You
and Your
Research

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A talk by
Richard W. Hamming

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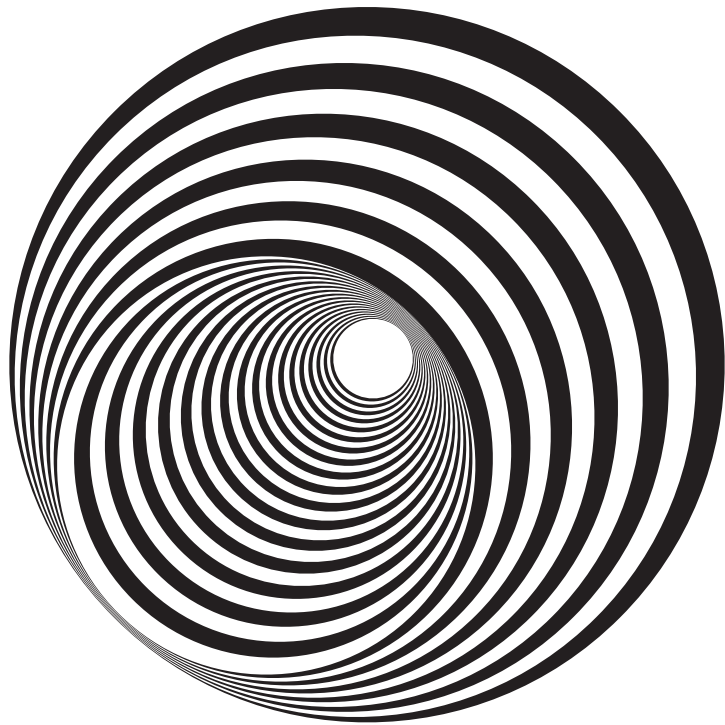Bellcore,
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"You and Your Research" is one of Richard Hamming's many lectures. In 1996, he published a write-up of his graduate course lectures in engineering at the U.S. Naval Postgraduate School in a book called *The Art of Doing Science and Engineering; Learning to Learn.* In Hamming's words, "the course centers around how to look at and think about knowledge," and is an extension of the wisdom of "You and Your Research." The book was republished this year, with a foreword from Bret Victor, by Stripe Press.

You and Your Research

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

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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 discouragements.

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, 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 observed 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!

Richard W. Hamming

11 February, 1915 –
7 January, 1998
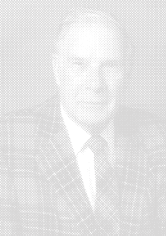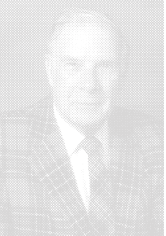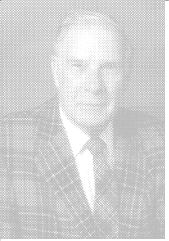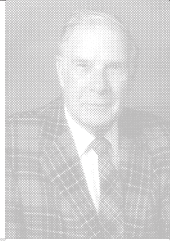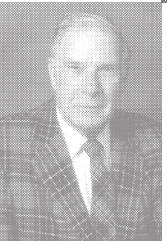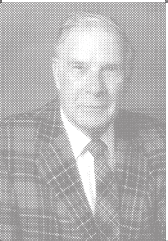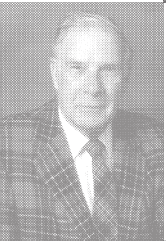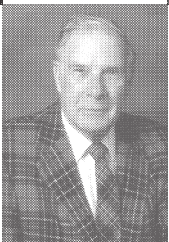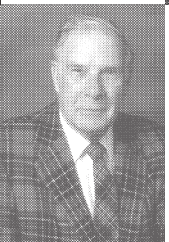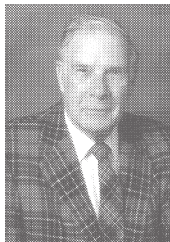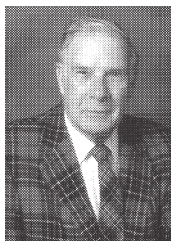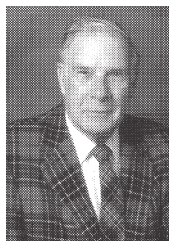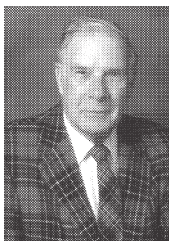
Richard Hamming was born in Chicago, Illinois, USA on February 11, 1915, the son of Richard J. Hamming and Mabel G. Redfield. He was brought up in Chicago where he attended school and realized that he was a more able mathematician than his teacher. He wanted to study engineering but the only offer of a scholarship came from the University of Chicago, which had no engineering department. He entered the University of Chicago receiving his B.S. in mathematics.

After his undergraduate studies he went to the University of Nebraska, where he was awarded an M.A. in 1939. He received a Ph.D. in mathematics in 1942 from the University of Illinois at Urbana-Champaign. His doctoral dissertation, *Some Problems in the Boundary Value Theory of Linear Differential Equations*, was supervised by Waldemar Trjitzinsky (1901-1973). Hamming, however, developed interests in ideas that were quite far removed from his study of differential equations when he discovered George Boole's *An Investigation of the Laws of Thought*. He found Boole's book interesting, relevant, and believable. The ideas in it would prove highly significant later in his life when he became interested in coding theory.

After earning his doctorate, Hamming married Wanda Little on September 5, 1942. He taught first at the University of Illinois, and then at the J.B. Speed Scientific School of the University of Louisville. In 1945, encouraged by a friend, he joined the Manhattan Project, a U.S. government research project to produce an atomic bomb at Los Alamos, New Mexico. A month after he arrived at Los Alamos he was joined by his wife, who was also employed on the Manhattan Project. Hamming was put in charge of the IBM calculating machines that played a vital role in the project. He came in contact with many leading scientists, including Richard Feynman, Enrico Fermi, Edward Teller and J. Robert Oppenheimer. The theoretical physicist Hans Bethe was his boss. Wanda Hamming began by doing computations with desk calculators, and later worked for Enrico Fermi and Edward Teller.

After the Manhattan Project ended Hamming remained at Los Alamos for six months, writing up details of the calculations they had done. He felt that it was important to try to understand exactly what had been achieved, and why it had been so successful. It was at this time that he realized that he had done the right thing by not studying engineering; engineers did much of the routine work, but mathematicians like himself were more critical to the cutting edge innovations. He formed a view of mathematics, arising from his Los Alamos experience, that computation was of major importance, but

it made him skeptical of the standard approach that emphasized formal abstract mathematical theories.

In 1946 he accepted a position in the mathematics department at the Bell Telephone Laboratories in New Jersey. However, he didn't entirely break his link with Los Alamos Scientific Laboratories, and made two week visits each summer as a consultant.

At Bell Labs he was able to work with both Claude Shannon, with whom he shared an office, and John Tukey. Some other young mathematicians had joined the Mathematical Research Department at Bell Labs just prior to Hamming. These included Donald Percy Ling and Brockway McMillan, who had been at Los Alamos at the same time as Hamming. Shannon, Ling, McMillan and Hamming called themselves the Young Turks. Hamming often related how they had all been affected by growing up in the depression, and all learned new skills with their war work. It led them, he said, to do unconventional things in unconventional ways. Hamming, for example, lunched with the physics group rather than his mathematics group, and they were fascinated by his unorthodox ideas and views. Not all his colleagues were happy to tolerate his unconventional ways. Some have described him as egotistical, saying he sometimes went off "half-cocked, after some half-baked idea." Unconventional ideas sometimes produce flashes of brilliance, but they sometimes also lead to failures.

Before discussing Hamming's highly significant work on error-correcting codes, we first note the many and varied problems he worked on in Bell Labs. These include problems involving design of telephone systems, traveling wave tubes, the equalization of television transmission lines, the stability of complex communication systems, and the blocking of calls through a telephone central office. He continued to work for Bell Telephones until 1976, although he became increasingly interested in teaching, and held visiting or adjunct professorships at Stanford University, the City College of New York, the University of California at Irvine and Princeton University between 1960 and 1976. After retiring from Bell Labs in 1976, he became a professor of computer science at the Naval Postgraduate School at Monterey, California. At this point he gave up his research career and concentrated on teaching and writing books. He believed that the way mathematics was being taught was wrong, and that the only way to change it was to write textbooks with a new approach. Here are two examples of his views on mathematics teaching:

We live in an age of exponential growth in knowledge, and it is increasingly futile to teach only polished theorems and proofs. We must abandon the guided tour through the art gallery of mathematics, and instead teach how to create the mathematics we need. In my opinion, there is no long-term practical alternative.

and

The way mathematics is currently taught it is exceedingly dull. In the calculus book we are currently using on my campus, I found no single problem whose answer I felt the student would care about! The problems in the text have the dignity of solving a crossword puzzle—hard to be sure, but the result is of no significance in life.

His attempt to move to a new way of teaching calculus is exhibited in his 1985 book *Methods of Mathematics Applied to Calculus, Probability, and Statistics.* He said that the book is "very different from the standard texts and its success or failure will tell us something about the prospects for change and innovation." Other texts he wrote all attempted to change conventional approaches to the areas they studied.

Richard Hamming is best known for his work at Bell Labs on error-detecting and error-correcting codes. His fundamental paper on this topic, "Error Detecting and Error Correcting Codes", appeared in April 1950 in the Bell System Technical Journal. This paper created an entirely new field within information theory. Hamming codes, Hamming distance and Hamming metric, standard terms used today in coding theory and other areas of mathematics, all originated in this classic paper and are of ongoing practical use in computer design.
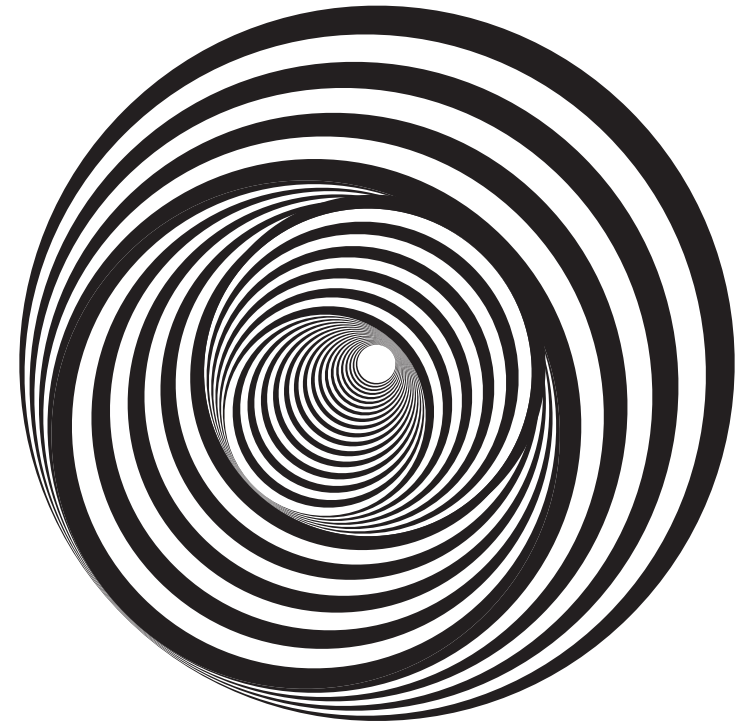
In 1956 Hamming worked on the IBM 650, an early vacuum tube, drum memory, computer. His work led to the development of a rudimentary programming language. Hamming also worked on numerical analysis, especially integration of differential equations. The Hamming spectral window, still widely used in computation, is a special type of digital filter designed to pass certain frequencies and discriminate against closely related frequencies.

In addition to the Turing Award, Hamming received many awards for his pioneering work. He was made a fellow of the Association for Computing Machinery in 1994. The Institute of Electrical and Electronics Engineers (IEEE) awarded him the Emanuel R Piore Award in 1979.

The IEEE created "The Richard W. Hamming Medal" in his honor. He was the first recipient of this $10,000 prize medal in 1988. He was elected a member of the National Academy of Engineering in 1980, and received the Harold Pender Award from the University of Pennsylvania in 1981. In 1996, in Munich, Hamming received the prestigious $130,000 Eduard Rheim Award for Achievement in Technology for his work on error correcting codes.

In 1997 Hamming retired from teaching at the Naval Postgraduate School and was made Distinguished Professor Emeritus. Shortly before he retired, he said that when he left Bell Labs, he knew that that was the end of his research career. It really would be the end, he said, when he retired from teaching. Indeed he was right, for having taught up to December 1997, he died of a heart attack in the following month. Richard Franke of the Naval Postgraduate School at Monterey wrote of Richard Hamming:

> *He will be long remembered for his keen insights into many facets of science and computation. I'll also long remember him for his red plaid sport coat and his bad jokes.*

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