

You and Your Research

A talk by Richard Hamming (https://en.wikipedia.org/wiki/Richard_Hamming).

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Transcript

I have given a talk with this title many times, and it turns out from discussions after the talk I could have just as well have called it “You and Your Engineering Career,” or even “You and Your Career.” But I left the word “research” in the title because that is what I have most studied.

From the previous chapters, you have an adequate background for how I made the study, and I need not mention again the names of the famous people I have studied closely. The earlier chapters are, in a sense, just a great expansion, with much more detail, of the original talk. This chapter is, in a sense, a summary of the previous 29 chapters.

Why do I believe this talk is important? It is important because as far as I know each of you has but one life to lead, and it seems to me it is better to do significant things than to just get along through life to its end. Certainly, near the end, it is nice to look back at a life of accomplishments rather than a life where you have merely survived and amused yourself. Thus in a real sense, I am preaching the messages that (1) it is worth trying to accomplish the goals you set yourself and (2) it is worth setting yourself high goals.

Again, to be convincing to you I will talk mainly about my own experience, but there are equivalent stories I could use involving others. I want to get you to the state where you will say to yourself, “Yes, I would like to do first-class work. If Hamming could, then why not me?” Our society frowns on those who say this too loudly, but I only ask you say it to yourself! What you consider first-class work is up to you; you must pick your goals, but make them high!

I will start psychologically rather than logically. The major objection cited by people against striving to do great things is the belief that it is all a matter of luck. I have repeatedly cited Pasteur’s remark, “Luck favors the prepared mind.” It both admits there is an element of luck and yet claims to a great extent it is up to you. You prepare yourself to succeed or not, as you choose, from moment to moment by the way you live your life.

As an example related to the “luck” aspect, when I first came to Bell Telephone Laboratories I shared an office with Claude Shannon. At about the same time, he created information theory and I created coding theory. They were “in the air,” you can say, and you are right. Yet why did we do it and the others who were also there not do it? Luck? Some, perhaps, but also because we were what we were and the others were what they were. The differences were we were more prepared to find, work on, and create the corresponding theories.

If it were mainly luck, then great things should not tend to be done repeatedly by the same people. Shannon did a lot of important things besides *information theory*—his master’s thesis was applying Boolean algebra to switching circuits! Einstein did many great things, not just one or two. For example, when he was around 12–14 years old, he asked himself what light would look like if he went at the velocity of light. He would, apparently, see a local peak, yet the corresponding mathematical equations would not support a stationary extreme! An obvious contradiction! Is it surprising he later discovered *special relativity*, which was in the air and which many people were working on at that time? He had prepared himself long ago, by that early question, to understand better than the others what was going on and how to approach it.

Newton observed that if others would think as hard as he did, then they would be able to do the same things. Edison said genius was 99% perspiration and 1% inspiration. It is hard work, applied for long years, which leads to the creative act, and it is rarely just handed to you without any serious effort on your part. Yes, sometimes it just happens, and then it is pure luck. It seems to me to be folly for you to depend solely on luck for the outcome of this one life you have to lead.

One of the characteristics you see is that great people when young were generally active—though Newton did not seem exceptional until well into his undergraduate days at Cambridge, Einstein was not a great student, and many other great people were not at the top of their class.

Brains are nice to have, but many people who seem not to have great IQs have done great things. At Bell Telephone Laboratories Bill Pfann walked into my office one day with a problem in *zone melting*. He did not seem to me, then, to know much mathematics, to be articulate, or to have a lot of clever brains, but I had already learned brains come in many forms and flavors, and to beware of ignoring any chance I got to work with a good man. I first did a little analytical work on his equations, and soon realized what he needed was

computing. I checked up on him by asking around in his department, and I found they had a low opinion of him and his idea for zone melting. But that is not the first time a person has not been appreciated locally, and I was not about to lose my chance of working with a great idea—which is what zone melting seemed to me, though not to his own department! There is an old saying: “A prophet is without honor in his own country.” Mohammed fled from his own city to a nearby one, and there got his first real recognition!

So I helped Bill Pfann, taught him how to use the computer, how to get numerical solutions to his problems, and let him have all the machine time he needed. It turned out zone melting was just what we needed to purify materials for transistors, for example, and it has proved to be essential in many areas of work. He ended up with all the prizes in the field, much more articulate as his confidence grew, and the other day I found his old lab is now a part of a national monument! Ability comes in many forms, and on the surface the variety is great; below the surface there are many common elements.

Having disposed of the psychological objections of luck and the lack of high-IQ-type brains, let us go on to how to do great things. Among the important properties to have is the belief you can do important things. If you do not work on important problems, how can you expect to do important work? Yet direct observation and direct questioning of people show most scientists spend most of their time working on things they believe are not important and are not likely to lead to important things.

As an example, after I had been eating for some years with the physics table at the Bell Telephone Laboratories restaurant, fame, promotion, and hiring by other companies ruined the average quality of the people, so I shifted to the chemistry table in another corner of the restaurant. I began by asking what the important problems were in chemistry, then later what important problems they were working on, and finally one day said, “If what you are working on is not important and not likely to lead to important things, then why are you working on it?” After that I was not welcome and had to shift to eating with the engineers! That was in the spring, and in the fall one of the chemists stopped me in the hall and said, “What you said caused me to think for the whole summer about what the important problems are in my field, and while I have not changed my research it was well worth the effort.” I thanked him and went on—and noticed in a few months he was made head of the group. About ten years ago I saw he became a member of the National Academy of Engineering. No other person at the table did I ever hear of, and no other person was

capable of responding to the question I had asked: “Why are you not working on and thinking about the important problems in your area?” If you do not work on important problems, then it is obvious you have little chance of doing important things.

Confidence in yourself, then, is an essential property. Or, if you want to, you can call it “courage.” Shannon had courage. Who else but a man with almost infinite courage would ever think of averaging over all random codes and expect the average code would be good? He knew what he was doing was important and pursued it intensely. Courage, or confidence, is a property to develop in yourself. Look at your successes, and pay less attention to failures than you are usually advised to do in the expression, “Learn from your mistakes.” While playing chess Shannon would often advance his queen boldly into the fray and say, “I ain’t scared of nothing.” I learned to repeat it to myself when stuck, and at times it has enabled me to go on to a success. I deliberately copied a part of the style of a great scientist. The courage to continue is essential, since great research often has long periods with no success and many discouragement.

The desire for excellence is an essential feature for doing great work. Without such a goal you will tend to wander like a drunken sailor. The sailor takes one step in one direction and the next in some independent direction. As a result, the steps tend to cancel each other out, and the expected distance from the starting point is proportional to the square root of the number of steps taken. With a vision of excellence, and with the goal of doing significant work, there is a tendency for the steps to go in the same direction and thus go a distance proportional to the number of steps taken, which in a lifetime is a large number indeed. As noted before, in Chapter 1, the difference between having a vision and not having a vision is almost everything, and doing excellent work provides a goal which is steady in this world of constant change.

Age is a factor physicists and mathematicians worry about. It is easily observed that the greatest work of a theoretical physicist, mathematician, or astrophysicist is generally done very early. They may continue to do good work all their lives, but what society ends up valuing most is almost always their earliest great work. The exceptions are very, very few indeed. But in literature, music composition, and politics, age seems to be an asset. The best compositions of a composer are usually the late ones, as judged by popular opinion. One reason for this is that fame in science is a curse to quality productivity, though it tends to supply all the tools and freedom you want to do great things. Another reason is that most famous people, sooner or later, tend to think they can only work on important problems—

hence they fail to plant the little acorns which grow into the mighty oak trees. I have seen it many times, from Brattain of transistor fame and a Nobel Prize to Shannon and his *information theory*. Not that you should merely work on random things, but on small things which seem to you to have the possibility of future growth. In my opinion, the Institute for Advanced Study at Princeton has ruined more great scientists than any other place has created—considering what they did before and what they did after going there. A few, like von Neumann, escaped the closed atmosphere of the place, with all its physical comforts and prestige, and continued to contribute to the advancement of science, but most remained there and continued to work on the same problems which got them there but which were generally no longer of great importance to society.

Thus what you consider to be good working conditions may not be good for you! There are many illustrations of this point. For example, working with one's door closed lets you get more work done per year than if you had an open door, but I have observed repeatedly that later those with the closed doors, while working just as hard as others, seem to work on slightly the wrong problems, while those who have let their door stay open get less work done but tend to work on the right problems! I cannot prove the cause-and-effect relationship; I can only observe the correlation. I suspect the open mind leads to the open door, and the open door tends to lead to the open mind; they reinforce each other.

A similar story from my own experience. In the early days of programming computers in absolute binary, the usual approach was usually through an “acre of programmers.” It was soon evident to me that Bell Telephone Laboratories would never give me an acre of programmers. What to do? I could go to a West Coast airframe manufacturer and get a job and have the proverbial acre, but Bell Telephone Laboratories had a fascinating collection of great people from whom I could learn a lot, and the airframe manufacturers had relatively fewer such people. After quite a few weeks of wondering what to do I finally said to myself, “Hamming, you believe machines can do symbol manipulation. Why not get them to do the details of the programming?” Thus I was led directly to a frontier of computer science by simply inverting the problem. What had seemed to be a defect now became an asset and pushed me in the right direction! Grace Hopper had a number of similar stories from computer science, and there are many other stories with the same moral: When stuck, often inverting the problem and realizing the new formulation is better represents a significant

step forward. I am not asserting all blockages can be so rearranged, but I am asserting that many more than you might at first suspect can be so changed from a more or less routine response to a great one.

This is related to another aspect of changing the problem. I was once solving on a digital computer the first really large simulation of a system of simultaneous differential equations, which at that time were the natural problem for an analog computer—but they had not been able to do it, and I was doing it on an IBM 701. The method of integration was an adaptation of the classical Milne's method, and it was ugly to say the least. I suddenly realized that of course, being a military problem, I would have to file a report on how it was done, and every analog installation would go over it trying to object to what was actually being proved as against just getting the answers—I was showing convincingly that on some large problems, the digital computer could beat the analog computer on its own home ground. Realizing this, I realized the method of solution should be cleaned up, so I developed a new method of integration which had a nice theory, changed the method on the machine with a change of comparatively few instructions, and then computed the rest of the trajectories using the new formula. I published the new method and for some years it was in wide use and known as "Hamming's method." I do not recommend the method now that further progress has been made and the computers are different. To repeat the point I am making, I changed the problem from just getting answers to the realization I was demonstrating clearly for the first time the superiority of digital computers over the current analog computers, thus making a significant contribution to the science behind the activity of computing answers.

All these stories show that the conditions you tend to want are seldom the best ones for you—the interaction with harsh reality tends to push you into significant discoveries which otherwise you would never have thought about while doing pure research in a vacuum of your private interests.

Now to the matter of *drive*. Looking around, you can easily observe that great people have a great deal of drive to do things. I had worked with John Tukey for some years before I found he was essentially my age, so I went to our mutual boss and asked him, "How can anyone my age know as much as John Tukey does?" He leaned back, grinned, and said, "You would be surprised how much you would know if you had worked as hard as he has for as many

years.” There was nothing for me to do but slink out of his office, which I did. I thought about the remark for some weeks and decided that while I could never work as hard as John did, I could do a lot better than I had been doing.

In a sense my boss was saying intellectual investment is like compound interest: the more you do, the more you learn how to do, so the more you can do, etc. I do not know what compound interest rate to assign, but it must be well over 6%—one extra hour per day over a lifetime will much more than double the total output. The steady application of a bit more effort has a great total accumulation.

But be careful—the race is not to the one who works hardest! You need to work on the right problem at the right time and in the right way—what I have been calling “style.” At the urging of others, for some years I set aside Friday afternoons for “great thoughts.” Of course, I would answer the telephone, sign a letter, and such trivia, but essentially, once lunch started, I would only think great thoughts—what was the nature of computing, how would it affect the development of science, what was the natural role of computers in Bell Telephone Laboratories, what effect will computers have on AT&T, on science generally? I found it was well worth the 10% of my time to do this careful examination of where computing was heading so I would know where we were going and hence could go in the right direction. I was not the drunken sailor staggering around and canceling many of my steps by random other steps, but could progress in a more or less straight line. I could also keep a sharp eye on the important problems and see that my major effort went to them.

I strongly recommend taking the time, on a regular basis, to ask the larger questions, and not stay immersed in the sea of detail where almost everyone stays almost all of the time. These chapters have regularly stressed the bigger picture, and if you are to be a leader into the future, rather than a follower of others, I am now saying it seems to me to be necessary for you to look at the bigger picture on a regular, frequent basis for many years.

There is another trait of great people I must talk about—and it took me a long time to realize it. Great people can tolerate ambiguity; they can both believe and disbelieve at the same time. You must be able to believe your organization and field of research is the best there is, but also that there is much room for improvement! You can sort of see why this is a necessary trait. If you believe too much, you will not likely see the chances for significant improvements; if you do not believe enough, you will be filled with doubts and get very little

done, chances are only the 2%, 5%, and 10% improvements. I have not the faintest idea of how to teach the tolerance of ambiguity, both belief and disbelief at the same time, but great people do it all the time.

Most great people also have 10 to 20 problems they regard as basic and of great importance, and which they currently do not know how to solve. They keep them in their mind, hoping to get a clue as to how to solve them. When a clue does appear they generally drop other things and get to work immediately on the important problem. Therefore they tend to come in first, and the others who come in later are soon forgotten. I must warn you, however, that the importance of the result is not the measure of the importance of the problem. The three problems in physics—anti-gravity, teleportation, and time travel—are seldom worked on because we have so few clues as to how to start. A problem is important partly because there is a possible attack on it and not just because of its inherent importance.

There have been a number of times in this book when I came close to the point of saying it is not so much what you do as how you do it. I just told you about the changing of the problem of solving a given set of differential equations on an analog machine to doing it on a digital computer, changing programming from an acre of programmers to letting the machine do much of the mechanical part, and there are many similar stories. Doing the job with “style” is important. As the old song says, “It ain’t what you do, it’s the way that you do it.” Look over what you have done, and recast it in a proper form. I do not mean give it false importance, nor propagandize for it, nor pretend it is not what it is, but I do say that by presenting it in its basic, fundamental form, it may have a larger range of application than was first thought possible.

Again, you should do your job in such a fashion that others can build on top of it. Do not in the process try to make yourself indispensable; if you do, then you cannot be promoted, because you will be the only one who can do what you are now doing! I have seen a number of times where this clinging to the exclusive rights to the idea has in the long run done much harm to the individual and to the organization. If you are to get recognition then others must use your results, adopt, adapt, extend, and elaborate them, and in the process give you credit for it. I have long held the attitude of telling everyone freely of my ideas, and in my long career I have had only one important idea “stolen” by another person. I have found people are remarkably honest if you are in your turn.

It is a poor workman who blames his tools. I have always tried to adopt the philosophy that I will do the best I can in the given circumstances, and after it is all over maybe I will try to see to it that things are better next time. This school is not perfect, but for each class I try to do as well as I can and not spend my effort trying to reform every small blemish in the system. I did change Bell Telephone Laboratories significantly, but did not spend much effort on trivial details. I let others do that if they wanted to—but I got on with the main task as I saw it. Do you want to be a reformer of the trivia of your old organization or a creator of the new organization? Pick your choice, but be clear which path you are going down.

I must come to the topic of “selling” new ideas. You must master three things to do this (Chapter 5):

1. Giving formal presentations,
2. Producing written reports, and
3. Mastering the art of informal presentations as they happen to occur.

All three are essential—you must learn to sell your ideas, not by propaganda, but by force of clear presentation. I am sorry to have to point this out; many scientists and others think good ideas will win out automatically and need not be carefully presented. They are wrong; many a good idea has had to be rediscovered because it was not well presented the first time, years before! New ideas are automatically resisted by the establishment, and to some extent justly. The organization cannot be in a continual state of ferment and change, but it should respond to significant changes.

Change does not mean progress, but progress requires change.

To master the presentation of ideas, while books on the topic may be partly useful, I strongly suggest you adopt the habit of privately critiquing all presentations you attend and also asking the opinions of others. Try to find those parts which you think are effective and which also can be adapted to your style. And this includes the gentle art of telling jokes at times. Certainly, a good after-dinner speech requires three well-told jokes: one at the beginning, one in the middle to wake them up again, and the best one at the end so they will remember at least one thing you said!

You are likely to be saying to yourself you have not the freedom to work on what you believe you should when you want to. I did not either for many years—I had to establish the reputation on my own time that I could do important work, and only then was I given the time to do it. You do not hire a plumber to learn plumbing while trying to fix your trouble; you expect he is already an expert. Similarly, only when you have developed your abilities will you generally get the freedom to practice your expertise, whatever you choose to make it, including the expertise of “universality” as I did. I have already discussed the gentle art of educating your bosses, so I will not go into it again. It is part of the job of those who are going to rise to the top. Along the way you will generally have superiors who are less able than you are, so do not complain, since how else could it be if you are going to end up at the top and they are not?

Finally, I must address the topic of whether the effort required for excellence worth it. I believe it is—the chief gain is in the effort to change yourself, in the struggle with yourself, and it is less in the winning than you might expect. Yes, it is nice to end up where you wanted to be, but the person you are when you get there is far more important. I believe a life in which you do not try to extend yourself regularly is not worth living—but it is up to you to pick the goals you believe are worth striving for. As Socrates (469– 399 BC) said, **The unexamined life is not worth living.**

In summary: as I claimed at the start, the essence of the book is “style,” and there is no real content in the form of the topics like coding theory, filter theory, or simulation that were used for examples. I repeat: the content of these chapters is “style” of thinking, which I have tried to exhibit in many forms. It is your problem to pick out those parts you can adapt to your life as you plan it to be. A plan for the future, I believe, is essential for success, otherwise you will drift like the drunken sailor through life and accomplish much less than you could otherwise have done.

In a sense, this has been a course a revivalist preacher might have given—repent your idle ways, and in the future strive for greatness as you see it. I claim it is generally easier to succeed than it at first seems! It seems to me at almost all times there is a halo of opportunities about everyone from which to select. It is your life you have to live, and I am only one of many possible guides you have for selecting and creating the style of the one life you have to live. Most of the things I have been saying were not said to me; I had to discover them for myself. I have now told you in some detail how to succeed, hence you have no excuse for not doing better than I did. Good luck!

Questions and Answers

A. G. Chynoweth: Well that was 50 minutes of concentrated wisdom and observations accumulated over a fantastic career; I lost track of all the observations that were striking home. Some of them are very very timely. One was the plea for more computer capacity; I was hearing nothing but that this morning from several people, over and over again. So that was right on the mark today even though here we are 20 – 30 years after when you were making similar remarks, Dick. I can think of all sorts of lessons that all of us can draw from your talk. And for one, as I walk around the halls in the future I hope I won't see as many closed doors in Bellcore. That was one observation I thought was very intriguing.

Thank you very, very much indeed Dick; that was a wonderful recollection. I'll now open it up for questions. I'm sure there are many people who would like to take up on some of the points that Dick was making.

Hamming: First let me respond to Alan Chynoweth about computing. I had computing in research and for 10 years I kept telling my management, "Get that !&% machine out of research. We are being forced to run problems all the time. We can't do research because we're too busy operating and running the computing machines." Finally the message got through. They were going to move computing out of research to someplace else. I was persona non grata to say the least and I was surprised that people didn't kick my shins because everybody was having their toy taken away from them. I went in to Ed David's office and said, "Look Ed, you've got to give your researchers a machine. If you give them a great big machine, we'll be back in the same trouble we were before, so busy keeping it going we can't think. Give them the smallest machine you can because they are very able people. They will learn how to do things on a small machine instead of mass computing." As far as I'm concerned, that's how UNIX arose. We gave them a moderately small machine and they decided to make it do great things. They had to come up with a system to do it on. It is called UNIX!

A. G. Chynoweth: I just have to pick up on that one. In our present environment, Dick, while we wrestle with some of the red tape attributed to, or required by, the regulators, there is one quote that one exasperated AVP came up with and I've used it over and over again. He growled that, "UNIX was never a deliverable!"

Question: What about personal stress? Does that seem to make a difference?

Hamming: Yes, it does. If you don't get emotionally involved, it doesn't. I had incipient ulcers most of the years that I was at Bell Labs. I have since gone off to the Naval Postgraduate School and laid back somewhat, and now my health is much better. But if you want to be a great scientist you're going to have to put up with stress. You can lead a nice life; you can be a nice guy or you can be a great scientist. But nice guys end last, is what Leo Durocher said. If you want to lead a nice happy life with a lot of recreation and everything else, you'll lead a nice life.

Question: The remarks about having courage, no one could argue with; but those of us who have gray hairs or who are well established don't have to worry too much. But what I sense among the young people these days is a real concern over the risk taking in a highly competitive environment. Do you have any words of wisdom on this?

Hamming: I'll quote Ed David more. Ed David was concerned about the general loss of nerve in our society. It does seem to me that we've gone through various periods. Coming out of the war, coming out of Los Alamos where we built the bomb, coming out of building the radars and so on, there came into the mathematics department, and the research area, a group of people with a lot of guts. They've just seen things done; they've just won a war which was fantastic. We had reasons for having courage and therefore we did a great deal. I can't arrange that situation to do it again. I cannot blame the present generation for not having it, but I agree with what you say; I just cannot attach blame to it. It doesn't seem to me they have the desire for greatness; they lack the courage to do it. But we had, because we were in a favorable circumstance to have it; we just came through a tremendously successful war. In the war we were looking very, very bad for a long while; it was a very desperate struggle as you well know. And our success, I think, gave us courage and self confidence; that's why you see, beginning in the late forties through the fifties, a tremendous productivity at the labs which was stimulated from the earlier times. Because many of us were earlier forced to learn other things — we were forced to learn the things we didn't want to learn, we were forced to have an open door — and then we could exploit those things we learned. It is true, and I can't do anything about it; I cannot blame the present generation either. It's just a fact.

Question: Is there something management could or should do?

Hamming: Management can do very little. If you want to talk about managing research, that's a totally different talk. I'd take another hour doing that. This talk is about how the individual gets very successful research done in spite of anything the management does or in spite of any other opposition. And how do you do it? Just as I observe people doing it. It's just that simple and that hard!

Question: Is brainstorming a daily process?

Hamming: Once that was a very popular thing, but it seems not to have paid off. For myself I find it desirable to talk to other people; but a session of brainstorming is seldom worthwhile. I do go in to strictly talk to somebody and say, "Look, I think there has to be something here. Here's what I think I see ..." and then begin talking back and forth. But you want to pick capable people. To use another analogy, you know the idea called the 'critical mass.' If you have enough stuff you have critical mass. There is also the idea I used to call 'sound absorbers.' When you get too many sound absorbers, you give out an idea and they merely say, "Yes, yes, yes." What you want to do is get that critical mass in action; "Yes, that reminds me of so and so," or, "Have you thought about that or this?" When you talk to other people, you want to get rid of those sound absorbers who are nice people but merely say, "Oh yes," and to find those who will stimulate you right back.

For example, you couldn't talk to John Pierce without being stimulated very quickly. There were a group of other people I used to talk with. For example there was Ed Gilbert; I used to go down to his office regularly and ask him questions and listen and come back stimulated. I picked my people carefully with whom I did or whom I didn't brainstorm because the sound absorbers are a curse. They are just nice guys; they fill the whole space and they contribute nothing except they absorb ideas and the new ideas just die away instead of echoing on. Yes, I find it necessary to talk to people. I think people with closed doors fail to do this so they fail to get their ideas sharpened, such as "Did you ever notice something over here?" I never knew anything about it — I can go over and look. Somebody points the way. On my visit here, I have already found several books that I must read when I get home. I talk to people and ask questions when I think they can answer me and give me clues that I do not know about. I go out and look!

Question: What kind of tradeoffs did you make in allocating your time for reading and writing and actually doing research?

Hamming: I believed, in my early days, that you should spend at least as much time in the polish and presentation as you did in the original research. Now at least 50% of the time must go for the presentation. It's a big, big number.

Question: How much effort should go into library work?

Hamming: It depends upon the field. I will say this about it. There was a fellow at Bell Labs, a very, very, smart guy. He was always in the library; he read everything. If you wanted references, you went to him and he gave you all kinds of references. But in the middle of forming these theories, I formed a proposition: there would be no effect named after him in the long run. He is now retired from Bell Labs and is an Adjunct Professor. He was very valuable; I'm not questioning that. He wrote some very good Physical Review articles; but there's no effect named after him because he read too much. If you read all the time what other people have done you will think the way they thought. If you want to think new thoughts that are different, then do what a lot of creative people do — get the problem reasonably clear and then refuse to look at any answers until you've thought the problem through carefully how you would do it, how you could slightly change the problem to be the correct one. So yes, you need to keep up. You need to keep up more to find out what the problems are than to read to find the solutions. The reading is necessary to know what is going on and what is possible. But reading to get the solutions does not seem to be the way to do great research. So I'll give you two answers. You read; but it is not the amount, it is the way you read that counts.

Question: How do you get your name attached to things?

Hamming: By doing great work. I'll tell you the hamming window one. I had given Tukey a hard time, quite a few times, and I got a phone call from him from Princeton to me at Murray Hill. I knew that he was writing up power spectra and he asked me if I would mind if he called a certain window a "hamming window." And I said to him, "Come on, John; you know perfectly well I did only a small part of the work but you also did a lot." He said, "Yes, Hamming, but you contributed a lot of small things; you're entitled to some credit." So he called it the hamming window. Now, let me go on. I had twitted John frequently about true greatness. I said true greatness is when your name is like ampere, watt, and fourier — when it's spelled with a lower case letter. That's how the hamming window came about.

Question: Dick, would you care to comment on the relative effectiveness between giving talks, writing papers, and writing books?

Hamming: In the short-haul, papers are very important if you want to stimulate someone tomorrow. If you want to get recognition long-haul, it seems to me writing books is more contribution because most of us need orientation. In this day of practically infinite knowledge, we need orientation to find our way. Let me tell you what infinite knowledge is. Since from the time of Newton to now, we have come close to doubling knowledge every 17 years, more or less. And we cope with that, essentially, by specialization. In the next 340 years at that rate, there will be 20 doublings, i.e. a million, and there will be a million fields of specialty for every one field now. It isn't going to happen. The present growth of knowledge will choke itself off until we get different tools. I believe that books which try to digest, coordinate, get rid of the duplication, get rid of the less fruitful methods and present the underlying ideas clearly of what we know now, will be the things the future generations will value. Public talks are necessary; private talks are necessary; written papers are necessary. But I am inclined to believe that, in the long-haul, books which leave out what's not essential are more important than books which tell you everything because you don't want to know everything. I don't want to know that much about penguins is the usual reply. You just want to know the essence.

Question: You mentioned the problem of the Nobel Prize and the subsequent notoriety of what was done to some of the careers. Isn't that kind of a much more broad problem of fame? What can one do?

Hamming: Some things you could do are the following. Somewhere around every seven years make a significant, if not complete, shift in your field. Thus, I shifted from numerical analysis, to hardware, to software, and so on, periodically, because you tend to use up your ideas. When you go to a new field, you have to start over as a baby. You are no longer the big mukity muk and you can start back there and you can start planting those acorns which will become the giant oaks. Shannon, I believe, ruined himself. In fact when he left Bell Labs, I said, "That's the end of Shannon's scientific career." I received a lot of flak from my friends who said that Shannon was just as smart as ever. I said, "Yes, he'll be just as smart, but that's the end of his scientific career," and I truly believe it was.

You have to change. You get tired after a while; you use up your originality in one field. You need to get something nearby. I'm not saying that you shift from music to theoretical physics to English literature; I mean within your field you should shift areas so that you don't go stale. You couldn't get away with forcing a change every seven years, but if you could, I would require a condition for doing research, being that you will change your field of research every seven years with a reasonable definition of what it means, or at the end of 10 years, management has the right to compel you to change. I would insist on a change because I'm serious. What happens to the old fellows is that they get a technique going; they keep on using it. They were marching in that direction which was right then, but the world changes. There's the new direction; but the old fellows are still marching in their former direction.

You need to get into a new field to get new viewpoints, and before you use up all the old ones. You can do something about this, but it takes effort and energy. It takes courage to say, "Yes, I will give up my great reputation." For example, when error correcting codes were well launched, having these theories, I said, "Hamming, you are going to quit reading papers in the field; you are going to ignore it completely; you are going to try and do something else other than coast on that." I deliberately refused to go on in that field. I wouldn't even read papers to try to force myself to have a chance to do something else. I managed myself, which is what I'm preaching in this whole talk. Knowing many of my own faults, I manage myself. I have a lot of faults, so I've got a lot of problems, i.e. a lot of possibilities of management.

Question: Would you compare research and management?

Hamming: If you want to be a great researcher, you won't make it being president of the company. If you want to be president of the company, that's another thing. I'm not against being president of the company. I just don't want to be. I think Ian Ross does a good job as President of Bell Labs. I'm not against it; but you have to be clear on what you want. Furthermore, when you're young, you may have picked wanting to be a great scientist, but as you live longer, you may change your mind. For instance, I went to my boss, Bode, one day and said, "Why did you ever become department head? Why didn't you just be a good scientist?" He said, "Hamming, I had a vision of what mathematics should be in Bell Laboratories. And I saw if that vision was going to be realized, I had to make it happen; I had to be department head." When your vision of what you want to do is what you can do single-handedly, then you should pursue it. The day your vision, what you think needs to be

done, is bigger than what you can do single-handedly, then you have to move toward management. And the bigger the vision is, the farther in management you have to go. If you have a vision of what the whole laboratory should be, or the whole Bell System, you have to get there to make it happen. You can't make it happen from the bottom very easily. It depends upon what goals and what desires you have. And as they change in life, you have to be prepared to change. I chose to avoid management because I preferred to do what I could do single-handedly. But that's the choice that I made, and it is biased. Each person is entitled to their choice. Keep an open mind. But when you do choose a path, for heaven's sake be aware of what you have done and the choice you have made. Don't try to do both sides.

Question: How important is one's own expectation or how important is it to be in a group or surrounded by people who expect great work from you?

Hamming: At Bell Labs everyone expected good work from me — it was a big help. Everybody expects you to do a good job, so you do, if you've got pride. I think it's very valuable to have first-class people around. I sought out the best people. The moment that physics table lost the best people, I left. The moment I saw that the same was true of the chemistry table, I left. I tried to go with people who had great ability so I could learn from them and who would expect great results out of me. By deliberately managing myself, I think I did much better than laissez faire.

Question: You, at the outset of your talk, minimized or played down luck; but you seemed also to gloss over the circumstances that got you to Los Alamos, that got you to Chicago, that got you to Bell Laboratories.

Hamming: There was some luck. On the other hand I don't know the alternate branches. Until you can say that the other branches would not have been equally or more successful, I can't say. Is it luck the particular thing you do? For example, when I met Feynman at Los Alamos, I knew he was going to get a Nobel Prize. I didn't know what for. But I knew darn well he was going to do great work. No matter what directions came up in the future, this man would do great work. And sure enough, he did do great work. It isn't that you only do a little great work at this circumstance and that was luck, there are many opportunities sooner or later. There are a whole pail full of opportunities, of which, if you're in this situation, you seize one and you're great over there instead of over here. There is an element of luck, yes

and no. Luck favors a prepared mind; luck favors a prepared person. It is not guaranteed; I don't guarantee success as being absolutely certain. I'd say luck changes the odds, but there is some definite control on the part of the individual.

Go forth, then, and do great work!

Source: <http://www.cs.virginia.edu/~robins/YouAndYourResearch.pdf>