RICHARD W. HAMMING

Recollections of mathematics and computing science at the Los Alamos Laboratory of the Manhattan Project and at Bell Laboratories, together with impressions of contemporaneous fellow mathematicians.

Transcript of an interview tape recorded

for

Bell Laboratories Archives and Historical Collections

and

The Mathematical Association of America

Bell Laboratories Archives
101 John F. Kennedy Parkway
Short Hills, New Jersey 07078

December 1981

PREFACE

The interview with Richard W. Hamming on which this transcript is based was one of two done cooperatively by the Bell Laboratories Archives and the Mathematical Association of America in early January 1981 in the Monterey area of California. Impetus for the project came from members of the Mathematical Association concerned with preserving sources for the history of mathematics during World War II. Not professional historians themselves, these mathematicians were interested in learning the techniques of oral history at first hand, in finding out the amount of time and effort involved, and in encountering some of the problems that typically turn up.

Another important purpose of the project was to begin obtaining interviews - particularly with older mathematicians while they are still available, able to remember and perhaps to help document events that are already from thirty-six to forty-one years in the past.

The interview was taped at Dr. Hamming's home on the morning and afternoon of 5 January 1981. It was structured around a series of questions that had been prepared by the interviewers and sent ahead to Dr. Hamming. The interviewers were: D. M. La Porte, archivist of Bell Labs; H. O. Pollak, director of Mathematics and Statistics Research at Bell Labs; and G. Baley Price, professor emeritus of mathematics in the University of Kansas at Lawrence. The recording equipment, comprising a cassette recorder and special microphones, was supplied by Bell Labs and operated by DML.

Because Dr. Hamming speaks so rapidly and clips many words, transcription was particularly difficult. The tapes were audited first by Gerri Markey, his former secretary, and then by DML. Reviews by HOP and GBP were critical to ensure mathematical accuracy. The transcript was then read by the narrator and very promptly returned with only a few alterations, mostly grammatical.

RWH: There´s a theorem in information theory that says that the most information is obtained when all the symbols are used equally frequently, When A,B,C,D, and E are all given equally frequently. While his range is up over here, there isn´t much signal. But he´s a sharp guy. Yes, he´s a very, very good man. You´re right. If there is some hesitation on his part, I would like to be.... On the other hand, it doesn´t do you much good, the rest of the people are also going to produce opinions at the conference. And if he says, "Ah, I don´t quite understand it," and shuffles around the way he does, the conference will go forward and the decisions will be made, and it can´t be unmade.

HOP: Well, then we need somebody else there who knows how to interpret that.

RWH: Ed, is, as I´ve said, possibly one of the most gifted mathematicians I´ve seen up close, bar nobody. He´s tremendous that way, but so much of his effect is vitiated by other traits of his, like his just being nice, just being an incredibly nice guy. You´ve got to be a bastard at times. I´m serious.

RICHARD W. HAMMING

Vita

Born: Chicago, 11 February 1915

B.S., University of Chicago, 1937

M.A., University of Nebraska, 1939

Ph.D., University of Illinois, 1942

Instructor, University of Illinois, 1942-1944

Assistant Professor, University of Louisville, 1944-1945

Member of Staff, Manhattan Project, Los Alamos, 1945-1946

Member of Technical Staff, Bell Laboratories, 1946-1964

Head, Numerical Methods Research Department, Bell Laboratories, 1964-1967

Head, Computing Science Research Department, Bell Laboratories, 1967-1977

Member of Staff, Naval Postgraduate School, 1977---

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There's an interesting story along that line. In the early days (I think it was probably `47; it must be `47 or `48), I was eating lunch at the cafeteria in West Street with a bunch of engineers. They were talking about salary raises during the war, "So many dbs, at various times...." Now you won't believe it, but I was nice and quiet the whole lunch, saying nothing - which is difficult to believe, but it was true. At the end, I said, "Pardon me, gentlemen. Will you tell me whether salary is amplitude or power, whether it's 20 log or 10 log you were talking about?" It turned out they had no idea between them. I realized then that jargon can mislead even experts in the field as well as others.

In statistics, the words "best fit" only mean "least squares." It <u>does</u> <u>not</u> mean "it is the best fit." The expert is misled by his own jargon as well as misleading everyone else. We should be very careful in our abbreviations and jargon, always.

Returning to understanding and Ed Gilbert, I once had a partial, nonlinear differential equation. I went to Ed and said, "I don't like to solve a problem when I don't know what the answer looks like to some extent. Would you mind looking at it and giving me an idea?" A couple of days later, he came back with the solution in closed form!

The other story about him is as follows. The phone rang one day, and the guy identified himself as a member of a certain university looking for a president. Now, he was careful to identify himself, and he said, "Mr. Hamming, you have a reputation for frankness. We are tired of getting lovely replies. Will you please tell us what you think about Ed David." After a long pause, I said, "Okay." and told them what I thought. After it was over (we had a long conversation), and I hung up, I sat back and decided, "Well I guess I really had recommended him very highly." But I had tried to say what it was. They would never call up Gilbert, because Gilbert will not say anything bad. In some sense, a recommendation is hard to get from him. If he says, "I don't quite understand it," he's probably saying "baloney." He's got a dynamic range that is so small that you have great difficulty in reading it.

HOP: Okay. But you can learn. I admit, it's not the same.

Incidently, coming back to Gilbert. Another thing wrong with Gilbert: he´s what you can call a "backroom scientist." You need to be able to do things occasionally, in the middle of a conference, to get up and say, "That is wrong for these reasons: bang, bang, bang, bang, bang," because the decisions will be made. Many scientists are unable to act on the firing line of decision. And while they´re valuable, they´re not as valuable as the man who extemporaneously will get up and say why something is wrong and make the case right then and there.

If you´re going to be a professor and turn out Ph.D´s, you should give a course on the presentation of results. They should be taught written, prepared, and extemporaneous oral presentation, - a whole course devoted to that. And if you can´t pass it, no degree. "We mean this course. It´s not something on the side, buddy. If you can´t learn how to present your material, we don´t want you - out! You´ve got to learn how to present material well." Now, the first problem is to find a professor to teach it. But I think this is one of the things we´ve got to do. As you know perfectly well, most of the papers written now are unreadable.

HOP: Always were.

RWH: No, I think again coming back to the *Monthly*, they were more readable years ago than they are now - or else I´m getting older.

HOP: Oh, the *Monthly* papers are still very readable.

RWH: There are some. Some of them are a little difficult at times. Once in a while, you find one in which the author is assuming too much. Incidentally, along those lines, I made an experiment once on a statistical book with all the abbreviations: LS, MLE, AUE, ARMA, etc. I made a calculation of how much bigger the book would be, how many more pages, if everything were spelled out instead of abbreviated. And it wasn´t going to add more than about ten or fifteen pages, if "Auto Regressive Moving Averages" were spelled out instead of ARMA, LS, etc, so that when you picked up the book, you were not snowed by these particular abbreviations. The abbreviations, we´ve got to stop it! I´m conscious of that in the navy, where we´ve got so many abbreviations. They talk that way, and they don´t know it themselves.

Riordan did. Riordan, to some extent, would look at enough special cases, write down the general case, and that's the end of the matter. But he did it after seeing how it worked.

GBP: If you really might be able to apply an induction, some of the general cases get to be.... It's horrible!

RWH: One of the things I learned from Henry was to do indefinite integrals - not that I couldn't look up the table, but by keeping myself sharp, I could do the one that wasn't in the table later on. You do have to do formal induction so that you can do the one you can't possibly follow through. Yes, you have to keep your tools sharp on small problems. That's the reason why, again, I think that some of the great scientists failed.

I did John Tukey in in a certain sense one time. For quite a few years, I buffered John from machines by taking all the details in my hands. He comes in one time with a great new system, which he starts telling us, from top down without telling us any of the guts. He finally comes down to where he's going to read the tape forward and then he's going to read the tape backward. We have to explain to him you can't read tapes backwards. A small technical detail at the bottom vitiated the whole idea. The great scientist needs to know the minute details as well as the generalities, and what fame tends to do to you is to protect you from grubby details.

Again at the Labs I observed the following phenomenon. Naturally, there are days you reduce a lot of data using computers. When the technical assistants who were gathering data went on vacations for two or three weeks, the boss had to go in and do something. Wait six months, and you find they're no longer measuring the data the same way! That is the reason why I think management has always kept the place in short supply of technical assistants. It seems to be necessary that a great guy know the details as well as the "big picture." You can't get along without knowing both, but of the two evils, I think it is better to have the big picture, because you can learn the details easier - if you have to. If you have only details, you can't seem to learn the big picture.

be done than I could do, after a short time I had a wide range of choice. Once in a while, I got ordered to do something, but that was rare at Bell Labs. Because of the military, once in a while, Bode or somebody else would ask for something, and I would have to do a particular thing. But to a great extent, I could choose after a little while. I chose, greatly, by the man rather than the mathematical problem.

GBP: I have forgotten who was supposed to have said it, but the advice was, "Read the masters."

RWH: Poincarè (I may be missing another name.)

GBP: Okay, I thought that was the one, but I hesitated to say it.

RWH: Yes, read the masters, go to the masters. Yes, that is the reason why I have a copy of one of Euler´s books. I have been unable to get his Analysis* in English. I can´t find a copy to read.

HOP: Learn Latin.

RWH: Yes, it is very glib for you, but I´m an old fart and can´t learn now.

DML: Eighteenth - century Latin is easy enough to read.

HOP: Yes, it is.

RWH: I may be driven to it yet. I have read Euler´s Algebra** several times. His famous algebra book, the one he dictated to his servant when he was blind, partly to teach his servant algebra. It´s got an appendix by Simpson and a few others. But it is very interesting to see how he works out many special cases, before doing the general case and goes ahead. Proof for him is often several special cases, when they work out right. He goes on, doesn´t bother to prove it in all generality, and it´s something like

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* Introductio in analysin infinitorum, 2 vols. (Lausanne, 1748).

** Anleitung zur Algebra (1770).

I´m now on the same side), and a lot of other guys who are almost as good fall slightly on the other side. What they´ve done is taken away from them. I resent this fact, although I acknowledge the truth of it. The tendency is to make the great much greater and push down the almost as good. Once you get on the right side of the Matthew effect you´re in. If you get on the wrong side, you are out. Now I have been on both sides and I know. I saw the effect of being on one side for a long while. When I wasn´t on one side, I was on the other. I saw things that I thought that I had done better, but that was all taken away from me. I might as well not have done anything. I have been on the right side for years, and things that I never did have been attributed to me nicely. Justice?

But it is a view I have taken, and in discussing with you all these names, you should understand what I am trying to come somewhere near doing justice to the less famous. I have been conscious for years of the tendency to make the famous more famous. Once you get a couple of prizes, it is easy to get more prizes. What happens when you are asked to recommend a guy? You look around at some other prize list and try the same names. It is a very, very unfair system, but it rests on the assumption you will look where you have had success in the past. That feature tends to produce that result, and it is an evil which I have no proposal of how to change.

HOP: What are you going to do about it? You haven´t time to read everything.

RWH: I know. I didn´t say that I had a way of solving it. I was just saying I have no way of solving it except making people more conscious of this phenomenon. Not that Shannon wasn´t good, but he was really only about six foot tall and a human being like you and me.

DML: You co-author with a famous person.

RWH: I certainly recommend working with famous people. When I came to Bell Labs, my theory was: You work with the good guys, some of their ability rubs off on you. Besides that, I was doing a study of what great scientist did and how they did it. So I picked my problems, to a great extent, not by the problem but by the person. Whether the problem was interesting or not did not matter half as much to me as who did it. In the early days, since there was more computing to of them, because we found that we would rapidly go through one and start the next one. The problems are numbered, and they show the tremendous, rich variety that used to drive Bode nuts. The variety of the problems we were doing that he couldn´t even know about bothered him for years. But he´d go out and talk about computing, and he didn´t know what was going on. There´s a source of information for you if you can find those logbooks from way back, from almost the earliest days in the computing center - early CPC days.

I suppose it took a couple of months before I realized I had better lay down a record. I couldn´t force anybody to write elaborate descriptions, but I could force the physical description to some extent, and a mathematical description. That is the best that I could do. And so you have the department number, case number, then the guy´s name, and some other things to track down. It ought to be useful for what early computing did. Because I think I told you, I think that the history of computing at Bell Labs should be the spread of influence, how it changed BTL, what was done, what kinds of problems were done, what new fields were opened up, which ways - rather than any one spectacular problem here, there, or yon. It´s the spread of knowledge in the whole Labs, it´s the way that things change in the long run that matters more than any individual first.

I had forgotten one other theory I have, which biases everything. In Science quite a few years back there is a paper called the "Matthew Effect." Do you know it?

DML: No I don´t.

RWH: It´s named after Saint Matthew, where in the Bible it says, "Unto everyone that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath" (Matt. 25:29). It´s a very real effect; it´s due to the way we ourselves behave. If you opened Physical Reviews and see a paper by Einstein and one by Joe Blow, you would read Einstein. Joe Blow might be just as good. And even if you did read Joe Blow and Einstein, when you talk to somebody else you would say, "As Einstein says,...." You tend to look where you have had previous success. This tends to make the famous more famous, the less famous less famous. It tends to divide like this: Shannon´s on one side (fortunately

BELL LABORATORIES ARCHIVES AND HISTORICAL COLLECTIONS

Interview with Richard W. Hamming (RWH)

Date of Interview:	January 5, 1981
Place of Interview:	Dr. Hamming´s home, 1140 Sylvan Road Monterey, CA 93940
Interviewers:	Deirdre M. LaPorte (DML) Henry O. Pollak (HOP) G. Baley Price (GBP)

Question No. 1

When you received your Ph.D. in mathematics from the University of Illinois in 1942, what sort of career did you envision for yourself? Why did you eventually abandon an academic career for one in industry?

RWH: When I was getting my degree in 1942, my vision was, at best, that I might become a head of a liberal arts department in mathematics in the Midwest and receive about $3600. The calibration number is that instructors received $1800 in those days.

HOP: Why the Midwest?

RWH: Because that was where lots of liberal arts colleges were, and I saw no better prospect than that. So that was where I started.

Why did I go from academe to industry? Chance and World War II. I was teaching (some of it military work), but I was twice approached to go to Los Alamos. The first time it fell through, the second time, I did go to Los Alamos. I had a vague idea only of what was going on. I went there, and after the war Bell Labs by chance offered me something.

There was a third reason. By that time I´d realized that I was too interested in good teaching, and that if I went to a university I wouldn´t get ahead. The funny thing is that in order to be a good teacher, in the long run you must engage in research. But in the short run, that's detrimental to teaching. If you engage in good teaching along the way, you finally are great at teaching, but you don't know what to teach. And that's the great paradox of teaching.

HOP: You have to get tenure first, and then you can do what's right.

RWH: In any case, I come into teaching now, late, in a good position. Actually, I went to Bell Labs telling the Bell Labs management I would come for three years and learn more about applied mathematics and then go back to teaching. Either I am stupid - it took thirty years instead of three years - or there was more to learn than I had thought, but somehow I stayed thirty years.

Question No. 2

Under what circumstances did you go to the Los Alamos Laboratory in 1945? What sort of work did you do there? Is there information available on the mathematical work done at Los Alamos?

RWH: Under what circumstances did I go to Los Alamos? Well, I had done some teaching, and there was a war going on, and I felt I had better do something. What did I do there?

HOP: Well, how did you get to Los Alamos particularly? Did someone come and recruit you?

RWH: No, no. Well yes, a friend Nicholas Metropolis, by letter, asked me. He had gone to school with me at Wright Junior College, also at the University of Chicago, and we had done a little graduate work together. He had written me a letter once, and it fell through because it turns out that Hans Bethe had a great objection to mathematicians.

HOP: Hans Bethe?

RWH: Yes. He had a great objection to mathematicians, and I was brought in to replace R. P. Feynman, Stanley Fraenkel, N. C. Metropolis, and Eldred Nelson - to run the computing machines. They swore to Bethe that I would do nothing but keep the machines going so they could go back to physics. So, I was really, strictly a stooge, and my job was to keep the IBM computing

pick it out then on telemeter data. I had exactly the same peak to peak as they had. I had the general structure right. Furthermore, the moment I showed them the curves, they knew what the error was. It pinpointed the error.

I always used that test when I finished a computation and handed the guy the answer. I would provide feed-back by going back three months later and say, "Okay, let's see what happened to those numbers that I gave you." I wanted to see whether my computations were right, to see whether they used them or not. And those guys who hadn't used the numbers didn't get more computing. Those guys who had, got more service. By regularly trying to check, "Did I calculate the right thing or not? If not, why hadn't I delivered the goods, if you needed them?"

[Tape 2, side 2.]

[While tape cartridge was being turned over, some words were missed.]

After a while, in order to protect myself, I started to keep a logbook. Whenever somebody came around for some computing, he would make some notes in the book indicating both what kind of physical work he was doing and what kind of mathematical computation he needed. For example, "solid-state physics such and such," and then, "differential equations" or "function evaluation" or something. And this book lay around. When people would come around and say, "What are you using in computing?" You simply opened the book; you could see. Even Bode could see! They saw every place in the Labs was there and how wide was the variety. So that's one source you could find out what was done.

The second thing I did was, I think, shrewder. When someone did some computing using one of my gals, I said to the guy, "When you write that paper, you are going to thank my girl. No if, ands, or buts, or else no more service, buddy." I waited a couple of years, and then I went through a year's run of the BSTJ (Bell System Technical Journal) and showed what fraction of papers had used computing; and that was devastating to management. I said "If the test of good science is being in the BSTJ, this is the effect of computing in the Labs."

HOP: What happened to this logbook of yours?

RWH: It must be with Bell Labs still, I could not take it with me. I think that you may still find them, typical bound notebooks. There must be three or four

RWH: C. A. Lovell was one of the good guys. Among other things, I believe he was the brains behind some early "throw-down" simulations.

I will tell you another story, about the Nike missile. In the early test shot days, they were doing half a dozen field shots every six months to get data to move various stages forward. Ling came back from one, and he was walking the hall looking unhappy. I said, "What´s wrong?" He said, "Two of the missiles have fallen out of the sky, and we don´t know what happened. We can´t fire the rest of them, but the next firing date is coming up and we need the data from this one first. The next design date is coming up; we have to do something." I said "Ling, give me the equations, and I will simulate." Well, he and Ed Norton sat down and gave me seven equations in seven unknowns. I put Clara Froelich to hand calculating.

Well, it´s a good example. I have no initial conditions. They were very simply throwing the Nike up and trying to get it to follow a particular trajectory. There was feedback to bring it back on its trajectory, so I said to myself, "It doesn´t matter really much where I start. I´ll start around the right altitude and about the right direction; it will bring itself in anyhow." Well, Froelich is calculating for almost a week. She comes to me one day. She says she has to start over; she has lost accuracy. I said, "Never mind, Clara. I absolutely insist that you go forward." She says, "The answer is no good." I said, "I don´t care about your argument. The answer is accurate for other reasons than your computations. Go forward." She never had much use for me after that, but the thing showed beautifully what was wrong. Exquisitely right! All because they gave me the right equations! With the right equations, no trouble!

That was one of the things, I think, that also broke down Bode´s prejudices a bit against machines, because they were really in trouble. And here, a simple simulation, a girl with a desk calculator computing along, gave the answer. Very obvious. What happened was pitch and yaw were both stable, but they had forgotten the cross connections within the system. Because this system could roll, when pitch settled down it threw energy to yaw, and when yaw settled down it threw energy to pitch. The instability grew. The oscillations simply pulled the Nike missile apart. I could pick out the peak to peak, and they could then

machines running from hour to hour and to get whatever calculations were needed done. I was to do nothing except manage the computing machines.

HOP: Did you say that Bethe objected to that in addition to...

RWH He wanted no mathematicians around the place.

HOP: Around Los Alamos at all?

RWH: So I understood. Unfortunately, from his point of view, there were a few around, but he wanted none and he was in charge of T [Theoretical] Division. He wasn´t going to hire any more.

HOP: Who were some of the ones that were around?

RWH: Ulam was there, but I don´t think he cared for Ulam very much.

GBP: He´s still around. He´s in Santa Fe now.

RWH: Yes he is. Living in Santa Fe, and he comes up to Los Alamos regularly. And between you and me,... by the way, I assume you want opinions, not politeness.

DML: Not politeness, yes.

HOP: Opinions, yes. If it gets too far, the language can always be edited out.

RWH: All right.

HOP: So, we´ll run with one of the mathematicians...

RWH: Richard Bellman was also there...

HOP: Bellman was there?

RWH: Donald Flanders was there. He was in charge of a hand computing group upstairs. D. A. Flanders, New York City. He committed suicide not long ago.

GBP: Yes, I know who he was.

RWH: He was a very good man. He was a big help to me. Periodically I would trot upstairs and talk to him and get myself oriented because he knew so much.

GBP: Were there one or two more? Wasn't there somebody from Wisconsin, probably came along with Ulam?

RWH: Probably. Joe Hirshfelder.

HOP: Isn't he a chemist?

RWH: Yes.

GBP: I think the name I knew fairly well. It's in Ulam's book.*

RWH: I will tell you a story about Ulam's book. I read it. Later I went to Los Alamos. I said to Metropolis, "Nicky, I just read Ulam's book. That isn't the way I remembered it." Nicky said, "I didn't remember that way either."

GBP: I haven't met anybody that remembers it that way.

RWH: Except Ulam. As far as I can see, each person has his own memories, the way he saw it. It was a very intense affair, and I am not surprised we saw it differently. You see, to me computing was the center of the whole place.

HOP: Sure, of course. Were there any other mathematicians around?

RWH: Oh, there was Milton Wing.

HOP: Was Milt Wing there at that time?

RWH: Yes, I think he was still there. I can think of some more now....

HOP: Because Milt Wing commutes. He is sometimes at Sandia and sometimes at New Mexico. He is still around.

RWH: Yes, he could probably tell you more about the mathematicians because they were upstairs, and I was down on the ground floor.

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* S. M. Ulam, Adventures of a Mathematician (New York: Charles Scribner's Sons, 1976).

and so on, and then solved for the angles to see whether they're correct. None of that was really involved. What was actually involved was front-back, left-right. And those were the simplest formulas to do. It amounts to affine geometry. But they didn't seem to know. And when I pointed it out, somebody said, "Whoever learns affine geometry?" I said, "I did." It's true I had learned that when I was a graduate student, to get a degree. It's not taught now; those things are simply not taught, those simple little things like that which are around every place to be done. But they don't seem to get applied. How can you teach students to get down to the fundamentals of things?

HOP: Well, one reason you don't is that you don't get a Ph.D. that way.

RWH: Well, I'm not in the Ph.D.-granting business as yet, although our department wants to go that way. I am sick and tired of thick theses. I want quality not quantity.

HOP: We keep motivating everything we do all the way along in favor of those few people who are going to go and get a doctorate.

RWH: True, but you can hold out for the guy writing a small, elegant thesis rather than a big thick one. Just like if we can get out of you a really good history, that's small not that thick [gesturing to indicate thickness], not four volumes. Yes, we choke ourselves up with the business of trying to be scholarly. Rather, the real contribution when you write a book is more what you leave out than what you put in. What is not worth knowing is best labelled "This is not worth knowing, forget it! I will tell you the bare guts," like Love's book. I got it during the depths of the depression. The professor adopted it because it was remaindered. We got it for ninety-five cents.

GBP: I studied it about....

RWH: You are a little older than I am.

GBP: No, I studied it about `23 or `24 along in there. You might be interested to know that Clarence A. Lovell was one of my early teachers as an undergraduate. He was one of the good guys. He grew up in my hometown, I knew all his family.

evaluate a polynomial at $a + ib$, where a and b are both real numbers. What I do is: I construct the real quadratic which has that root, $a + ib$ and the root $a - ib$; divide the real polynomial by the real quadratic; get the linear remainder; and then put the complex number in.

HOP: Yes, but finding quadratic factors has always been a tough business.

RWH: No, No, No! I constructed the quadratic factor. I constructed it from the given root, then I divide, and I evaluate the polynomial. It's a remainder theorem for quadratics. It takes just half the arithmetic. For a while, I was ahead of them, for a little while.

Now, you're going to ask the question I've asked many times. How was it that all my predecessors with girl hand calculators never observed that simple property? They were grinding it out the hard way, Instead of doing the obvious thing: construct the real quadratic factor, divide by it, get the remainder, and evaluate the remainder at the complex point. I don't know. But it's obvious that I had to know abstract algebra to do that trick. It's a small one. Just doubled the capacity of the machine, almost. I've puzzled many times: Why did my predecessors not see all these simple things?

There are lots of simple things left to be found like that. For example, at Irvine, in the hidden view problem. Here you are; you're looking at various objects. What's hidden and what's not? They were calculating distances. I said, "Forget it! Consider first the normal form of the equation. Put this edge in the normal form of the equation and evaluate it. If it is plus, it's farther out. If it is minus, it's this side. But now, you really don't have to divide through by the square root, because you only care whether it is front or back. You don't care how far. There's no real distance involved." Next, is it left or right? I said, "You don't really care what the angles are. All you've got to do is take that triangle to determine your three points. If the area is positive, it's one way; if the area is negative, it's the other way. Right? Since the origin is one of the points, what it comes down to is $x_1y_2 - y_2x_1$. If this is plus, it's on one side, and if this is negative, it's on the other." That's really affine geometry. They would have solved for the distances to find out whether this distance is greater than that,

HOP: Had you caught the name of Milt Wing before, Baley?

GBP: Well, I have heard the name before, but I don't know the man. No.

RWH: He is in Los Alamos some of the time.

GBP: I don't think I have ever met Milton Wing, although I have heard the name a number of times.

I am trying to think of the name of a physicist, who got a Ph.D. at Harvard, but he went to Bell Labs. No, not Bell Labs, but IBM eventually, but he may have come after the war was over.

Did anybody write down the history at that time?

RWH: There is almost nothing. I have a few notes.

GBP: Do you know the history written by David Hawkins?*

RWH: Yes. I read that, too.

HOP: Do you know David Hawkins?

RWH: Yes, slightly. Yes, I am not sure he knows me, but I knew him slightly. I don't think that there was anything that I read - I read quite a few stories - that seems to resemble what I remember. Remember I was tied up with machines. I saw the whole thing depending upon the computing machines. As a matter of fact, when I protested one time about the vast amount of money we were wasting on computing, somebody said, "Okay, come out to the field shot." (This was not a real atomic bomb, but it was a test shot for certain experiments.) We went out there. We left the motor of the truck running. We went into the blockhouse. We fired the test shot. We waited the required ten minutes or so, and we ran for the truck to get out of the radiation. The truck was running; we zoomed off. And as we were departing, he said, "Well, there went $100,000 worth of equipment. And if we didn't have

* "Some years later he [David Hawkins] wrote two volumes, since declassified, about the organization and the scientific history from the early days of Los Alamos until the end of the war" (ibid., p. 160).

your numbers, we wouldn't know what we saw." So, I quit complaining about the cost of computing right there.

HOP: Besides these people that we've been talking about, what other names would you suggest, what ways would you suggest, of getting information about mathematical work at Los Alamos? How would one go about it?

RWH: I've thought about that a good deal, and the answer is: I don't think you can. There were some books written, just like the series for radiation. We were required to write a series of books at Los Alamos.

HOP: Oh, you were?

RWH: Yes. The ones I wrote were all on "How do you make machines do certain things?" They tell you negligible about the mathematics.

HOP: Right. But where are those books now?

RWH: Presumably the AEC has them. I don't know. I've never seen them. I have a copy of a draft of what I wrote. It is highly technical, details of IBM machines and <u>nothing else</u>.

HOP: Whom would one ask? Metropolis?

RWH: Metropolis, or write to Los Alamos and say there are in existence a whole bunch of books we wrote. The computing one was, I think, volume 18 or 17. There was a series of books written by people who stayed on. That's one of the reasons why I took six months more before I came to Bell Labs, to try to get something down on what had happened. There was some attempt to do so.

Hans Bethe dictated long notes. I remember watching him. He would sit, dictate with small file cards, and tell the girl to leave two inches, leave three inches, and so on, refer back for her all the way down. Out would come perfectly typed stuff. He would fill in the equations, and he never had to go back and do anything else after that.

Placzek was there after Hans Bethe.

HOP: How do you spell that?

algebra, although I'm not very high on complex variables, which is the old classic stuff with endless sequences of epsilons and deltas. I'm not very strong on that one.

I should tell you another story you may not know. I think that Bode was <u>told</u> to spend a year at Princeton by his management at one time. So he went there. He spent the year and came back and says to us (this is shortly after Shannon had done things), "We really ought to survey mathematics and see what else is useful." So some of us tried. We found nothing. I think you have to start with the problem and find the mathematics. There have been very few to start with mathematics to try to find the problem. It appears to be the other way, almost always - in the past.

Oh, I should tell you another lovely story. Classical network theory involves finding complex zeros of polynomials - very heavily. It's a major problem. And the way I came upon it was as follows. In the history of computing, when I came to the Laboratories, there were four hand-computing groups that I am aware of: one in the Network Department, one in the Math Department, one in Quality Assurance and there would seem to have been one connected with Central Office simulations.

HOP: You mean the throw-down computer at West Street.

RWH: Well, that kind of thing, yes. It was highly specialized. There were these four hand groups. Well, the Network Department got a relay computer, Model VI. I used it occasionally. Well, I wanted to get more time on it. I could only get time when they weren't using it. I could easily see that if I went through and speeded up any individual program, it would only take them a couple of days to find another problem to occupy the time I had freed. I would have to get at the central problem of speeding up all the programs, then I would have a little gap before they filled the time up. The central problem was evaluating real polynomials at complex points. So I looked at it.

Now a complex number is really the real field with a quadratic irrationality adjoined. Every element in the field is representable as $\underline{a} + \underline{b\theta}$. Therefore, I should be able to find the value of a polynomial at a complex point, by staying in the reals down to the bitter end, not starting in the complex. So I want to means. It means either extreme specialization or we change our methods.

GBP: This recalls the last chapter, I believe it is, in Ulam's book, Adventures of a Mathematician. The key words, if I exactly recall them, about our present activities were "Nobody knows what is going on." Does this make sense? We're spending large amounts of money, but there is nobody that understands it very well, or evaluates it.

RWH: John Tukey was a scientific generalist and I often say that I was one too. But we are a dying breed, and we are on the defensive almost always because the expert can drop Lebesgue integration or something else on you and outfox you on some fancy stuff that is totally irrelevant. And he can use jargon that beats you down. The scientific generalist has to have incredible gall to say "Bah!" For example, at Bell Labs I had a lovely rule, which I used many times. You know what "high-falutin" means? I had a thing called the "falutin index." I would say, "All you have done is raise the falutin index. Let's get right back down to the bottom." When somebody says "Lebesgue integral" or "Put it on Banach space," I say, "Let's get right back to the problem. You added nothing to it."

Once in a while, abstraction and generalization add tremendous clarity, but many times they do nothing. Bourbaki was only half right and half wrong. The idea of abstraction as a value in itself, no! Sometimes it's valuable, sometimes it's worse. Sometimes it really obscures everything. It's a very real problem, and the scientific generalist has some real struggle involved.

I was a very lucky guy. I came to Bell Labs at a time when it was a very exciting place to be. There were very exciting people, the war had stimulated the mathematics group through so many people and in so many directions. Everything burst forward. I didn't work on radio telescopes. I didn't work seriously on lasers very much. There were a few things I missed, but almost all of the real things that happened - space flights, so on - I worked on a corner of them, worked with people who worked on them. And it was very exciting to be in on it. The only thing that got me through was mathematics. It was the same calculus, everywhere. Although you'll notice I'm high on abstract algebra. I'm high on analysis, and abstract

RWH: P L A C Z E K. He came to replace Bethe and was head of the department then.

HOP: Which department?

RWH: The Theoretical Division, which I was part of. My wife worked for Fermi and Teller, who were on the other side, across the road. She ran a hand calculator for them. Well, shall we go on down the line?

HOP: Well, let me go back to the university just a little. As I recall, you went to something like seven different institutions...

RWH: Yes.

HOP: ...in the process of getting your degree. Why did they keep throwing you out of places?

RWH: Well, the first and third both closed their doors and never opened again; the second, I went broke; and the fourth was the junior college I graduated from. Remember, it was the depth of the depression. So, that's how four got going, then Chicago - I got a bachelor's degree. I went to Nebraska for two years and got a master's degree.

HOP: How did you pick Nebraska?

RWH: They gave me a scholarship, $500 a year. What do you think? It is that simple!

GBP: Then that explains everything.

RWH: That explains everything.

HOP: I didn't say there wasn't an answer, Dick. I was just kidding.

RWH: I had a job offer from the Western Electric Hawthorne plant in Chicago, at the same time, at significantly more money. And this offer from Nebraska came through. I called up the employment of Western Electric and said I had another job offer. He asked about it, and I said, "Well, $500 a year." He seemed surprised, and I told him why. He said, "You're right, take it. Come back when you're through." But I never did.

Is there anything more before I go on to who brought me to Bell Labs?

Question No. 3

Who or what brought you to Bell Labs?

RWH: It was Deller and Fry. They came out to Los Alamos on a visit. Now [R.A.] Deller was the head of Personnel.* And Fry was Fry.**

I was told by somebody, "Why don't you stop by theater number 2 and talk to some guys in Bell Labs?" And I thought it was nonsense. I didn't have time. But we went to work every day and every night, and as I was walking to work in the evening, I said, "Well, it won't hurt me to stop by and talk to them." So, I chanced to talk to them. And they made me a job offer and...

HOP: Right then and there?

RWH: Well, no. A letter came back sometime later. But it was a very short interview.

HOP: You didn't come east to interview?

RWH: No, I was a shock to H. W. Bode. I was a big shock to Bode. BTL offered me a salary raise in December 1945, before I came in July 1946. Four people were hired from Los Alamos that I knew of.

HOP: Yes, who were they?

RWH: Frank Schnettler, Brockway McMillan, myself, and I can't think of the fourth name.

HOP: So Frank Schnettler came at that time?

RWH: Well, he had been at Bell Labs before.

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* Technical Employment. - Ed.

** Thornton C. Fry, who had been head of Mathematical Research before the war. - Ed.

shrink from the war size. Maybe you can run a small installation with these kinds of people, but maybe you cannot run a big one of 18,000. Maybe you have to have a different kind of person in management. So I am not saying it's wrong now, but certainly there is a big change in who gets promoted now and for what reasons. Ian Ross - whom again I knew quite well at the beginning - there's a very good development man, not a great researcher. Before then, usually there have been great research people. It may be necessary, but it certainly is a change which I find bothersome.

I find this is likely to be an apocryphal story. The story is that the Personnel Department found out what makes good scientists - what would keep them well-balanced. So they went out and hired all the well-balanced people. They found, after ten years, or thirty years, or something like this, that all reports were on time, everything is fine, but no great ideas were arising. In some sense, it is the trouble-makers who produce great results. Unfortunately it is true. Certainly I was a trouble-maker; certainly Terry was a trouble-maker; certainly Shannon was difficult; so is Tukey! If you go into something unusual, you have to be unusual in more than just one direction.

There is another guy you haven't asked about. You wouldn't know Jack Kane, whom I still write to occasionally. Jack Kane was crazy as a fruitcake, absolutely crazy as hell. He really was crazy for a while, after he left Bell Labs. But he once said to me, he'd been calculating the size of Phys Reviews. He said, "You put it on a shelf each month; you push them across to make room for the next month's issue; it falls off the other end." He said he calculated that in some year, I think it was 2020, the volumes would be moving with the velocity of light! He said, "But it doesn't matter, there'll be no content."

Now it wasn't very far from that time when I once went to a talk down at Princeton by the IEEE in which the editor said, "If our plans for publication as we now have them come true, a conscientious member of the IEEE could start reading January 1 and read all year long and still not read everything the IEEE had put out." There would be no time to do anything but read! That was many, many years ago and this other story was quite early. Kane extrapolated various things from his Reviews which made me realize that we are eyeball to eyeball with infinite knowledge, and almost no one is willing to contemplate what infinite knowledge

RWH: Yes, I admire the navy system. It's either up or out. Past some point, it's up or out, buddy.

GBP: About 52 I should think. Or is it twenty years?

RWH: No. Above a certain rank, you have to get a promotion within a certain time, or out.

HOP: But in the same breath, you complain that among the physicists, all the best people got promoted.

RWH: Well, I possibly have.

HOP: Whom should you promote instead?

RWH: You have me in a difficult position. I told you I thought that Pierce was a good man. I'm inclined to some extent to think we might have destroyed him. I have long thought of the following. Had Pierce appeared twenty years later, could I have formed a group whose function was to be a machine for the other people - every time Pierce had an idea I would hand him back the answer as fast as I could - to drive the master unmercifully. How much more could we have gotten out of his talent? How long would he have lasted? Would he have degenerated into more and more pious generalities and vagueness and more and more books? Was it inevitable or could he have been kept highly productive in the scientific sense? I don't know the answers.

I didn't dare discuss this with McKay. I worked with McKay when he first came. I worked with him for a while and I was well acquainted with him. And since then I have had several meals with him recently. I've not dared to ask him, "Was it a net gain or loss when you became a vice-president?" I don't know. I have been puzzled by it very much. I don't truly know the answer.

As I said, boy, there is no substitute for experience! I wish that my management knew what it was to be creative, and what trouble it was to try to be that way. What it really was. On the other hand, in defense of the present Bell Labs.... When I came to Bell Labs, it was generally true that scientific excellence resulted in promotion. All the people up my line of command had done great scientific work. More and more it is true that "professional executives" get promoted. But remember, I came to BTL when it had a staff of 6500, and they were trying to

HOP: I see.

RWH: And they all went in January, but I stayed on because the whole computing was collapsing. We couldn't find anybody to replace anything, so I stayed on. BTL sent me a telegram saying that I had a salary raise in December. I thought, "Oh well, that's kind of interesting, give me a salary raise. Heck, I'll try it."

HOP: They did not interview you on the premises?

RWH: No. I was a shock to Bode, I am sure, a complete shock. But he had great faith in Fry, and I can't blame him.

HOP: Fry at that time was no longer connected with the mathematics department.

RWH: No, he was an executive assistant to a vice-president.* He was very active in a lot of ways. I bumped into him a number of times, and I will tell you more when we come down the list to him.

Question No. 4

How adequately did your experiences as a student and teacher and as a researcher at Los Alamos prepare you for your work at Bell Labs?

RWH: Well, at Los Alamos I learned several things. Most important, I learned all I didn't know. It was the first time I'd seen really classy people up close. And I realized I knew nothing. I really mean it.

HOP: Who were the classiest there?

RWH: Fermi, Teller,...

HOP: Yes.

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* Immediately after the war, Fry was made director of Switching Research. By 1950 he was an assistant to Executive Vice-President M. J. Kelly, who became president of Bell Labs in 1951. - Ed.

RWH: ...Bethe, Feynman. I was learning computing from Feynman from moment to moment. He was always around under foot. Watching him, I realized how little I knew. Mind you, I had minored in physics. Secondly, I became much more receptive to nonmathematicians.

GBP: Von Neumann?

RWH: Yes, I saw him only at a distance. I saw him later at Bell Labs. But I rarely saw him up close, anymore than I saw Bohr up close.

HOP: First, you said,...?

RWH: I had learned nothing about math and physics, and secondly I was receptive to the fact that nonmathematicians might know something. Mind you, I found that the average doctoral training gives you the impression that only mathematicians are worth knowing, and they know the truth, and anything else is inferior. And it is true of every field I have found since then. Every field inculcates in its practitioners, without saying so, that this is the greatest thing out. The BTL History will indicate that BTL is the greatest thing out, won't it? See what I mean? Well, I've learned that.

HOP: Well, that was a leading question.

RWH: Well, another thing, I had learned is that the use of a computer can solve what could not be done in the laboratory. You cannot make a small-scale bomb and try it out. You either simulate or you don't do anything. You can't really do small-scale atomic bomb experiments. You have got to have a critical mass. I saw the essential role of computing in experimentation, in that one lesson.

GBP: Did you know Kolsky in Los Alamos?

RWH: Kolsky? No.

GBP: Harwood George Kolsky. He probably came after your time. He was the man I spoke of that went to IBM. I heard him talk about computing. He was a physicist by training, but they made a computer out of him. And he said he used to have trouble with his budget. However, when he reminded them that he had computed some answers they needed and thereby saved them the $1 million which an experiment (the detonation of a bomb) would have cost, he received the budget he needed for

RWH: No, I agree with you. A counter example - partly! Remember
his moon guidance! On the other hand, J. B. Johnson did. But I think I've done pretty well. After all, some things in digital filters and some other things in fields which I have taken up recently are named after me. I think I have been more productive than I expected to be by following my own advice.

But, let's go back. I used to fuss at Schelkunoff and say to other people, "How can I avoid being like almost everybody else in the laboratory? Why can't I be like Darlington, J. B. Johnson, and a few others?" And one day Schelkunoff says to me, "If you are worried about it, you don't have to." But I think, Henry, you have a real problem in management there to make people conscious that this is the normal pattern. Do they wish to follow it? If not, what do they intend to do so they will not end up like so many people? For most of my years at Bell Labs, I argued that you should retire everybody at 55 on full salary (research department only), and everybody would be ahead. We would lose some good people, but we would also get rid of all the others.

There are two concepts. You know the concept of critical mass, critical mass of scientists. But there is another concept, I call "sound absorbers." Any scientist who has a new idea has the urge to go tell ten people. Now if you bump into some smart guys, they'll say, "Yeah, I thought about that," or "That reminds me of this," and so on, or "Hey, I'll look into that." You meet these damn sound absorbers, and they say "Yeah, very interesting, da..." And the idea dies away. If you have too many sound absorbers in the place, ideas just vanish. The trick at Bell Labs was for me to learn to avoid all those sound absorbers. Just simply walk by them and not speak to them. Just talk to the guy who has something to bounce back to you. I think that, on the average, you would do more good, by getting rid of all those big sound absorbers at 55 to 65 than the harm you do losing a few guys like Darlington and a few other productive people. The bulk of them are less productive. IBM, you know has a retirement rule, retirement for executives. I can only commend it to everybody at Bell Labs, that the top management retire early.

GBP: The navy sort of pushes them out early.

Incidentally, I've got a letter, which I should have mailed today, which I wrote over the weekend. I talked with Fred Reines about an experiment in physics. I told a lot of other guys. Reines is the only guy who seemed to respond. He said, "Put it in a letter. I'll see what I can get done." I am proposing a simple experiment to be done in physics. It won't take very long, and it is the kind of experiment which - the letter says and I think it is true - will wind up in textbooks, whoever does it. It is a very vital experiment on the foundations of quantum mechanics, but almost nobody will lift a finger. They'll say, "Yes, very interesting," but they'll walk on. But a guy of Reines's stature, who is really good to begin with, is able to say, "Yes, that's not a bad thing. Let's look at it." It is very interesting how only great scientists seem to be able to look at the important problems. That's why they're great.

HOP: Obviously this sort of characteristic in an individual is unstable, because if somebody once has it, by your own description, it disappears.

RWH: No, not if he recognizes what I have. Now, look what I have done, Henry. Knowing these things, I have <u>deliberately</u> shifted from hardware to numerical analysis, to software; I got out of that business into other things. I have done digital filters. I have gone back and written books on various things. I have consciously and deliberately moved a reasonable amount. In fact, were I president of Bell Labs and were it possible (it isn't), I would say, "The terms for employment in the research department are: you must make a moderate change in your field every seven years, or within ten we reserve the right to force you. You may not work in the same field more than seven years or, at most, ten years - to clean up - but roughly seven years. Maybe, if it drags on a bit more, we'll give you a couple of more years to finish it off. But you may not go down the same path endlessly." Because I think this is one of the things that does people in. They've already learned all the reasons why things cannot be done, so they can't do anything great. I feel strongly about this. I would say just that, at Bell Labs, for years, I would say it regularly. But it didn't make any of those guys change what they were doing.

HOP: And the one guy that you describe as continuing to be productive to the end, Sid Darlington, did not change his field.

his computing department.

HOP: Well, before we go to Bell Labs,.... Really, I have been asking some of the questions about the military part, but that's Baley's main interest here. Have we gotten into the things that you wanted to know?

GBP: Well, what we want to know is how to get a hold of the history, and you say there is no way to do it.

RWH: Well, the only thing I can say is there must be these <u>n</u> volumes. There are at least 17 or 18.

HOP: Personal?

RWH: No, these were volumes like the radiation series, written by individuals, but describing group activities. I was to summarize computing, how you did it, what we had done, and try to find out what was computing all about..

GBP: And there is no book that's labeled "Mathematics at Los Alamos?"

RWH: No, I don't think there would be any labeled "Mathematics." You see, I only had one, computing. I was busy doing that.

GBP: And you don't think David Hawkins tells the whole story.

RWH: I don't think anybody can.

GBP: Nobody knew it.

RWH: With an exception. Oppenheimer may or may not have known. Oppenheimer would come down, once a while, to our place and sit around and talk leisurely. As a result, instead of working sixteen hours a day, we would work eighteen hours a day for the next several weeks. He had a gift of inspiring people. I think he knew almost all aspects of Los Alamos. Now, I was also a member of the coordinating council, so I sat in meetings which coordinated all activities, and I was aware of everything around the place, but my job was in my little corner. And, I think there were many people like me that had, in principle, the vision, but everybody was so busy doing his piece that I think that very few people could have had the total picture.

HOP: How big was this coordinating council?

RWH: I guess twenty-five or more people. Oh, with deputies and so on, maybe as large as fifty, but I don't know how many were there. I can sort of picture a room with twenty-five to fifty people at a meeting, but there may have been more people who couldn't always attend. You know meetings, everybody doesn't attend all the time. Nevertheless, we were expected to come, so we were aware of what else was going on to some extent.

DML: And these met monthly?

RWH: I think something like monthly or oftener, I don't remember. In a sense, I knew what was going on at remote locations, just as in a sense they knew what was going on in computing. But how much can you know? You say, "We've got another two bomb designs done," or something else like that. They could not know the details of the common picture.

GBP: Where did Brockway McMillan work at Los Alamos?

RWH: I don't know where he was. I think he was out in one of the X Divisions* but I don't know. I've often wondered.

GBP: Los Alamos is one place in the United States I have never been to.

RWH: I was there just last December.

GBP: Never been there. I saw it from a distance in an airplane flying from Santa Fe or something like that, but I have never been there.

RWH: Well, I am a consultant there now, and I don't have an idea what's going on still. But after all, Henry's been a boss at Bell Labs for all these years, and he doesn't know what's going on in much of the place.

HOP: Much less than I used to, in fact.

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* Explosive Division, a different mesa from ours.

encourage that to happen?

RWH: Well, I'll give you an answer and really what you should have done. I told you, I went to lunch with the physics table. The Nobel Prize broke it up. Also McKay and some other guys got promoted - another one of the guys was Molnar. They all disappeared, changing jobs, retiring, and I was left the dregs. No use eating lunch with them. In the far corner was the chemistry table. So I went over to the chemistry table. "What's new in chemistry? What's important in chemistry?" Pretty soon, "What are you working on that's important?" Finally one day - and I am a son of a bitch - I say to the whole table, "If what you are working on is not important, nor do you think it is going to lead to something important, why are you working on it?" I wasn't welcome any more. But, six months go by, and one of the guys (the guy who married our secretary), Dave McCall, stops me in the hall and says, "Hamming, you really got underneath my skin with that remark." He said, "I spent six months thinking about it. I haven't changed my research, but it was well worth while." So I said, "Thank you," and walked on. Two weeks later, I found he was made boss. The only one in the bunch who was willing to think beyond his nose. The rest of them could not.

I'll say again, almost all scientists spend almost all their lives working on problems which they themselves know are not important and which they do not believe will lead to something important. With one life to lead, why the hell they do it? Unless that is the life they want!

HOP: Well, why do they do it?

RWH: Because I think they will not plan, they will not stop to think, they are unable. Or just like Miss Gray, like Kaiser, or various other cases, they are unable to go down that lonely path of trying to do something great and run the risk of failure and have nothing. They want the steady, immediate gratification of safety, and I can't say Bell Labs puts any pressure on you at all to be safe. No boss of mine ever told me I should do that. No, they choose the path themselves. You are not told this by management. Nor does the president of the university say that to his hired help. They make it clear they want you to do great work. But how few people do it.

a tremendous sea of detail. He is a genuine genius, but it´s minutia. Does anyone want to know 125 pages on random numbers? Does anyone want seventy-five pages on Euclid´s algorithms, when the real problem of Euclid´s algorithms is the following one (which I leave to you to work on, as an exercise)? Given real numbers (I mean numbers in a computer), how do you decide when Euclid´s algorithm gives a common factor or when it doesn´t? When do you decide if two polynomials have a factor, given real numbers? Nobody´s worked that out. But that´s the problem. He´s got seventy-five pages on Euclid´s algorithms, with that left as a starred exercise somewhere, with no discussion in the text whatsoever of the real problem. He did what amused him, rather than what the problem is in computers. It´s fundamental, because that´s a very, very fundamental result, and one needs it. It´s the function and the derivative having common zeroes which gives you the multiple zeroes. Right? But given real numbers, how does one choose? In this case, you can´t say, "Well, it is bounded; I don´t know." You´ve got to choose one side or the other. It has or has not a factor. How can you make a rational choice, because there is a great deal of recursion to the numbers all the way down the line? It is not a simple answer. Bounds will not get you anywhere, you must make estimates. He doesn´t come near it. He drifts, as throughout all things.

I will say again, a genuine genius. I´ll give you evidence of his genius. Genius often is simplicity. After years of Stanford´s refusing to teach FORTRAN, and insisting you must learn ALGOL, he was a genuis to look to see that almost all problems were in FORTRAN. Simple, and it´s only a genius who can do things like that. He finds that 95 percent of the problems are in FORTRAN. He says, "Well, if they are all in FORTRAN we´d better teach them a little bit of FORTRAN." He has the gift of cutting through a bunch of garbage down to things, but he has only a minutia view of the vast range of knowledge. He has not a firm overview.

HOP: Over and over again in your discussion and in your talking about people, you have emphasized keeping your eye on what is really important, ...

RWH: Of course.

HOP: ...the most fundamental, the thing that really makes a difference. Now how in the world do you run either a university department or an industrial one so as to

RWH: Any large organization is bigger than anyone can know.

GBP: I spent forty years at the University of Kansas and I am unknown there.

RWH: You are known, to some extent.

GBP: And I don´t know the university anymore.

RWH: I happen to know more about Kansas State than I know about Kansas, but you were known to me long ago.

<u>Question</u> <u>No</u>. <u>5</u>

What areas of research were being emphasized when you joined the mathematical department after World War II? Would you agree that Shannon and Tukey shaped the character of the mathematical group in the postwar period? Who else determined the tone of the department?

HOP: Anyway, you decided to go to Bell Labs for three years, what did you find when you got there?

RWH: What I found was, network theory had dominated the math department.

HOP: You went into Bode´s department?

RWH: Yes, and as I said, he was shocked. Now, those who were there at the time were people like Blackman, Bode, Darlington, Dietzold (who was my immediate supervisor), Froelich, Gray. Lakatos was there. MacColl was there, Schelkunoff, Shewhart, and an old guy named Zobel, whom you´ve missed.

HOP: We had him yesterday when we talked to Thornton Fry.

RWH: Now there were four young Turks: Shannon, Ling, McMillan, and Hamming. I hadn´t known that we were called the four young Turks behind our backs, but we were. We knew we were the four troublemakers. Now, in a sense, of the four I am the failure.

HOP: Well, in what sense were you troublemakers?

RWH: We didn´t do things properly. We didn´t do network theory. We did everything else, everything we shouldn´t have been doing. For example, every mathematician knew that computing was for those who couldn´t do mathematics. You did network theory by complex variables, the way Bode did. Anybody who resorted to numbers was beyond the pale. I was a troublemaker that way. Shannon obviously was a troublemaker producing information theory. Ling was very good in military work and at algebra. Both Ling and McMillan, you know, ended up vice-presidents. And Shannon is world-famous. I'm just the failure of the four.

HOP: You are just as famous.

RWH: Oh no! Anyhow, I came there, and I found network theory dominated the department, but we were moving into missiles. (Oh, Tukey was there, too.) The first practical thing I did of any size was I took some diagrams from Tukey, went up to MIT to use the MIT differential analyzer, the RDA No. 2, to study trajectories for the Nike guided missiles. It was a beautiful machine, and Tukey had supplied beautiful wiring diagrams plus a very good plan of computing.

HOP: This was the beginning of the Nike business?

RWH: Yes, back before the fall of '46, probably. Ling and Tukey had given me these things. I went up and started to run trajectories. Now, I had twenty minutes to look at a trajectory develop before I committed my next trajectory run, and the total trajectory took forty minutes, so I had lots of time to think and regret the choice I had just made. And I discovered that a vertical launch was better than slant launch. I found the proposed wings were much too large, and I began to realize that the formulas that Ling had given me for swapping wing size for drag and so on must have been local linear approximations. So I came back and said, "Look, these formulas can't be right. I am going much too far. I am going down to one-third wing size." Ling said, "Yep, you're right." So they gave me some new numbers, and I went back and I got some new trajectories. And they were happy about the vertical launch, because they had planned for a slant range launcher. They were wondering how they would swing it around and point it in the right direction. I had found that a vertical launch is much better. Well, that means I <u>really</u> learned analog computing right after <u>digital</u> computing. I had a very strong experience with analog stuff (which we'll come up to in a little while).

HOP: You mentioned Tukey and Ling here as your primary contacts.

you are going down the path of 340 years, a million-fold growth of knowledge. You cannot stand it.

GBP: Nobody sits down to think about 500 years in the future, or even 100.

RWH: Why not? I do.

GBP: Why, I think they should.

RWH: You shouldn't say nobody. I think we're both exceptions. To quote a theorem.

GBP: There is one conspicuous computer scientist that neither one of you has named. And I don't think you should skip him, since you've sort of covered the field. <u>Knuth</u>.

RWH: All right, I'll take him on. In mathematics, students who do well up through calculus often do not do well in advanced mathematics. They are what I call the "minutia people." In order to do a multiplication, you've got to be able to take care of a lot of little details. In order to do calculus, you have got to pay attention to a sea of minutia. But advanced mathematics is not a sea of minutia. Knuth is a genius, but he is a minutia man. Because programming, which requires incredible attention to detail, is the entrance way, computer science tends to attract minutia men. Knuth is the greatest. He is a genius, but a minutia man.

Now, the evidence is as follows. I was commissioned by several guys at Bell Labs to talk to Knuth about writing volume seven next, not in order.* So I go in, I stick my feet up on the desk, and he's got his feet up on the other side - you know he's six feet six or something, a big man. I had written a rather nasty review of volume two, and he had the gall to bring it up. I said, "I'm not taking back one word of it, but I am perfectly willing to discuss it." And we set out, hammer and tongs with no punches spared as it were. I beat him down to admitting that what is in those books is what has amused him, not what a computer scientist should know. They are filled with

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* <u>The Art of Computer Programming</u>

RWH: The second edition and the third edition were worse.

GBP: The great big theoretical calculus - it was too difficult to be taught in the ordinary school - was the one written by Osgood. It had about 400 pages in it, but it was too difficult, too theoretical.

RWH: Granville was widely used, though.

GBP: Granville was widely used, but if you look at George Thomas...

RWH: That was a thousand pages, or close to it.

GBP: Yes, they were too heavy to carry around.

RWH: That's right, they're depressing.

HOP: Okay, but the reason for that is very simple. Because you've got an audience that is forced to buy the darn thing so, if you increase the price, you make more money.

RWH: The professors do not have to adopt the book.

HOP: The parents of the kids....

RWH: The professors are the ones responsible.

GBP: I think the professors are responsible.

RWH: It's easy to fall into the syndrome. For a calculus course, somebody gave me the outline to cover every day. I used one of these thick books, not Thomas but the equivalent, Thomas and Finney. What a stinking book!

GBP: Finney helped to make the later edition.

RWH: Helped ruin it.

GBP: You could try Lipman Bers's book, which is real heavy.

HOP: Quite a parcel.

RWH: They are filled with result, result, result. Nowhere will you understand how to do mathematics, how to create theorems, how to find proofs, or anything like that. You are given all the published theorems and results and a bunch of exercises. You are never told how to do mathematics. If you continue on that path,

RWH: For Nike, yes, but otherwise, Dietzold.

HOP: ... what about Bode and MacNair?

RWH: Well, let's go back. We will work on down here. You indicated you believed that Shannon and Tukey shaped the character of the mathematical group. And my answer is "no." Bode had a tremendous effect on me, and above Bode were guys like Bown, Fisk, Kelly, and Baker. All these people had a heck of a lot of effect, because they set the tone. For example, Shannon would come in at ten o'clock, play chess until one, and go home for a whole year. Without Bode and those people above him to protect him, he wouldn't have done the information theory. They made possible what we did. All things considered, they gave us a remarkably free hand.

I disliked Dietzold for years, until one day he said that he never had had an idea of his own, all he'd ever done was take Bode's ideas and translate them. And then I realized that Dietzold had to be really smart; he saw what he could and should do and he did that very well. He took me around to all kinds of places. He took me to West Street, this, that, and the other kind of place, and dumped me into the Nike business, and so on. They exposed me widely to a lot of things, and from that, gradually, where I could react, I did. But they took a great deal of effort to do that. And I think Dietzold, while he may not have had great ideas himself, was very smart in recognizing that Bode needed support as well as in guiding me. Ling, you know, became Bode's chief translator after awhile. Bode was totally incomprehensible to most of us - yet he was very effective!

HOP: But I had always had the impression that it was Bode and [W. A.] MacNair that...

RWH: No, MacNair had a great deal to do out at the military side. But I didn't see much of him then. At Whippany, where I went one day a week for many, many years, I didn't see MacNair very often, although I saw him when he was in Sandia. He had me come out for a week at Sandia one time. No, I suppose he did have a lot of effect, but I don't really know. It was Bernard Holbrook, Hendrick Bode, and John Tukey who did the first trajectories by hand and laid out some of the original design of the Nike, in a short period of something like two or three weeks. The three of them laid it out. And the final design was remarkably close to the original one, although it deviated quite a lot along the way, it came back to it.

HOP: One of the things I remember being told once is that it was Clara Froelich together with a team of computresses who computed these.

RWH: Sure. Froelich was in charge of the hand group.

HOP: You know. John would sit there and look at them.

RWH: John is definitely difficult that way, too. Well, I will tell you what did happen. John used Milne's method. Well, years later, I found that Milne's method was unstable in the middle range. I had just taken the method and assumed it always worked. It was an unstable method. It worked sometimes. Clara Froelich was in charge of the hand computing group. (When her mother was alive, and living with her, Clara was a bit of a bitch. The moment her mother died, Clara became a very lovely person.) She adjusted for the instability of the method.

HOP: Clara had been hired by Thornton Fry way back.

RWH: Way back when, and I believe gossip has it that she thought she was going to marry him at one point, between various marriages of his. But we won't get into his sex life, I hope. It's spectacular. We'll just leave it alone. There are some marvelous stories.

HOP: But I am curious now about the division of labor in computing trajectories. You took some to MIT. Clara Froelich did some with a team she organized, by hand,...

RWH: Some of the girls, yes.

HOP: You and John and others...

RWH: I did the analog ones. John had a very clever arrangement for the RDA #2 from which we got calculations of variability off the true run, so it told me, more or less, how to perturb things. Froelich did the simplest hand calculations, crude trajectories, but they exhausted her computing resources. They couldn't do very much by hand. After all, you get around 2000 operations a day out of a girl, and if that doesn't get you down the trajectory, you can't put a team on it to do it faster. There is

go down that path on which we are now marching.

GBP: Well this is certainly what has been happening to the mathematical scientists since World War II. We used to have one subject and now we've got the Conference Board of the Mathematical Sciences.

RWH: For my doctorate, I had to pass exams on applied mathematics, geometry, algebra, and analysis. I had to know all four subjects to get a degree at Illinois in 1942. Come on, now you can get by with only topology. What do you propose to do, sir, in 340 years with a million-fold doubling? Alternatively, science is not going to continue at its present rate. One of the two. Actually, one of the three things: you're going to have a million times as many fields of expertise, the rate is not going to continue, or you're going to do something different to cope with increased knowledge. It's a favorite topic of mine.

HOP: I'll give a Hamming solution: burn the library.

RWH: Well, that book I'm trying to write, Methods of Mathematics Applied to Calculus, Probability, and Statistics, says clearly in the preface and in the text, there is so much mathematics now needed - both pure mathematics and needed applications - that we can no longer hope to tell you what you need, we must teach you how to find it for yourself. It is hopeless to try to cover mathematics now. We must give up the results and teach the methods. Mathematics as now taught is like taking you through an art gallery of finished paintings; nobody tells you how to paint the picture or compose. Nobody tells you how to find the theorem or anything else. You are only shown finished theorems and finished proofs. But that is just plain wrong for teaching. It may or may not have been good in the past. We'll let that go. But it is hopeless for the future.

GBP: Look at the calculus book that I studied, I really studied Love's calculus.

RWH: The little red book?

GBP: Yes.

RWH: It was the same one I did!

GBP: A thin red book.

stimulating, and in the long run, you are more likely to know what to work on than if you are left alone to work on what you think. Contrary to the theory, constant interaction with the real world and interruptions, which are annoying as hell, are desirable, I think, for most people most of the time. There may be a few exceptions.

GBP: Maybe that's why the war developed so many good scientists.

RWH: Right. I believe in getting out of the structure. I think that the war forced us into other things. As a result of this belief, when Prim was reorganizing the Math Department I said, "Don't put me among all the numerical analysts. Put me next to Gilbert or somebody else in another department, where I can, by association, learn new things and be forced to new things." You tend to go to lunch with the guy next door to you. Don't go to lunch with your own people. In fact, I had a fixed rule for years. I used to eat with the physicists, a well-thought-of bunch of guys, McKay, and so on. And it worked out great. I learned a tremendous amount by eating with those guys. Don't go with your own people; you won't learn anywhere near as much. Go out where the stimulation is high. Even if you believe that great science is luck, like lightning, you can at least stand on the mountain tops where lightning is likely to hit you, rather than in the valley where you're safe. The average scientist spends all of his life working on safe things, and this goddamn government grant system encourages it.

GBP: You turn in a proposal on something you've already worked on?

RWH: It encourages you to work on safe things all the time, and that is not what we want.

Incidentally, I can give you a couple of numbers which I find devastating. To a great extent, since Newton's time, knowledge has been doubling every seventeen years. Bell Labs doubles its population every seventeen years. That's a very accurate figure, back even to when it was an AT&T division. Never mind what they say, it's been doubling every seventeen years since I came, and before. Well, up to the present, we have coped with it by specialization. Project 340 years forward, twenty doublings, one million. We will have a million fields of specialization for every one now. We do not want to

no possibility. A pregnancy requires nine months, and putting more women on the job will not speed up the delivery. Similarly, it takes you a long time to get a trajectory by hand. You can't put nine girls on a job and speed it up.

Incidentally, in case you have prejudices, let me point out my prejudices. There was discrimination in Bell Labs in those days, against men. A man had to have a Ph.D. to get into the Math Department. A girl could come in with a bachelor's degree, in almost anything, get a job doing hand calculating, and some of them were ultimately promoted to members of staff elsewhere. There was discrimination against men in the sense that no man could get his foot in, women could.

On the staff, there were Sallie Mead and Clara Froelich and Miss Gray. I am not sure that Miss Froelich was on the staff, but Miss Gray and Sallie Mead were both on the staff.

HOP: Packer.

RWH: Margo Packer, yes I guess, but she reported to Froelich. She's still in Washington, I understand. Packer was a headache. There were some other ones.

HOP: Of course, there was Marian Gray who had a Ph.D.

RWH: Marian Gray, she was a very lovely person. We'll come down to her in a little while. I have some comments on her.

Shannon had an effect on me in several ways. One is, I was busy doing computing for others all the time and watching him do things. I said to myself, "You know, I could have ideas if I would take the time. I've got to get loose from all this running from moment to moment. Then I can have ideas, too."

Secondly, he taught me the importance of publicity. I can tell you the story. He built this first mouse maze. It was a crude thing built out of mechanical parts with an arm moving above the maze. Being in West Street, I'd seen a reed relay, and I came back and said, "Look, Shannon, at these reed relays. You can put them below, and the magnet above would tell the circuit where the mouse was, so you wouldn't have to have everything built above." So he rebuilt the maze, and it was much better. And then pretty soon I found him having a shop order to build a very good third model. That's probably the one you saw. I said to myself, "Why does a man with his talent waste his time; there is absolutely nothing new on a beautifully engineered job." Well, I let it go, I just wondered. I saw it put down in the concourse. I saw the effect, and I saw that he knew it was not sufficient to have ideas; you've got to advertise.

The same way, everyone wanted to call it communication theory, but he insisted on information theory. He knew it would be more accurate the other way, but it got much more publicity with information theory as a title. He knew the art of advertising much better than most people did. And I learned from him that it's not sufficient to have ideas; you've got be be able to market them, or you might as well not have them. So I learned a lot from him, but not directly.

He was an extremely private person. He did his work, and he told nobody about it for long periods of time. Then he would come out with this, that, and the other theory, but he was a lone wolf all the way down.

HOP: Who was with him at that time?

RWH: Nobody. Nobody ever worked with him.

HOP: Well, Hagelbarger hadn't come yet.

RWH: Hagelbarger was a friend of his like...

HOP: Ed Moore?

RWH: I worked with him closely in the sense when he got a Mechano set for Christmas from his wife I used to go over to his house some evenings and play with the set on his living room floor. I shared an office with Shannon and Sallie Mead for a while, a great big office up in the attic in Building 1, before Building 2 was completed. I probably knew him then as much as anybody did.

He was essentially a lone wolf. His closest friends were Barney Oliver and Pierce, and it was an association of minds rather than subject matter. But Shannon was very able in lots of ways. But I don't think he had as much influence as he could have had, because he was a lone wolf.

way any of us remember it either. He says he's got file cabinets full of evidence. I have only memories which say exactly the opposite. He says Von Neumann invented internal programming. We have evidence he didn't. Von Neumann never claimed it. He was only a consultant to Mauchly-Eckert. All kinds of things, he's gotten totally wrong.

GBP: That's what I've always heard about the book, that it's full of errors, in spite of the fact that he claims his documentation in the preface.

RWH: That's why I cannot write a history. I do not have three file cabinets full of data. But I have a memory which is quite different from his. But in justice to him, the last dozen or so times I have met him, he has not pulled any of this Von Neumann business with me. He transcended it, but it took him a very, very long time. I think I might have too. It's a little hard not to. It took me quite a long while not to quote Tukey. Tukey was always in my conversation for many, many years. "Tukey said this, or Tukey did that."

GBP: Goldstine has been writing on history. He wrote the one on computing machines. He has just published a book on The History of Numerical Analysis.*

RWH: He has? He probably thinks numerical analysis is simultaneous linear equations.

GBP: And I've heard comments from the top people at the Institute that they don't think it works.

RWH: Incidentally, in my opinion, the Institute for Advanced Study has ruined more great scientists than any other place has created - judged by what they did before, what they did after. That's the criterion. Look at what they did before and what they did after. The Institute for Advanced Study is not good working conditions. The trouble is, what all of us think from moment to moment are good working conditions, are not. I've said, a closed door with no interruptions sounds good. You get on with your work; that is bad. Constant interruptions from other things are much more

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* From the 16th Through the 19th Century (New York/Heidelberg: Springer Verlag, 1977).

It's a very common thing, which partially explains the phenomenon that everybody knows. In mathematics, theoretical physics, astrophysics, almost all the best work a man does is done very young. It's a well-known phenomenon. How can you explain it? That's one of the explanations. It's a very difficult thing to manage. Scientists need management. Ed David straightened me out one time, along that line. He pointed out that I hadn't done anything good for a while, and I said, "Oh yeah, I've written something up." I sat back, thought for a while, and said, "It is, important to write up small things." It's a very common thing. Also, if you can't learn to abandon a bad project, you're through. The first lemon you meet, you're dead. You must learn to abandon bad ideas.

Shannon clearly got caught on this syndrome of What do you do for an encore for information theory? You can no longer do simple things. Obviously, he was very talented in lots of directions. Plus this other curse I told you about just did him in. Now I'm not saying, having done information theory, he wasn't entitled to all the leisure he wants in the world. We're talking as scientists. As a productive scientist, he was done in by a simple feature like that. But perhaps he had a happy life.

GBP: You made only a passing comment about Herman Goldstine.

RWH: I used to have a game. When someone and I were coming up to Goldstine, I would say, "I bet you within three minutes he refers to Von Neumann." And for years I was right. Suddenly, I started losing. He finally outgrew his association with Von Neumann. For a long while, he could not avoid bringing in "Johnny and I did so and so," or something else like that. He could not avoid doing that, for years and years and years. But he outgrew it.

GBP: He wrote a book on the history of machines, I believe.

RWH: I know. From Pascal to von Neumann.* That ain't the

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* Herman H. Goldstine, The Computer, from Pascal to Von Neumann (Princeton, New Jersey: Princeton University Press, 1972).

Tukey had a lot of effect, too, but he was only there one or two days a week and he spread himself much too thin. But we will worry about that later.

I think Don Ling had a fair amount of effect on me because he was a guy who also was an algebraist, and he did what the job needed. That's the thing I learned to do. He worked heavily on Nike with Bode.

HOP: What had Don Ling done during the war?

RWH: I don't know. I don't know his background. He's still alive, you know, in Albuquerque.* But I don't think it'll do you any good.

HOP: In New Mexico.

RWH: He's in Albuquerque. I can give you his address. I phoned the other day, and he's able to walk ten steps only now.

HOP: Oh, no.

RWH: And, in the conversation, I finally cut it off because his voice was going downhill rapidly.

HOP: Is he able to play the piano at all?

RWH: Not if he can only take ten steps each time. He's in very bad shape. I was going to talk to him. I said "I can catch a cab and come out to see you", but as the conversation went on I decided I better not catch a cab and go out to see him.

HOP: Who's taking care of him?

RWH: At the time of the phone call he had a full-time nurse. His daughter was there for a while, wrote some Christmas cards, but I just don't know. There are some other people around, I heard, to keep an eye on him somewhat. But I just don't know. I was afraid to push the conversation. As I say, his voice was getting weaker and weaker even in the five-minute conversation I last had with him.

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* Ling has since died, in July 1981. - Ed.

HOP: You say he had an influence?

RWH: He had an influence because from him I learned you do what needs to be done, never mind anything else. Whatever needs to be done, you go do it. As an example, he reduced data at White Sands by eye when the machine busted; he could reduce it by hand! He did whatever needed to be done. When Nike got going and well along, he spent a lot of time, perhaps a year of his life, in Washington trying to persuade the military that it would neither solve all problems nor it would it be nothing. He tried to get them to make a just evaluation of what it would do. He saw that was necessary. I learned a fair amount from him.

I learned negligible from McMillan.

HOP: Well, what did McMillan start to do?

RWH: McMillan. Well, we'll come up with some other things first. You understand, having retired makes you think about your career. And having picked up several honors including this latest one, this National Academy of Engineering, makes you wonder, "Why me?" I don't know that McMillan ever had a great idea. He proved some variations and put some mathematical rigor into Shannon's work.

HOP: Yes, and there is a basic lemma that he's credited with in this theory.

RWH: That is, Shannon already has got all the results. McMillan merely put some rigor into the thing. I don't know anything that he did. But that doesn't mean he didn't do something. I only said I don't know anything that he did that really mattered. It seemed to me he talked a lot, said a lot of things, but he was frequently flat wrong, and he'd insist upon some things when he was wrong. But perhaps that makes a good vice-president. He may very well be good, but I just don't know. But in mathematics, I don't think he had much influence at all. He certainly was not one of the strong people in the bunch.

But I don't think that Shannon and Tukey completely shaped the character of the mathematical group. I think it was really shaped to a great extent by management indirectly by managing us. At the same time, we weren't manageable. It's clear that Bode would have preferred me not to have messed with computing. They tried to get me to do classical

takes so long to realize that you have got to be able to sell an idea. You have to be able to write the theorem up well. If you can't....For example, my doctoral thesis. I gave one talk, and I realized that it was a subject nobody could care about again. If I wanted to do research anybody would listen to, I'd better change subjects. So I did.

Well, apparently Shannon told management that he didn't want to be fussed over. Every visiting guy that came to Bell Labs wanted to see Shannon, and so the Labs gave him protection. But the truth was that he wanted to be fussed over. He thought if he went to MIT, he would be fussed over - an endowed professorship and all that sort of thing. But believing that he was modest, he stayed at home, didn't come down to campus. Dave Rose, who went up there from BTL, says, "You know I never saw him on campus. Neither did some other people." Now, as proof of my contention that I am right in this situation, I spoke to Barney Oliver one day. I met him by chance, just after he made a trip to Russia and to Israel, and said, "How's Shannon doing? How's my old pal Shannon doing?" He said, "I've never seen Shannon happier." I said to myself, "I'm not surprised. He just went out and has been fussed over again." There is a trivial thing, a belief that you want to be left alone, but the truth is that you want to be fussed over.

Plus, the other syndrome: What do you do for an encore after you've done information theory? Einstein had the same trouble. If somebody at the Institute for Advanced Study had said to Einstein, "Al, old boy, just for my sake, drop this unified field theory for six months and go do something else, something that is totally different in physics." Nobody had the guts to say that to him, so for the rest of his life, from 1917 on, he worked on a subject which produced essentially nothing. Essentially nothing in the following sense: when a new paper of his came out, I asked every physicist at Bell Labs, "Have you read it? No? You can't believe it is important then, if you haven't read it." By the test: did they read it? No. The physicists did not read what Einstein was doing. It is a common syndrome that great scientists have great trouble with. Even I have had trouble with it at times. When I do something big, then I begin to work on simple problems. It is out of small things that big things grow.

RWH: Very puzzling. I used to go to lunch with <u>Backus</u>, Greenstat, Herrick, and the rest of the guys when they were making FORTRAN. I was using the 701 machine with Weiss, and we'd go to lunch about once a week over to one of the various restaurants down there. All the time, I tried to persuade him there was a problem with debugging, but he would never admit there was a debugging problem. Nevertheless, when you consider the hostility he faced, everybody said, "You can't do it. If you can do it, we don't want it. In any case, it's no damn good, and no decent programmer will use it if you do." He persisted and put together a very good job. That you simply can't take away from him! On the other hand, he has had tremendous leisure since then, and he really hasn't delivered the goods. His Turing prize speech was a promise of something, but he has never delivered the goods. He is a puzzle. He obviously had the ability once to put his mind to something and stick by it and deliver very, very good things. But he hasn't done it any more. He's like this guy Gomory coming here tomorrow.

HOP: Ralph Gomory?

RWH: I'm supposed to have lunch with him or something, for a half hour or something.

So many people do something...<u>Shannon</u>...let's go back to Shannon. The theme I've been <u>concerned</u> about all my life: What makes great scientists and What happens to them. When Shannon was offered this Donner Professorship at MIT, I said, "If he takes it, that is the end of his professional career. He'll never do a damn thing again." Gilbert, McMillan, Slepian, everybody else gave me a hard time. (Just before I left, I went around and gave them a hard time back.) He hasn't - not that he couldn't.

HOP: Why did you expect it?

RWH: Well, I had inside information of a lot of kinds. In the first place, it takes one to know one. But I could see he has good manic depressive cycles - a real pronounced one - so I knew this about him. Gradually, I recognized that, although he believed he was modest and didn't want to be fussed over, the fact was he did.

This was part of what I learned about advertising. I asked myself, "Why does this guy know so much about that? How did he learn it? Why was I so dumb?" It

mechanics, which is what J. A. Lewis ended up doing. I took a* couple of courses at Columbia from Ray Mindlin. So I did a few problems in that area. But what kept happening was that important problems in computing arose. For example, I might as well tell you one which I have used again and again. I was asked, "Can you compute Bode's phase-gain integral?" I don't know anymore than you do what it is. I said "Is it linear?" They said, "What do you mean, 'linear'?" I said, "Twice as much in, twice as much out; the sum of two in, the sum of two out." They say, "yes." They will have sixty measurements and want sixty answers. I say to myself, "Every linear transformation is a matrix multiplication. There must exist a matrix 60 by 60 which will do the job." Now I know that the Accounting Department has some IBM 601 computers, the same type we used at Los Alamos. I know the speed is about one multiplication operation a second (additions were free). I say, "Yes, I can run off one data reduction per hour." They say, "Fine, let's see you do it."

I take the integral home. I look at it. It's the Cauchy principal value integral! Since I said I could do it, I'll do something. I finally interpolate cubically over the singularity nicely, but I find after a while that I can't do the computing at the ends of the interval. I have to have more data beyond where they want the answers. So I come back and say, "Look, I cannot do 60 by 60. You have to give me 63 measurements and I'll give you 57 answers." They agreed. "Furthermore," I say, "you've got to tell me whether it's zero, constant, or linear tapering, or what beyond the ends of where you make measurements. You've got to give me some information out there." "Fine." And so I have to design a program which will do any case they want.

Lastly, I look at the thing for a while and I see that the values that they had given me, arithmetically spaced, are not going to be calculated equally accurately I think. So I come back to them and say, "Look, you need these values geometrically spaced in frequency, not arithmetically." And they protest, and I fuss a bit. They go talk to the theoreticians and

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* A BTL consultant and Columbia University Professor.

then say, "We now know why you want the frequencies logarithmically spaced. Put your integration that way rather than arithmetically." So we agreed.

Well now, the reason I bring this out to you is - what it comes down to - consider the training I needed to do this job. I redesigned the whole experiment. I never went down the manhole and measured a single thing, but I needed to know a fair amount of abstract mathematics in order to do that trick. That's the role of abstract mathematics; you don't use it directly, but you need it so that you can see instantly: of course, there is a matrix; there is a Cauchy principal value; yes, you can interpolate locally to integrate, two sides will cancel out; and yes, if you want equal accuracy, something other than arithmetic spacing might be better. It's a very impressive thing to me how simply it was done.

Now they gave me some hand help to calculate a matrix, a Mr. Kingsbury from their network department. We ran them off. Later, after about a half a dozen cases (because I had only agreed I would do a couple), I went to Bode and said, "Look, Bode, I agreed to calculate a couple of these, but I didn't expect to spend the rest of my life at them." He grabbed the phone and, after about a five minute conversation, he says, "Tomorrow there will be a girl there. You show her how to do it, and you're out." So I got out. Years and years later they were still using the same method. They hadn't learned a thing!

Well, that is some of the sort of network stuff I got caught in. But you see, when that arose, they had to come to me to do some computing. Dietzold came to me one time (again hating himself) and asked me could I do some calculations of some other thing on the machine, which really came down to creating some orthogonal polynomials. I said, "Yes, but you've got to give me some money to pay for machines at the IBM Service Bureau in New York." And I continually did this until they realized that they were spending more money renting machine time than it would cost to buy me my own machine or to rent me a machine on the grounds. Well, they rented one, an IBM-CPC (card-programmed calculator), but they put it in the attic. They put it in the attic, where no one could see it, while the analog computer was down there on a main floor with windows so everyone could see it. But this digital computer was to be hidden. It was disgraceful.

GBP: Oh, that's what Peter Lax did. I knew Peter Lax had a connection with the Manhattan Project, but I didn't know what he did.

RWH: He pushed cards.

HOP: And Kemeny did also?

RWH: Yes, the two of them pushed cards. They were GIs. We got them off with all kinds of trouble. The army put in all kinds of rules about this, that, and the other thing, and we wiped the rules out.

HOP: After all these hours, you thought of some more names from Los Alamos!

RWH: They were not mathematicians then; they were undergraduates, or at best, beginning graduate students. But both of them were energetic and eager to learn. They took every opportunity to learn.

GBP: That's where John Kemeny connected with computing. And it came out later. The only thing he talks about is being assistant to Einstein and things of this kind.

RWH: Oh yes, he was a student of Einstein. He made it clear he was his student. We used to call him Von Kemeny. I visited Kemeny once for two weeks at Dartmouth. I helped him get the money to get the computing center up at Dartmouth, so he invited me to come up when they were putting in BASIC, and I spent two weeks up there. He has many talents, a very able guy.

GBP: He is retiring as president of Dartmouth.

RWH: Yes, but you know all the time he was president he insisted on doing some teaching to keep his foot on the ground. That is a very realistic view. So many top management don't do this. I admired him tremendously for that. I admired him in many ways, although admittedly he was snobbish in a way, and as I said, we called him Von Kemeny.

HOP: Greenberg?

RWH: No.

HOP: Didn't know him? Backus.

His numerical analysis book is unreadable. There is a lot in there. There are things which I, for the last five years, have looked at again and again, trying to figure out, "What was this man saying?" He probably has got it there, but I sure can't figure it out. Very nice guy, another real gentleman. I wish I could be like him, but I'm not.

HOP: Tommy Hull.

RWH: Genuine, second-rate, energetic. Incidentally, energetic is no small thing, like Joe Traub.

Joe Traub isn't first class by any stretch of the imagination. When I had him, I couldn't get him to see what the problem was. He did the wrong thing sometimes, which had to be undone. But because he had energy, he got a great deal accomplished. Energy is a good substitute for ability. Although, when you have a lot of energy and really no vision, you can be a curse. Traub had a moderate amount, but he really didn't see the problem. For example, that book of his is simply Newton's method ad nauseam. We really don't want to know that much about Newton's method; we really don't want to. And he couldn't see that, so we let him publish the book.

HOP: Bernie Galler.

RWH: I don't know much about him. I knew him as a person. I can't tell. He apparently had the brains to create MAD, but didn't even have the brains to say, "Since MAD didn't get going, why should I continue to teach my students a thing which isn't that much better than FORTRAN? Why should I teach them a goofy dialect?" But they persisted for a long while, and then they finally had to abandon it. But I don't know anything great he ever did beyond that. But that's no small thing, like Kemeny.

Kemeny's great contribution was BASIC plus doing a good job at Dartmouth and getting a Ph.D program in computing going there and putting in a good model. You know, he pushed cards for us at Los Alamos.

HOP: No, I didn't know that. Was there someone else who was at Los Alamos?

RWH: Kemeny and Peter Lax. Yes, Kemeny and Lax pushed IBM cards for us in the machine. They were GIs.

And there was no air conditioning! It was really very hot in the summer. So I got W. O. Baker up there one summer day and held him there for half an hour until his shirt was stuck completely. And he said, "Well, you know, the bright lights are kind of hot." And I said, "Yes, but if I change the lights down to small bulbs, we would fall over cables and couldn't see the holes in the cards." Next year, we had air conditioning! But not from Bode!

Question No. 5

What effect did the invention of the transistor have on the kind of work done by the mathematicians?

RWH: None, that I know of. Now, I did a lot of computing for them, and I got some interesting problems, but it never influenced the math department one bit that I know of.

HOP: Should it have, using your hindsight again?

RWH: No, no, no.

Question No. 6

You worked with Pfann on zone refining. Was this kind of collaboration with physical scientists mathematically interesting?

RWH: You mentioned Pfann. Recently I got out my copy of the book by Pfann and looked at it. Yes, let me tell you about Bill Pfann. Since you're probably interested in such things. He came to me. He looked dumb. He certainly knew negligible mathematics, and he was inarticulate (then). But I had earlier resolved the thing to do (which I had learned sort of from Los Alamos) was work with good guys, never mind the problems. And for some reason or other, I thought that he had a good idea. But his whole department, over in chemistry, thought that zone melting was no good. That was one of many failures of his colleagues to recognize good work. So I helped him out.

Now, he looked dumb (mathematically). If you remember what the index register is, it was the hardest thing to explain how to run the index register on the IBM 650. So I explained it to him in terms of a zone moving down the strip, and he got it immediately. And while his book indicates I did a lot of calculating, I really only helped him get started and let him do most of the work. I helped him get on the machine and helped him get things going. I did a little analytical work for him, to get a few closed solutions, but they were impossibly messy ones. But there again, the mathematics wasn't terribly interesting at all. It had no effect on mathematics.

Now, we come down to question number 7.

HOP: Before you go on to that, you started to talk about the CPC. How successful was that?

RWH: It was fabulous. By sheer luck, at that time I was going out to Rand with David C. Bomberger to look at the possibility of buying some commercial Gypsy-type equipment, some more analogue equipment. We were looking at their differential analyzer. I heard about the CPC machine they had at Rand, so I went around to talk to the guys just a bit, because I knew a CPC was coming to BTL. They had a gorgeous general-purpose board, a description of which they gave me, with all the wiring diagrams, to do the general purpose work. I changed it a bit (because they had had a mathematician who tried to · do everything mathematically instead of practically). I knew some of the stuff he put in was wasting time, so I threw it out and put some more useful stuff in. But essentially, I took their thing and with the help of Peabody got into general purpose computing promptly with their general purpose board, taking it directly from Rand. It was a very useful thing.

But to back up a bit. You see, one of the other things I did early, by about December of '46 or so,... I was asked by Bode, with Miss R. A. Weiss, to go in and help write some software for the relay computer number 5, which we were delivering to Aberdeen, because that contract required software as well as hardware. Furthermore, Bode asked to get some acquaintance with it, so I did a partial differential equation on the damn thing. The way the magnetic field went into a nonlinear magnetic material, and we got answers out of that by running it whenever nothing else was going on, as a backlog order problem. And that did win Bode over a little bit, because he suddenly saw how it was that saturation occurred. I had these B-H curves on tapes and I looked up the values; and I did the essential functional look up via paper tapes.

He was a very able guy, but by the time I saw him, he was already coasting down hill. Only by looking back and asking other people, did I understand what his great days had been. He obviously was a very good guy. He had a stroke then. And that's where you sons of a gun got me back to be a department head for a while again.

HOP: Howard Aiken.

RWH: That son-of-a-bitch, I couldn't stand him because I was one too. But he gave me a lot of good advice. One of his best ones: we were standing, I think, looking at one of his machines, and he said to me, "This is my last computer." I said, "Why?" And he said, "I'm tired of building them; I want to get back to using them." I said to myself, "It's good enough for Aiken; it's good enough for Hamming." Let's not get confused with the machine and the use. The use is more important than the machine. He helped me, by that remark, find my way too. Damn it, it's the use of the machines that counts, not the machines themselves.

Mauchly and Eckert, want to go on to those guys?

HOP: Yes, why not.

RWH: Mauchly and Eckert were two good guys. When they were bought up by UNIVAC, Mauchly took his vice-presidency to do what he pleased, and Eckert plunged in to do the job that they wanted him to do. As a result, Mauchly was gradually eased out, and Eckert is still very influential. When you go work for a new guy, for heaven's sake go work for him! Mauchly didn't understand this. Mauchly also, demeaning his old age, lied periodically in public about whether he had known J. V. Atanasoff's work. And his signature was right there! Bang! a visitor's signature with a date. But he tried all kinds of ways to get out of this. On the other hand, Mauchly and Eckert between them invented internal programming.

HOP: Householder.

RWH: A man with one interest, in one topic. Again a good personal friend of mine, but so myopic. Very good, but maybe that's all he had, and he played his one talent as best he could with that one little thing. It's better to do one thing well than nothing. But he was awful narrow, wasn't he? And almost impossible.

talk to him, and as happens when people are too great, he gave me the answer I already knew. But he couldn't hear the problem as to why it was inappropriate. And I have found this happened to lots of people when they become too famous, they think they know the answer rather than listen to why the problem's different. (I've done the same in my way!) And this is part of the explanation of why old scientists are unproductive. They really can't hear the problem, just as Bode couldn't see computing. The math department was changing to something different; he wanted to keep it in the image of the complex variable. Old scientists, having identified the problems, cannot see the problems have changed.

For example, to get back to Nyquist, Nyquist had proved that, for the same bandwidth, FM is no better than AM (frequency modulation, of course). Everybody at Bell Labs knew it, but they forgot to read the small print, <u>the same bandwidth</u>. There were plenty of people in Bell Labs, in my judgment, who could have developed FM, but they all knew it was no better than AM.

HOP: Bernie Holbrook.

RWH: <u>Holbrook</u> is a very, very able guy. He was also, in <u>old age</u> (long since he passed his peak), a great comfort to me, besides Walter Shewhart, who was my other source. I'd go over and weep on his shoulders, as it were, about my troubles and the difficulty of getting along with the goddamn math department and all their damn funny ways. And he would soothe me down nicely.

What happened to him was the following. When they wanted to build Electronic Central Offices they had, of course, to know what the relays ones were in great detail. The relay guys wouldn't tell them. Holbrook is assigned the job to find out. Literally, they would not tell him. He was reduced to asking questions of them, which they didn't understand, so that he'd find out, so he could build an electronic office to match. That job, spending a couple of years possibly, in a hostile environment, working like that, really destroyed his energy. Afterwards, he coasted through until he had a stroke. His coasting wasn't bad mind you, but he was coasting, against what he'd done before. I have no complaints. It was a very, very difficult task to have done. It was a very unpleasant one, and he apparently did it very well.

HOP: Computer number 5, was that Stibitz's?

RWH: Well, Stibitz was one, two, three. He'd already left BTL by then. Stibitz was gone before I arrived. I looked into Stibitz because, to some extent, I identified with Stibitz. I tried to find out, from Dietzold and others, why Stibitz left, what the trouble was. Dietzold sensed the real trouble. He showed me some memos written by Stibitz while he was at Bell Laboratories and some other ones that Stibitz had written now that he was a consultant. At Bell Labs, he was rather arrogant and didn't write any decent descriptions. Once he was working for himself he was more careful, so....

HOP: Why did he leave?

RWH: Fry, trouble, and because he wouldn't really compromise with the telephone system. He wouldn't write decent memos, and Fry and he weren't too happy together. He wanted to do computers, and Bell Labs didn't want computers.

HOP: Why?

RWH: Computing isn't something mathematicians do. We forget what the situation was then. He [<u>indicating GBP</u>] can tell you.

HOP: Now, but you said computers were something that Bell Labs didn't want. Bell Labs didn't consider it apparently proper...

RWH: Well, Stibitz was in the math department. His bosses didn't like digital computing machines.

HOP: And yet, Fry always had a tremendous interest in computers.

RWH: Well, but remember Fry, I think, had a hand in building an analog machine, which I have never seen, the <u>Isograph</u>. I have never seen it, in all my years. I'm <u>waiting</u> for somebody to pull it out of storage just to see it. It was a total disaster. MacColl told me what happened. I can see also from how Clara Froelich calculated zeros of polynomials digitally that it was an analog machine which should never have been built. They didn't know what they were doing. It worked, but they didn't understand that we didn't want to do that problem that way. The isograph would draw the function of a complex value - you ran the z = x+iy around a contour, and you got the w contour. And the number of times w(z) circled something is the number of zeros inside. So you change the radius until you tried to get a change in the count, and just where it came in showed you the complex values as well as the absolute value. And that's no way to find zeros of a polynomial. That's not a good thing to do.

They buried it quietly, and that was one of the difficulties I had, you see. That was a disaster they had built, publicized it, and it was a lemon, an embarrassment to the math department.

GBP: Not even under the pressure of the war did they build computers.

RWH: They built Model 5 for the military at NACA and Aberdeen. Oh yes. Relay number 1 was the complex computer that Stibitz had built in '39 or something like that and exhibited in '40.

GBP: I saw it June 1940 at 463 West Street, and I saw it, the terminal, at Dartmouth.

RWH: Yes, that was basically a multiplier of complex numbers. Then, number 2, I think, was a complex interpolator. Stibitz, being on the NDRC (National Defense Research Committee), saw that they had to prepare tapes to drive testing equipment, and this computer was simply a device for preparing tapes, an interpolator. The next one was a little more complicated. It did the same sort of thing with fixed point arithmetic. No. 4...

GBP: Who was it built for?

RWH: You'd have to look up the history....

GBP: I am wondering if Aberdeen had a hand in building some of these things?

RWH: Five, No. 5 was Aberdeen's. There were two copies of five, one went to NACA* at Columbus, I believe, and the other one to Aberdeen. Now the Aberdeen one actually

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* According to the second volume of A History of Engineering and Science in the Bell System (ed. M. D. Fagen. Bell Telephone Laboratories, Incorporated,

Wannier, but that is one thing he frustrated me on, thoroughly. Because I could never understand - nor would he adequately explain - how he did that mysterious thing.

But I learned a tremendous amount from Gregory, he was a big help. I worked a Monte Carlo problem with him, which was very nice. It was the only truly successful Monte Carlo problem I did - by John Tukey's definition. He came to me in the card program calculator days, with an integral equation in which each functional value was obtained by integrating over a plane, the value here depending on the normal of the plane. I could no more do that with a CPC than I could fly. But I asked the source. It was a charged particle that was accelerated by an electric field.

I wanted to do a Monte Carlo, but, unlike my friends, I waited until I had a problem which Bell Labs needed done. So I converted this to a Monte Carlo problem. We had 10,000 cards made up; we got the numbers from the proper distributions. I got IBM to bid on the job, and run it off. I let them throw away a few cards at random when they got card jams, because they had a great deal of trouble. Like idiots, they had bid on the thing, and they didn't know what they were bidding on. They were losing money, so I let them off the hook. Well, I plotted the curves and gave them to Wannier, and the first thing he does is complain about the accuracy. I tell him, "You agreed to this." Secondly, he says, "Ah ha, they're really Maxwellian distributions, off-center, like that." I said, "Yes, anybody can see that." And he grabs a piece of paper, furiously starts writing (ignoring me completely), and derives the whole thing analytically. By John Tukey's definition: the only good Monte Carlo is a dead Monte Carlo, that's the only one that worked out for me. Now he went on. When he had the actual answer and compared it with mine, mine were systematically off a little bit. Well, I didn't like that, so I looked into the matter. I found out that the very unlikely events had been preferentially removed by the cards they had removed from the deck. Never run a Monte Carlo and pick out a removable random card, never mind how random. But that was a very good model. It was my first really big Monte Carlo. It was my first really successful Monte Carlo.

I went to Von Neumann over that. Tried to pull a "swindle". I went down to the Institute, tried to year. And you don't do that by going to seminars. If you set out to so something .....

Incidentally, on the whole subject of people leaving the Labs for a year, I think it is highly desirable, but not always. I think in general, one ought to start out with a clear vision of what you want. Both times I set out to write a definite book, to get something accomplished. I've watched some other people take a year and have nothing to show for it. When they come back, I can see no effect of their having been gone. They might just as well never have left. Nobody gained anything from it, as far as I can see. For example, whether Slepian had gone to Hawaii or not I can see no particular gain one way or the other. He'd have done the same damn things no matter where it had been. On the other hand, I think our Nobel Prize guy, [P.W.] Anderson, profited greatly.

There were a couple you haven't named. There was a guy named Gregory Wannier from Bell Labs.

HOP: Yes, but I didn't talk much about people outside mathematics.

RWH: Well, he had a great effect on me. I found him early. Whenever I did a problem, I wanted to understand what I was computing before I computed. I don't believe in computing blindly, just turning out numbers. I believe in understanding first. I would go down to him, and he would say, "Well, you start with Newton's laws" and he derived what I needed. Everything but one, I learned. One thing I could never learn from him, is illustrated in the following simple story. I solved a second order differential equation for somebody, and he was gloriously happy. As I handed him the answer I said, "Of course, I can solve it with diffusion." He said, "You could?" And I say, "Yes, you write the equation." So a month will pass or two weeks will pass and he comes around with a diffusion equation, which raises the degree of equation by two. And I would say to him, "You've raised the degree by two, I need two more boundary conditions." And he would say, "You're crazy! We haven't got any more boundary conditions." And I would say, "You know very well I have to have them." So after a little bit of argument, I saw I was getting nowhere. I would go down to Wannier with this guy in tow. We'd sit down. Wannier would find them, but I could never find out how, because each time he seemed to do it differently. Outside of that, I learned a tremendous amount from

had two computers, two mainframes, in the same computer, so it was really two machines cooperating on one problem, if you want. That was one of the problems I worked on with Weiss. We worked, had to go to West Street regularly, and we did problems on that in `46, early `47.

HOP: At any rate you got your CPC.

RWH: Yes, and it worked very well - all things considered! We soon had it completely loaded up, and I got a guy from Drew University, a theological student, to run it nights - thus almost doubling its productivity. All he had to do was put the cards in it. I presume he prayed or something, because we never had troubles at night. The thing didn't break down while he was running it. But in a sense it was a real lemon. It was built from parts of four different machines, and it was very hard to maintain. And when I finally overloaded it sufficiently, I got one 650 [in 1955].

Now, there was to be an overlap of the two machines for a month or more. Well, within two weeks - I remember this one guy - I took Bode down to this guy and said, "Listen to what he says." This guy says, "I'm going to run my program on the CPC until I have to convert." Bode says, "You'll have to convert sometime?" He answers, "Yes, but I'm going to keep on running as long as I can on the CPC." Bode says, "Take it out." So the overlap lasted two weeks instead of a month.

Now, immediately the workload on the IBM 650 built up because, looking at a distribution of problem lengths, I found the distribution of problem lengths didn't change, and yet I had just gotten a machine ten times faster! The problems we then did were simply ten times bigger! I could see disaster coming, and I saw I had to have a second 650.

1978), the first unit of Model V went to the National Advisory Committee on Aeronautics (NACA) Laboratory at Langley Field, Virginia, in 1946. The following year, the second unit went to the Army Ordnance Ballistic Research Laboratory at Aberdeen, Maryland (p. 170).

HOP: How soon was that?

RWH: I can't tell you the number of months, but it was very soon. But Bode obviously wouldn't move. I went to Tukey and Tukey says, "Order it." I said "Yeah, but how are you going to get authority?" He said, "Just order it, and I will take care of it." And sure enough, just as the second machine is coming in the door, Tukey has taken care, somehow, of the paperwork at the top. So I got a second 650. I never knew how he did it, but I was rescued.

Bode? No, one machine was enough. Although, give him credit. Very early he said to me, "These machines haven't helped my girls, my hand calculator girls at all. You only give them, the machines, big problems." Thus he put my attention on the question, How do I do small problems on these machines? Second, and much more important, one time he said at a department meeting, "Hamming and his girls shouldn't worry about the fact that they can't do all the problems." I went home and said, "Bode's crazy. Look at all these important problems not being done." But then, I said, "Well, Bode isn't dumb. He must have meant something." It finally dawned on me that really what he wanted was for me to find out what machines could do rather than to do it. My real problem in the research department was to find out what computers could do. And so, I shifted to some extent, to try and find out what range of science the machines could do was rather than merely getting work done.

HOP: How was work on the 650 paid for?

RWH: Originally it was loaded on all the vice-presidents' areas equally. And I used that fact by talking to various vice-presidents, saying "Of course, you realize you're paying for part of my 650, and you aren't getting any machine time in your whole vice-presidential area."

"What?"

"Oh yes! It's loaded on the whole company; you're paying for it."

In about two or three months or less, then somebody from his area comes wandering around saying, "Say, could I use your machine?" Then we moved the charging down to departments; then we moved it down to cases.

HOP: Abe Taub.

RWH: Didn't like him at all. He was at Illinois, where they once tried to get me to come back. I would never have gone back, because Wanda wouldn't let me. But Taub, he tried to live on having worked with Von Neumann too long, like Goldstine.

HOP: Forsythe.

RWH: You mean George Forsythe?

HOP: George Forsythe.

RWH: He was the reason I went to Stanford for the year in 1960-61. Very nice guy. Let me back up and speak of the subject. At Los Alamos, I became envious of these other guys like Feynman, and so on, and I wanted to know the difference between them and me. I told you I envied Gilbert some talents. I envied Forsythe's being a gentleman. I just never could be the gentleman he is, although I tried to learn a little bit about being polite. He was a very nice guy. He didn't do great mathematics, but he caused other people to, which is almost as good, if not as good. Like Oppenheimer. Oppenheimer could inspire people to do good work. He was a great leader. He was a great inspirer, a great teacher. Forsythe likewise. He didn't do great mathematics himself, but he was a great man anyhow. "There are many ways to heaven" and Forsythe, I thought, was very good. As I say, he was the reason I went there as against other places.

I could have gone almost any place, because after all, Bell Labs was paying the bill then. But I wanted to write that book.* I didn't dare to go to MIT, and I didn't dare to go to some other places. I didn't dare to go to Berkeley. And the second time, in 1970-71, I went to Irvine; I didn't dare to go to Santa Cruz because I thought the redwood trees would make me too idle, and MIT would get me to go to too many seminars. I wouldn't get the book written. With the second book, I ginned out two pages a day, seven days a week for a year, because I turned out 732 pages in the one

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* Numerical Methods for Scientists and Engineers (New York: McGraw-Hill, 1962; 2nd ed. 1973).

footnote for a few years, and disappear. He's one of the greatest talents. Man, am I envious! I'm annoyed by it, because I love him as a person. I'm just envious as hell. I wish I had his talent. But there you have it, another one of those small character defects that does a man in, keeps him from real greatness.

HOP: We're finished with the list of people at the Labs.

RWH: Let's see, you must have missed a lot of guys.

HOP: I probably did, but....

RWH: I can think of one, Henry Pollak.

HOP: Go ahead.

RWH: Well, Pollak was like McDonald. Hank McDonald and I could hardly go to lunch together before we'd be screaming at each other, arguing violently. Nevertheless, when we came to merit raises, there was very little disagreement between us about who was good and bad. Same way with Pollak. We didn't agree at all on what mathematics is, but we agree on what is good mathematics. My objection to the son of a bitch is that he thought the mathematics department is best managed by no management, and I think he should have managed the department. And that's a big difference of opinion.

HOP: Alan Perlis.

RWH: He's one of the best people in computing to this day, despite all his physical handicaps. Probably one of the most influential guys for ideas. He didn't write. He has features that are a nuisance. He didn't answer letters. He didn't do this or that. But man, he was a fount of ideas, very, very influential. I learned a lot from him.

I learned a lot from this guy Pollak too, by the way, learned a lot of mathematics.

[Anecdote lost at end of tape.]

RWH: ...Wallace Givens did make me vice-president of the AAAS mathematics section once, but I never thought a great deal of him.

But along the way, I protested very strongly about the following point. Somewhere, I think, in the 701 days, a T.A. [technical assistant] girl could run up a bill of $5,000 whereas even department heads couldn't sign for that amount. And we couldn't do anything about it. There was a very basic imbalance there. (Still is!)

HOP: When and why was the switch coming from the two 650s to the 701s?

RWH: I don't know the date, but I used the 701 in N.Y.C. on a big military job. And I had demonstrated an important point, that analog computers couldn't do what digital computers could do. Weiss and I spent a year on that problem. That was where I burned my fingers and found in the middle of an absolute binary coded program on a 701 (which has a mean free time of fifteen minutes between failures), that Milne's method is unstable, and I had to create another method. Well, I got it going, and we were getting trajectories, and I realized that I was showing just what I told you, that digital machines could do what analog couldn't. Therefore, I knew the report would have to go to every big analog computer location. I was using a crummy method of integrating. It was effective, but not elegant. I sat down (going back to and fro on the Lackawanna train) and developed a very elegant method, which is known as Hamming's method of integration. It was defensible logically, and that worked out fine. So I had Miss Weiss change a few instructions along the way, and we were in.

But after watching all that she had to do in the whole project, I said, "I will never ask another human being to do what she did for a year's work, coding absolute binary on a machine." Solutions needed a half hour, but mean mean-free time between computer failures, fifteen minutes. An absolute binary program means you put the bits in and look to the cathode ray tube. It was murder, and she worked very hard.

HOP: **That brings us to question 8, does it?**

RWH: No, 7. Let's go to Gypsy first.

Question No. 7

Would you discuss the development and use of Gypsy, the general-purpose analog computer designed by Emory Lakatos?

RWH: It was known I was a digital character, and I would say the whole Gypsy was designed without telling me anything. McMillan had a hand in it partly. All they did was take gun director parts, make them of a matched impedance of one megohm, bring them up to a double patchboard of the telephone switchboard type, and remove some of the windage, drag, and other things, and replace them with more sines, cosines, and idiot hyperbolic functions on potentiometers.

I stayed away from it as much as possible, but I had several problems I wanted to do and as that was a suitable machine, I used it a couple of times. Bode asked me one time - I think he'd rigged it. He said, "I've asked Lakatos to work full time for a couple months and finish the military report. Will you run the thing?" I says to him, "I see the problem. I get it running well enough to get credit, but not so well that I'll get stuck with it." And he laughed in my face. So, what happened is, within a couple of weeks, I cleaned up the style of running it, and there was no more backlog at all, and everyone was happy. And the machine was idle! So I never got rid of it.

When I found I was stuck with it, I started redesigning the thing. In the first place, to take one example, they had "overload lights" on a panel.

HOP: Why did people insist upon keeping it?

RWH: Oh, it was very valuable. It did all kinds of problems.

But let me tell you. It had overload lights over here on the mainframe, but over there, is where you watch for the trajectory coming out,.... Well, you weren't looking when it overloaded, naturally. So I said to the maintenance man, "I want sound." They fussed, and fussed, and fussed, and they finally connected a little thing up, a charged-condenser to run a little one-inch speaker that went "Naah!" Sufficient! If you were looking here, and it went "Naah," you knew to step over there, stop the thing, look at the overload lights, and not to go ahead. Simple device. Greatly increased productivity and reliability of the answers.

Next, all the "time constants" of the integrators were changed by girls reaching in and changing the corresponding wires by hand among the high voltages, which I didn't like. Furthermore, all the feedback

RWH: Very, very ingenious guy. If somebody could've disciplined him from the superficial into profundity, I think you'd have had more, but perhaps somewhat worse. What could've been done? He certainly is a talented guy in a lot of ways, but what has it been? A lot of slick stuff. Clever, ingenious, but Bode once said, "Show me the guy who writes the definitive book," and after getting mad at that and saying, "Just because you wrote a book like that,..." I came around to believe that he was right.

HOP: Ruth Weiss?

RWH: Very fond of her, and I said several times I was more dependent upon her than I was on my wife many times. Very, very limited. We finally separated when Schelkunoff pointed out to me that we had worked together so long that she took me as being too infallible, and that was a mistake. And after thinking it over a little while, I said, "Yes." In the early 650 business I did something to her differently. I told her, "You're going to do the programming. I'm not going to learn how to program the machine. I'll tell you what I want programmed, but I won't do the programming." She didn't believe me. I'd say I want a program to do this. She would show me, I'd look at it. I would see that there was an error there. I wouldn't tell her. She found out then, that she had to be dependent upon herself and that made her grow a great deal. But it took Schelkunoff to point it out to me, and then I had to set about the business of getting her independent of me. And she had limited talent. She never could see the big picture really well, like Gwen Hansen, although she was somewhat better than Gwen Hansen. I don't think that, in some respects, she was as good as Leagus.

HOP: Ed Gilbert?

RWH: There's a guy who hurts. Next to Tukey, he's the most talented mathematician I've seen up close. You see, I haven't seen Von Neumann up close, but Gilbert I have. If you mean by mathematician a man who can take an ill-formed problem, formulate it, and lay down an attack, Ed Gilbert's got it. On the other hand, the son of a bitch would never get emotionally involved with anything. I argued with him many, many years ago, "Ed, for heaven's sake, take these pearls of wisdom you have published, here, there, and yon, and string them together in a book showing the underlying method." He doesn't care. He's going to die, be a worked on the wrong problems. He didn't really understand what to do. He hasn't done a thing since he went to Wisconsin. He could only do what amuses him at the moment. He has no sense of self-discipline.

HOP: Roger Pinkham.

RWH: Personal friend of mine again, as you know.

HOP: Yes.

RWH: Two failures. He's a Harvard graduate, very talented, very able, talented in many directions and unable to select among them. So that he's done too many different things, rather than a few things well. Plus, he's let his private life interrupt his professional life. Again, we're talking from the scientist's point of view. Perhaps the individual has a right to put his personal life ahead of his professional life. Speaking professionally, his private life has occasionally gotten in the way of his public life. For example, when I came out here, they were looking for a department head. They wanted me to be department head. It was the last thing I wanted to do. Pinkham would have been a good one, but he affected a long beard, hippie, such other things. I knew he was unhappy at Stevens to a great extent. He had already told me before then. He had antagonized so many people, so that he only had to say one thing, then all the rest of the department heads would vote the other way. Again, he was dressing in hippie form and such things. The NPS (Naval Postgraduate School) could not use him because of that trivial feature!

I learned my lesson many, many years before at IBM. I was using the 701 then, and I noticed that I wasn't getting good service. I said, to myself, "What vice-president said, 'Give Hamming a bad time'? No vice-president. Why are you getting a bad time? It's the clerks at the bottom who are assigning the time. Now, why would they do that to you? It's because, Hamming, you're going there with riding breeches and dressing kooky, that's why." I thought for a long while, I had a choice: either assert my ego and "be me" or smother my ego a bit and get my work done. I decided I'd better appear to conform, so I could get the work done. John Tukey hasn't learned that to this day.

HOP: Dave Hagelbarger?

amplifiers were opened or closed by throwing a switch on the amplifier inside the cabinets, so you never knew what was what. I wanted it all out in the front control. They said you can't do it, blah, blah, blah. And I said we could do it. And they told me why I couldn't, because the long lead wires would produce parasitic errors. I said, "Well, you can put a reed relay down there, and a switch up there, and the hell you can't do what I asked for. Let's do it." And, I finally got it done, but I had resistance all the way. So you finally had the state of the machine at that moment right in front of your eyes, not hidden in fifty different places.

So I really had to human engineer the machine. I did no real design engineering in the sense of the electrical voltages. I designed human engineering. They had not the faintest conception. Lakatos did not know how to run it "humanwise." The design was poor human engineering. I had the hyperbolic functions all taken off, because they are no things to have on a machine, I had more sines and cosines and linear potentiometers which were general purpose. They really didn't understand that it was a general-purpose computer. And they really didn't understand anything about human engineering.

So that's what happened to that damn thing, and why I got stuck with it.

HOP: Where had Lakatos come from, by the way?

RWH: He was there when I arrived. He was another guy,... Which brings up this point. He was quite able, in a certain sense, but he had no vision. He could only do that which was in front of his face. If I may quote the Bible or something, "people perish with no vision," or something like that.* I think that what makes a good scientist is a vision of more than the obvious. And so many people have very little vision. And Lakatos was one of the many guys who had comparatively little vision. He finally left when he got a better job offer in the West, California. He's still out there in Los Angeles, or somewhere.

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* "Where there is no vision, the people perish" (Prov. 29:18). - Ed.

HOP: Is he?

RWH: Yes. Well, some more trouble with that. By that time I was running so successfully that I soon had both bigger problems than I could do as well as more problems. So the problem came up of building a second Gypsy. We looked out first: Can we buy commercially? Well, we were at least one bit better in accuracy and for one bit, we built a second copy! But I went to Whippany, where they were building it, and I said, "I don't care how you build it" (because they wanted to build it fancy), "but it must be exactly like the first Gypsy. If you're going to change the second, you've got to change the first." And we had a little trouble over that, but I insisted that the two had to be exactly compatible. If they wound up creating a new one, they had to reach in and upgrade the old one at their expense.

HOP: Who built it?

RWH: I don't know. I can't say. I went out there with Bomberger. Bomberger and I did a lot of work together in those times, and I wanted him to help test the new machine before they brought it over, since they had assembled it at Whippany. He said, "Oh, it's perfectly safe." So I patched up y" + y = 0, which is supposed to draw circles if you plot y against y'. It didn't draw circles. We patched a couple of different integrators. We tried various things. We finally called the guys that built it in and showed it to them. When we went to lunch, they were busily looking at what was wrong. It's a very simple thing to upgrade the integrators, amplifiers, and so on, and they had not put in a heavy enough bus, a copper bus, so that the leakage current was running around through back circuits. All they had to do was to put in a great big copper bus. But that was one of the things I insisted upon, compatibility. When we put in heavy copper it worked fine. We had the connections from one to the other; we connected two as one, or separate.

HOP: You've mentioned Dave Bomberger several times.

RWH: Yes.

HOP: My recollection of him is that of a power engineer. What else had he done?

"Voyeurism is no substitute for experience," not only in sex, but in research.* If you have not done it, you really can't know to some extent what it is. Pierce had done it, not once but a lot of times.

He would come in your office when he was boss and say, "What's new?" And if you didn't start telling something interesting, if it was dull, you looked up, and he was already out of the office and gone. After a little while, you realized that you had, at any moment, to be prepared to say what you were doing that was interesting, in a form that was interesting to him. He was a real good boss that way. But he didn't fill out forms, he didn't do all kinds of things, and in that way he was a bad boss. I think he's great.

HOP: McIlroy.

RWH: McIlroy is a curious thing. He worked for me when he came in first. I sized him up. I said, "Hamming, don't tell him anything. Leave him alone." I helped him get a blackboard. I did everything else. I helped him with all the mechanics, but I wouldn't tell him what to do at all. He did some very good work in the beginning. Then he petered out. He hasn't been managed properly. There's a talent there. (I saw him this time, when I was back.) There's a talent there, but somehow or another it got lost. He could've continued to be very productive - not that he's unproductive - but I think he's got much more, particularly in view of what he already did in the early days. It's not been gotten out of him somehow. It's a shame, 'cause I don't know how to do it.

HOP: Ed Moore?

RWH: Ed Moore. I used to enjoy him until somebody said, "We ought to have a case to charge time that Ed Moore wastes." And I stopped and thought awhile and decided that after that I was no longer going to listen to Ed Moore. He'd come by and tell you the most fascinating thing about type fonts, about this or that, but he

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* R. W. Hamming, "We Would Know What They Thought When They Did It," in A History of Computing in the Twentieth Century, ed. N. Metropolis, J. Howlett, and Gian-Carlo Rota (New York: Academic Press, 1980), p. 8.

Kaiser and said, "Kaiser, you´ve done your thesis in the matter, and you´re the greatest living expert in the matter. You ought to write a book. This is the time in your life to do it, blah, blah, blah, blah, blah!" He agreed, "Yes." Nothing happened, so I said to Kaiser, "Well look," I said, "why don´t you and I write it and we´ll call it `Kaiser and Hamming´?" "Fine!" So I started going to lunch with him to find out what it is all about and started writing my part and doing what I could. Nothing happens on his part. I finally say to him, "Look, Kaiser, if you don´t do something pretty soon, we´ll have to call it `Hamming and Kaiser´." He says, "Fine." It finally gets done. I say to Kaiser, "Look, you haven´t done a darn thing. I can thank you in the preface, but...." He says, "Fine." So that´s how the book came out! I´m now going to work on the revision. The publishers want a revision of the darn book.

At the same time he could have been writing the book, Kaiser was spending enormous time compiling a bibliography. He knows what he should do. He gets sidetracked with the trivia. It´s a common story. People know what they should do, and they will do, as I told you, like Miss Gray. She would go for the thing with the more immediate gratification rather than the loneliness of long-haul research. It´s a minor character defect, but a real one.

HOP: John <u>Pierce</u>?

RWH: Son of a bitch and a very great scientist! I´m glad he was my boss. When I was calculating travelling wave tubes for him, he was always complaining that I didn´t get the answer fast enough. I said, "Look, you know I can only compute when the Accounting Department is not using the machines for our paychecks. What do you want, solution of travelling wave tubes or paychecks?" He still gave me a hard time over it, and I don´t think he ever thanked either Miss Weiss or me reasonably for all the effort we put into travelling wave tubes for him on the damn machine. Still, with all the difficulty and the fact he didn´t do all kinds of things he should, one knew that the son of a gun knew research; and it´s desirable, I think, to have a boss like Bode or Pierce where, even if you get a bum decision, you at least feel the man knew what he was deciding. Whereas, if you come back to Morgan and some other bosses, I don´t think they know what first class research is, never having done it. There is an expression which you´ll find in that paper I gave you

RWH: Well, he was. He´s a good example, he had done very good work in the military business. Though I had known him earlier (the RAND trip for example), I really got involved with him first on this 701 job I told you about, the navy intercept. He conned me into that one beautifully. He was good, but he was one of the many guys who did not grow. And so they moved him to power engineering when he would not move forward. He didn´t like digital machines at all. And, furthermore, he just didn´t develop. Now he´s a good friend of mine. (Incidentally, you understand these people are all so many friends of mine.) He quit, retired about the same time I did. It´s marvelous the difference! He is disgruntled as hell, while I think Bell Labs is a great place. We both had a very similar experience, except that I moved forward. I left at the time of my choosing; he was sort of pushed out. We got a Christmas card from him. They´re still living where they were. He was left behind and this is a very, very important problem for BTL to worry about.

On the last [day] of my seminar course, I give a lecture on what knowledge is worth knowing. I gave a talk at Princeton years ago, "What Knowledge is Worth Knowing." And I told the class, of all that I learned in school, almost everything was wiped out, vacuum tubes, all kinds of things, except mathematics. The calculus had not changed. If there was one thing which would have a long-term value, judged from past experience, it was mathematics. And it´s the only inference you can make for the future! Technologically, we´re due for great change. But in mathematics, it is not likely.

HOP: Dick, you always maintained that one of the best things that had happened in history was the burning of the Library at Alexandria.

RWH: Yes.

HOP: Do you still believe that?

RWH: Sure. The dead hand of the past! In fact, I´ve got a book over there on it - a couple. Let´s go forward.

<u>Question</u> <u>No</u>. <u>8</u>

Why were L1 (Wolontis and Leagus) and L2 (Hamming and Weiss) developed? How successful were these languages both inside and outside Bell Labs?

RWH: Number 8 is a delicate question. Let me give you my version, which is not the official version, but I think it is the correct one.

Wolontis and I went to the one-week 650 school IBM ran to learn about it. I saw immediately that I did not want to run in that language. Bode had got to me. (Down a way [on the list of questions] is "What is the most important thing that happened to computing?" I think it was Bode's prodding me, "It isn't helping the girls," He made me think about what computing was, and my favorite saying, "The purpose of computing is insight not numbers," is possibly the most important single thing I did. The purpose was to advance Bell Laboratories, not to get numbers.) Well, I saw that I could not use that crazy 650 language. Furthermore, it was a fixed-point machine.

Within the week, I saw that I could make a three-address system like the CPC, like Stibitz's machine, A times B equals C, three digits for the first address, three for the second, and three for the third. But that meant that I could not refer to the upper 1000 registers. Okay, so I could put the software system there. I saw that much. At first, I only saw that we could have ten instructions, for one decimal position, but then I saw that most instructions didn't require the second address, A times B equals C, but sine A equals C. There's no second argument. So I could use the zero operation to say, "Look at the second argument for more details of the instruction you are to do next." Well, I got this thing quite a way along. Now I did this using top-down philosophy.

Wolontis was working for me. He was panicked over the logic at some stage along the way. But we had gotten the whole thing laid out: where it was going to be, this, that, and the other thing. He came in one Monday with all the logic built, running. Oh, I couldn't stop him, so he published the L1. I was trying to build L2 at the time. In order to be reasonable, we simply stopped doing the symbolic system, which I was trying to do. This was an absolute address, and absolute instructions. I wanted symbolic instructions, so debugging would be much easier. I had to wait for about a year or so, and then produce the L2, because to produce the L2 directly, when Wolontis really had stepped up and done it suddenly, was troublesome. So we simply let go that way, but really the evidence is nifty. One day, a while back, Alan Perlis asked me about that, and I

HOP: What's your guess?

RWH: If you get left behind - you do good work but get left behind - you feel unappreciated. I think that's it for the most part. About Rice, I'm not sure. He didn't speak too well of Bell Labs. I don't know. He's such a nice guy, he probably wouldn't say evil of anybody. But, a lot has passed him by.

HOP: George Baldwin?

RWH: I tried with Baldwin repeatedly to get him not to believe what he was told when he was managing computers, but to look. George didn't want to look. Furthermore, George would promise A to the first guy and non-A to the second guy. George wanted to be liked. I think he was able, but his desire to be liked vitiated almost anything he could do.

HOP: Jim Kaiser?

RWH: Kaiser obviously is a good friend of mine. He was out here recently. You know that book Digital Filters by me?* Let me tell you a story about that. Being interested in what people know in this business of being obsolete and the analog computer, I stopped W. O. Baker in the hall one day and said that I had watched the analog guys not convert to digital. I had watched various other nonconversions. I had watched the earliest ones. The old relay guys would not learn electronics. They were pushed aside. I said to Baker that they were an economic loss, but to my mind, worse, they were a social loss. They were disgruntled. "The telephone company is rapidly going digital," I said to Baker, "and if we don't get these analog guys and other people over to digital computing and a digital way of thinking, with digital filters, we're gonna have the same thing." He looked interested. I said, "I think what we need is a good elementary book which will help convert them." He said, "Yes, I think you should."

I knew I'd been had. I walked off. I got repeated feedback from Baker through Tukey that Baker was glad I was looking into matters and so on. So I went to

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* Published by Prentice-Hall in 1977. - Ed.

going to argue with Tukey when Tukey said he was, at that time, the greatest living combinatorialist. It´s very likely he was.

HOP: You mentioned Nyquist.

RWH: Nyquist was interesting. I met him very early, like so many people. There´s no question he´s good. He was worried one time and had his whole department, including Riordan, screwed up on the following question: "If the power series is convergent or divergent on the boundary of stability, does the convergence prove it´s stable and the divergence prove it´s unstable?" Good example of a question that hasn´t any meaning. In the analog business, you don´t have stability that much. All the department was fiddling around with the question when it was the wrong question. How he slipped up there, I don´t know. But in general, he is a very shrewd guy, who could stick his finger on the right thing very well. I met him on numerous occasions both through Riordan and through E. G. Edwards.

HOP: Did you know Hartley?

RWH: A pain in the ass. Yes. He came by my office periodically. Remember, he had some new quantum mechanics?

HOP: Yes.

RWH: He´s another lesson. I hadn´t the energy to find out whether he had a good idea or not. I do not know whether he was right to have stayed around the place. I just didn´t have the time. He´s one of the reasons why I resolved not to go back and hang around at the Labs when I retired. Oh, by the way, you know Blackman. When he retired, although he lives right across the grounds, right on the edge, Mary said to me one day, "He has never been back in the Labs." Now that takes some effort to stay away. It shows how much he was disgruntled before he quit.

HOP: Not only that, but we invited him to Christmas parties, and he never came.

RWH: Well, it´s a very interesting question. Why some people look back on Bell Labs and on pleasant careers and some don´t.

told him the story. That´s why he´d never heard of Wolontis again in computing, because he really hadn´t had the idea at all. L1, L2 were very successful.

Let´s take up questions 9, 10, and 11.

Question No. 9

Would you list the highlights of the development of computer science at Bell Labs that led to the establishment of a separate Computing Science Research Center?

RWH: I told you more or less how we crept up on the thing. I would not order a 701 until I saw that a modern software system could be built so that you could sequence problems; make the machine do it. The way we were using it in the IBM New York City location was that each person got the machine, then he went downstairs, he set all the console switches, mounted all his tapes, and put in all the plug boards himself. When he finished, he took it all back off again. That´s no way to run the place. In fact, I made the 650 run smoothly because, among other things, there was only one console setting and one set of plug boards.

HOP: Was this the difference between trying to do a problem and trying to do everybody´s work?

RWH: Yes, I was interested in all problems. All of next year´s problems was my goal. "How do I do all of next year´s problems?" is the basic question. The purpose of computing is insight, not numbers. It is not a matter of getting the numbers out: it is getting the right problems done. That means you must make the machine available to the man who´s got the problem, so he can see what can be done. Well, I didn´t actually build it, George Mealy built the software system, but I wouldn´t order a 701 until I saw that it was possible. And I got out, ´cause, as I told you earlier, I saw that that was no way to get rewarded at Bell Labs. Service was no good, the math department wanted research. It paid off on research. Properly so.

HOP: Wasn´t Gwen Rowe involved in this?

RWH: Yes. She retired the other day with no party, no nothing. So she must have been disgruntled too.

HOP: Well, I never knew it, that she was retiring.

RWH: I knew. I've still get the gossip.

HOP: She was working in BIS (Business Information Systems) at the end.

RWH: I have more gossip than you have about Bell Labs. Well, you asked, "How did the Computing Science Research Center develop?" Let me say this. For more than ten years, I bitched to my management regularly, "Get the damn machines out of the research department. We can do no research." For example, I complained and complained and complained every time a message came back, "We must have it to do research on it." Finally, it seems to me they got the message. Just as no Library Science Department should run the library, no Computing Science Department should run the computer. They finally got the message. I think that, plus - possibly much more influential, I can't tell - the budget was now so large. It was obvious that the Math Department, or even the Research Department, shouldn't be controlling that much budget. It was inappropriate. I wouldn't be surprised, if it were sheer dollars, if the vice-presidents looked at the budget and said, "Hey, now wait a minute, we need someone to manage that thing directly. We can't let that be hidden down there." Well, I certainly complained for more than ten years, regularly, that the machine was stopping all research. We finally got rid of the computer. The moment we did and we got the guys loose, UNIX™ came out of it and such other things.

HOP: There was some time gap between there.

RWH: Not much. My version there is the following. I went to Ed David and said "Look." (They all knew I tried to get rid of the computers. I was a persona non grata, 'cause the toy was going away.) I said to Ed David - with the door shut - "Give them the smallest machine you can, that you can get away with. If it's too small they'll sulk, but those guys are very good. They'll be very ambitious, they'll get great work out of a small machine. Bigness is not what's required." He gave them that, and UNIX™ came out. By setting the stage, Ed David caused UNIX™ to appear. Now nobody knew what they were going to do. But we knew that these guys with ambition would have to make the small machine do big things.... Everybody really knows a small machine can do practically as much as a big

HOP: Steve Rice.

RWH: Rice obviously did some very important work. But at the back end, he seemed to me long since out of date. He tried messing with machines. I've got to give him credit as one of the few old scientists who ever took up computers. Nevertheless, while he could use them, I don't know what he produced of any great importance late in life. Although he worked hard. Again, see, the essence of doing important work is doing the right problem, and that's the danger. He did the same thing as Shannon did, and others. If you have to get a job done next week, you close your door or you work odd hours or something. You can get more work done in the short haul while your door is shut and you're on your own. But if you do that too long, you no longer know what to work on. Rice worked by himself. He came in early you know, in the morning. After years, he no longer knew what to work on.

Riordan had a tough life. Of his most important paper, probably the most widely cited, Nyquist said, "Well, if you want to publish it all right, but I don't see why you waste your time on it." I learned a great deal from him. During much of his life, he was stuck at Bell Labs. He had no degree, and he couldn't go to a university. So I resolved that I would watch myself, and anytime Bell Labs couldn't get along with me I could go elsewhere. So I never had to.

John Tukey said to me one time, "Why don't you ask the greatest living combinatorialist instead of me." I said, "Who?" He said, "Riordan." But Riordan is curious. I used to have lunch with him once a week for years. He was writing the combinatorial book. I asked him, "Will you please really give the rules for the symbolic stuff?" He says, "They're all in E.T. Bell's book." And I said, "It isn't exactly intelligible, and besides that, sometime or another, you admitted it wasn't there." So he hemmed and hawed. He would never put down - nor would Gilbert - just what the rules are. Riordan was like Euler. He worked out many particular cases. Beyond when I was bored he'd work on another case before he came to a general one. One time he made a derivation, got down to the end symbolically, and said "That's the wrong answer. Let's start again." He derived it a different way, got a different answer. He said, "Okay, that's the right answer." Why one and not the other? He would not do the mathematical business of trying to find out why one instead of the other. But I'm not and Terry both subconsciously left them dependent. So as a result, years later, Terry is always on the phone Saturdays and Sundays - a crisis he has to handle - whereas I am left alone. Isn't it curious that even teachers, although they're supposed to tell the student everything, will often suppress things, because subconsciously they don't want the student to be an equal? It's a very, very common trait of suppressing, subconsciously or consciously, some of the things. I think Tukey did it a lot. Milt certainly did it.

On the other hand, I think Milton Terry had a tremendous effect where you [Pollak] would not have seen it so much, in the whole system. For instance, one of the stories I love. There's a production line trying to make some pressed ferrite ceramics, and Kansas City just can't make it work. So he's going out there constantly. One Monday morning he's standing there with the supervisor and he shoves his elbow in this guy's ribs and says, "Look right over there." He says, "There's the barrel of powder we've just mixed carefully, and the machines are vibrating and separating the powder. That's why you're having trouble." He had that gift. He could see through it. Remember, somebody* had this stereovision. Terry observed promptly that he could use that to find out whether the inspectors from Western Electric were, with binoculars, actually seeing binocular vision. And he showed a lot of the inspectors were not! He also could say immediately, we could use this as a test on children, very young, for eye tests, to find out if they are using both their eyes. He had this gift. I don't know what else to call it. The gift of doing the right thing. He didn't use fancy statistics, although he could do it. He depended upon the simplest stuff. But man, he got to the heart of things many a time!

I think he was very influential at spreading the use of statistics around in the field, even above Tukey in some respects. Although Tukey certainly was very effective with military business.

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* B. Julesz. - Ed.

machine. You don't have to have big ones; and it worked out very well, once we were freed from routine maintenance.

Once Bob Morris spent a year speeding up the FORTRAN compiler. To what end? Each thing with a trickier gimmick on it ran 30 percent faster. So IBM issues a new machine or a new assembler, and we've got to start all over again. No real profit. It's a kind of development work, but it's not research work at all. We were constantly being sidetracked into keeping the machines going.

However, I'll tell you a story on the side. One time, I think in the 650 days, some military guys wanted a job done by Friday. I didn't want to do it. They went to Schelkunoff, who was the boss then, and Schelkunoff told me I had to. I thought for a while and said, "Okay, but Friday afternoon you're sitting in your office past your usual evening time, and I'm sitting with you, and you're watching." So we did. I took everything else off the machine and ran that problem only and I said, "There's the guy walking out the door, and you see he's got nothing under his arm." (I'd delivered the answers to him.) Monday morning, Schelkunoff called the guy up and said, "Did you come to work this weekend?" The guy said, "No." Schelkunoff said, "The stuff was delivered to you on Friday." The guy said, "Yes." I said to Schelkunoff, "You know, I could've done it easily over the weekend." After that Schelkunoff rightly said to everybody, "You set the deadlines, you can change them." He never again asked me to displace research for military work. He let me do research sensibly.

Incidently, another story about Gypsy, the same way. One summer, we had demands on Gypsy that far exceeded what could be done. So I invited all the guys to a conference in the Math Department. I stepped up and said, "Now, you want so much." I wrote down the amount, so and so, so and so. I drew a line and added them all up. On the other side, I calculated the number of hours available in the summer, and I sat down. I didn't say a word! They started compromising, they began to see they had to compromise. I let them settle it among themselves, work out a whole schedule. They each had a week or so. We did the first week. Somebody came to the place where he could get off and make some room for somebody else. I went right back and settled the schedule myself and cut them down to size by making them all see the impossibility of that many hours.

But Bode never understood this. Bode kept asking me if he could put another problem on the machine. "Could you look at this problem too without dropping something else?" Would I? I've got to drop something else. He never could understand that a machine has a finite capacity. He always wanted me to do another problem squeezed in somewheres. I had trouble with him.

HOP: When did Gypsy finally disappear?

RWH: I don't know the date. But we got rid of it, along with the maintenance man. We gave it to Stevens, isn't it?

HOP: Brooklyn Poly.

RWH: Right, Brooklyn Poly.

It was an interesting problem. My first experience with management in a certain sense. I got the damn Gypsy with this maintenance man. Now it ran fine when other analog computers didn't. Question: Is it the maintenance man or is it that these parts were debugged by the war and the arctic and the deserts and so on? Was that the reason it ran well? So over his objections, I lashed up cables, and I did this, that, and the other thing. I gradually got acquainted with him and the machine, and I concluded he was totally incompetent. With a positive feedback loop, he would adjust the amplifier so, as it were, the needle would stand right on the point, instead of recognizing that you shouldn't be doing that problem. He could never understand Gypsy as a whole. He knew all the pieces very well, but had zero conception of what an analog computer was for. My problem was, not knowing electronics, to decide whether a man was good or bad, and I decided he was no good.

But that's one of my many lessons. So many people have no vision of the whole. They see not the total purpose. Gwen Hansen was like that. I could never get her to see a problem on the Gypsy as a whole. She could only see the pieces. Consequently, if the problems were formulated wrong by the physicist originally, they'd do the wrong problems. Instead of seeing, from the way the computing goes, that it is the problem that is wrong.

else to go to. He is much younger than I am, too.

<u>Tukey</u>. I worked closely with Tukey for five or seven years, and much that I learned about mathematics I learned from him. He was creating power-spectrum theory at the time I was working with him. I thought, in a way, he was as talented as Von Neumann, but Tukey - well, I don't know how to say it - he spread himself too thin, maybe. He's too desirous of being the center of attention. He subconsciously didn't tell you everything, so you had to come back and ask him. Only late in life has he realized that he has to reform. I think he has now; he's writing some books. But he just tried to do too much, too many different things. He may have wasted one of the greatest talents I've ever seen, bar none.

GBP: He wrecked himself?

RWH: Tukey ruined his own talents, destroyed himself by trying to do too much. He could've been much greater, I think, if he tried to do less. He misunderstood what makes great science. He's one of the ones that bothers me the most, because in some sense, I owe so much of my education to him. But I also realized after a while, I had to get loose from him, otherwise I wasn't going to be me. So I gradually pried myself loose.

You haven't got down there the name of Milt Terry, which you really should have.

When <u>Milt Terry</u> turned up, I was in charge of computing. He wanted to do statistics, which involved keypunching; we had a 101 statistical sorter. Looking him over for a while, I said to Milt, "Look, you run half of the computing center; I'll run the main computing part. You take the keypunch and so on. Why should you ask me always for this, that, and the other thing?" Well, Milt was a difficult guy to get along with. I'm difficult. We never had one bit of trouble between us in all the years we divided it up between us.

Now Milt had this feature. Like Tukey, Milt would consult with a guy, but he kept the main control to himself. Whereas when somebody wanted a problem done, I would say, "Fine, we will sit down; we will do the thing together; we will get the program running on the machine; and when we finish, <u>you</u> will know how to run it!" I tried to make them independent of me. Tukey had an office far away from the rest of the math department and, while legally in it, he was excluded. I felt very much the same. He did statistics, which wasn't proper mathematics, and I did computing, which wasn't proper mathematics. I would go down and listen to him now and then. I found him valuable just to go talk to.

Slepian. He's a very good example. You wonder why he was not another Shannon, although just the other day, he got a $10,000 prize. I finally decided what happened to him was he always worked on problems which somebody else had more or less laid out. He did not go out and find the problem, which is ultimately the great thing in mathematics. The man who finds a new field: Von Neumann finds game theory, although Borel knew something about it; Shannon creates information theory.... Slepian, with great ability and hard work - there's no question he worked hard - he always seemed to work on things that had already been somewhat explored and laid out. Therefore, well, what happened happened.

Shannon. We talked about Shannon.

Vyssotsky I thought was very, very good. I was one of those who when Vyssotsky left for South America, kept track of him. I kept writing letters, now and then, till I got a letter back from him indicating he was ripe - independently, I think, of Hank McDonald. We both lured him back to Bell Labs. So we have him back. So that's how good I thought he was.

I still think he's pretty good. Although, he's another man, who may not deliver all I think he has in him. He's gone through now, I think, two divorces. It may well be, his personal life will get in the way of his professional life. We're discussing scientists; we're not discussing whether you shouldn't do this. That's another question. We're discussing scientists as scientists, and his personal life may get in the way. Perhaps one's personal life shouldn't.

Wolontis was an extremely ambitious guy. I don't want to say brilliant. What do I want to say? Flashy in a way, with a bit of a bad temper, and I think in the long run, that did him in. He's retired now, and it may be for health, I don't know. It's a little odd that he should retire at this time, when he's got nothing to go to. When I retired, I had something

"Let's go down and explain to him what is happening, and why he's got something..."

"O yeah, I got that term wrong. Sure, you're right. Let's change the term and I'll do it right."

They could never learn to connect what happened in computing with what was going on in the physics, chemistry, or engineering. Gwen Hansen was one of the bigger lemons that way.

HOP: You used to maintain that it was a difference between doing the right problem the wrong way and doing the wrong problem the right way. I think you ought to talk about that.

RWH: I pulled a variant of that on Kelly and Bode. Bode was not yet a vice-president, and Kelly was the president. I'm giving a talk, and I say "The open shop (being where each guy does his own problem) tends to do the right problem the wrong way (meaning they're inefficient), but the closed shop tends to do the wrong problem the right way." Kelly nodded his head like this after a moment. And I saw Bode's head go like that [nodding], and I knew I was through arguing with Bode. I settled Bode's hash once and for all with that one saying. Kelly saw it immediately. You know how he smoked, with smoke coming up past his face like that, and his cigarette hanging down. He nodded his head like that, and I knew I'd won. That was beautiful!

But that's part of the same thing. Fundamentally, the problem was to get the machine in the person's hands so he could propose the right problem. That's why I think we were very early in getting computers into the labs as lab tools. I had educated a generation of physicists to the use of computers. They knew what a computer could do. They could see that it could do their lab work for them. At one point, the computing department had a corner [on the market] - you couldn't buy a computer without their permission - a computer being probably $10,000. So they bought a $9500 computer. Six months later, they bought another $9500 in parts, and they had the computer in their lab without the Computing Center's knowledge. I knew about it, but the Computer Center didn't know about it. And I encouraged them in this thing. I encouraged them extensively to do this. The big organization had to be circumvented.

Question No. 10

Computer science is now developing as a separate discipline. Is this right? Is a background in other fields necessary? desirable?

RWH: Well, let me continue a little further, and we'll take these in order. That computing science is developing as a separate discipline, I have no comments beyond the following. For years I preached to deans and prexies and such things in universities, "You are creating a Computer Science Department so that computer science can be created." I didn't maintain that it was, but I said, "You are putting the department together do it." I am now embarrassed, to say the least.

On the other hand, no. When I look at computer science, it doesn't look like there's much unity, emphasis, or coherence. It doesn't look like there's much. But when I look at other departments.... Let us look at say math, the guy who gets a Ph.D in topology. What is there there? Is there so much more there than there is in computer science? No. If I judge where the field ought to be, the unity it ought to have, computer science doesn't exist. When I look at other fields, it isn't so bad.

Still I really am mixed up. I am a member of the Computer Science Department. I am planning, quietly, not officially, but I've already made contact. By '85 I expect to be in the Math Department. I may take a detour through Electrical Engineering, but I expect to end up in the Math Department, because I am very unhappy in the Computer Science Department.

My whole department - all of them are busy, in action. They get a machine; they play with it; they want to get more machines, do more things. They don't want to stop and think what it is they should be doing. I'm struggling to get them to tone down a bit, but you can't stop them from having their playthings, what they want. They assign theses which become doing activities, and all the navy officers love it. They are used to well-structured situations in which they know what to do. When the battle occurs, they know all the ground rules and everything else. They love well-structured situations. But this is supposed to be an educational institution, and they need training in ill-structured things, which means you sit and think; you're not told what to think.

Labs took him back, he never did a damn thing compared to what he had. And I believe he could have if he had the guts to try.

Now Schelkunoff was another case entirely. Schelkunoff was in West Street. He was in the math department, but he was separate. He and Miss Gray had this office on West Street, which I used a great deal 'cause I used to go to West Street a good deal. Since he didn't come to work till three o'clock, after a year or so, he gave me use of his office, which was a lovely big one. He got up at six in the morning (he lived up on Eleventh Street on the West Side) and worked all day, and when he was through work, when his energies were gone, then he walked to work and processed pieces of paper there.

As I understand it, and this is only an understanding, the idea of the waveguide was around somewhat. He was looking at the thing, and he found that the eigenvalues showed that the impedence decreased with increasing frequency. And he says, "Man, bandwidth is the name of the game! This is it! This is what we should investigate." He put a great deal of effort behind that, much effort behind waveguide and such other things, although I don't think he was the first. But he wrote the damn book of his in his own notation. He rediscovered much of electromagnetic theory in his own notation. He didn't understand, as Shannon did, the selling problem. If he had written in common notation, he'd be a much bigger name now. But he didn't do it.

Shewhart is another person who's missing. Shewhart discovered Quality Control. If you will look (you can check up on it) when I last looked the Society of Quality Control was bigger than the Statistical Society. Recently I worked two days a week for two or three years at Princeton in the Statistics Department. I bugged them regularly about the neglect of Q.C. How come? Because quality control does not involve fancy mathematics, it isn't popular. Neither were, for a long while, error-correcting codes, because they didn't then involve fancy mathematics, as Shannon's information theory did. Shannon's information theory attracted a great deal of attention. Only when error correcting codes got more algebraic theory in them, did they become more legitimate.

Shewhart was very valuable in lots of ways. I would often go down to Shewhart's office. Shewhart computer shows he really didn't understand the thing too well. He was good, but not really great.

Leagus. She was, when she came, very, very mixed up. She worked fourteen to sixteen hours a day in order to avoid facing life. So from moment to moment, she was highly productive, but it took her years to get herself on the track and become a human being. I think she has by now. She's doing pretty well. For years, she caused us trouble. She'd work late nights, and then we risked being caught on the state laws about employing women. We had to order her never to work past midnight, but she would anyhow. She could've got us in serious legal trouble, because of her desire to hide behind work rather than face life.

Ling, we talked about. I still think he's one of the great guys.

MacColl was a mathematician's mathematician. He was an obdurate, stuffy, difficult Scotsman who would do only pure mathematics. But he was a mathematician to whom, when I came, everybody else turned for mathematics.

By chance, I had actually known of his work before I came, because my thesis cited him. But one day, I went to Bode and said "Gee, in a few years it would be nice if I could be like MacColl," and he said, "Um hm?" And I went off and thought for a long while and decided, I didn't want to be that way. He was a personal friend of ours. We went to Thanksgiving dinners at his house frequently, and we had his wife down just the other day. I realized that, by his stubborness, he'd greatly shrunk himself. He could've been much more effective than he was.

McMillan. I told you, I don't know anything he ever did.

Morgan. He never did much.

Prim was the very same thing. He was a very close friend of ours. When he came, we got acquainted with him early. I thought he had a great future. He was going up the line rapidly, as you know. He was the director of Sandia Research and then he quit and took the job with Litton. He was very ambitious. He nearly got killed in Vietnam, but I think this was already after he knew he wasn't going to succeed, him off to Vietnam to do operations research. While the

GBP: This recalls an experience of my own.

HOP: Well now Jack Borsting's got to agree with that.

RWH: Yes, Borsting did, and all the admirals do. Although the present one refuses to be quoted, I'll quote him anyhow. He says, when he went through here, the most valuable thing was that he emerged a changed person, which I interpret to mean he was educated rather than trained. Now clearly, this school should have more training than most universities. I think justly we can defend more training than the average school. But we have still too much training. The average school has too much training. The average school is so busy training students, it doesn't educate them.

Question No. 11

What do you consider to be Bell Labs greatest contributions in the field of computation?

RWH: I thought that the contribution of Bell Labs to computing was a realization which was - as I said, the purpose of computing was insight not numbers - that computers are a means to do Bell Labs jobs.

Now I can tell you several stories about it. I'd forgotten these until somebody reminded me. Anyway, apparently at one time I said (you know Bell Labs, this was back in the vacuum tube days) "A double wing from basement to attic will be filled with computers." They knew I was wrong, because I was only a computer mathematician. I said, "At present, nine out of ten experiments are done in Laboratory, one in ten on computers, and it'll be the opposite way." They knew I was crazy because mathematicians don't realize one has to look at what goes on in the real world. And I also said, "More than 50 percent of the people at Bell Labs will be pretty directly involved with computers." I turned out to be right. I saw early where computers involved humans, and the continuing use of machines was more important than just volume and production and most other things. And that's why I tend to consider the best thing that I did around the place was to try and order priorities in that way.

Question No. 12

Would you describe the origin of your work on the theory of error-correcting codes? How has algebraic coding theory developed?

RWH: I've been asked this for so often, I've got a standard speech, practically. I was using Stibitz's error-detecting machine. It was a Model V relay computer. It and the Model VI had error detection on them. If they didn't work, they would halt and try twice more before they abandoned the problem. I got to use the machine over weekends. I would assemble a bunch of problems, one after another, after another to do. When I came in Monday one time, no answers, because something had gone wrong. It had picked up problems and dumped them one after another. Well, I tell my friends, "You have to wait another week." Next week, the same thing. This time, we leave Friday; the machine drops, fails promptly.

Well, I'm angry. I'm sufficiently angry to say, "Damn it! If the machine can find out there is an error, why can't it find out where it is?" Now the essential element is that I have high emotional involvement, and it caused me to say something different. As soon as I say it, I know; of course, I can build three computers, and by comparing circuits, I can do it. So there's no question, Can you build the machine? A little thinking shows me that a rectangular array with parity checks horizontal and vertical, would give me the coordinates of any single failure. That comes about because, in Pasteur's words, "Luck favors the prepared mind." I had thought about why the two-out-of-five codes that Stibitz used worked. I had generalized it to find, in general, n bits with one parity check would do the whole job. So I have that. Now, the semiperimeter is naturally best for a square shape, and I'm pretty smug about the whole thing. I'm driving in the company car to West Street one day and suddenly - I cannot come up with the details - I realize that a triangular one, where I put the check with the row and the column on the diagonal, would have less redundancy for the same area. The moment that arose I said, "Ah ha! What is the best possible?" A few minutes and I say, "a three-dimensional cube, check the planes, and I'll have just the three edges; $3n$ against n^3 would be even better. If three dimensions are good, why not four or six? After all, I'm not going to put the relays in that arrangement; I'm only going to wire it that way. By the time I arrived there, I know that 2x2x2x... is really good. But it doesn't take very long to realize that 2x2x2... will give me $n+1$ checkbits, and that gives 2^{n+1} different possible syndromes (I didn't have the word syndrome) whereas I only need $n+1$. I needed a syndrome for the right answer and one for each wrong

He worked on Nike, he produced a book one time, but the book was long out of date before he did it.

Bode was the head of the department. I found great inspiration from him, but some people didn't. I found him highly inspiring.

Sid Darlington. I never could understand, in spite of the fact I'd go to his office and ask, I could not understand what he says. And to this day I cannot. I cannot read his published papers. But he is still going, and he and J. B. Johnson were the only two elderly scientists who, I thought, ever had new ideas in Bell Labs.

Bob Dietzold was my boss. I told you, for years I didn't like him, but he finally said, that All he ever thought of was carrying out Bode's ideas. And then I thought he was very good.

Clara Froelich we discussed.

Marion Gray was superb, but she had her faults. She was trained in computing like nobody else was. She was well educated. She was very smart. She was hard of hearing - and I excuse a lot of that because my mother was hard of hearing - but she would not step forward. When I first came to BTL, I met her in West Street only and I thought she was great, but ultimately I ended up as her boss. One time, she said to me, she should do more research. I said, "Yes, Miss Gray, I wish you would. I wish you'd quit doing problems for other people and do research." She looked dubious. So I said, "I will get Bode to come and tell you. I will get the director, I will get Vice-President Baker to come in and tell you, 'Miss Gray, will you please do more research and less service?'" Result, she started crying.

Now there are two interpretations. One interpretation is that she cried because apparently so many people cared about her. That's a possibility. The other is that she needed the feedback from moment to moment, like a mother. She needed the moment-to-moment gratification of doing work for other people. She couldn't take that lonely path of doing research without the feedback regularly. I'm inclined toward the second theory, but of course I can't prove it.

Lakatos was a nice guy, but I don't think he really had the big picture. The way he designed the analog teach them the concept of an algorithm, that is the main thing. It´s much more difficult than to teach the people a programming language. The teacher starts in computer-programming courses teaching a language. They never appreciate the fact that the student doesn´t understand the idea of an algorithm. And furthermore, he doesn´t really understand what the language he´s got to reduce the algorithm to is. Given a language, it´s a question of expressing this algorithm in this language. Mathematicians don´t understand it either.

About half a dozen times at Bell Labs I had to reduce an elliptic integral. None of the books up until, I would say, about fifteen years ago, actually told you how. You thought they did, but when you tried to do it and follow all the steps, something went wrong. Only in recent years have there been real, careful descriptions: How you take this damn integral, break it down to a type: one, two, three. If it comes down to the third type you´re licked. If it comes down to one and/or two you´re all right, but I had trouble with that third type.

So I think that a constant emphasis on what it means to express something definitively in some fixed language is about the chief thing we need in computer appreciation.

Plus, I think you need to realize how millions of operations, somehow or other, do effect it. There´s a saying of mine (which everybody knows, I guess, it depends on me): When you change something by an order of 10, an order of magnitude, you produce fundamentally new effects. If you can increase magnifications simply by a factor of 10, the field changes. Well, computers have produced a million-fold change in what you do by hand, and it´s hard to understand what millions of operations can mean.

<u>Question</u> <u>No</u>. <u>16</u>

Would you care to give us your impressions of any of the following people:

RWH: I will, providing you understand, this is a personal opinion and gossip, and I´m not giving you friendly reviews.

<u>Blackman</u>. I was best man at his wedding, but <u>I never saw anything that he did that seemed worthwhile.</u>

position. So I´ve got almost twice too much.

The problem then is, How do you find what is least? I talked to various people, including Shannon and others. I came up pretty soon with this simple code. If I want just as many syndromes as I have labels, what do I want? I want the syndrome to name the label of the error position. How can I get the syndrome to name exactly the position with all zeros being no error? So there you are.

Any idiot could do it. I did it in odd moments in less than two or three months, while spending all day long on other things. But it was held up a couple of years for patents, and we finally had to find Holbrook to draw up some circuits to do it. So that´s how that came about.

<u>Question</u> <u>No</u>. <u>13</u>

Is there still a place for analog computers?

RWH: Is there a place for analog computers? There never has been much intellectual interest in them. So analog computers, I don´t think, have got a great intellectual future. On the other hand, the john upstairs has a little device which integrates the volume of water in the toilet tank. I have great faith in the future of analog computers. The little thermostat there on the wall is an analog device which bends in proportion to the temperature. I think there´s a big place commercially. But intellectually I don´t think there´s much room for analog computers. They don´t have much sex appeal. They´re difficult to run; they´re valuable for particular problems, when they work; but intellectually the digital computer is all we have.

<u>Question</u> <u>No</u>. <u>14</u>

Do you still believe that "computing is relevant to every field except mathematics"?

RWH: I don´t know that I ever really believed that. On the other hand, there is some truth to it. Calculus, to a great extent, is how to avoid computing. How do you find slopes and various other things? How do you find the maximum and minimum without vast amounts of function evaluation and so on? Mathematics is basic. Much of it is concerned with the infinite. Lehmer says some place, "A mathematical theorem must cover an infinite number of cases." A theorem that 34 is the smallest number of such and such a property is not a theorem. You just look at the first 33 and see how they got it - provided you make the test in a finite number of trials.

Since mathematics is so intimately connected with infinity and machines are so obviously finite, it doesn't help a great deal. I was an eager devotee of Von Neumann who said, "We'll learn all kinds of things from the particular cases we compute; we'll learn lots." And I knew that Dickson had done a lot of computing. He had two or three girls at all times employed at desk calculating finding special cases to conjecture general theorems. And we have done some good work in algebra, but I put it up to Lehmer, who's used a great deal of computing. He didn't find anything really significant such as Gauss's congruences, and so on. No. They've elaborated things, they found more details, but I don't think they've added anything to the real heart of mathematics - including the four color problem.

HOP: Well what do you think of the four color theorem?

RWH: Well I don't think anybody gives a damn one way or the other. When Ed Moore was working on it at Bell Labs, I said to him, "We don't care about the theorem. If you find an interesting way of proving it, the method of proof may be important, but the result is not. If you did succeed, for a couple of more years you'd go around and give speeches around the country. And then what? Unless the method of proof is important, the result is not.

HOP: Well, Ed spent his time trying to find a counter example rather than trying to prove it.

RWH: But he didn't even find a counter example. Had he found one it would have been useless, unless he found it by an ingenious method. The method would count. The method they used to prove the theorem isn't terribly extensive. It isn't terribly important. It's a little bit better than finding a million digits of pi, but not a hell of a lot. That really isn't mathematics. This book I'm trying to write on calculus really has as a title, <u>Methods of Mathematics Applied to Calculus, Probability, and Statistics</u>.

GBP: I think of William Shanks, who spent twenty years of his life more than a century ago computing pi to 707 decimal places; modern calculations have shown that about the last two hundred digits in his value for pi were wrong.

RWH: I don't really think, as most people do, that mathematics and computing have much in common.

<u>Question No. 15</u>

Do you still think highly of computer-appreciation courses?

RWH: Yes. I think highly of computer-appreciation courses. The average person in the United States, the average educated person should have some acquaintance with computers. I don't think they need to be taught, though, all about them. I think computer-appreciation courses should be taught. They should be taught differently now than of the book I wrote.*

HOP: When should they be taught?

RWH: High school and college. Somehow or other you have make the average person aware of the larger problems, such as, Can machines think? They are after all, going to be the heart of the revolution, the computer revolution.

HOP: Why does the average person have to think about that? What difference does it make?

RWH: Emotional attitude towards it, just like the industrial revolution. Why does the average person have to be aware of <u>it</u>?

HOP: Well then, you ought to teach it in elementary school.

RWH: Possibly. Now I don't say where, but I think the average person needs more than he has.

HOP: What sort of computer education should the average person have all over?

RWH: Well, I guess they should know how to run programmable computers at least, if not microcomputers. If you can

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* <u>Computers and Society</u> (New York: McGraw-Hill Book Company, 1972).