without some principles for selecting among all these equilibria. Schelling (1960) argued that, in a game that has multiple equilibria, anything that focuses the players’ attention jointly on one equilibrium can cause them to expect it and thus rationally to act according to it, as a self-fulfilling prophecy. The focal factors that steer the players to one particular equilibrium can be derived from the players’ shared cultural traditions, or from the coordinating recommendations of a recognized social leader or arbitrator, or from any salient properties that distinguish one equilibrium as the focal equilibrium that everybody expects to play. In my view, the cooperative solutions which I was studying in my early papers were theories about how welfare properties of equity and efficiency can identify a focal equilibrium, which an impartial arbitrator could reasonably recommend, and which may be implemented by the players as a self-fulfilling prophecy.  I understood, however, that some general principles for eliminating some Nash equilibria might be also be appropriate. In particular, I knew that the theory of Nash equilibria for strategic-form games could yield predictions that seemed irrational when interpreted back to the framework of dynamic extensive games with two or more stages. The problem is that, if an event were assigned zero probability in an equilibrium, then the question of what a player should do in this event would seem irrelevant when the player plans his strategy in advance, and so a Nash equilibrium could specify strategic behavior that would actually become irrational for the player if this event occurred. In graduate school, I recognized that such irrational equilibria could be eliminated by admitting the possibility that players might make mistakes with some infinitesimally small probability. Reinhard Selten (1975) was also developing similar ideas about refinements of Nash equilibrium. When I read his important paper on perfect equilibria in the *International Journal of Game Theory*, I sent my paper on proper equilibria to be published there also. The motivation for these refinements of Nash equilibrium was to become clearer a few years later, when David Kreps and Robert Wilson defined *sequential equilibria* of dynamic extensive-form games (1982). In this sense, perfect and proper equilibria were attempts to recognize in strategic form the equilibria that would be sequentially rational in the underlying dynamic game. But Kreps and Wilson cogently argued for dropping our reliance on strategic normalization and instead analyzing sequential equilibria of games in the dynamic extensive form.  Learning to analyze incentive constraints in games with communication Basic questions about information and incentives in economic systems were very much in the air at Northwestern in the late 1970s and early 1980s. There was great interest in Leonid Hurwicz’s ideas of incentive-compatibility, and these ideas influenced my search for a cooperative theory of games with incomplete information. But from my student days, I had learned to see games with incomplete information in the general framework of Harsanyi’s Bayesian model. After I heard Alan Gibbard’s early version of the revelation principle for dominant-strategy implementation, I became one of several researchers to see that it could be naturally extended to implementation with Bayesian equilibria in Harsanyi’s framework. The revelation principle basically says that, for any equilibrium of any communication system, a trustworthy mediator can create an equivalent communication system where honesty is a rational equilibrium for all individuals. With the revelation principle, we can generally apply some mathematically simple constraints that summarize the problems of incentives for getting people to share their information honestly. So I wrote a paper showing how the revelation principle for Bayesian incentive compatibility could extend and simplify the feasible set for Harsanyi and Selten’s (1972) bargaining solution for two-person games with incomplete information. This idea was the basis of my first article on the revelation principle, published in *Econometrica* in 1979.  In the Harsanyi-Selten bargaining solution, however, the objective is to maximize a little-understood multiplicative product of the players’ expected utility gains. In discussions with [Robert Wilson](https://www.nobelprize.org/prizes/economic-sciences/2020/wilson/facts/) and [Paul Milgrom](https://www.nobelprize.org/prizes/economic-sciences/2020/milgrom/facts/) about auctions, I began to see that there might be more interesting economic applications where other objective functions are maximized over the same feasible set. I realized that the revelation principle could become a general tool for optimizing any measure of welfare in any situation where there is a problem of getting information from different individuals. During a visit to the University of Bielefeld in Germany (1978-9), I wrote a paper that applied these ideas to the problem of designing an auction, where the objective is to maximize the seller’s expected revenue, subject to the incentive constraints of getting potential buyers to reveal information about their willingness to pay. When I returned to Northwestern, I worked with colleagues on other important applications and extensions of these ideas. David Baron and I worked on optimal regulation of a monopolist with private information about costs. Mark Satterthwaite and I analyzed efficient mechanisms for mediation of bilateral trading problems, involving one seller and one potential buyer for a single indivisible good.  With some simple but natural technical assumptions, we were able to derive powerful *revenue-equivalence* theorems, which show how the expected profit for any type of individual with private information can be computed from the expected net trades for other possible types of the same individual. In this calculation, an individual’s profit is seen to depend on the way that his private information affects the ultimate allocation of valuable assets, but not on the details of how asset prices are determined in the market. Thus, people who have private information may be able to earn informational rents under any system of market organization in which the allocation of assets depends on their information.  The problem of providing incentives for people to share private information honestly, which has been called the problem of *adverse selection*, was not the only incentive problem that economists were learning to analyze in the 1970s. There was also a growing literature on the problem of providing incentives for individuals to choose hidden actions appropriately, which has been called the problem of *moral hazard*. Robert Aumann (1974), in his definition of correlated equilibria for games with communication, formulated a version of the revelation principle that applied to moral-hazard problems. In 1982, I published a paper to extend the revelation principle in a unified way to problems with both moral hazard and adverse selection, yielding a general theory of incentive-compatible coordination systems for Bayesian games with incomplete information. Later, in 1986, I showed how to extend the revelation principle to dynamic multi-stage games where sequential rationality is required in all events.  In these extensions, I began to see a basic methodological dichotomy between the revelation principle and the principle of strategic normalization: If we use one then we cannot use the other. When we allow that players can communicate with each other through mediated channels that are not specified explicitly in the game model, then the revelation principle can be applied, and the complicated nonlinear mathematics of Nash equilibrium can be replaced by the simple linear constraints of incentive compatibility. The set of incentive-compatible mechanisms includes all equilibria that can be achieved by adding any mediated communication system to the given game, and so this set can be viewed as the feasible set for a focal arbitrator or leader who can design the communication system. But the existence of implicit opportunities for communication among the players during the game invalidates the argument for strategic normalization, and so one-stage strategic-form games can no longer be considered as the general model for all situations. Thus, when we analyze games with communication, we study Bayesian games and more general extensive-form games, setting aside the principle of strategic normalization, as Kreps and Wilson suggested.  Bengt Holmström and I published a paper in 1983 on how economic concepts of efficiency should be extended to situations where people have private information. According to Pareto’s basic concept of efficiency, the functioning of the economy is efficient when there is no feasible way to make everyone better off. But we must re-think many parts of this definition when we admit that each individual may have different private information, which is represented in our Bayesian games by a random privately-known *type* for each player. An economist’s evaluation of efficiency or inefficiency cannot depend on information that is not publicly available, and a realistic concept of feasibility must take account of incentive constraints as well as resource constraints. Thus, the concept of incentive-efficiency should be applied to the entire mechanism or rule for determining economic allocations as a function of individuals’ privately-known types, not just to the final allocation itself. Holmström and I suggested that an incentive-compatible mechanism should be considered *incentive-efficient* (in the *interim* sense) when there is no other incentive-compatible mechanism that would increase the expected utility payoff for every possible type of every individual. If a change would make even one possible type of one individual worse off, then an economist cannot say for sure that the change would be preferred by everyone, when each individual privately knows his or her own type.  In the early 1980s, I returned to the problem of defining general cooperative bargaining solutions for Bayesian games where players have different information. To identify one bargaining solution among the many mechanisms on the interim incentive-efficient frontier, we need to define some principles for equitable compromise, not just among the different individuals, but also among the different possible types of any one individual. In particular, to avoid leaking private information, a player’s bargaining strategy may need to be an inscrutable compromise between the payoff-maximization goals of his actual type and the goals of the other possible types that the other players think he might be. Such an inscrutable intertype compromise must be defined by some principles for measuring how much credit each possible type can claim for the fruits of cooperation. For example, when a player could be a good type or bad type and would prefer to be perceived as the good type, so that the incentive constraint that bad types should not gain by imitating good types is binding and costly in incentive-efficient mechanisms, then an inscrutable intertype compromise might put more weight on the goals of his good type than his bad type. In the early 1980s, I began to see that this idea can be mathematically formalized and measured by using the Lagrange multipliers of the informational incentive constraints in the mechanism-design problem. Economists have long recognized the importance of Lagrange multipliers of resource constraints, which correspond to prices of these resources, but economists had not previously done much with Lagrange multipliers of incentive constraints. With this mathematical insight, I was at last able to solve the problem of extending the Nash bargaining solution and the Shapley value to Bayesian games with incomplete information, which I published in *Econometrica* and the *International Journal of Game Theory* in 1984.  New directions In 1980, I met Gina Weber, and we were married in 1982. Our children Daniel and Rebecca were born in 1983 and 1985. Everything in my life since then has been a joint venture shared with them.  The biggest focus of my work in the later 1980s was on the writing of a general textbook on game theory, which was published in 1991. In this book, I presented, as coherently as I could, the general methodology for game-theoretic analysis that so many of us had been working to develop. Other textbooks on game theory were also published in the early 1990s, and general textbooks on economic theory also began to treat game theory, information economics, and mechanism design as essential parts of microeconomic analysis. When the importance of game theory in economics was recognized by the Prize Committee in October 1994, by its award to John Nash, John Harsanyi, and Reinhard Selten, I opened bottles of champagne to celebrate with my colleagues at Northwestern.  The last section of my game theory book considered markets with adverse selection, where theorists since Michael Rothschild and [Joseph Stiglitz](https://www.nobelprize.org/prizes/economic-sciences/2001/stiglitz/facts/) (1976) have found that simple equilibrium concepts can have complicated nonexistence problems. I suggested that equilibria may be sustainable in great generality by a version of Gresham’s law, that bad types may circulate more than good types in the market, and I formalized this argument in a paper in 1995.  In the late 1980s, I began to work on game-theoretic models of politics. I had always felt that game theory’s applications should go beyond the traditional scope of economics. In constitutional democracies, political constitutions and electoral systems define the rules of the game by which politicians compete for power. So game-theoretic analysis should be particularly valuable for understanding how changes in such constitutional structures may affect the conduct of politicians and the welfare-relevant performance of the government. I saw some analogy here with well-known ideas from the economic theory of industrial organization. In particular, the ability of political organizations to extract profits of power (corruption) should be expected to depend on barriers against the entry of new competitors into the political arena.  My first paper on comparison of electoral systems with Robert Weber was followed by several more papers in which I explored various models for evaluating the competitive implications of electoral reforms. For example, a politician could try to appeal broadly to all voters or could try to concentrate narrowly on appealing to small subgroups of the voting population; and I developed game models to show how different electoral rules can systematically affect politicians’ competitive incentives to appeal more broadly or narrowly. In other papers, I showed how some electoral rules can yield a wider multiplicity of equilibria, including discriminatory equilibria in which some good candidates may not be taken seriously by the voters. I argued that such discriminatory equilibria can become barriers to entry against new politicians, thus allowing established political leaders to claim more profit from their political power. Democratic competition is intended to reduce such corrupt profit-taking by political leaders, but my analysis suggested that the effectiveness of democracy against such corruption can depend on the specific structure of the electoral system. In several models, I found that approval voting could induce stronger incentives for politicians to compete vigorously at the center of the political spectrum.  The constitutional distribution of power among different elected officials can also affect political incentives. Daniel Diermeier and I wrote a paper to show how presidential veto powers and bicameral subdivision of the legislature can decrease legislators’ incentives maintain coherent discipline in political parties or legislative coalitions.  The analysis of games with incomplete information has shown economists how probabilistic analysis of decision-makers’ uncertainties can offer practical insights into competitive strategies, but my MBA students at Kellogg did not seem well prepared to apply such analysis. So I felt that it was important to seek new ways of teaching that could make practical probability models accessible to them. After trying many different approaches throughout the 1980s and 1990s, I found that the best pedagogical solution was to use simulation modeling in spreadsheets. Models and techniques that seemed too difficult for students when I wrote on the blackboard became intuitively clear to them when we worked together in electronic spreadsheets. So I wrote an MBA-level textbook on probability models for economic decisions which was published in 2005.  Since 1996, I have written several papers on the history of game theory itself. I had the privilege of speaking about the importance of John Nash’s contributions to economic theory at the American Economic Association’s luncheon in honor of his Economic Sciences Prize in January 1996. This presentation developed into a longer paper that was published in the *Journal of Economic Literature* in 1999, for the 50th anniversary of the day when Nash submitted his first paper on noncooperative equilibrium.  As the end of the 20th century approached, I began to ask whether the advances in competitive analysis that meant so much to me might actually offer hope for a better 21st century. So I wrote a theoretical retrospective on the history of Germany’s Weimar republic, the failure of which was so central among the disasters of the 20th century. The establishment of the Weimar republic was framed by the treaty of Versailles and the Weimar constitution, which were written in 1919 with expert advice from leading social scientists, including John Maynard Keynes and Max Weber. I wanted to ask whether any recent advances in political and economic theory might offer a better framework for understanding such practical problems of institutional design, so that mistakes like those of 1919 should be less likely in the future. In this retrospective, I found that the advances that seemed to offer the most valuable insights for improving international relations were based largely on the ideas of Thomas Schelling and Reinhard Selten, in particular, the focal-point effect and the analysis of strategic credibility.  So when America set out to invade Iraq in 2003, I applied Schelling’s ideas about credible deterrence to show that how America’s rejection of multinational military restraint could exacerbate threats against America. When a powerful nation uses military force without clear limits, instead of deterring potential adversaries, it can actually motivate potential adversaries to invest more in militant counter-forces. Thus, I have argued, the greatest superpower in the world may have the greatest need to articulate clear and credible limits on its use of military force, according to rules and principles which the rest of the world can judge.  In recent years, my research agenda has been increasingly shaped by a broad study of political history. To understand the great problems of political change and economic development, we need new theoretical models that can help us to better understand the functional logic of the traditional systems from which many nations are now evolving. To have any hope of finding such broader models, however, a theorist needs the broadest possible understanding of different economic and political institutions of different societies throughout the world, from ancient times until today. For such a program of study, I found Samuel Finer’s *History of Government* (1997) to be a particularly valuable introduction to the global history of political institutions, but it needs to be followed by much more reading. From this historical perspective, I have come to believe that new theoretical models of oligarchy or feudalism or tribalism could become as important for political analysis as the Arrow-[Debreu](https://www.nobelprize.org/prizes/economic-sciences/1983/debreu/facts/) general equilibrium model for analysis of competitive markets.  In searching for universal principles that underlie all kinds of institutional structures, I have come to see agency incentive problems and reputations of political leaders as critical factors that are fundamental to the establishment of any political institution. The rules of any political system must be enforced by government officials, and getting these officials to act according to these rules is a moral-hazard problem. The incentives for lower government officials depend on rewards and punishments that are controlled by higher political leaders. So we find, at the heart of the state, a problem of moral hazard for which the solution depends on the individual reputations of political leaders. The institutions of any political system must be organized by political leaders whose first imperative is to maintain their reputation for rewarding loyal supporters. The problem of cultivating a democracy can then be recognized as a problem of creating opportunities for more politicians to begin cultivating good democratic reputations, that is, reputations for serving the mass of voters even as they reliably reward their active agents and supporters with patronage benefits.  As an application of these ideas, I have written critically about American policies to establish democracy in occupied Iraq. I have argued that the first priority of the occupation authority in 2003 should have been to create elected and well-funded local councils, in which local leaders throughout the country could begin building independent reputations for responsible democratic governance.  In 2001, I accepted a job in the economics department at the University of Chicago. This was only my second academic position. Northwestern’s long tradition of great strength in economic theory made it an ideal home for me to work with great colleagues in game theory and political economics. But after 25 years in one school it seemed time for a change, especially when the alternative on the other side of town was a university with another outstanding tradition of scholarship in economics. I still value my former colleagues at Northwestern as much as my new colleagues at Chicago. Also, I am proud of another connection that I still have with Northwestern University, as my wife Gina is now a dean in Northwestern’s McCormick School of Engineering.  In 2007, the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel was awarded to Leonid Hurwicz, Eric Maskin, and me. I am very proud to have my name so prominently linked with Leo’s and Eric’s, and with the advances in analysis of coordination mechanisms and incentive constraints to which we contributed along with many other great economists. The opportunity to share this honor with my wife and my family and to celebrate with so many friends and colleagues has been a wonderful experience for me. But I understand that the true Laureates are the ideas of incentive analysis and mechanism design, and I want to continue working to understand them more deeply and to present them more clearly. There is much that we still need to learn about how our social institutions operate, and how they can be better designed. |
| Autobiographical |  |
| Podcast | Belonging to a community is wonderful, but for economic sciences laureate Roger Myerson sharing outside your community is more important. By crossing maths with history and politics, Myerson arrived at new insights about economics, found his love for game theory and moved onwards into new ways to describe situations where markets don’t work properly.  This Nobel Prize conversation was conducted in March 2020. The host of the podcast is nobelprize.org’s Adam Smith. |
| Telephone  interview | 0837=  [Eric Maskin] – Hello.  [Adam Smith] – Oh, hello, may I speak to Professor Maskin please?  [EM] – Speaking.  [AS] – This is Adam Smith, from the Nobel Foundation’s website in Stockholm.  [EM] – Hi.  [AS] – Hi, congratulations of course on the news.  [EM] – Thank you.  [AS] – And we have a tradition of recording extremely brief telephone interviews with new Laureates, for our archives, and I wondered if you’d mind just speaking for a couple of minutes.  [EM] – That’s fine.  [AS] – Thank you. Well, you’ve been awarded the Prize, together with Leonid Hurwicz and Roger Myerson, for laying the foundations of mechanism design theory.  [EM] – Uh-huh.  [AS] – Now, as I understand it that’s a way of studying the design of institutions implementing collective decision-making.  [EM] – That’s right.  [AS] – Can you give us some examples of where it has been most effectively applied?  [EM] – Well, probably the most dramatic example in recent years was the various auctions that have been used for decentralization. In various countries around the world, assets that had previously been in the hands of governments were sold off to the private sector in the hope that this would lead to a more efficient allocation, that these assets would be put to better use. And the way that they were sold off was via auctions, in the hope that the auction mechanism would help promote a better application. So auctions are a particular example of a mechanism that has been used very effectively.  [AS] – And these are serious auctions, where social good matters. It’s not like an Ebay auction where it doesn’t really matter too much?  [EM] – Right, right. I mean to take an example in my country, the FCC, the Federal Communications Commission, had some auctions designed for selling off radio spectrum. And these auction sales made possible the telecommunications revolution in the US. So now cell phones and Blackberrys, and the like, are all using radio waves that had previously been in purely public hands.  [AS] – Goodness. So it really enabled a technological revolution. That connection between economic theory and, sort of, everyday use probably isn’t seen by many people.  [EM] – That’s right.  [AS] – And the list of places where mechanism design theory has been applied just goes on and on, doesn’t it. It extends into social systems, social policy.  [EM] – Yes. Well for that matter you can think of a tax system as a mechanism. Indeed James Mirrlees, who was a Laureate from about 10 years ago, did his work on the design of optimal income taxes. Again, there you have particular social goals, you are interested in using taxes to improve the income distribution, to help those at the lower end. But you don’t want to choke off the incentives of individuals to earn income, that is you don’t want to stifle initiative. And so a properly designed tax system can strike a balance between helping the poor and, at the same time, giving people the incentive to work.  [AS] – Right, so your work, and the work of your fellow Laureates, is a further step on the road to designing institutions that align individual incentives with overall social goals?  [EM] – That’s right. That’s what we try to do.  [AS] – Are there further roads to go down? Do you think that there’s …  [EM] – Oh, gosh, yes. I mean this is a vast subject which has only begun to be explored. Leo Hurwicz, the founder of the subject, started work on this, well, close to 50 years ago. And it remains a field which is extremely active. Mechanism design is one of the most active areas in economic theory still, and should continue to be.  [AS] – And I imagine it’s a special delight to you to be awarded the Prize together with Leonid Hurwicz?  [EM] – Yes, well, Leo and Roger both. They’re both friends, and collaborators for that matter. I’ve written papers with both of them. But Leo is the father of the field, and I can acknowledge now that for many years I put his name forward, I nominated him, for this Prize. I’d begun to worry that it was too late. He’s 90 years old.  [AS] – Yes, indeed.  [EM] – So, I was enormously relieved when the news came this morning that he’d won and yes, it’s a tremendous thrill to be able to share it with him, and with Roger.  [AS] – How nice. In fact we just spoke to him and I pointed out that he was the oldest ever Laureate, at 90 years old …  [EM] – Is that right? In all fields?  [AS] – In all fields, in all time.  [EM] – That’s remarkable. Well, I’m awfully glad that it wasn’t too late.  [AS] – His wife’s comment to him, which we overheard on the telephone, was “Well, are you pleased to have lived that long?”  [EM] – [Laughs]. There was a very nice celebration, which actually Roger and I both attended in Minneapolis, last Spring, in honour of the 90th birthday. And I’m happy to say that, although Leo has been in somewhat fragile health in recent years, his mind was as sharp as ever. And although he found it somewhat difficult to speak, on those occasions, when he said a few words his rather devilish sense of humour was still intact.  [AS] – Good. Well hopefully they’ll be another celebration come December when all three of you gather in Stockholm. And at that point we conduct rather longer interviews with Laureates, so I’ll look forward to speaking again then.  [EM] – I’ll look forward to that too.  [AS] – OK, thank you, and once again, my congratulations.  [EM] – Many, many thanks. Bye, bye. |
| Interview | Interview with the Laureates in Economics Eric S. Maskin and Roger B. Myerson, 6 December 2007. The interviewer is Adam Smith, Editor-in-Chief of Nobelprize.org.  **Eric Maskin, Roger Myerson, welcome to this interview with Nobelprize.org and to Stockholm. You’re co-recipients, together with Leo Hurwicz of the 2007 Sveriges Riksbank Prize in Economic Sciences in honour of the memory of Alfred Nobel. You’ve been awarded it for your work contributing to a mechanism design theory which essentially provides an analytical framework for analysing institutions making collective decisions?**  Eric S. Maskin: Right.  Roger B. Myerson: Well put.  **I think I’ve just paraphrased your words, I’m sure. We should perhaps start by mentioning Leo who can’t be here because of health reasons. But he’s the founder of the field, he invented the mechanism design theory and he’s also the oldest ever Laureate at 90 years old. He started this field half a century ago in a very different climate. Then people were considering the differences or trying to compare economic institutions in capitalist and communist situations for instance. Was that a very different time to be doing economics?**  Roger B. Myerson: I don’t think 1972 was that different a climate. I think he was responding. Much of his career was in response to this early 20th century capitalist versus socialist debate where it can’t be a recognition that all of our wonderful results about the efficiency of the free market didn’t necessarily mean that a socialist command economy was bad. In fact they didn’t say anything about the socialist command economy and that economic models simply weren’t broad enough.  Leo Hurwicz was, I think, largely responding to this and [Frederik Hayek](https://www.nobelprize.org/prizes/economic-sciences/1974/hayek/facts/)’s observation that the debate between capitalism and socialism in an article of 1945 by Hayek argued that the real distinction had to be on understanding the economy. The market is a mechanism for communicating information. People who need things. People who have resources. The supply and demand are communicated in the market and Hayek said the virtues of the free market, that he said in his opinion, that socialism couldn’t imitate, were virtues of communication and this is what needed to be studied. Hayek also said that of all the economists of his time, the people who were worst about this were the mathematical economists.  You are talking to two mathematical economists and in particular Leo Hurwicz was a mathematical economist from the start. Leo understood, as I think we all do correctly, that whatever mathematical or non-mathematical economist may or may not have understood in 1945, if you think your models are not general enough to understand the broader phenomena, mathematics which is versatile and you know it’s the vigour and versatility of mathematics is a good place to go. It’s a useful tool. If economic models were not broad enough to be able to compare capitalism and socialism, mathematical models were worth pursuing and this is the challenge that Leo took.  Eric S. Maskin: Just to amplify what Roger has been saying. The debate between von Hayek and von Mises on the one hand and Lange and Learner on the other about capitalism versus a planned economy was very important and very interesting but also very frustrating in a sense because none of the terms they were using, decentralisation, a command economy. For that matter a market economy were defined precisely enough so that the debate could be assessed and actually Leo got into his great work in part because he was trying to understand just what these terms might mean. The word decentralisation sounds as though it has an obvious meaning, but when you think about it for a moment, you see that it’s really quite a subtle concept. He spent a good many years just puzzling over what decentralisation means.  **And then framing it in some kind of mathematical terms?**  Eric S. Maskin: The advantage of mathematics is that it’s quite ambiguous. It gives a precision that sometimes ordinary language doesn’t provide.  Roger B. Myerson: Put it another way if you want. I would argue that in some of this debate, you can find Hayek saying things that we can now say in our models. But because Hayek couldn’t say it in formal economic models, he’d tell them to get lost. If you don’t have it in an analytical framework, the words fly by. When you have a systematic framework I think the real breakthrough was in Leo’s 1972 paper when he introduced incentives to communicate. Suddenly then things changed.  **That’s the idea that the market is not dominated just by the amount of resource around, it’s also dominated by the incentives that people have to use it, to work with it, is that right?**  Eric S. Maskin: Incentives are a critical part, not just of markets, but also of alternatives.  Roger B. Myerson: Exactly. They’re both.  Eric S. Maskin: And to do a proper comparison of a market institution versus a non-market institution one has to take into account the incentives of the individuals in that institution to behave as we would want them to behave.  Roger B. Myerson: In a nutshell, I think in economic theory, the models that people taught their students to work with, in trying to help satisfy the desires and needs of human beings, increased human welfare, the economic problem is constrained by their own infinite resources. And resource constraints that we don’t have enough to make, enough oil to do everything we want in the world. Resource constraints were the essence of the economic problem. Before now the economic problem, as we understand it in our models, and theory also involves incentive constraints, the incentive to communicate information honestly. The incentive to work hard and to exert effort when nobody’s watching. It is these incentive constraints when information is imperfect are the essence of daily life. But now we’ve added to resource constraints in the economic problem we also have incentive constraints, and economists understand that too, not just ordinary people.  **Ok, we’ll come back to mechanism design theory a little bit more later, but was Leo Hurwicz’s work instrumental in making you move into this field? What was your path to the field yourselves?**  Eric S. Maskin: In my own case, it was a direct influence because I started out in pure mathematics which I still find a very beautiful subject. But wandered almost by accident into a course that one of Leo’s close friends, [Kenneth Arrow](https://www.nobelprize.org/prizes/economic-sciences/1972/arrow/facts/), was teaching on just this sort of stuff. Ken Arrow called it information economics, but a large chunk of the course was devoted to what we’ve now called mechanism design. I thought this was great stuff because it combined the rigour and clarity of mathematics with a real attention to important, social and economic issues, and I thought this was an unbeatable combination. So, I can truthfully say that Leo Hurwicz changed my life.  **Right. So it was a way of capturing both the practical application and the beauty of the theory of your mathematics?**  Eric S. Maskin: Right. As it was presented in that course, that it was theoretical. That is that we did not look at particular applications to which the theory might be directed but it was obvious that there would be such applications and in fact, in the years since we’ve seen quite a few of them.  Roger B. Myerson: Both of us are theorists, there’s no question. We start from looking for conceptual frameworks that can help people to understand applied problems better. While Eric in graduate school was reading Hurwicz, I was reading more [John Harsanyi](https://www.nobelprize.org/prizes/economic-sciences/1994/harsanyi/facts/). We were at graduate school together, but I’m trying to remember when I first met Leo Hurwicz and I’m sure it was within the first year of being an assistant professor after I finished. But I was at the same time reading Harsanyi who was trying to develop the most general framework for the analysis of competitive behaviour in competitive situations, in games. Harsanyi had this great breakthrough in how to model games where people have different information. So both cases, Hurwicz and Harsanyi you were both working on information.  I think we when we got these frameworks together, we realised Harsanyi was developing tools for analysis of a given game, and Hurwicz was giving us a framework for how do you design a game. I think that stream came together in the late 1970s where two of the groups of then young economists who jumped into the breach with the game theoretic tools in place, and Hurwicz’s mechanism design in place and at last economics was prepared to really deal systematically and analytically with interactions between people who have different information, the creation of institutions where people have different information.  **So you actually felt aware of the fact that there was suddenly new tools available to work with?**  Eric S. Maskin: Well, not having been around in the days without these tools, I’m not sure that we appreciated, at least I didn’t appreciate it at the time. The extent to which all of this was new. I certainly do now. I realise now, as I didn’t then, that this was a very special time. Maybe you knew at the time then?  Roger B. Myerson: I got into the game theory at the end, I think you may have been in the course, Howard Raiffa taught a wonderful decision analysis course.  Eric S. Maskin: I took that.  Roger B. Myerson: It was analysis of rational decision making. He made it very practical. You wake up in the morning and you have a utility function and how would you assess it? But at the end of course, he said supposing there are two people interacting and they each have utility functions that they’re maximising. That’s game theory and there isn’t very much known about game theory. Now by the way we’re about to celebrate the 50th anniversary of Howard Raiffa’s book with Duncan Luce from 1957. It was the best book we had available but when he said …  Eric S. Maskin: It’s still a very good book.  Roger B. Myerson: It is a good book, but when he said, Actually not enough is known about this, about how to analyse games, how can life go on? I’m sorry, how can we do anything if we don’t understand how to analyse these structures. I had a great sense that fundamental work was needed, and the truth is I was lucky to have been born at a time that got me in graduate school at a time when the breakthroughs were ready. There’s still more work to be done on fundamental game theory, but we’ve made a lot of progress on the tools.  Eric S. Maskin: The landscape has definitely changed. When I started teaching in 1977, my first job was at MIT. The MIT economics department had never had a game theory course before. I taught the first game theory course that the department had ever offered. Now it’s inconceivable that any department in the world worth its salt shouldn’t, not only must these departments offer game theory, but game theory has to be a central part of the curriculum, so you can see how over the years the landscape has changed with it. But I didn’t appreciate at the time that that was going to happen.  Roger B. Myerson: May I amplify that with one? At my 25th college reunion, the president of Harvard University when we came back spoke about the new humanities centre that the university was investing in and he said that Harvard took humanities seriously and not just feels like for example game theory. When we were students there were humanities courses taught at Harvard University. There were no game theory courses taught. It’s nice to … I felt a certain vindication. I thought it was important too. Humanities is important also. I want to be very clear about that.  **OK. You came out of the same milieu, you were students together and then you separated and went your own ways. But then reunited as collaborators but presumably also competitors?**  Roger B. Myerson: Oh yes, absolutely. Coming up with the same ideas for instance. I just discovered, preparing for this, and looking at Eric’s vitae I realised there’s something I want to do on the soft budget constraint and Eric’s already written the great paper on the subject. There’s more work to be done of course but we have three decades now of history of coming back to the same ideas and that’s good.  Eric S. Maskin: One instance where some collaboration might have occurred early was on auctions. It turned out that at the same time Roger was writing his now universally acknowledged seminal paper on the design of auctions. John Riley and I were thinking along very similar lines. Would have been nice if we could have joined forces.  Roger B. Myerson: Yes, yes.  Eric S. Maskin: Early on. Competition is a great thing, but collaboration is perhaps even better.  Roger B. Myerson: I worked for 25 years at Northwestern. I was very influenced by my colleagues and I really thought the way I framed it was very much a Chicago Northwestern approach. But I was approaching it the same way as Eric. We must have got a lot out of school together or great minds just think alike, but whatever. We have one joint paper, and as I’ve mentioned to you before I owe my Erdős number to that paper.  **That’s the source of the Erdős number.**  Roger B. Myerson: Yes.  **So this is your collaborative distance from Erdős?**  Roger B. Myerson: Yes.  **OK.**  Roger B. Myerson: He’s the close runner up.  **And you’re a 2?**  Eric S. Maskin: I’m a 2 because Peter Fishburn actually wrote a paper with Erdős and I wrote a paper with Peter.  **I read somewhere that there’s a baseball player who signed a baseball at the same doctoral ceremony that Erdős got a honorary doctorate at Emory University.**  Roger B. Myerson: Does that make him a collaborator?  **That’s the question. Does he have an Erdős number of 1 or not?**  Eric S. Maskin: I don’t know. Might need a referee.  **Probably not. Let’s think about the mechanism and design theory for a while. It’s something that actually touches peoples’ daily lives in an enormous way, because it’s used for so many decision-making processes that affect people. Auctions being one that’s already been touched on. Would you each like to choose an example of where it’s been applied that would help to illustrate its far-reaching impact?**  Eric S. Maskin: Shall I go first? One current example is traffic congestion. I think this an example of what most people can relate to, at least city dwellers. Many cities, New York among them, have a problem with too many cars at the same time, and the question is how to deal with that problem. There are many alternative conceivable policies. One is to ban traffic outright, at least from certain parts of the city. Another is to raise tolls leading into the city. Subway, tunnel tolls or bridge tolls. A third possibility – and this has been pursued here in Stockholm and a number of Australian cities – is to equip cars with transponders and keep a record of where the cars go in the course of a day and charge them accordingly. Not only as to where they go but also the time of day.  A fourth policy is to invest in public transportation, and there are many other possibilities as well. What mechanism design enables us to do, it gives us the tools to evaluate these alternative policies and to see which one of them or combination – it’s usually some combination – is best suited for the particular circumstances of a given city. It gives us an analytic framework to evaluate policy.  **How does that work, how do you apply that analytic framework? I’m sure there are obviously many levels of detail to the answer to that question, but one can see how one could work out the income from for instance a toll system, but how do you use mechanism design theory here?**  Eric S. Maskin: Income is one consideration, but you also want to understand how the toll system is going to affect peoples’ behaviour. If you change the tolls you’re going to presumably change peoples’ driving behaviour, so mechanism design gives you a method for evaluating the sensitivity of that behaviour to the policing instruments.  **In general is it used when people are trying to make these decisions? Are cities such as Stockholm do you think using mechanism design theory when they make their decisions?**  Eric S. Maskin: Typically, at least some cities. I can’t speak about the Stockholm case but Melbourne, Australia, for example did seek out the advice of well-trained economists when designing their system, so, yes, I think that’s an example of mechanism design in action.  **Excellent, thank you.**  Roger B. Myerson: My favourite example to think about is mediation and dispute resolution. When a mediator is going back and forth between two parties and the media is essentially creating a game by which they provide information to the mediator and a deal is put together that has certain characteristics that depend on what they say at various stages or there’s some positive probability of failure of the negotiation. That process should be understood as a mechanism design possibly. The mediators look in the middle. I think there’s some awareness of this. At Northwestern I worked with people in dispute resolution from a variety of methodologies and I think there’s still more that needs to be communicated. It’s very popular to study bargaining, how to negotiate something, but I think from my perspective, pedagogically, I think the study of bargaining should begin with a study of mediation and the theory of mediation begins with the concepts of mechanism design.  On the one hand you need to find out peoples’ trade-offs in order to find what they call win-win opportunities. What am I willing to give up that doesn’t mean so much to me but means a lot to me in exchange for something that you’re willing to give up that means a lot to me. But also, and that’s sort of the old-fashioned economics, the trade-off between two utility curves in an Edgeworth box diagram. Then there’s the problem of getting people not to pretend that they need everything and for that some positive probability of disagreement may be of the essence of the matter, and a mediator who doesn’t understand that is going to make more trouble than help sometimes. I think mediation in dispute resolution is a fundamental problem and it is an area where the ideas of mechanism design have been studied and have been applied to some extent but could be much more.  **That seems to open up the ground for an extension of mechanism design theory into political science and legal science or legal theory, and that’s already going on?**  Roger B. Myerson: I think the extension of economics methodology with game theory and mechanism design has enabled the boundaries between political science and economics which were academically artificial and to some extent not so productive. Because after all social problems are both in the market and in politics at many times. Those boundaries have been lowered, people can apply ideas from both sides and I think that this is very helpful. Much of what we do in political modelling is not necessarily mechanism design because it’s not necessarily having the kind of universal perspective. It’s often of the form as you change the constitutional structure, you’re changing the rules of the game the politicians play and how will that change political behaviour? That is the way we should approach any specific reform.  The question then is what would be an optimally designed constitution? That’s a very good question. I don’t think we’re very close to that. The most important part of mechanism design I see at this point is an understanding within politics itself, a political leader is creating an organisation, he’s got a faction of supporters, he’s got people he works with, and they all need to trust each other to make the campaign go for high office. Understanding the nature of politics by understanding the political leaders within their factions are solving a mechanism design problem. That I think we have something useful to say.  **Dwelling a little bit on the artificial academic separation between political science and economic science, is the separation very exact in university departments? Is a political science department and an economics department, are they very different entities still?**  Eric S. Maskin: I’m afraid in many universities they are. In fact, one of my complaints about even such a great university as Harvard – where I was for many years – is that people are very much separated by departments, that there’s not enough opportunity to talk across disciplinary lines. For that matter there are many political science departments which themselves are divided. There are the people within the political science departments who take what’s known as the rational choice route which is relatively close to the economics approach. But then are the institutionalists who eschew that kind of analysis and then there are the political theorists who are largely philosophers. Some political science departments are actually three little departments in one and there may not be very much communication amongst the three.  Roger B. Myerson: Political science departments typically don’t have a unifying analytical methodology the way that economics department do, so they do many things. The rational choice approach, let me say for a moment, I don’t think I have not lived my life in a perfectly rational manner, to the standards that we normally assume in economic models so I don’t see why I should think anybody else is perfectly rational, but the truth is for analysis of institutions, it helps to assume. You want to ask if you’re going to reform the structure of a constitution for example or change the structure of promotions and pay within a firm. You’re changing the rules of the game and it’s very useful to assume that people are rational and selfish, I should say, in the analysis of how the institutional structure will affect individual behaviour.  I think the fact that much of political science, they don’t invest in. There’s been a tradition of most political scientists not investing in the calculus of rational choice analysis. That has meant that analysis of alternative institutional structures. What would happen suppose we designed a constitution that combines presidential and parliamentary aspects and will we get something that’s the best of both worlds or will we get the worst of both worlds? For that we need some theoretical analysis and I think rational choice is the best or almost an essential tool for such. There are many ways of approaching truth in life. Political science departments have typically not gone into comparison of political institutions sometimes in the way that they could with a more rational choice analysis.  There are two ways of getting around that and both are happening. One is political science departments in recent decades have started more and more to have rational choice analysis, and the other is economics departments have promoted professors whose research was using economics, rational choice methodology, to study political institutions. That is to say the questions migrate to the economics department. There’s a third approach which is people in economics and political science departments could actually talk to each other. That does happen too. But I think in Europe in particular there’s been a very good expansion of interest in economics departments of studying questions that are about the nature of politics and political institutions.  **I think on the radical idea of economists and political scientists getting together and talking we might draw the interview to a close since the tapes are ending. But thank you very much indeed for taking the time to speak to us and I wish you a very pleasant Nobel Week in Stockholm.**  Eric S. Maskin: Thank you so much.  Roger B. Myerson: Thank you. |
| Interview |  |
| Q72 | It seems that regulation theory and applied regulation in general always tries to capture and control some market inefficiency or malfunction. But it seems this process goes in cycles: At the very moment the regulation is good enough and applied, the market creates a new inefficiency or product and/or learns how to escape from the grip of the applied framework. Do you believe your theory helps smoothen this dynamism in the long-run and how? |
|  | I like your suggestion that some part of the business cycle may be driven by evolving change in our financial markets and the need for the regulatory system to adjust with some lag. This is an interesting idea. Mechanism design is relevant to the question of designing regulatory systems, but the most important element in the phenomena that you describe is the great complexity of the evolving financial markets themselves. |
| Q72 | Is it possible to apply and have practical daily use of the mechanism design theory or similar analytical means in everyday governmental budget planning and execution decisions? |
|  | Budget planning and execution involve quantitative measures and benchmarks to guide and monitor agents’ activities, and these are central elements in the analysis of mechanism design problems. |
| Q18 | Is it economically viable to measure the performance of an individual based on the performance of otherwise interested subordinates? Most specifically, I refer to how many school systems budgets are affected by the performance of students, many of whom do not yet fully appreciate the value of education and therefore do not fully apply themselves to their studies. |
|  | The problem that you describe, of multiple agents who all affect the same outcome measure, is called moral hazard in teams. Unless we can find ways to measure individuals’ separate contributions, such situations often have free rider problems. |
| Q3 | Who, or what, inspired you to enter your field of achievement? |
|  | My parents and teachers and my friends encouraged and inspired me to work hard throughout my life. Reading Isaac Asimov’s book “Foundation” inspired me to think about a career in mathematical social science. |
| Q7 | In one word, can you describe your reaction when you knew you had been awarded the Nobel Prize? |
|  | Grateful. |
| Q11 | 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? |
|  | We all get tired and frustrated sometimes. But I do not remember seriously thinking about giving up my curiosity for questions in economics and other areas of social science. |
| Q3 | Congratulations for your well deserved awards. My question is: Who, of all the other Nobel Laureates in your field, would you most want to meet and why? |
|  | Among the Nobel Laureates whom I’ve never met, I would have especially liked to have met [Friedrich von Hayek](https://www.nobelprize.org/nobel_prizes/economics/laureates/1974/), whose beautiful 1945 paper was so influential in the area where I have worked. |
| Q26 | 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! |
|  | Thank you. We will not make any big changes in our lifestyle, but will be more comfortable in thinking about long-run planning for retirement. |
| Q4 | 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? |
|  | You are asking an important question, but it is hard to explain the answer. I tell students that their intuition about what to work on is their greatest asset as scholars, even if they cannot explain their intuition. You may start with a big important question that you know society needs to understand better, and then you carefully take a small part of the big question and try to find a way to make progress in it. Or you may start with a methodological innovation that seems very promising to you, and look for important problems where it can be applied. And always you keep reading about the good work that other people are doing, because that is the most common source of inspiration, if we can see a way to complement or extend their work. |