Richard Hamming

" You and Your Research"

Transcription of the
Bell Communications Research Colloquium Seminar
on March 7, 1986

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At a seminar in the Bell Communications Research Colloquia Series, Dr. Richard W. Hamming, a Professor of the Naval Postgraduate School in Monterey, California and a retired Bell Labs scientist, gave a very interesting and stimulating talk, 'You and Your Research to an overflow audience of some 200. Bellcore staff members and visitors to the Morristown, NJ, area received the talk via a live television broadcast on March 7, 1986. This talk centered on Hamming's observations and research on the question "Why are few scientists make significant contributions and as many are forgotten in the long run? From his more than forty years of experience, thirty of which were at Bell Laboratories, he has made a number of direct observations, asked very pointed questions about research and scientists, and has drawn some conclusions. These conclusions suggest ways to improve the quality of research. The following is a transcription and edited version of the recording. The talk is about what he has learned in terms of the properties of the individual scientist, their attitudes, traits, working habits, attitudes, and philosophy.

In order to make the information in the talk more widely available, the tape recording that was made of the talk was carefully transcribed. This transcription includes the discussion which followed in the question and answer period. As with any talk, the transcribed version suffers from translation as all the inflections of voice and the gestures of the speaker are lost, one must listen to the tape to recapture that part of the presentation. While the recording of Hamming's talk is very conversational and fairly completely intelligible, that of some of the questioner's remarks were not. Where the tape recording was not intelligible I have added in parentheses my impressions of the questioner's remarks. Where there was a question and I could identify the questioner I have checked with each to ensure the accuracy of my interpretation of their remarks.

INTRODUCTION OF DR. RICHARD W. HAMMING

As a speaker in the Bell Communications Research Colloquium Series, Dr. Richard W. Hamming of the Naval Postgraduate School in Monterey, California, was introduced by Alan G. Chynoweth, Vice President, Applied Research, Bell Communications Research.

Dick is one of the pioneers in computer mathematics, and also in many of our former colleagues from Bell Labs who, I understand, are here to be with us today on what I regard as a particularly felicitous occasion. It gives me very great pleasure indeed to introduce to you my old friend and colleague from many many years back, Richard Hamming. Dick Hamming as he has always been known to all of us.

Dick is one of the great mathematicians who have contributed so much to the development of computer mathematics. In recognition of his early education at the Universities of Chicago and Nebraska, and join Ph.D. at Illinois, he then joined the Los Alamos project during the war. Afterwards, in 1946, he joined Bell Labs. And that is, of course, where I met Dick - when I joined Bell Labs in their physics research organization. In those days, we were in the habit of lunching together as a group of people who had offices in the area of the Bell Labs in Manhattan. And it was in those lunchtime discussions that I first became acquainted with the breadth of Dick Hamming's interests and his ability to express himself on almost any topic.

Dick's most professional paths have not been very close over the years, nevertheless I've always recognized Dick to be a leader in the field of Bell Labs and have always had tremendous admiration for what he was doing. I think the record speaks for itself. It is too long to go through all the details, but let me point out, for example, that he has written seven books and of those seven books which tell of various areas of mathematics and computers and coding and information theory, there are already well into three of them classics in their respective fields. I'm referring to books such as the one on numerical analysis.

I think I last met him - it must have been about ten years ago - at a rather custom little conference in Dallas, Ireland where we were both speakers. As always, he was tremendously entertaining. Just one more example of the provocative thoughts that he brought to bear on the subject at hand. I'm very pleased that he has accepted our invitation to be guest speaker today. Dick, we are very much looking forward to your talk. And, maybe you can begin by telling us what you are doing at Monterey.

THE TALK: "You and Your Research" by Dr. Richard W. Hamming

It's a pleasure to be here. I doubt if I can live up to the introduction. The title of my talk is, "You and Your Research." It is not about managing research, it is about how you individually do your research. I could give a talk on the subject - that's a talk I've, in about my free time, prepared and delivered to various audiences - but I've never prepared such a talk formally. Several years ago I was invited to Washington to talk about my research, and I spent the whole week preparing that talk. Several times during the week I would be invited into someone's office to discuss some aspect of my research, and I would pull out my notes for the talk and we would spend the time discussing those kinds of things which we perceive are significant things. Relatively, of you, I was Shannon's information theory, any number of outstanding theorems - that's the kind of thing I'm talking about.

Now, how did I prepare for that talk? I took my notes and I threw them in the wastebasket. From then on I talked from the heart rather than from the notes. Of course, I had to do my homework to be able to talk about these things, but I'm now talking about how you do your own research, not how I do mine. I'm trying to tell you what I think you should be thinking about if you want to do something like what I do. The way I do it may not be the best way for you, but it is the way I do it. The fact is that I've been doing research for a long time and I've learned a great deal about how I do research, and I'm willing to tell you about it.

Let me start out logically, but psychologically, I find that the major objection is that people think great science is done by luck. It's all a matter of luck. Well, consider Einstein. Note how many different things he did that were good. Was it all lucky that he married his first cousin? Consider Maxwell. He had a quite unusual education for the Scottish upper class. Was it lucky that he had that quite unusual education? I'm willing to argue that none of his things were lucky.

You see again and again, that it is more than one thing from a good person. Once in a while a person does only one thing in his whole life, and we talk about that, but a lot of times there is repetition. I claim that luck will not cover everything. I'm willing to argue that point with anyone else. I claim that luck will not cover everything in the case of Einstein, and I'm willing to argue that luck will not cover everything in the case of Maxwell and many others as well.

For example, when I came to Bell Labs, I shared an office for a while with Shannon. At the same time he was doing information theory. I was doing coding theory. It is impossible that two of us did it in the same place and at the same time - it was too much luck to have two people in the same place doing almost the same things at the same time - but we did it, both of us. Shannon has said that his mouse was as good as mine. I'm willing to believe that. I'm willing to argue that he's better than I am, but we both were doing it and we were doing it with about the same quality of work. Although I'll come back several more times to luck, I want to dispose of this matter of luck as being the sole criterion whether you do good work or not. I claim you have some, but not total, control over it. And I will quote, as I have many times before, the Roman philosopher who said, "Fortune favors the prepared mind."

One of the characteristics you see, and many people have it including great scientists, is that usually when they were young they had independent thoughts and had the courage to pursue them. For example, Einstein, somewhere around 12 or 16, asked the question, "What would I see if I rode along with a beam of light?" He was young and he had the courage to ask that question and he pursued it. Maxwell, Shannon, Carver, and many others have had this quality. Some of them lose it as they get older, and others don't. I'm not quite sure why, but I'm trying to tell you what I've observed. You can't do great work if you don't have some quality of this sort. It doesn't quite guarantee that you will do great work, but it's a necessary condition that you have to have at least some of this quality.

In order to get it you must obviously, I must talk in the first person. I have to get you to drop modesty and say to yourself, "Yes, I would like to do first-class work." Occasionally forces on people who set out to do really good work. You're not supposed to be, but it is supposed to decrease and you will you do great things by chance. Well, that's a kind of dumb thing to say. You can't depend on luck, you can't set out to do something significant. You don't have to kill other people, but shouldn't you set yourself, "Yes, I would like to do something significant."

In order to get to the second stage, I have to drop modesty and talk to the first person about what I've seen, what I've done, and what I've heard. I'm going to talk about people, some of whom you know, and I feel that when we know, you won't quote me as saying this about them.

Let me start out logically, but psychologically, I find that the major objection is that people think great science is done by luck. It's all a matter of luck. Well, consider Einstein. Note how many different things he did that were good. Was it all luck that he married his first cousin? Consider Maxwell. He had a quite unusual education for the Scottish upper class. Was it lucky that he had that quite unusual education? I'm willing to argue that none of his things were lucky.

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But let me say why age seems to have the effect it does. In the first place if you do some good work you will find yourself on all kinds of committees and unable to do any more work. You may find yourself as I am Braman when he gave Bobbi Fano a tribute on her retirement, he said that she had been on committees so long that she had become committee material. And I'm on committees so much that I'm committee material. Well, committees are the place where you can't do great work but you can kill great ideas very easily in committees, and so you get committee material that doesn't do great work. So I'm not saying that committees are the reason for age, but I'm saying that committees have something to do with it.

When you are promoted to leader you will find you have less and less time to think about the problems you want to think about. Management is a very time-consuming business - I'm speaking from experience. They still want you to think, but they want you to think about their problems, not your own. They want you to be their leader, their representative, their voice, their image. The more you are in that position, the less you can think about the problems that you want to think about.

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The management will also select problems for you which are quite different than those you would select if you were free to select your own problems. In the first place they will select problems they think they understand something about, and secondly they will select problems that they think need to be solved badly enough so they are willing to devote resources to them. In contrast, the problems you select to work on in your own field, you understand more about the importance of those than anyone else does at first, and secondly, you can't get anyone else interested in them until you've been able to show how important they are. Thirdly, you can't get much done on your own in your own field if you have to depend on others to help you. You can't make it happen from the bottom very easily. It depends upon small groups and what direction you have. And so things like, you have to be prepared to change. I chose to avoid management because I like to think about the problems I want to think about, and management will take you away from that. Of course, you can't do great work in science unless you know what is important and what is unimportant.

Occasionally, one can do great work in something that is not important, but it is very difficult. Let me illustrate with a story. Suppose there is a field in which everyone is trying to climb a fence. They are climbing the fence in various ways and they spend their time climbing the fence. Now, I come along and I'm a good fencer and I climb the fence in a certain way. I'm climbing the same fence they are climbing but I'm climbing it in a different way. If I'm climbing it in a different way I can see that they are all climbing the fence in such and such a way and I can point out to them that there is a better way to climb fences. If I'm climbing the fence in a different way I can see that they are climbing the fence in such and such a way and I can point out to them that there is a better way to climb fences. If I'm climbing the fence in a different way I can see that they are climbing the fence in such and such a way and I can point out to them that there is a better way to climb fences.

Now, suppose everyone is interested in climbing fences and everyone is climbing fences, I think that if you look carefully you can find other ways of climbing fences. Now, suppose there is a new kind of fence. Now, I can see that they are climbing this fence in such and such a way and I can point out to them that there is a better way to climb this fence. Now, suppose there is a field in which everyone is climbing fences and I can climb fences in a different way and I can see that they are climbing fences in such and such a way and I can point out that there is a better way to climb fences, there is a good chance that I can get a paper published. There is a good chance that I can get a paper published in a new field. There is a good chance that I can get a paper published in a field in which everyone is interested. There is a good chance that I can get a paper published in a field in which I am the only one who understands what is going on. Now, suppose I want to climb a fence that I think needs to be climbed and I find that there are no fences in this field. In this case I may have to climb the fence in such a way that no one else understands what I'm doing. In such a case, I may find that I climb the fence and there are no other people who are interested in climbing fences. In such a case, I may climb the fence and I may not get any credit for it at all for many years, possibly never. But if climbing fences is what I think is important, then I may be willing to climb fences in such a way that I'm the only person who understands what I'm doing and I'm climbing fences that I think need to be climbed and there are no other people climbing fences. In such a case, I may be climbing fences that are important but I'm climbing them in such a way that I'm the only person who understands what I'm doing and there are no other people climbing fences. In such a case, I may be climbing fences that are very important but I'm climbing them in such a way that I'm the only person who understands what I'm doing and there are no other people climbing fences.

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Now the matter of drive. You observe that most great scientists have tremendous drive. I worked for ten years with John Tukey at Bell Labs. He had tremendous drive. One day about dinne time or four years after I joined, I discovered that John Tukey was not married. John Tukey was married to his research. He would come to the laboratory at 8:00 in the morning and he would leave at 2:00 in the afternoon for a quick nap before he came back at 8:00 in the evening. This went on for weeks before I discovered that he was married to his wife one day when I met her in the hallway. She asked me if I had seen her husband lately. It seemed that I had not seen John Tukey for some weeks.

What Bush was saying was this: "Knowledge and productivity are like compound interest." Given two people of approximately the same ability and one person who works ten percent more than the other, the latter will more than twice outproduce the former. The top-quality people will not work for second-rate people for any length of time. So what happens? The second-rate people will hire third-rate people so they can be the boss. The third-rate people will hire fourth-rate people so they can be the boss. The fourth-rate people will corrupt the system. Doctors will admit patients and then not cure them so they will have something to cure next week. I could go on and on and on about how this works. It's a well-established fact that the top-quality people will not work for second-rate people, and so on. When I was at Bell Labs, I had a very good boss named Bob Pierce. He was a very good boss. He had the quality of being able to hire people who were better than he was. It's a very difficult quality to have. I've watched many managers and I've seen that they don't have it. They want to be the top dog. Bob Pierce wanted to be sure that he had the best people he could get. He had the quality of being able to hire people who were better than he was. I'm quite sure that when I left Bell Labs, I was not the worst mathematician in the place because Bob Pierce had hired people who were better than I was. I'm quite sure that when I left Bell Labs, I was not the worst electronic engineer in the place because Bob Pierce had hired people who were better than I was. I'm quite sure that when I left Bell Labs, I was not the worst physicist in the place because Bob Pierce had hired people who were better than I was. If you want to work with the best people you can find, you don't want to work for second-rate people. You don't want to work for third-rate people. You don't want to work for people who are worse than you are. When I was young my mathematics teacher used to say, "Go where the wise men are." There were always a few wise men who knew what was worth doing and what was not worth doing. You want to be with those people. It's very important.

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