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Interviewee: Howard Aiken (1900-1973)
Interviewers: Henry Tropp and I.B. Cohen
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TROPP:

This is the 26th of February 1973, and we're holding a discussion with Dr. Howard Aiken in his home in Ft. Lauderdale. The other participants are Professor I.B. Cohen and Henry Tropp. Would you like to begin, Professor Cohen?

COHEN:

Sure. Well, I've never done a taped interview like this, exactly.

AIKEN:

Neither have I.

COHEN:

I think, therefore, that I ought to say that I would like to put my terms of participation on record. And they are, that in the first place, that quite clearly, whatever we record here is the literary property of H.H. Aiken, and I think it has to be understood that if he says when we are finished, "You know, I was indiscrete", or something, "and I think we better not let anyone ever hear that until I am out of this world or out of this world for a time", or whatever he wants to do. As a historian, I've got to go on record that this is said.

TROPP:

Well, I'm glad you went on record, but this is standard procedure for my operation: that this is a privileged piece of information until Professor Aiken decides what use, if any, can be made of it.

AIKEN:

And I'll add at this point that unless I think that it is a mistake to add further remarks on watches, ____________…

TROPP:

(Laughter) I think that's a comment almost worth recording.

COHEN:

Well, let me begin by two rather general questions. I've made a few more specific ones here. This is something I think I really already know, but I think that I would like to get it authenticated. The story is usually told, which as I recall, I've heard you tell, is that you really became concerned with problems of mechanical computation during the thesis you wrote with Leon Chaffee.

AIKEN:

That's correct, yes.

COHEN:

That is right, isn't it?

AIKEN:

Yes. I had done a thesis on space charge, and this is a field where one runs into cylindrical coordinates, or in parallel cases, into ordinary differential equations in nonlinear terms, of course. Actually, the object of the thesis almost became solving nonlinear equations. Not completely, but there was some of that in it. The only methods available were __________ methods and they are extremely time consuming. And it became apparent to me at once that this could be mechanized and programmed and that an individual didn't have to do this.

And it was at that time that it became clear that in the words that I used then, there were different kinds of computing procedures— computing by columns, in which one listed all the values of the independent variable, and then made all the first steps and then all the second steps and so on. And the punched card machine was really ideal for this. One didn't have to do a thing. _________________ when the data had the n plus the first row of the table____ you had to compute across this and this and then this. ________________ ____, which no other computing device at the time could do. And this was so elementary and it seemed so simple, that you wonder why I take the time to discuss it, but it was a completely personal observation of the first importance to me at that time. Because in the analog machinery, there was nothing about this. That last point determined the name of the game when we got controls.

COHEN:

Yes. But even at that early stage, you were aware then, I take it, of the punched cards.

AIKEN:

Oh, yes, that's right.

COHEN:

Why?

AIKEN:

Why was I aware of them?

COHEN:

Yes. I wondered how many people. Well now, Leon Chaffee, for example, would he have been aware of them?

AIKEN:

No.

COHEN:

I wouldn't have thought so.

AIKEN:

Well, I became aware of it because having decided that something should be done of this. I found in the library these two books: Napier's "Exhibition"…

COHEN:

My favorite book.

AIKEN:

That thing, and even more important in some ways, "The Catalogue of the South Kensington Museum." And those two things provided me with an enormous amount of background information. But these I didn't find at once, and this led to a rather peculiar amusing incident. The faculty had rather limited enthusiasm about what I wanted to do, if not almost downright antagonism, and we had an assistant in the Department of Physics. If I remember right, his name was Lancsos, Can you remember?

TROPP:

Cornelius Lancsos? He's at the Institute for Advanced Study at Dublin now. Was he at Harvard then?

AIKEN:

Oh, no, no. This was in the Laboratory.

COHEN:

Lanza.

AIKEN:

Lanza. That was the name.

COHEN:

Yes. There were three of them.

AIKEN:

That's right.

COHEN:

Cornelius was one. Yes, there were three brothers, and they were mechanics.

AIKEN:

That's right. Well, anyway, after the proposals for the Napier Mark I had been set forth and people were talking about it, and the faculty had begun to make it rather clear then, that they had no interest, I guess it was Saunders who mentioned it to Lanza, who added his viewpoint. And he couldn't see why in the world I wanted to do anything like this in the Physics Laboratory, because we already had such a machine and nobody ever used it. I heard about this, and demanded to know where we had this machine and it was in the attic of the old research laboratories. And sure enough, we had two of Babbage's wheels. Those were the wheels that I had later mounted and put in the body of the computer.

But this was the first time I had ever heard of Babbage, and then, from there, I found Babbage in this thing: "Passages of Life in the _______________", and from that, I got it out of these, and there's my education in computers right there, this is the whole thing. Everything I took out of a book.

TROPP:

That's practically all there was. There really wasn't anything else.

COHEN:

Well, that's extremely interesting, because I've often wondered about that part of it. In fact, I was going to ask you another question, which I don't need to anymore, whether you had ever by chance on a vacation, gone to the South Kensington Museum.

AIKEN:

I went to the South Kensington Museum after the war, while the place was in still a shambles. Well, I went to England and I always stayed with the Comries, and Comrie made arrangements for the Museum to be opened up so that I could get up and see the analytical engine and it was the greatest disappointment of my life. But that's the way I got to see it. Did you know Comrie?

COHEN:

It was one of the great regrets of my life that I didn't. I've read his papers and what a wonderful man he was.

AIKEN:

Oh, he was fantastic. He was a crusty so-and-so, you know. When he published a mathematics paper, he always put a number of round-off errors in it, and then recorded these errors and wrote a letter to himself which went through the mails, and then deposited that letter with the Secretary of the Royal Society. So that when subsequently, somebody copied his table, he could have this envelope opened and show them the round-off error. (Laughter).

He one time wrote a paper, and I can't remember where it is. It was entitled: "Four or Five Different Ways to Publish a Mathematics Table." One way was to find fragments of the table at different places, and then carry out interpolations so as to get the increment and the independent variable the same for all these tables, round them off to the same precision, and then publish them without making reference to any of the previous authors. And this method, he said, is favored by the United States Coastal Geodetic Survey. (Laughter).

Another way was to find fragments and then compute the likes. And that method is favored by people who he mentioned. But then, the least favorable technique was to find some defining expressions and do all the work yourself, and there were very few people like that.

TROPP:

I was talking to Wilkes about Comrie's work, and he said that Comrie did things that nobody else had ever done in terms of computational techniques. You can find marvelous work like the Mathematical Tables" group, but they were using and they were grinding things out with Taylor series or something equivalent, and getting their precision by just a lot of sweat and a lot of heavy labor. Comrie was just about the only one of his era who really tried to devise computational techniques in order to gain insight into what was happening with the tables he was constructing.

AIKEN:

You may know that he found an accountant sheet made by National Cash Register, and it had a number of registers in it that you could read from one register to another ______________________________ And he adapted that machine to table making and taught the young women to run it, after he left His Majesty's Almanac Office, and opened up Scientific Computing Services at #30 Bedford Square.

TROPP:

Is that the posting machine that shifted at each register?

AIKEN:

That's right, that was the machine.

There's another story about Comrie and I don't know whether it was the truth or not, it may be ________. At any rate, the Royal Navy needed some tables of some differential equations or something. During World War II, they made a contract with Cambridge, and also, to back it up, with Comrie. And the Math Department at Cambridge went to work on this, as did Comrie. Comrie worked on it for a few weeks and then turned in the results. So then the Royal Navy forgot they had another contractor working on it, and a year later, Cambridge walked in with a solution to the problem which they didn't need anymore, and the result being, that they invited Comrie to come to Cambridge and discuss the methods that he used. He took his machines with his girls and he went there and gave a lecture, and finished by saying, "Now this shows three things: First, these young ladies have never attended your university. And second, because they did not attend your university, they were able to learn some numerical methods, and third, because they didn't attend your university, they had not thought that what they knew was after all, quite indecent______________________________" (Laughter.)

TROPP:

Of course, that must have been the attitude about computation at Harvard when you were first beginning to talk about machines.

AIKEN:

Oh, yes. The attitude was that a machine to do what I wanted to do would have so many parts that, based on elementary probability, some of the parts would after all, never work. That was one view.

Another view was that if you got it to work, it would only be a short time and you would have carried out, executed all the work that one could impose on such a device, and thereafter, it would be just a white elephant. It would be very much like a museum and a university knows that it should not breed too many museums.

This led to Connant's letter to me, and I can't remember the date of it.____. Connant wrote me a letter and said that I was one of these "faculty instructors". Do you remember that terrible time when people were supposed to instruct the faculty? (Laughter.) I was a "faculty instructor" with a three-year appointment. Connant's letter was to the effect that the committee that he had appointed to investigate had __________________________, and that I could look forward to being promoted in the near future and become an associate professor, but that I would never advance beyond that rank. That was a bitter letter.

TROPP:

It was almost saying, "Get on to something else if …"

AIKEN:

"Or get out".

COHEN:

It's incredible, you know, the kind of manners people had. Leontief, when I interviewed him, had a letter when he came to Harvard, in which they said that they wanted him for certain things that he was doing, but as far as mathematical economics was concerned, they couldn't understand it, they didn't think it worth doing, and he wanted them to know that no one there would be interested in whatever results he might have obtained. That is the same kind of attitude, but I suppose, as we were talking you see, you see, before I went into physics, I was an undergraduate in mathematics, and I did also mathematics in my first year of graduate work, which took me through 1938. Now there was no one there who ever did any computing. The nearest that I was saying that anyone was interested in even numerical processes was G.D. Burkhoff, and he only because of his concern with the theory of finite differences. Did you ever run into that with him?

AIKEN:

Yes.

COHEN:

But people like Marshall Stone, for example, and others, they never even calculated a definite integral.

TROPP:

Well, Professor Menzel told me that when he was interested in a computation in astronomy, Burkhoff was the only one on the faculty who talked to him about problems that he had.

AIKEN:

Well, there were a number of members of the Math Department even up to the time of my return, who had never been in the computer business. There was one who publicly stated that he had never set his eyes on one.

TROPP:

(Laughter). Never had and never will.

COHEN:

It's really quite amazing. A young lady, who has been doing some research into the revolution in which you were associated, came up with an interview with a Mrs. Rice, Mrs. Edmund Rice, who was apparently a student of yours at Radcliffe in those days of separation, in apparently 1935 to 1936.

AIKEN:

1935 to 1936, that's when I was giving the first course in physics.

COHEN:

They didn't do much in separate physics courses. I taught that course once too.

AIKEN:

Yes, they had the first course in physics, the lecture course demonstration. And Cathy always gave that course, and then he was away for a couple of years and I was given the opportunity of this.

COHEN:

Then Harry Clark took it over.

AIKEN:

Yes.

COHEN:

And then, since I wasn't a real physicist, they decided that I could teach the girls.

Well, this girl says, and I think she is crazy but I think we ought to get it from the horse's mouth, that you even then talked one day about the possibilities of what she called scientific calculation with a machine. She remembers you having a roll of paper under your arm, but I thought that was too early.

AIKEN:

I think so. Either she's wrong about the time, or… I'm afraid this is just not right.

COHEN:

Because you were then working on your thesis.

AIKEN:

Yes. This was the period in which I was beginning to form my future. I didn't know it of course, but this was the period in which I was beginning to form my future. But I can't imagine having conveyed those interests to the girl.

TROPP:

It didn't sound right. Now, you know, there are a number of things in that period. There was this paper of Professor Menzel's on computing, but it has two dates on it: it has a 1935 date and a 1948 date, and you can't tell which was which, and you can't tell what his attitudes were towards computational needs, which I think is the essence of the paper. The need for better means of doing computation.

AIKEN:

I can tell you something about that. Harlow Shapley was, in one way or another, associated with IBM, and due largely to Ted Brown, I went to see Shapley. Shapley ___________________________, as Shapley did in most anything. Shapley was a man who as long as what you said was not obviously nonsense, would give you a hearing and take interest, and if he could help, he would do it. So Shapley, in Harvard College Observatory, became interested in computing long before Menzel— long before that. Because I can remember going out there to talk to Shapley and running into Menzel, and Menzel would be completely quiet as long as we were together and ________________________.

COHEN:

When I interviewed Ted Brown, he told me (I don't know whether I ought to call him Ted, but everybody does, so I slipped into it, you know. I don't want you to think I'm presuming to have more acquaintance with him than I have) that he was a student of the other Brown at Yale in mathematical astronomy. He went to a talk that Shapley gave after Shapley had seen the work of Eckert, not J. Presper Eckert, but Wallace, and talked about the possibilities of using machines, business machines and machines for computing, and that this interested him tremendously. But then when he came to Harvard, he originally wanted a job in mathematics and he saw G.D. Burkhoff and Burkhoff arranged for him to do part mathematics and part business school. Then, he told me about his acquaintance, and I guess he was one of Watson's favorites in many ways for a variety of reasons. But what I couldn't understand about his relation to you was, how did you get to Brown?

AIKEN:

How did I get to Brown?

COHEN:

I could see that once you met him and he got you to talk to Shapley, and you'd come back and he'd get you to Watson, but it's hard to get your foot on your ladder.

AIKEN:

It's a good question and interesting. I recognized very early that the thing that would be required to make this computer go was money and a lot of it. By then, I had decided that I should build the first machine out of somebody's existing parts, rather than to start out with metal and wire or go ahead, but try and use somebody's pieces. Well, there were lots of different pieces to be used. There were the pieces of the computer industry at that time. There were the pieces of the step switches and so forth of the telephone industry. The telephone industry also had teletype and that was wire control printers, and they had punched paper tape, and I could foresee that it didn't make any difference whether you used tape or cards for input. The tape was harder to edit and you couldn't sort with it, but nevertheless, it would work and it had advantages.

So all these different techniques— printing telegraph techniques, telephone switching techniques, computer industry techniques— were all griss for my mill, at that time, largely as a promoter, trying to find out where to get these pieces so that this machine could be put together.

To this end, I went to the Monroe Calculating Machine Company. That was the first step, and what was the name of the charming man I met there? I'll tell you in just a minute.

COHEN:

Mr. Chase?

AIKEN:

Chase! My gosh, where did you get that? Let's see.

COHEN:

Well, the only reason I happen to know about Chase is that Chase has published a history of mechanical computing machinery, and he describes how Aiken outlined to me the components of a machine which would solve his problems. His plans provided automatic computation and the four rules of arithmetic, pre-established sequence control, storage and memory of instold or computer values, sequence control which could automatically respond to computer results or symbols, together with a printed record of all that transpires within the machine, and a recording of all the computed results. I recognize the feasible construction in the outline of the mechanism he proposed to build. It was not restricted to any specific type of mechanism. It embraced a broad coordination of components which could be resolved by various constructive medium."

AIKEN:

He's just saying what I said a moment ago, only much better.

COHEN:

Well, I just thought you might like to know where I got it. You said, how did I get it.

AIKEN:

Is this the paper?

COHEN:

Yes, that's the one. That's the symposium.

AIKEN:

Okay. It's the symposium at Pittsburgh. It was John… what's his name? He's dead now. John Bowman.

Well, anyway, I went to Chase, and did just what he said.

TROPP:

What's the date on that visit? Is that dated? Was that in 1937?

COHEN:

It was April 22, 1937.

AIKEN:

Okay, there it is. I went to him, and Chase was Chief Engineer at Monroe, and a very, very, scholarly gentleman. He took almost immediate interest, and we kept up an association for quite a few years there. He wanted in the worse way, to build Mark I. He would supply me with the parts and we would collaborate and do it together, that's what he wanted to do.

He also foresaw what I did not. I did not foresee the application to accounting as coming out of it, and he did. He went to his management at Monroe and he did everything within his power to convince them that they should go ahead with this machine because, although it would be an expensive development, it would be invaluable in the company's business in later years. Now this is the thing that Tom Watson was never able to see.

COHEN:

He was not.

AIKEN:

Oh, no. I'll mention that in just a few minutes.

But Chase could see this. His management, however, after some months and months of discussion turned him down completely. They wouldn't have a thing to do with it. They said it was totally impractical. In fact, they sounded almost like the Harvard faculty. But in his moment of disappointment, it was he who said, "Go to IBM, because IBM is very successful and they more apt to undertake a thing of this kind than anyone else."

And I said, "All right, who I should see at IBM?" And he said, "Why don't you see Professor Brown? He's at the Business School at Harvard. He's right there." So I said, "all right", and I went to see Ted Brown.

Ted immediately came to life and suggested that I see Shapely. He immediately came to life and off to IBM. That's the way it happened. Now it's interesting that although IBM spent money for the parts and provided men to help with the reducing practice, none of the people who worked on this job or Watson could ever see that Mark I had any meaning whatsoever at all, other than an advertising venture to satisfy this peculiar guy from Harvard. The men who were detailed workmen found it interesting, but didn't want to spend much time on it. They felt that they had to work on something that had some commercial value, because if they didn't, on one of his trips to Endicott, Watson would find out that they hadn't produced anything in the way of a new product, and they'd be in trouble. So that after the basic ideas had all been laid down and we'd gotten to the place of execution, I had to make a trip every month or so to Endicott to beg these people to get back on the job and do a little bit more. That's the reason it took seven years. Even after I went into the Navy, I still made those trips to Endicott to plead: "Come on, let's do a little more."

COHEN:

That's very interesting. Again, you know, you are so apt to read things backwards, and they think of the involvement of IBM after their SSEC in the computer industry, and they just assume that this was their vision of it. But as I was saying to him, it seemed to me that I still don't fully understand why they did it, unless their parts were available. It would be, if it worked, a great feather in their cap. It would give them a lot of good publicity, they had a charitable instinct to some extent. Watson, despite all of his bad qualities, after all did have some idea of helping knowledge and new technology, and if this was a great feat that you could do, let's sort of do it. Was it more than that

AIKEN:

No. Absolutely not.

COHEN:

Because one can see that none of those engineers, even the ones that IBM talks about in terms of their contribution— Lake Hamilton and so forth…

AIKEN:

And Durfee.

COHEN:

And Durfee. You never hear of them doing anything in this line afterwards. That they just didn't decide that this was…

TROPP:

Well, I think you give IBM too much credit for vision.

COHEN:

No, I'm not, you see.

TROPP:

No, but the vision didn't start until much later than the SSEC, it was in the 1950's.

COHEN:

Oh, I see. That's their statement.

TROPP:

It started after the defense calculator came into being.

COHEN:

Oh, yes.

AIKEN:

IBM got going in the computer business when young Tom was made president.

TROPP:

That's right.

AIKEN:

And as he told me one time, the first thing that he did when he became President of the Company, replacing his father, was to sense the embarrassment the Corporation faced because Remington Rand was getting all the credit for going ahead. So that if the senior Watson had remained active for say, another two years, chances are pretty good that Sperry Rand would be the big computer company today. It was changed by young Tom.

TROPP:

That's right. He didn't have the technical knowledge so much as the vision. He had been exposed during the war, in aviation and to radar particularly, and I think this was where his knowledge of electronics came from.

AIKEN:

And he was also being needled by all the large customers: "What's Remington Rand doing? What are you going to do?" That sort of thing. So that Watson actually couldn't understand what all the publicity was about. He couldn't see that it had any practical value. He felt that he had been slighted when the credit was handed out, very much, he felt slighted. In fact, he felt so slighted, he tried to get me fired from Harvard, despite the fact that at no time had I ever tried to take any credit away from him.

COHEN:

Well, that's a terribly interesting thing to me, because that answers one of the always biggest questions I had of your getting to IBM and why IBM did it. By the way, since this is being recorded, I didn't mean to say that SSEC was a value judgment, but IBM always, in their official handouts, talk about going back to their SSEC, you know. I don't want to get into that, but that's another question and let's not worry about their later history.

I was interested in another thing which was connected with that. Of course, Monroe would come to you. As I remember, we had in the top floor of Cruft, a whole battery of Monroe's, and had them at the Observatory. It was either Monroe or Marchant or sometimes Burroughs, but we all at Harvard, ran on Monroe's. It was an old one that I learned to compute, under Fred Whipple at the old laboratory which they knocked down, you remember, by the old tennis courts. They had old ones that were so old that the gears were worn. Instead of watching, you could listen and hear the thing turning over and count the clicks. (Laughter.)

But in your thesis, you talk about the methods of solution of expansion in infinite series, and numerical integration by iterative methods. You obviously, whether it was experiencing your computing with a Monroe or what, immediately turned to the concept of digital computing. And that interests me because of two things and now let me tell you them, and you'll see how wrong I probably ought to be in my guesses. At that time, of course, the only people who were thinking in terms of computers were people who were doing analog computing, and while it's true that we've forgotten about that, after the war, Bush and his group did make a very complex analog computer taking over all the things that you had been developing, and that had come out of the ENIAC, and you can, after all, program an analog computer to do a great deal. And I've seen that as I look back, that if you look even as late as the forties, it wasn't clear to people as to whether the future lay with digital or analog. And during the war the development of fire control, radar, and so forth, bomb sites, all use analog computers and everybody is thinking about it. Well, it seems to me very odd that you should not, and especially so in that when I spoke to Vannevar Bush and asked him how he really got started on this, he said the big push came when they began to make power networks, and they were terribly worried about possible black-outs such as we eventually had. They couldn't do the computing, and they set up and began to devise these analog computers and that's how he said he got into the thinking about these differential analyzers and so on.

Now, you had a back ground in electrical power engineering. You lived in a world of analog computers. It would seem to me to be the obvious conclusion of that simple syllogism that you should have been what you weren't.

AIKEN:

Well, as I said, I got going on non-linear differential equations, and non-linear differential equations are not the easiest to deal with on analog computers, and it was for that reason that I began being interested in digital machinery. This was it, really, and this was the thing to do.

Also, I guess there was another point. The analog machines are two or three digits, and I foresaw the enormous libraries of mathematical tables, not realizing that the very thing I wanted to create to make the Mathematical tables ____ was necessary. (Laughter.)

COHEN:

It's easier to compute your results than to look them up.

AIKEN:

Yes. And it was for this reason that the early phases of the computer had such a strong table connotation on the work that was done there.

COHEN:

I remember as the youngest member of the Physics Department at lunch, your coming in with the first volume of the Bessel functions and tables, and everybody terribly excited by it.

Well, going on with the astronomy, I suppose at a certain stage, you must have come on the work of Eckert. Perhaps not Comrie until later, but …

AIKEN:

No, I found Eckert before Comrie. In fact, I went to see Eckert at Columbia. Somebody at IBM and I can't remember who— Rice. And I can remember to this day walking into Eckert's office, leaving my overcoat on a chair in the anteroom, and going into the office and sitting down. After we'd been talking for four or five minutes, he said, "Where's your coat?" And I said, "It's on a chair on the second floor." And he jumped up and dashed up and retrieved my coat and brought it into the office, and he said, "You mustn't leave things lying around Columbia". (Laughter).

COHEN:

Even then.

AIKEN:

Well, what was very clear to me when I saw Eckert's Laboratory and talked to him was that he had made excellent use of _______________ machine, even as Comrie did of the National Cash Register Machine. Eckert was not a man to redesign the machinery or change the machinery. He was a man to use it and extract the maximum benefits from it. But what he was doing was computing _____________. (Laughter.) Isn't that amusing to say it that way?

TROPP:

Yes, except when you think about iterative procedures, you realize you can't go to vertical methods.

AIKEN:

Of course, there are so much more elegant means of expressing these ideas today, but it was ___________________ to me. So I left Eckert's _____________ Laboratory with a feeling that as a competitor he was just doing fine. There was plenty for me to do.

COHEN:

It was a problem that hadn't been solved.

AIKEN:

That's right.

TROPP:

Did you have contact with Ben Wood in that same period?

AIKEN:

No, I never met him. He was a consultant to IBM and I had never met him, but I've heard about him.

TROPP:

Because he had computational needs in handling all the data from all the tests that he was working with.

COHEN:

There was a famous dinner that I've been told about. I was a graduate student, of course, and I knew nothing of it at the time, in 1938, and I was interested in the people who were there. The list seems to include Dean Westergard, of course, who was at the engineering school, E.B. Hunington, who I guess was still active then, and getting kind of old…

AIKEN:

Yes, this was a Harvard dinner.

COHEN:

Yes, a Harvard dinner. There was Stern, and that must have been Ted Stern, who was, I suppose, the best of the young mathematical astronomers that they let go.

AIKEN:

This was the dinner in which I was asked to describe what I wanted to do.

COHEN:

Right. There was Bridgeman.

AIKEN:

Yes.

COHEN:

Funny choice, though I must say that I would love to tell Bridgeman anything and I always did. There was Ted Kimball, yourself, Brown, and J.G. Phillips. But what's interesting is that the only man from the Math Department there was E.B. Huntington, and I suppose it was because he had done statistical tables.

AIKEN:

He was more conscious of the fact that ____ than anybody else in the Math Department. He was also more …

COHEN:

____

AIKEN:

Yes. Actually, that dinner, which I had forgotten all about, was probably the ____ _______________. These were the kind of people_______________________________________.

COHEN:

No, as a matter of fact, I remember as you must certainly do, and I don't know if you are aware of this. After the War, at the time that you were holding those great conferences, and I don't remember whether this was the first one or the second one, but I remember that there was a great discussion between you and Norbert Wiener as to whether the computer was really a brain, and this kind of question which newspapermen like to ask, you know. Obviously, you took opposite sides on it.

But I remember that I was lunching with some people in the Math Department and I don't remember who they were, but I remember that there was just general agreement that this had nothing to do with mathematics as the Mathematics Department could see it. They could never think of a mathematician being interested. I remember that somebody there and it might have been Garrett Burkhoff's eventual "von Neumann is", and somebody said, "yes, he was one of the greatest mathematicians of the twentieth century, but obviously, he was getting old." And I think that if there was one thing about which those mathematicians would have agreed, it was that. And if I'm not wrong, it was Marston Morse who said this, and he was a very able mathematician.

TROPP:

Well, the way you describe the mathematician's attitude is accurate. I remember it even into the fifties. These things were for other people, but not for mathematicians.

AIKEN:

Well actually, this attitude prevails today. The computer establishments in the universities are almost never in the math department.

COHEN:

Well, as I see it, the mathematicians weren't interested. The engineers were interested in the engineering problems of it, but they had yet not been taught to think in terms of what the computer might do. They solved their problems with big slide rules and tables and for them it was extraneous too. The only people that I can see as a group who really were interested were astronomers, and of course, as I was telling you, they were the big computers and the people who did the tables. Physicists didn't really do much computing except on a simple level.

AIKEN:

That's right. It was astronomers who essentially extended the techniques of Adams and Ashforth.

COHEN:

Ashforth is a man whom I admired. Do you know about Ashforth?

TROPP:

No.

COHEN:

You ought to look at his books sometime. There was a real brain and an unsung hero over the modern world. He was a great mathematical astronomer, who also designed machines, tide machines. Of course, they were all analog, but very interesting ones.

AIKEN:

And they solved the differential equations defining _____________shaped drums.

COHEN:

Yes.

One question that I'd like to ask you about is a double one here. We've talked about the fact of digital, but my question then was: was it simply the IBM contact that drew you away from vacuum tubes? After all, you were Leon Chaffee's student, in a sense, and his life was a life of vacuum tubes. I take it there was some thought of quenching circuits of some kind that would use vacuum tubes in the Mark I, wasn't there at some stage?

AIKEN:

Yes, for radio radar parts. Well you question really is: having grown up in space charge in arbitrary-like Cruft, why was Mark I an electronic device? Again, money. It was going to take a lot of money. Thousands and thousands of parts, and using the techniques of the digital counters that had been made with vacuum tubes just a few years before I started, for counting cosmic rays, it was very clear that this thing could be done with electronic parts too. But what it comes down to is, if Monroe had decided to pay the bill, this thing would have been made out of mechanical parts. If RCA had been interested, it might have been electronic. And it was made out of tabulating machine parts because IBM was willing to pay the bill.

COHEN:

Looking back, of course, if you can compare what you were doing with Mauchly and Eckert, they were getting speed and you were getting reliability. Isn't that what occurred to you then?

AIKEN:

Oh, yes. In fact, …

COHEN:

Because you used to holler about reliability. The one word as I told him when he said, "What do you first remember about Aiken?" I said, "Reliability".

AIKEN:

Yes. You see, a computing machine is not reliable. It's worthless, absolutely worthless. You can't trust the results. And that was the reason that we checked everything that we did. Even today, people don't go to all lengths to check what they did. Just think of _________________, nobody ever questioned it. Nobody ever questioned anything. I did an awful lot of checking even on the machines that we had, to be sure that there wasn't something ____. Well, we were conscious that we had ________ and the ENIAC didn't. We had program facilities which they didn't. We had input and output which they didn't bother to worry about. And last of all, we had a precision so that if they used Simpson's Rule to integrate ____________ tiny little steps. And we could do the same integration with Mark I with twelve thicknesses, and then suddenly ________________________ and make great big steps, you see, and we could do it once and they had to do several times to see if they could get the same number s twice. So that we used to say, "What's all this speed for? What does it accomplish? We get there sooner."

And yet, you know, I really question if electronic computation would ever have become a great success had it not been for <u>the transistor</u>.

COHEN:

I know. I've said it too, without being as wise as you are. There are people who argue against it because they point to Sage, because Sage did work with hand picked vacuum tubes, but it's the unusual one. It's that reliable transistor that really made the difference and made it go.

AIKEN:

It was that and the memory. I mean, the memory suddenly went from the least reliable to most reliable almost overnight.

AIKEN:

____ magnetic bubble memory just as sure as anything, and ten to twenty bits was not impossible. Just think of the problems that were going to get solved with that memory. You've heard all the talk about mechanizing the Library of Congress back in 1950, people were talking about that. Now let's mechanize the ____________________. All of this back in the fifties, there was consideration being given to problems of this kind.

There was research on information retrieval techniques. My student, Jerry Saltman, is one of the most distinguished people in this area. That began before we had the memory, and really these information retrieval techniques have been put together by Saltman and others over the last decade and will come into its own in its mechanization. I have no question a practicing attorney will have the law sitting on his desk with some keys and information retrieval techniques to assist him in making briefs. This is just going to happen so clearly. Accountants, lawyers, physicians— you can imagine a physician having at his desk a key approach device that will give him all the side effects not of one drug but a combination of drugs. It will be invaluable.

The Encyclopedia Britannica up there might be on your desk with some keys. Why will you want all those books?

COHEN:

And as the new information comes and you revise it, you're up to date; you don't have to wait for the next edition.

AIKEN:

That's right, and you know that the chances are pretty good when the __________ are reduced to practice, that you can build a device of that kind for less than it costs to make those books.

I know you're not talking to me about what's coming in the future.

TROPP:

Well, I really wanted to talk about the future as well as the past.

COHEN:

Yes, well let me do my three more past, and I want to say that I think this is a fascinating thing, because I think it's astonishing always to see what your insights are.

TROPP:

And it's not divorced from the past.

COHEN:

No, it's not.

I had one question which I don't know really how much you want to put on record about, but it had always interested me about it, and that is what really … I guess Bryce didn't contribute anything in the way of ideas.

AIKEN:

Bryce? He was a very astute inventor. A very astute inventor.

COHEN:

So I gather.

AIKEN:

And he was a valuable senior advisor. If you started down a road that didn't look very practical, he could put his finger on it just about that quick. He was worth all the other IBM people put together.

COHEN:

But Lake, Durfee and Hamilton, did they contribute any real ideas?

AIKEN:

Lake contributed absolutely nothing. He was the head of the department in which Durfee and Hamilton worked. Durfee contributed sweat. I must tell you an amusing story about these gentlemen, which I think will go far to make it clear to you.

I went up to Endicott after this began to be formalized at IBM over the years. During the conversations with Lake and Durfee about what kind of machine this was that we were talking about. It was, oh I guess I made eight or ten such trips before I learned that IBM didn't know how to divide. And that was a terrible blow. They could add, they could multiply, but by god, they didn't know how to divide. It was almost like the bottom had dropped out. Maybe I ought to get back to Monroe—they knew how to divide.

So I stayed at a hotel in Binghamton and I can't remember the name of it. That night when I found out that they didn't know how to divide, I was up nearly all night, and it was that night that I invented the technique of dividing by computing by <u>reciprocals</u>. This is a scheme by which you can compute <u>reciprocals</u>, knowing how to add and multiply and you know how to do it. You could add and multiply by computing reciprocals and all you need is a first guess, and a first guess, when you are dealing with an independent variable that proceeds by fixed intervals ________, the first guess is always the reciprocal of the last name, and the convergence is beautiful. You double the number of digits of precision at each iteration.

So it was around three o'clock in the morning when this all came clear. The next day I walked into IBM and said "Well, you don't need to worry about not being able to divide because I know how to divide with an adder and a multiplier." And I started to derive this expression using the ________________________________. And it became very clear that this was a waste of time because these men couldn't understand this. They knew no mathematics. Even high school algebra was too much for them.

I came in the next day and said, "You know, it doesn't make any difference if you can divide because I can divide with an adder and a multiplier", and I started to derive an expression with this, which was meaningless. So then I said, "Well, you do this. You want to find the reciprocal for the number, and you guess any number whatever that you think is a first approximation, one digit of accuracy ________________. You multiply these and you subtract from two, and then you multiply them a guess, and you'll have a number which is closer than the guess. Then you take that and repeat it all over again, and keep on going until you get the accuracy you wanted to get the reciprocal."

Well, that helped, especially when the example I used was to compute the reciprocal of three on the assumption that three tenths was the first approximation. That would give you .33 and the next time, .3333 and up. Durfee and Hamilton thought this all over, and Durfee said that he wanted to go away and think about it. He disappeared and didn't show up until 5:00 that night.

He came back with an enormous piece of drawing paper, and it was covered with pencil arithmetic on all sides, and he threw it down on the table in front of Hamilton, and he said, "Ham, it works every time." (Laughter.)

COHEN:

I am so glad to know that's a true story. Harry Clark told me that. Do you remember old Harry Clark?

AIKEN:

I sure do.

COHEN:

And he said to me, "And it works. It works every time."

AIKEN:

And then, on the basis that you can't patent mathematics—this is what we began talking about—Hamilton designed a mechanism to do this, and I believe he has a patent. He made an application and I suppose he has a patent on attaining the values of reciprocals for this thing. He used the credit, not me.

TROPP:

That took care of division.

AIKEN:

Yes. Well, now then there was a lot of talk at IBM about this, and Bryce, whom you mentioned, became confident that IBM ought to learn how to divide, and that being satisfied by approximating, he designed a dividing machine. This was then incorporated into Mark I. So we ended up having a Bryce divider in Mark I, which we took out three or four years later because we built a fast relay multiplier, a fast relay multiplier with this gimmick back here. And with that fast relay multiplier, we could divide faster than the dividing machine. This was the arithmetic unit right in here, and by taking out that divider, we had all that space and we could use the counter reels and increase the storage capacity, which went from 72 storage registers up to 100 and some. Oh boy, this was a tremendous _______________.

TROPP:

I had seen the piece of Mark I that's in the Comp Lab, but I hadn't seen that portion of it. The fast relay multiplier is not there.

AIKEN:

This is the relay multiplier, and this thing here, this cabinet had in it some subsidiary sequence devices that would run the machine for 22 instructions, the instructions being determined by plug board. And it also had in it a whirligig, an arm rotating in synchronism with the main shaft of the machine, so that a revolving lamp would… (Interruption)

Let's see, what were we talking about?

COHEN:

We were talking about the rapid divider.

AIKEN:

Oh, yes. I was starting to say that we had this revolving lamp and we could put that across the cams that took the sparks and turned on the circuits of the relay and those cams would get burned. So Saturday morning was reserved for maintenance. One of the things that you did was to connect this thing and all the cams in sequence, and make sure that they were in pretty good shape so we could probably run for the next week.

COHEN:

I take it that although they did provide a divider other than the one you provided, that all of their contributions were of that kind. I mean, they were mechanical linkages and so on.

AIKEN:

Well, their contributions were _______________ arithmetic, using their techniques.

TROPP:

It was pretty off the shelf, then, technology.

COHEN:

Except for the one — divider.

AIKEN:

Well, the divider became a standard technique in their technical machine design thereafter.

COHEN:

Oh, it did? That was going to be my next question. And that's the Bryce. They never used your method.

AIKEN:

No.

COHEN:

Did you ever write that up that you remember? I'm just interested, because methods of dividing happen to be a little hobby of mine.

AIKEN:

No, I don't believe I did.

COHEN:

And it was never used so far as you know, either.

AIKEN:

Oh, yes, it's been used quite a bit, but always by programmers. Quite a few programs have used it. You see, very few machines have square roots, and you use the same technique for square roots and reciprocal square roots. Yes, I'd say the technique has been very widely used, but not so much for division because most machines have dividing in them.

COHEN:

I'm not surprised at your ingenuity in devising a method, but I was just a little curious too as a side, it isn't a major issue obviously, as to how you happen to think of using that rule? Had you had experience with it?

AIKEN:

I had had a good deal of experience with Newton graphs, because I had already recognized that I had to use Newton graphs in square roots.

COHEN:

I see. Being interested in Newton, you see, that's what fascinated me.

AIKEN:

So having already decided that Newton graphs would be used for square roots, it wasn't too much of an extension to say, "Well, if I can't divide, well I'll see what I can do with Newton graphs."

COHEN:

Well, that's extremely interesting and I think very valuable, because no one is really ever entirely sure about what contribution they made, and as you know, afterwards, that became such a silly question overshadowing the primary one.

AIKEN:

Well, one should not belittle what IBM did by any means. They had all this equipments that in totally different form was used, but it did do a performance of arithmetic, or Field's arithmetic, ________ Bryce's then. And once those equipments were made available, then the overall organization of the machine and the design and techniques for printing, final printing, and the design of techniques for automatic control was about what was left that had to be done to put this thing together in one piece. So I wouldn't belittle what IBM did by any means.

TROPP:

Given the attitude at Harvard that I don't think is any secret even in terms of their attitude towards what you were doing, how did you convince Harvard to get involved in this venture with IBM?

AIKEN:

I suspect that by having gone so far that they couldn't quite see how to stop that. I've always thought that my experience at Harvard in that regard was very much like Hickman's experience. Hickman was a three-year instructor_______________________, and he was due to be told that this was the last appointment, when he found a Navy radar station ____________________________ transmitting __________________________ and he could get this stuff_____________ service equipment. He went out and scrounged this equipment and presented it to Harvard, and Harvard didn't have the heart to say, "Gee, good-bye." So he managed to make a career out of it.

And I think my experience with the University was about the same. Nobody wanted a computer and nobody thought it was going to be any good, but I had gone so far with the thing that it just didn't seem right to have me give it up.

COHEN:

You know, it's really amazing. The war came and then once it was there, there was no more any problem, but I was present, again in my junior capacity, when it was decided to scrap the battery room to make room for the Mark I, which was due to come up. It was all ready to come on and there was no place to put it. And there was a long discussion: "Well, you know, do we really…", and "is this for…?" I remember one question that was asked, I wouldn't remember by whom at that time, was, "Well, will there be money when we get rid of this and the wars over, to restore the battery room?" (Laughter) Well, we had a famous Harvard professor of physics, Joseph Lubbering, who built all kinds of things and was a great man in many ways. He was a professor who had the Bridgeman Chair, Nat Hollis' Chair, for fifty years, and at a meeting of the faculty, they asked whether they should introduce electrical engineering, and he said, "No". He said, "It's just a spurt. I've seen these things come and go."(Laughter.) And that's the way people are, and that, I think you're not going to change. It may be that the Moore School was different, but I would really like to know what they thought down there. My guess is that there wouldn't be any academic group with a vision to see what this was. Well, who even outside of academic groups? In fact, I was going to ask you how many people other than yourself really did have a vision as to what the many, many kinds of things computers could do other than just tables?

AIKEN:

It was pretty slim. Comrie could see it. He saw it from one end to the other.

COHEN:

Because I don't think Eckert ever did, and he was interested in the Air Almanac and simplified things, but that the computer would ever do anything more than tables…

TROPP:

I think Stibitz had vision.

COHEN:

You do. I don't know much about him. Did you ever run into Stibitz?

AIKEN:

Oh, yes, I know Stibitz quite well.

TROPP:

I mentioned to you that in 1946, he was already designing an electronic computer for business applications.

COHEN:

Yes. Well, I was sure you knew him afterwards, but you didn't know him during the planning days.

AIKEN:

No, I didn't.

COHEN:

No one even knew what he was doing, I take it.

AIKEN:

I met Stibitz at meetings because we were both interested in the same field. I had never heard of Stibitz until Stibitz and Williams, and I got to know Williams better than Stibitz.

TROPP:

E.G. Andrews was there.

AIKEN:

Williams, I guess, is the man who did Stibitz' work.

TROPP:

But in terms of ideas and vision, I think Stibitz did have it.

AIKEN:

Is Stibitz still in Vermont?

TROPP:

He's still at Dartmouth in the Medial College.

AIKEN:

He's at Dartmouth now? Stibitz is?

TROPP:

Yes.

AIKEN:

I didn't know that.

COHEN:

Well, he has a new career in medicine.

TROPP:

Yes. He's following your outline of staying active by finding a new career.

AIKEN:

Well, he must be working with Miles Hays there then. Miles Hays is one of my students.

TROPP:

And E.G. Andrews is retired and living in the same area. He was also involved in the actual building of this machine.

AIKEN:

Yes. And when he retired from Bell Laboratories, he went to work for some space contractor, I think ____________________.

TROPP:

I think another man who had vision, and he was kind of analogous to the astronomers, and that is John Mauchly, and that's because of his interest in weather. When you have been interested in statistical analysis for a long period of time, you have a different viewpoint of computation, and I think he too had vision of eventual needs in computation.

COHEN:

Well, I remember when you used to talk about all possibilities of language, linguistics, automated factory and so forth and all kinds of things. It was just "Aiken again, you know".

AIKEN:

"Running off at the mouth again."

TROPP:

There really were only a handful of people who could see over the next hill. Right now, I can't think of any other names in that early period.

COHEN:

Well, of course, as I look at the history of the computer, which has become sort of an interest of mine. I mean, we live in it, and I think you have to do something with your own period if you are a historian. I look back on those great conferences that you held and the attraction that they had as being an enormous personal contribution of yours, apart from just the existence of the Mark I.

Now I happen to be looking through the file of correspondence in the archives, and I came upon your correspondence with Leontief. I don't know if you remember it, but it's extremely interesting. You invited Leontief to come and participate because you realized that what he was doing was relevant and you wanted to attract him, you see. Leontief wrote back and said, "But I've never, myself, done any work with the Mark I." You wrote back, "Well, it doesn't matter. We've got another one. Come and do that." And so forth. Well, this was the way, you know, of bringing people in and propaganda, where you may say, "Well, that isn't important", but in history, it is. Unless you get people interested in going, the uses won't come by themselves. They've got to be made, and they won't be made by traditional people because they're satisfied with what they've got. They're making a living, they're making a career, and they're doing it, but these other people interest me.

There were two people I wanted to ask you about. Did you ever have anything to do with Wilbur, at MIT, who made a large analog computer in which there were these large mechanical frames that would vibrate and the solutions of 12 by 12 matrices could be got because the vibration would cease when you would get to a position of stability. He was a professor of civil engineering who was interested in building design. You never ran into him, I take it. I found him because the only person who ever used his machine was Leontief, and he realized, Wilbur did, that either he went into this full time and give up civil engineering or give it up and go back to civil engineering, which is what in turn, he did. There must have been a lot of people like that. He had no vision, but I just wondered if you had met him.

The other person that I was interested in, I know that you had at least two contacts with him, but for reasons that are fairly obvious, I think it would be fascinating to know what your relations with him were, when you first heard of him, was von Neumann. Now I know that Warren Weaver sent you some computations for von Neumann for the Mark I on inclusion. Secondly, you were with him on that National Academy of Sciences Commission. I have no idea what kind of relations you might have had or where you first heard of him.

AIKEN:

Well, let's take the Commission first. Who all was in that Committee, let's see.

TROPP:

There was you and von Neumann and Stibitz. Was Sam Alexander on that?

AIKEN:

Maybe. But there was a mathematician and what was his name? He's presently at Miami University.

TROPP:

Oh, John Curtiss.

AIKEN:

That's right— John Curtiss. John Curtiss proposed that we should all get together and start an association for people interested in computing machines, a new scientific society. And I said, "No, we shouldn't do that because computation was a universal thing." I said that what we should do was to help the mathematical economists to publish papers in their journals, using computational techniques and astronomers with theirs and the physicists with theirs and so on. That our best interests and the best interests of the scientific community as a whole would be better served to assist everybody to use machinery and to publish their work. And after all, we were tool makers, and it didn't make very good sense for all of us specialists to get together and talk about the tools they used.

Von Neumann agreed with that completely, and so this proposal of Curtis’s' was voted down. Curtiss then went out and formed the Association for Computing Machinery.

TROPP:

That was the Eastern Association, the original one.

AIKEN:

Yes. And neither John von Neumann during his lifetime, nor I have ever joined the Association for Computing Machinery. We opposed it and I still oppose it. Now, the basis for the opposition today is, of course, far less, because now you get all the machine builders and there are thousands and thousands of specialists, and they do have something going for them. But as of the time that it was proposed, it didn't make very good sense, and as I say, von Neumann refused to have anything to do with it and I'm just as stubborn as von Neumann, and I never joined.

COHEN:

Did he ever come up to your Laboratory?

AIKEN:

Yes, many, many times. We did several problems for him. Then, we had a lot going back and forth. As long as he was using computing machines and helping to lead us in the discovery of numerical methods, which in some of the problems that we did, Dick Block did the programming by the way…

TROPP:

Dick showed me some of the early programs that he wrote.

AIKEN:

It seems to me that that was pretty good programming even for today. This was done by Dick with the assistance of von Neumann.

TROPP:

Well actually then, you really introduced von Neumann to automatic computation. That was his first exposure.

AIKEN:

Yes, that's right.

TROPP:

And that, if Dick remembers right, was about 1943.

AIKEN:

Yes, yes, that's right.

TROPP:

When he was talking to you, and this was even before the machine was delivered.

AIKEN:

Okay, so then von Neumann made what I've always thought was a grave mistake, as a computing machine builder. He had some very good ideas on the mathematical structure of machinery, and if had stuck to his _______ in that area, we would have profited from him. But by going off and starting to build machines himself, he ended up partly like Babbage. He was not an engineer; he didn't know how to organize a thing of this kind. Input/output, the necessity for that was just something that was dragged in, you see, _____________________________, and he obviously had a very, very loud voice in the sense that he was very influential. The result was that things which he proposed, good, bad, or indifferent, were/in some cases followed.

He was responsible for the observation that a string of binary digits represents ___________________, and also represents ____________. So why differentiate between the two. You could have one store. The desirability of having access from data store into information __________ store and vice versa is very, very necessary. But just because strings of binary digits were indistinguishable, doesn't necessarily say that the best thing to do is to dump all this stuff in one pile.

First thing you do is divide the speed of the machine by two. You can get an instruction to find out what you are going to do and then go and get the data and bring it up_________. They call this Murphy's Law—if it can't happen, it will. So you've got ___________ as possible.

People are getting away from this now, after all these years.

TROPP:

Well, you got away from that on the Mark III.

AIKEN:

Oh, A. I never built a single instruction machine, B. I never built a binary machine _________________ arithmetic I regarded binary arithmetic as being a simplified concession to the design of the machine. And I couldn't see why in the world you wanted to simplify the design of the machine and then complicate the user's problem for the next ten years. That never made any sense to me. I have no idea what part of IBM's total has been derived from converting numbers into decimal system from the binary system and back again. (Laughter.) I have no idea what part of their total volume was derived from this. It's a very interesting question, because you see, you always put _____________ information into the ____________ in decimal as a matter of form. You want to find out what a man's paycheck is, he worked so many hours at such and such a rate and you've got to multiply these. And both these data are given in decimal forms. You put both of them in and translate them to the binary number system, multiply them to get a product, translate it, and put it back in.

Now, IBM now has a simple decimal form, decimal arithmetic for things like this _______________, but there they did it in their original machines.

COHEN:

But you, in a sense, had a form in the revised Mark I of stored program, already.

AIKEN:

Yes, that's right.

COHEN:

I mean it wasn't in the shape that von Neumann did it, but it certainly was a stored program, there's no doubt about it, because I remember that tables of sines and cosines for example, and so forth. I mean, they were there.

AIKEN:

Oh, we had stored program. You see, this thing here was dedicated to instructions, telling the machine what to do. These things, these three, were called interpolators, because you could put in those punched tables of superior functions, and the machine would search in those for some value of the function, and then read out n values of the function on either side and perform an interpolation to evaluate in here, the value of that function. These things could also be used for instructions.

Then, these sub-sequences back here where there are 22 steps put this thing in the position of being able to direct the overall program, call these in for iterations or these, so that you could run with small loops inside of big loops and all the things which von

Neumann made a considerable point of. Now, of course, this is by no means as elegant as what you can do when you are given superior storage devices which we didn't have.

COHEN:

And these are fixed, whereas the von Neumann ones, I suppose, you can operate on and alter them as you go along. But still, you don't expect the first one to do everything.

AIKEN:

That's right. And there's another thing that maybe should be said here. We never altered instructions in any of the machine that we built. I always believed that one should design these machines so that they looked like mathematics insofar as possible. The multiple subscript techniques for writing and expressions and manipulating expressions represented our techniques and were for the basis of machines. And the business of going ahead and getting the instructions, that is take the number out of such and such a register and then changing that instruction by adding something to it to get another register, we didn't do.

Our approach was to have a subsequent register that you called the I Register to write the instruction, pick the number out of the Ith Register, and the number you got depended on what the value of I was, in the I Register, you see. So that using the programming machines which we built for Mark III and Mark IV, you could program pretty much in terms of elementary algebra, and I always believed that programming should be a branch of mathematics and that one should design machines essentially for copying expressions under the keys taken right out of algebra.

Now, I spent a great deal of time too, in trying to devise methods for determining programs from given mathematical expressions. You see, after all, all a program is, is another algorithm for computing. That's exactly what the algebraic expression is. So there should be some way of taking an algebraic expression and deriving a program from it

TROPP:

I got the impression when I looked at Dick Block's early programs and Bob Campbell's early work, that you had designed the machine so that if I solved the problem in a logical fashion, it was immediately transferable to the machine procedure. It was the way my brain operated in the mathematical context.

AIKEN:

Yes, that's right. Programming, you see, as it has grown up, has something ________ about it. You don't use mathematical symbols in programs. You know why? Because a certain IBM keypunch had a given set of symbols and you had to use those symbols because there were no others. To me, this is disgusting. I say, "Tear the damn punch apart and rebuild it. Make it the way we want it." (Laughter.)

End of Side One, Tape One TROPP:

I wanted to talk a little bit about some of the people that you were involved with, because like Professor Cohen, I think that these conferences like the two at Harvard, particularly and you lectured at the one at Moore School in 1946 also, didn't you?

AIKEN:

Yes.

TROPP:

And I think those three conferences are very important in terms of spreading the word, letting people know what was going on. I just wondered if you cared to comment on some of the people that you were involved with in that period. You mentioned Norbert Wiener and …

AIKEN:

Well, first of all, let me say that I guess I ran those conferences on a single handed basis, which as I look back on, at that time, in the development of this subject was a very good idea, but one which I don't think I would approve of today. I made up my mind who was going to be on the program, and I called him up on the telephone and told him he was going to be on the program and what he was going to speak about. Now I didn't put words in his mouth, but I told him the subject about which I wanted him to talk. As far as I was concerned, dealing with that subject was important in meeting the needs of that program. I did this because I knew we had colleagues that had something positive and that these symposiums should be so organized that we brought out all of these matters. I felt that this was essential to getting us ahead at the time.

Now I'm sure that if you tried to run a scientific conference in general on that basis, there would be a lot of people who would tell you to go to hell, and probably some people should have told me, although nobody did. I was refused by only one speaker in all the time I invited to these conferences, and that speaker first accepted and I printed the program up, and then he called to give his refusal. He dramatically announced in this article that he wrote that he wouldn't have anything to do with warmongers. This was still under the Navy and we were operating with a Navy contract. So he wrote some articles on it.

COHEN:

But he came as I remember, but he wouldn't speak.

AIKEN:

That's right, he came, and he wouldn't speak and he sat in the front row. Well, needless to say, this provoked me ____________________. Norbert was a rather overbearing individual in many ways. You remember E.B. Wilson? E.B. Wilson was way ahead of us in assessing the situation and he'd say, "Well, he's a bore, isn't he?" (Laughter.) So he would be very apt to tend to monopolize a situation and always get off on the tangent of machines that think. And at this point, that was an absolute block. I do not believe that machines can think, and there was no way for me to communicate with Norbert anymore. I tried to tell him what I thought about it, and if we tried to adopt the notions of thinking as he stated them, then maybe elevators could think. Maybe a very large part of the electrically controlled equipments of the world were all thinking machines. And the reason that he focused his attention on a computer was simply that it thought more and perhaps more dramatically.

COHEN:

Well, he was pushed by McCulloch and Pitts to some extent, and by god, once you get contact with Pitts, you're finished. Well, maybe we should erase that too. I don't really want to have that recorded. But it's true.

TROPP:

You know, there are all kinds of things, like, for example, it was pointed out to me in Isaac Asimov's new revised edition of "The Encyclopedia of Science" or something like that, the only person connected with the invention of the computer is Norbert Wiener. I can't find any evidence anyplace that Norbert Wiener had anything to do with the development of computers.

AIKEN:

Have you seen this thing here?

TROPP:

No, what is it?

AIKEN:

"Prospectives on the Computer Revolution."

TROPP:

No, I haven't.

AIKEN:

Well this man … this is a collection of papers, and these people found my prospectus for Mark I that Tony Attlinger had, so that's the reason that I'm in here. The editor who picked Norbert Wiener to write on points out that maybe Wiener was one of the few of the distinguished mathematicians who lived long enough to become very distinguished, and then to become undistinguished because people found out that many of the things that he had advocated were fallacies. That view was expressed in here.

COHEN:

Well, in his book called (and this I don't mind keeping on the tapes) I think it was "The Human Use of Human Beings" or something like that, there's a long preface. And in the preface, he says that in the thirties, late thirties, "I was advocating and discussing the following. And then he lists everything that was done in the world of computers up to about 1950. Now I asked Julian Bigelow, of whom we ought to say a word in a moment, whom I must say, I was very taken with. I liked him very much, and he worked very closely with Wiener, out of all things, fire control and antiaircraft, because Wiener, despite his later: "I will have nothing to do with the military", like Albert Einstein, when the War came with the Germans, caved in on his principles. In a sense, I don't believe in that kind of principle, but to simplify terms, I think you ought to stick to it or not, but he didn't, you know, and I think regretted it. Both of them did in the same way. But I said to Bigelow, "Did you ever hear during those days, of his expression of these particular ideas." And he said, "No, but afterwards, I heard him make that statement on many occasions."

It's a very interesting list. It includes everything from the stored program, you know, to binary and so forth.

TROPP:

Of course, Vannevar Bush says in "Pieces of the Action", that if you go back and read his early expository articles, you can credit him with foreseeing everything that happened. It just isn't so. I mean, these were just what they were. They were pipedreams written for general consumption exposition. And it turned out later that many of the suggested directions of twenty years from now did turn out to be true. He said it would be false to credit him with having conceived of these as ideas.

AIKEN:

One thing that has turned out not to be true is the thinking machine.

TROPP:

Wiener, I know, was involved with Harold Hazen in this same period and Hazen remembers driving him to meetings during the wartime years, and also can't remember discussions related to these ideas.

AIKEN:

There is a beautiful apocryphal story. You went to either Wiener or George Burkhoff and said, "George (or Norbert), who do you think is the second greatest mathematician in the world?" (Laughter.)

COHEN:

At The Math Colloquium, you might be interested in this, Wiener not only slept and snored very loudly during the talk, but then when it was over, got up and began the discussion, and always, no matter what anyone had done, the roots of that were in his Ph.D. thesis. He would even sometimes tell you the page. (Laughter.)

AIKEN:

"Young man, what is the result that you have reported to this colloquium, and how are they associated with my such and such paper of 1906?" (Laughter)

COHEN:

Still, in talking to some people in the computer field, they seem to have the idea that whatever cybernetics meant and it isn't entirely clear what it meant, that somehow or other, the thought. It got people thinking about control and automatic control, and they therefore alleged that this was an important ingredient in the environment of general ideas. There is nothing specific as he alleges, and you know, that, as an historian I can tell you, is the hardest thing to evaluate. Just the general thinking of possibilities and one can't really prove or disprove it.

TROPP:

Well, Stibitz, for example, thinks that one of Norbert Wiener's contributions is prediction theory, even though it had no positive impact on any work that he knows of or was connected with, rather. He still thinks that's a very important work and an important milestone in terms of what it stimulated. I think that is what is difficult in evaluating ideas.

AIKEN:

Well, I think you've got to look at many of the things that Norbert said in the same way that you look at Jules Verne. Who is to say that Jules Verne didn't stimulate a lot of people to invention? I would hate to say so. I think you have to say the same thing about

Norbert Wiener, because he said things in a very startling way. He didn't hesitate to hit the table and say, "This is the way it's going to be", and he was the final authority, you know. ______________________________. And I don't know how this could have not stimulated some people to think, but just as Jules Verne did. Oh, how Norbert would puff up if he ever had heard me say that. He would be so indignant. The word pressure would be become so great that …

TROPP:

What contact did you have in the early running days of Mark I say, with Bell Labs, with the Moore School, with other projects that were going on about the time Mark I had become operative.

AIKEN:

All right, Mark I became operative, and the first contact we had with any other project was Jay Forrester. Forrester came to the Laboratory with several people and he had a contract to build a large analog machine for the Navy.

TROPP:

A flight simulator.

AIKEN:

Yes. And they took rather more of my time in meeting after meeting than I wanted to give. But they spent a lot of time, so they never built the analog machine and they switched over to my digital computer business. So at least we did that much for them. So that was the first contact.

Then, to the Moore School lecture and see what they were doing. Bell— I'd been in and out of there for a long time, and that came about years later as these volumes of reports up here, which were written on switching theory by my students and by me for Bell Laboratories. They supported us to the extent of about $100,000 a year, and I believe that money is still going into the laboratory. I think it still is. It went a long way towards establishing switching theory as something that everyone learns a little of.

TROPP:

Philosophically, the early relay machines that Stibitz and Williams and Andrews built, and Mark I, were strong in many of the same things that you were concerned with— reliability, checking, and accuracy. They had so much in common that I wondered what kinds of contacts there were between you.

AIKEN:

No, it wasn't a matter of contact. They knew from running the telephone system that you have to be reliable or it would be no good.

TROPP:

They put in all the checking codes.

AIKEN:

Well, that's an interesting comment on that. They did their checking by checking the equipment. I always did my checking by checking the numbers, and I argued that "Why check the equipment? If I get the wrong answer, I'm in bad shape, but if I get the right answer it would mean that the machine is making a mistake, so what? Let the machine go wrong, just so I get the right answer. And when I check the results, I check the precision of the results themselves, the program, the machine and the whole works. So why throw all this trash in the black box to check itself? If it's possible to put so much equipment in for checking purposes, then the checking equipment gets into trouble too.

TROPP:

Of course that's why there was so little checking equipment on the early electronic machines. It would have just been outlandish what they would have had to have put in.

AIKEN:

Yes. So, since there is always an identity or some kind of algorithm in the mathematics, such that when you compute the number x, you can subject the number xx x to mathematical scrutiny to show that it's right. For example, early trigonometric tables would be computed for our own use. We wanted sine x, we computed cosine x too, and we squared them and added them to make sure that they were equal to one. I can remember being told one time that, "my goodness, how does your memory come in back there?" I can remember being told one time that our method of operation was like a Japanese tunnel builder. A Japanese tunnel builder starts building from each end, the same way we do in this country, but he misses. And when he gets all done, he says, "Well, at least, we get two tunnels." (Laughter.) I can remember being told that when I used the example of sine x and cosine x and the idea of showing that they were equal to 1 for checking purposes. Well, all right, we've got two functions.

COHEN:

That's very good. I'm glad to hear that.

TROPP:

I hadn't heard that.

COHEN:

No. Of course, as I remember, the method as it seems to me, that it's a great one, because you do it. And I remember reading somewhere that one of the interesting things you always insisted on was, that even if the results were just being tabulated for your own sake for storage on the tape or cards, you always had the typewriter going too, so that you could see the results so you could see the results and be sure that they were adding up to one or whatever the operation was.

AIKEN:

That's right. We had a check counter, and if you subtracted two numbers in that check counter, and the absolute value of the difference was greater than the pre-assigned counters, the machine stopped.

COHEN:

Was it stopped or did a bell ring?

AIKEN:

It stopped right then, because we didn't want to make mistakes beyond that point. We found that this check procedure, quite aside from its immense value for sheer accuracy, was a tremendous value in another way. In the course of the computation— addition and so forth between fifty and one hundred instructions no more - and then perform a check on the last values computed in that sequence, and if you passed that check, go on to the next step and go on. And when the machine stopped, you had only to look at the instruction number at which the machine stopped and then look at the operating instructions _____________program, and see what equipment was used in that last fifty or sixty instructions.

If the multiplier wasn't used, it made a mistake, ________________ ________________the circuits of the multiplier. You knew that the certain multiplier didn't cause the error. So it became an error finding aid as well as insuring the results.

You see, Mark I could run sometimes 12 hours and not make a mistake. That's pretty good. And sometimes, we'd run 12 hours and make a mistake and go back and repeat the calculation and then go right on and compute sporadically. In fact, most of the errors were of that nature, which we could deal with very well.____.

TROPP:

Somebody told me the story occasionally of using Grace Hopper's mirror to check to see where the sparks were occurring.

AIKEN:

Yes. The cams were somewhat difficult to see, and when cams were in trouble, Grace would come out and pull her mirror out of her pocketbook and stick them in front of the cams and look for sparks. Grace was a good man.

Incidentally, have either or both of you run into my secretary, Jackie Sill, her name is now?

COHEN:

Yes. Sill … in Gloucester.

AIKEN:

Yes. She can tell you more about the people…

COHEN:

Well, she's on our list.

AIKEN:

She can tell you more about the interactions between people than anyone. In many ways, Jackie ran the project.

COHEN:

Were people like Dick Block and Grace Hopper just sent to you, or did you pick them?

AIKEN:

I picked them. Let me tell you about Dick. I went to the Naval Research Laboratory, Bryce and me, to get the backing of Admiral Ventura for some calculations which we wanted to perform but _____________________________. And _________ said "yes", <u>it sounded reasonable to him</u>, but he'd get one or two of his young men to look at things. And the man he called was Dick Bloch. So I expressed my full appreciation for what Ventura had done for us by recruiting Bloch and taking him away. (Laughter.)

TROPP:

Where did you find Bob Campbell? You know, I asked Bob, and he couldn't remember.

AIKEN:

Bob was a graduate student at Harvard, and when I went into the Navy, we needed somebody to keep his finger on this program and Chaffee asked me if I knew Bob Campbell, and I said I had, and he said that he thought that he might be interested, and so we had him. Bob Campbell is now at Mitus Corporation now, isn't he?

TROPP:

I should remind myself at this point to erase some indiscrete comments about Dr. Hickman and Norbert Wiener that have been discussed earlier.

COHEN:

One of the topics that we discussed at lunch which seems to me to be of special importance, as was indeed one thing we talked about before lunch, which didn't get on the machine, is the really important influence that Howard Aiken had outside of the United States. I would record, just for the record, the translation into Russian, Russian correspondents, students who have come from Sweden, France, Belgium, visitors from England, students from Germany, and Italy. Then, we also wanted to explore just a little bit further, the contact made by lectures that were given, for example, at the Moore School in 1946, and visits from Americans in other groups.

TROPP:

Right. And along with this, I think the topic that I was concerned with, and that's the impact of your visits to Europe in terms of how it affected the early machine developments, particularly in Western Europe and even in England.

AIKEN:

Well, I don't know whether you're interested in this thing or not. _________________. That is a pretty good summary of the effect of my operations in Europe. Wherever you see an award or anything like that, it was because of something I did for that country.

TROPP:

I should say for the record, that we're looking at essentially the second and third pages of Dr. Aiken's curriculum vita. The item is entitled "Honorary Degrees and Awards", essentially. There are degrees from the Technische Hochschule in Darmstadt and continuing on for another page and a half through awards in Belgium, France, Spain, Sweden; membership in societies in Germany and Sweden; a Fellow of the Royal Society of Letters and Sciences in Gittburg.

But the thing that was interesting during lunch, is that you were very specific in mentioning some of the individuals who worked with you and some of the lectures that you gave in Europe, and some of the visits and I think perhaps, even some of the philosophy that you were trying to expound on those visits.

AIKEN:

All right, let's see if I can recapture it all. In Sweden, we had two associates named Schoran Shobert and Gustove Nayobious, that were sent to us by Sweden to study computers. And after a couple of years with us, they went back home and built the first Belgian computer.

Then, coming south Professor Balther, from Technischule Hochschule in Darmstadt had spent a great deal of time in our laboratory, and on going home, pretty much took the responsibility for getting research and development of digital computing machinery going in Germany. The result of that was I visited Darmstadt on a good many occasions and lectured there. The first time I lectured there was shortly after the war, and Darmstadt, as you may know, was the reprisal target for Coventry. And there weren't two bricks in the Technischule Hochschule still standing together, and the room in which I lectured had no windows and the roof leaked. It was raining all over the whole place. At one end of the blackboard, I couldn't go there because the water was pouring down. I could only use one piece of the blackboard. And the lecture room was completely full of people sitting on the floor because there were no seats, and some of them in puddles and the doors were locked. And I couldn't help but think that if I lectured at Harvard under the same circumstances, I'd have to bring a tape recorder if I expected anybody to hear what I said. That's the way it was in Europe during the war.

All right then, further to the south in Belgium, we had Willie Poulliar and Marcel Lindsmund. Lindsmund came from one of the schools in Belgium, but I can't remember which one. Both of these men were picked by Louis Henri, who was director of Earsia. Poulliard was an engineer with Bell Telephone Laboratories in Antwerp. Bell Telephone Laboratories in Antwerp had absolutely nothing to do with our Bell Telephone Laboratories. And these men were sent by Earsia to spend a year with us and go home and found a computer industry, which they did.

They were followed by another man, and Professor Mananbeck and Mananbeck was accompanied by Lito… I can't remember his name now… Bito Bellovich. Bito Bellowvich went home and became the first director of the National Computation Center of Belgium.

From France we had many visits from Louis Couvignon. Couvignon was a great pioneer in France at Institute Blaise Pascal. We also had a young man whose name was Philippe something, and I'll try and remember it in a minute. He went home and became a designer of electronic computing devices for the __________ Company. He was essentially the number one computer man France, after Louis Couvignon.

We had Professor Jose Santasmassas from Spain— Jose Garcia Santasmassas. He is Professor of Computation Science at the University of Madrid and director of the computing center there. I made many trips to Spain at his request. A, to help him get started, to lecture, and to attend conferences there.

There was a young man from Italy— Michael Conneva, who was sent us by Andreano Olivetti, from the Olivetti Company in Piraea. Michael stayed with us for several years, and went home to Olivetti. Then, when the Olivetti Company bought the Underwood Typewriter Company, he got shipped back, and I guess he's a permanent fixture of this country now. That's all I'm able to think of at the moment in the way of these people.

TROPP:

Well, it might be interesting, while you've thought of the people in the countries, to recount in terms of what you were doing at Harvard, the impact of that on what they did when they returned to their countries, because about that time, you had either Mark II was either in being or was in design, Mark III was also…

AIKEN:

Mark III was being built, and some of these people overlapped Mark IV. They all came to us under sponsorship of some academy or school or from their government. They came to our laboratory to get packed up in the field of computers.

COHEN:

What about England?

AIKEN:

We had a constant stream of people from England through the place. They never sent a representative to the laboratory. As I've already commented, in dealing with people from England, there were always two sides: A, an appreciation of the work with us, and B, I guess a resentment that it hadn't been done at home. We always experienced this.

I don't know what more you want me to say. Universally, these people made careers out of…

TROPP:

I guess what I'm interested in is that this is a period of time when there's a lot of philosophical argument in the United States in particular and also in England about how machines should be built.

AIKEN:

Well, either because of their association with us or alternately, because they had looked at these debates and decided which side they wanted to belong to, and hence, came to us. At any rate, we were all pretty much in agreement on how a machine should be built and what side of the philosophical fence we sat on and we didn't have any internal dissent.

TROPP:

I got this feeling as I looked at Western Europe, that the early machines reflected what was going on at Harvard, more than they did what was going on in England and other parts of the United States.

AIKEN:

There is in question about it. Absolutely.

TROPP:

And even in England, there were at least one or two instances, the National Physical Laboratory is one of them where the philosophy is in the Harvard mode. This argument continued, I guess, until the transistor when everything seemed to fall apart for a while, and now the same questions are beginning to arise again.

AIKEN:

They are, and the decisions now are beginning to be more along the lines of the machines that we built at Harvard.

TROPP:

Well, I think that this one point that you made this morning, and I think Mark III is an example of it, a separation of instruction and program, even though you realize you can treat them both the same, that their characteristics are identical and the machine itself can't tell one from the other. This decision on your part to have the program one place and the data someplace else.

AIKEN:

We decided to make the machines look like they were an algebraic instrument. Look at the thing that Hewitt-Packard built. That's exactly what it's doing.

TROPP:

Well, it's also interesting in that if you look at the West Coast environment that's developing in the late forties and early fifties at the same time, they too characterized an algebraic as opposed to a kind of a geometric concept or conceptual way of looking at the machine, the work in the aircraft industries primarily, and this is, again, an algebraic orientation.

COHEN:

But the aircraft industries, now that you've mentioned them, are really extremely important, because there’s no doubt that set up, I suppose, the first large consumer demand for computers, other than in academic settings. Isn't it more or less?

TROPP:

I think it's that the needs of that particular industry as well as things like Atomic Energy and other groups, that if you really want to look at it, got IBM into the business. It was servicing that need, first with the CPC, then …

COHEN:

Yes. Atomic Energy as a single entity and you take all the designs of the aircraft…

TROPP:

Atomic Energy needs, though.

COHEN:

… aircraft engines, and you take all the multiple demands that were being made on design, as I understand it, they lived or died according to their computer facilities, and this may have also directed a certain need. Now there were certain people, though.

AIKEN:

Louis Ridenhauer.

COHEN:

Yes. Whose a forgotten figure, you know, but shouldn't be.

TROPP:

Not according to what I've looked at, he's not forgotten.

COHEN:

No, no. He shouldn't be. As I told you, when I was reading this book of Hartree, it was he who got him to come over and to try to give a survey and he particularly picked Hartree because the picture was so complicated with all the competing Americans.

Now, what about Iverson? He was a partial product of yours at any event.

AIKEN:

He was one of my Ph.D. students.

COHEN:

He was a Ph.D. student, wasn't he?

AIKEN:

Yes. Here's his thesis.

COHEN:

He overlapped with Brooks, did he? Or did they meet up afterwards?

AIKEN:

Yes. Iverson and Brooks were there at the same time, and Iverson was interested primarily in programming languages even at this time.

COHEN:

Was he really?

AIKEN:

Yes. Really, this thesis is the beginning of the book on computer languages, which Iverson wrote, and which was the basis of his reputation. As you know, he is a Fellow at IBM, which means that he draws a salary and has no responsibilities other than to produce what he chooses to work on.

TROPP:

One of the people that you mentioned at lunch that I would like to get on this tape is Blau.

AIKEN:

Oh, I forgot all about it. In going through the countries, I overlooked the Netherlands.

TROPP:

You mentioned Vanderpoel.

AIKEN:

Vanderpoel and what was his name at the Mathmatishce Sintrum in Amsterdam? It's a man whom we all know. We traveled back and forth to the Netherlands with him. Oh, what was his name. I've forgotten.

Ridenhauer, by the way, closed the second symposium.

COHEN:

Have you talked to Iverson?

TROPP:

No, I haven't.

COHEN:

He always interested me for a very special reason. Not only because of his great success and his work with computer languages, but you know, the problems that he were dealing with here were not just conceived abstractly, but came out of the research that Leontief was doing in input/output economics. Now, this was made into a book by the Harvard University Press later on, but I had never seen the thesis form, and I see that he acknowledges that in the beginning.

TROPP:

Here's the use of the Mark IV Calculator in the solution of a new dynamic, economic models, continues the previous work of Dr. H.F. Nitchell

AIKEN:

Yes. I've always liked his inscription.

COHEN:

Yes. Oh, yes, I do too. I got to remember that.

AIKEN:

Ben Weingarten, that's the name of the man from the Netherlands.

TROPP:

Ben Weingarten, that's right.

AIKEN:

At the mathematical center in Amsterdam. They had one of these high-speed mental calculator type individuals, and like stage people, he had a professional name and it was Pascal. (Laughter.) And on one trip to Amsterdam, this man was brought to me to demonstrate his skills, and the offer was made to take any two five digit integer numbers and write these one below the other and draw a line as if for long multiplication, and by the time the line is drawn, Pascal would dictate the digits ______________.

So I took a piece of chalk and wrote and waited until it happened. And I turned around to look at Pascal and he had been standing up and he sat down. He took his handkerchief and started to mop his forehead and people said, "Well, what's the matter?" "Why", he says, "both of those are prime." (Laughter)

TROPP:

They thought he was marvelous because he knew how to multiply.

COHEN:

__________ recalls in his memoirs that when he was first putting out his compometer, that many companies had these calculating wizards and the test of his machine was whether he could not surpass them but do as well.

AIKEN:

Do you know that you've reminded me of a tale that without that story, I would never have remembered? When Mark II was finished, that machine was built essentially to make firing tables for Navy big guns. And there was a feisty admiral who came up to look at it and said, "How do I know if the results are ready yet." So we checked and ________________________________. So we were getting desperate. It was time to ship this confounded thing, and we wanted to get rid of it and get on with our work for Mark III. So, finally I said, "You give us any range table for any gun with the make, together with the initial condition, and we'll _________ for you. And he agreed that that would be a test of the _______________. And by that offer, I got myself in very, very serious trouble, because we couldn't find range table ______________________. (Laughter.) They would all start off and then they'd begin to deviate, because of the accumulated error.

COHEN:

Each time you put it in another term.

AIKEN:

Oh, I had a hard time getting it in. (Laughter)

COHEN:

And you couldn't explain that to him either. Isn't that marvelous.

AIKEN:

Albert Worthheimer finally got us out of trouble. He finally came to the rescue and straightened it out. I don't know what we'd have done without Albert.

COHEN:

He' still living.

AIKEN:

Albert is? No, he's dead.

COHEN:

Now who is the man I met? I gave a seminar at MIT, and I met a very nice man with a name that sounded very similar to that. He must be about your age, who said he was an early admirer of yours and had been instrumental in helping getting you, when he was in some naval position, out of the Bureau of Ships, and back to Cambridge. He lives on the North Shore somewhere.

AIKEN:

Oh, yes. You're talking of … what was his name? You're talking about the naval officer who got me into the Naval Reserves. Did he tell you that he did that too?

COHEN:

No. But I wouldn't be surprised from the way he was talking.

AIKEN:

Yes. You see, at Croft Laboratory, we had three or four naval officers every year. This man whose name will come, said to me, "You know, you ought to be in the Naval Reserve". And I said, "Why?" "Well", he said, "if there's never a war, nobody will ever bother you. And if there is, it's much better to go in as an officer in the Navy than to get drafted as a private." And I thought that was pretty good sense, so I went down and applied for a commission in the Naval Reserve on his recommendation.

I was asked what rank I wanted to apply for and I said that I didn't know. I said, "What is the lowest commissioned rank in the Navy?" And they said, "Well, it's an Ensign." And I said, "Okay, I guess maybe I better apply for that." The recruiting officer, who was so amused by this whole thing, said, "No, why look at your age. We'll make you a Lieutenant Commander." (Laughter.) And I'm sure that if I had said, "Well, I think I ought to be a Lieutenant Commander", he'd have said, "Well, we'll make you a Lieutenant J.G.," or something. But by not having any demands, I did better.

Now, the man you are talking about, became Commanding Officer of the Naval Research Laboratory, which was located over in the Biology Building.

COHEN:

That is right.

AIKEN:

And when Mark I was finished, I was School Officer of the Naval __________ Warfare School, and this man who was a retired Naval Commander… what is his name? He called up Yorktown ____________ School, and said, "Why don't you come up here and run this computer?" I said, "Well, I've got orders." And he said, "Well, I'll get that corrected immediately." And he did within just a matter of hours. I had orders to pack up and go back to Harvard and become the officer in charge of the United States Naval Computing Project. I guess I'm the only man in the world who was ever Commanding Officer of a computer. (Laughter.)

COHEN:

I was thinking that when you came up, we ought to see him, along with Mrs. Sill and so forth, and fill in the rest of all that background.

AIKEN:

What is that man's name? I'm indebted to him. It's embarrassing that I can't think of it.

COHEN:

Well, I can find it, no problem.

TROPP:

I guess I'm interested in the chronology of Marks II and III, because it was 1944 when Mark I was commissioned, it was 1944 when you were reassigned from Yorktown to be in charge of the computer, and I kind of get the feeling that Mark II and III were almost planned simultaneously, and Bob Campbell has trouble separating this development.

AIKEN:

No, Mark II … It's a wonderful thing to write books, isn't it? You don't have to remember so many things.

TROPP:

Right.

COHEN:

But if you used the test that you used for those firing tables, you don't know which one you're testing.

AIKEN:

"Description of a Relay Calculator! Mark II was built for the Naval Proving Ground at Dahlgren on the recommendation of Professor Clinton Bramble of the United States Naval Academy, who was a mathematician and who was on duty as a Naval Officer. And Bramble was able to foresee that they had to quit this hand stuff in the making of range tables. That's why we built the computer. And Albert Worthheimer found the money for it and signed the contract.

"In November of 1944, the Bureau of Ordnance requested the Computation Laboratory, then operating as a naval activity, undertake the design and construction of an automatic digital calculator for installation at the Naval Proving Ground."

TROPP:

That's the beginning date.

AIKEN:

Yes, and that 1944 was probably around September or October, so that we had just about gotten going and learned how to run a computing machine, when we were given the added responsibility of a second one. We started the second one down in the basement of the Research Laboratory of Physics, so that Grace Hopper, when I came in one morning, she'd been there most of the night struggling with Mark I, and I said, "What have you been doing here all night?" And she said, "Chaperoning these two damn computers." (Laughter.) Typical of Hopper, eh?

TROPP:

Yes. One of her comments on the early days was that she was the only one she knew who had to program a brand new machine every day, every time she would come up in the morning.

AIKEN:

Now let's see. Marks III: "Description of a Magnetic Drum Calculator." This one is dated January 1948, prior to the completion of Mark II.

TROPP:

Oh, I see.

AIKEN:

"The Bureau of Ordnance requested the staff to undertake an investigation of automatic digital computing machinery with particular reference to electronic components." And work on the magnetic drum calculator, Mark III, was started in the summer of 1948, four years after ______________.

TROPP:

That answers my question.

COHEN:

But they did overlap.

TROPP:

There was an overlap only in the construction and conceptual periods, because it seemed like very difficult to have done those as close in time as my poor records had indicated. Bob also mentioned how you had decided on the specifications for a special relay that you hoped would be the only kind of relay you would have to use in Mark II. He said that you ended up having to use two or three.

AIKEN:

Yes, there were two of them. We designed the relays, and they were built by the Autocall Company.

TROPP:

Right. This was the latch relay.

AIKEN:

It was the latch relay for storage in the.

TROPP:

I'm sure that took a while to get the specifications and to finally get a company that would build them.

AIKEN:

That's right. And then, Mark IV was the last machine, and I can't remember when it was but it was around 1950. The second symposium, I guess, the one on switching theory. ____________________________ Symposium.

TROPP:

I'm really glad about the chronology of that, because you know, one of the myths…

AIKEN:

There's only one more point to be made, the second symposium. It was September, 1949, the second symposium, and it was at this symposium that I announced that when Mark IV was completed, we would never build another computer, because it was apparent that industry was beginning to build computers. The job was done, and any further effort of mine would put us in a position of competing with industry, and good god, they could do the job better than we could at that time. So we quit.

TROPP:

X The dates of conception and design of Mark III is interesting because part of the mythology in the literature is that you resisted going into the electronic aspect, and it's clear that in 1944, there were no electronic machines operative when you were building Mark II. When you were talking about building Mark III, this is the beginning of when everybody else was talking about building. So there really wasn't even any time delay.

AIKEN:

There was no resistance of any kind.

TROPP:

I say, that does burst one of the myths.

AIKEN:

No. No, there was no resistance.

COHEN:

But this was still vacuum tube.

AIKEN:

Mark III was. You see, I never had anything to do with the transistor.

COHEN:

Did you know John Bardeen? He was a junior fellow. You overlapped in time.

AIKEN:

Yes. I didn't know him well, but I knew him.

COHEN:

Did he ever come around?

AIKEN:

No. I don't think that Bardeen foresaw what his product was going to be used for anymore than I did.

COHEN:

Or anyone else for that matter.

TROPP:

I don't think anyone else did at that time. They were expensive, totally unreliable, relatively …

COHEN:

Walt Bracken told me once that there was a group at Purdue under Lock Horwitz…

TROPP:

Right. I was there during that period.

COHEN:

…who were working on this, and they thought it was practical, more practical than he did, and that it was a great race. He told me of a terrible moment and I'm sorry I didn't have a tape, but a meeting of the American Physical Society, when Lock Horwitz told them about the difficulties they were having at Purdue and said to Bratten, "How are you fellows getting on?" And they were filling the patent specifications, and he didn't want to lie and he couldn't tell them that, because the patent hadn't been filed yet, and he didn't want them to go back and pour on the steam. He said it was the most embarrassing experience of his life.

TROPP:

I remember that when I was there. I discovered after the fact that a good friend of mine who got a Ph.D. in physics told me a year or two afterwards that he was part of that group and that he had been working on the transistor at Purdue under Lock Horwitz, of which their work was later by some time period.

COHEN:

Well, it happens and so forth, and it doesn't have to be much later.

TROPP:

But I do remember that there was a pilot project at the University of Toronto in the 1949 period — UTEC—in which they were trying to build a parallel type machine, and it was strictly a pilot machine and they bought a few transistors. I remember Calley Gotlieb talking about fifteen dollars a piece, totally unreliable, they kept them in the safe and nobody knew what they could do. They just had them as curiosities and that's really all they were in that early period.

AIKEN:

The first transistors we purchased in the computer laboratory were purchased in France.____, and they were yea big, and they looked like little plastic things with three wires sticking out of them. That happens to be exactly what they were. They sold those damn things in a number of plastic molds with three wires sticking out of the thing; they had no circuit capability whatsoever at all. And like everybody else, we didn't find that out until we paid for it.

Then I wrote to the United States Chamber of Commerce in Paris to complain, and several other people must have done the same thing because I got a letter back, saying, "Well, these people are no longer in business." But they ripped quite a few people with their technique.

No, as I said when we began this discussion, I never had any pre-conceived notion about what kind of components you should use to build a computer machine— mechanic, relays, electro, vacuum tube, or what. The thing to do was to get the machine built and running so that it would make numbers.

COHEN:

You mentioned the three possibilities of Monroe mechanical, IBM relay or electromechanical, RCA (perhaps) electronic, of course, I think it was just your good fortune that Monroe wasn't interested, because I think mechanically, you would have been finished, perhaps, or I mean, limited, terribly limited. Have you ever thought about that?

AIKEN:

I'm sure that tabulating machine parts came closer to what I wanted to build a machine than Monroe's machines did. The best thing that could've happened is that if somebody like Automatic Electric had said, "Well, here's a Carlin reflatch." That's the best thing that could've happened, but of course, nobody said that.

COHEN:

But I did know this, and looking back through some of the records, that it was thought at one stage that IBM would supply money and parts and you would do the construction.

AIKEN:

That's right.

COHEN:

But that was dropped.

AIKEN:

Yes, that was dropped. Watson wanted to keep all his marbles at home.

TROPP:

As part of the Mark I project, he also gave Harvard some money to keep it going, didn't he, once it was installed?

AIKEN:

About a $100,000 gift that came with the machine.

TROPP:

Did Mark I ever do any problems for IBM? Were any computations?

AIKEN:

No.

TROPP:

There really were no strings attached to that gift then. There was no computational time.

AIKEN:

IBM had no computation requests.

TROPP:

The hours were split then, primarily for the Navy project and for Harvard.

AIKEN:

Yes, for quite a few years, it was the Navy.

TROPP:

Was it 18 hours for the Navy or 24 hours?

AIKEN:

24 hours for the Navy until after the war. Then there came a time when there was this split between Harvard and the Navy. Then we got machines built for the Navy so they didn't want Mark I anymore. The Navy Department negotiated with the Atomic Energy Commission to operate Mark I, so the Atomic Energy Commission ran the laboratory operatively for years.

Then the Air Force came in and they wanted Mark IV, and so we rang the whole gambit, from one agency to another.

TROPP:

How did the environment at Harvard and attitude towards computation change once you had a machine that was really performing, was solving problems and doing them with some speed and great reliability.

AIKEN:

Well, then we entered into a phase in which "Howard Aiken had a computing machine and nobody could get near it, nobody in the University could get near it", which, in part, was true. However, since we had paid for it ourselves or raised the money to pay for it, it didn't disturb me very much. I used to answer, "Well, I can't give you ______________, either, can I?" But there began to be some resentment that all of this was going on and the University didn't have any access to it.

Then the next thing that happened is that computers began springing up all over the school. The Medical School was getting computers, the Astronomy Department wanted computers, and the Economics Department wanted a computer.

TROPP:

Yes, but during Mark I, II, III, and IV, there weren't any other computers.

AIKEN:

That's right.

TROPP:

That's all there was then.

AIKEN:

It was Mark IV when the interest on the University as a whole to get hold of a computer took hold.

TROPP:

Did Professor Shapley ever get to do any astronomy calculations on Mark I?

AIKEN:

No, he never did. Well, of course, Shapley wasn't computing at the time. Some of his associates were, but there wasn't too much.

TROPP:

Well, one of the early problems of the lens problem…

COHEN:

That's Jim Baker.

AIKEN:

And that was in Mark IV days.

TROPP:

Was that Mark IV?

AIKEN:

That was Mark IV, Air Force Mark IV.

COHEN:

Jim Baker was designing special telephoto lenses for the Air Force for Reconnaissance.

TROPP:

It was that late, it was Mark IV.

COHEN:

And although he was at the observatory as a junior fellow in astronomy, that was not really observatory.

AIKEN:

No, that was an Air Force contract and it was quite a while, and he absorbed a third to a half of the operative Mark IV before his funds ran out.

COHEN:

Well, I think it fair to say that that was the beginning of lens design by computer, which has since revolutionized the whole of photographic optics and everything else. ______________________ (Interruption)

COHEN:

… or I wouldn't have told it to you. But he was a backwoods boy. But I think it's interesting that you often hear about the great developments of computer designed lenses, and that's where it started.

AIKEN:

That's where it came from.

COHEN:

Right in the comp lab.

COHEN:

One of the lines that I think, while we have our machine going…(Are we tiring you?)

AIKEN:

No.

COHEN:

… that I think I'd like to record because I think it's very very fascinating, are your relations with Comrie, even though this isn't directly in the line of the development of Mark I or II. I'd like to do it for a number of reasons, and as I think I mentioned at lunch, Comrie has always been a hero of mine, and particularly because not only the fact that he had a disability, and as I mentioned at lunch, on Armistice Day, learned about computation and tables from Carl Pearson in person, which is rather a nice thing marking history. But also, because it seems to me that he had a particular genius of making do with what had developed. I should add because I didn't say it at lunch, that one of my hobbies in the history of science is the fact that an enormous amount of the advance of science has depended on a spill-over from industry. We tend to think of industry today as being influenced by the development of science. I remember when I was a student and teaching assistant to Newton Henry Black… By the way, no man ever had the names of more illustrious predecessors than Newton, Henry and Black. But he used to tell us, and he was right, that the thing that characterized science in the days when I was a graduate student and an undergraduate in physics, were breadboards.

Everybody made breadboards. Do you know what a breadboard is? I don't know if it is still used.

TROPP:

Yes, it is still used.

COHEN:

And it was filled with these frightfully inexpensive ammeters which you would put on shocks and series and make into voltmeters and so fort. Well, the reason that these were available and made research so easy was, according to Black, the fact that the Model T Ford in those simple cars, needed a meter to let you know if the battery were charging or discharging. The Wesson Instrument Company primarily, which made its fortune, made these very inexpensive meters which then were available for the sciences.

Now, the capability developed by the National Cash Register posting machines, the Mercedes Euclid, which he used, and he used the Babbage principle as I mentioned, where the output of one machine like in the difference engine, would become the input for a second and you would have three of these mounted one right alongside of the other. In fact, one of his papers deals with the great potentialities for science.

Now, as far as I know, there was practically no one in the scientific world that was aware of what had been developed as commercial business machines, and Comrie had that tremendous insight. So that's by bias, which I just wanted to mention. So I was very much interested in your contact with him. I take it you didn't meet him until after you yourself were established.

AIKEN:

We were in the Battery Room at Cruft when I met Comrie.

COHEN:

Oh, you were? He came to see you?

AIKEN:

He came to see me.

COHEN:

Was that during the war?

AIKEN:

Yes. He was a very quizzical individual. He didn't believe everything that he was told; he had to see it for himself completely. He wanted to see Mark I, and when the first volume of tables from Mark I was published with the modified ____________ functions and these things, Comrie got one of the first copies and he differenced those tables to see if he could find some errors.

COHEN:

He did? Oh, I see. He was looking for second differences and third differences.

AIKEN:

Oh, seventh or eighth differences, because that table was very coarse, due to the fact that it is a complex______________. If it had a fine interval, it would have been a whole library. So he had to go to very high ordered differences, which he did, looking for errors in that book and he wrote a review on it, which I have.

TROPP:

That's the one that's in Nature.

AIKEN:

Is it?

COHEN:

No, the one that's in Nature is of Volume I, on the operating manual. Maybe this is in Nature too.

AIKEN:

Well, I wouldn't be surprised if it was, but I can't tell you where it was. At any rate, he differenced it and reported that he had found no errors, grudgingly almost. Well, that was the beginning of my association with him, and, as I said, when I went to England, I was usually his guest. He made arrangements for me to get into the Kensington Museum when it was essentially closed. We had a very pleasant association, very constructive on the one hand, and on the other hand, this thread of envy that what we were doing hadn't been done in England, and the British Association that published these things, how stupid they were that they had never fostered any developments of this kind. That went on over and over again in our conversations.

COHEN:

In the first review of his of your work, which I read some time ago in Nature, which is the review of the Volume 1 of the Annals, which is called an operating manual, as you know, he says a number of things. The two things that struck me very forcibly were (1) he said, "This has been financed for Harvard University by IBM." And he said, "It's just a pity that there isn't anyone in England as enlightened a company director as there is in America to make such a thing possible for us." Now it's interesting. He didn't say, "It isn't that your government shouldn't do it." It was perfectly obvious that the British government wasn't going to do it, by the way; I mean he didn't have to say it, but there was tremendous envy about that. You could see that this was very very strong in his mind.

AIKEN:

Yes, and this welled up in our relationship over and over again. Now there was another thing that came out of my contact with him. If you go back before the time of Charles Babbage, you come to a conclusion that many distinguished mathematicians and physicists were interested in computation: Pascal, Leibnitz, Kepler…

TROPP:

Gauss.

AIKEN:

Gauss, yes.

TROPP:

Gauss did millions of calculations but he did them all in his head.

AIKEN:

Yes, and let's go on with that. Why? Just pick the distinguished names of that era, and you find a man who was interested in computing machines. Then, we come to Jacquard, whom I think should be recognized. Then, Babbage went across the English Channel and picked up Jacquard's work, and started it off in a computer machine. And at that point, with the end of Babbage's work, there was no scientific interest in computing machinery until the day I started.

TROPP:

I agree with you. I was talking with Bob Multhauf, who is the editor of *ISIS*, who had done a paper on calculating by steam. It was kind of a draft of a paper and both of us pretty much came to the conclusion that there just wasn't any scientific interest in the nineteenth century and early twentieth century.

You begin to have a glimmer during World War I, when Veblen and Moulton and a few people were at the Proving Grounds and starting to have problems with ballistics tables, but the war didn't last long enough.

COHEN:

Do you mean in England or in this country?

TROPP:

No, no, this country.

AIKEN:

Well, Comrie's view was that this dark age in computing machinery that lasted one hundred years was due to the colossal failure of Charles Babbage. When Babbage spent all that money and nothing came of it, academic people simply threw up their hands and said, "well, let's ____." This was the thesis of Comrie.

COHEN:

I don't know that I believe that because of two things: I have a sort of line of the giants in the computing field. I put Pascal first because he made a public demonstration of something which may seem obvious to us, but you had to do it to prove it. Namely, that you could have a machine that could do arithmetical operations and do some kind of computing, and to make it go is tremendous.

Another one of my great hero's next is (Charles de Colmar?) Toma De Coalma, because while his arithmometer wasn't an enormous breakthrough, he showed that you could make a reliable instrument on a commercial basis, and he sold thousands and thousands and thousands of them, insurance companies and everywhere else.

My next one is Otto Steiger, because he made the Millionaire. Now the Millionaire and Bolay had a rivalry for direct multiply and that was widely used. My first computing was done on a Millionaire. I computed lunar pulses with Alexander Pogo. I was reading that when Lowell was convinced that there must be a planet producing perturbations, when he looked into the problem of the orbit. You see, how could Uranus' orbit be what it was. There was Uranus and there was Neptune, and Neptune's orbit you just could account for. The first thing he did was to go out and buy a Millionaire. So there were some small groups. Maybe it wasn't big, and I never found evidence that Lowell used the Millionaire, but that was very good news to go out and buy one.

Then, Jacquard and Babbage come along, you see. Then, as near as I can make out, really nothing much happens on a big scale until you came along and showed that you can really do this thing on a proper all-purpose general scale. But during this era, while you may not find any direct multiplying as you have in the Millionaire or the Bolay series of pegs which he never pushed through, although there was one made for the Belgian railway system which was operative, nevertheless I don't think it's true to say that. I think what may be truer to say is that most of the problems in academic mathematics were solved without the use of computers and there wasn't the pressure on them.

TROPP:

Well, mathematicians, and I'm one of these who is guilty, if I ran across a large system of equations, I said to myself or my students, "I'm not going to live long enough to find the inverse of this array. It's just too big." So I ignored certain classes of problems expect theoretical.

AIKEN:

Let me say in Comrie's defense now and what he said about Babbage failure bringing about a dark age from the point of view of designing and building computing machinery in academics. This was one way in which he justified his position in his bitterness against the fact that this was left for being done in the United States rather than in England.

COHEN:

I think it fair to say that if Babbage had been successful, it would have been different, but to say his failure had an effect would imply that people knew about him. I don't think that anyone had heard of Babbage.

TROPP:

Yes, the mathematical literature had tended to ignore this attempt almost totally.

COHEN:

Yes. Astronomical, too.

TROPP:

Yes. Then, of course the fact that the difference engine which he didn't complete, was completed by Schuetz and his son and operated successfully in both England and this country, indicated that Babbage' ideas were worth looking at, but still nobody looked at them.

COHEN:

Yes. I'm not sure that they operated so successfully in this country. The only one I know of went to the Albany laboratory; I forget what it's called.

TROPP:

The Observatory, but I don't know the name.

COHEN:

Dudley Observatory, yes. But there was no evidence that it was ever used for any problems.

TROPP:

I don't know either.

COHEN:

The director— I forget his name— left soon after it came. Now, there was a man, however, named Grant, an American from Salem who got a patent for a variation on that difference engine. Have you run into him?

TROPP:

Well, I've located the fact that that engine was built; it was on exhibit at the Philadelphia Exposition in 1876. I have a picture of it on the floor…

COHEN:

Yes, this is Grant.

TROPP:

Yes. The Grant Difference Engine. And the picture, you can't really tell where it is. There's no record of it in the catalogue, and it seems to have vanished from the University of Pennsylvania, where it supposedly went. I have absolutely lost track of it from the Centennial celebration in Philadelphia.

COHEN:

Well, at the Crystal Palace Exhibition, both Tomar de Comar who had a large piano-shaped arithmometer… I've seen the one he had on exhibition. It was built just like a big player piano with a keyboard. That shared the prize with Babbage's difference engine.

TROPP:

Not Babbage's; you mean the Scheutz.

COHEN:

Yes. Well, I mean …

TROPP:

Well, Scheutz says it was Babbage's and Babbage says Scheutz…

COHEN:

I know. We just heard about that in terms of G.W. Pierce and Katey, each one saying the other was the inventor. But basically, it was the Babbage idea. Realize that there was a lot of publicity being given and I think you put your finger on it. This is reasoning after the fact and not really cause.

TROPP:

Well, we're getting away from Comrie.

COHEN:

Yes. Well I was thinking of Comrie, because after all, of course what Comrie was up against was the tremendous power of the vested interest of a bureaucracy. He found it when he was Deputy Superintendent of the Nautical Almanac. I don't think he was ever Superintendent, was he?

AIKEN:

Wasn't he? I thought he was.

COHEN:

Wasn't he Deputy Superintendent? Maybe he was Superintendent. But even there, all these women and so on, you know, I mean they didn't want to be replaced or have their jobs altered.

TROPP:

Where do you see the role of Hollerith in terms of say, the 1890 Census and the idea of record keeping and handling masses of data in this development of computation, as you outlined the various stages when you began Where would you plant him?

COHEN:

Well, you see, that's, I think, something which eventually joined the computer data processing. But there really wasn't computing done by Hollerith, it was sorting.

AIKEN:

It was a rather rudimentary device. The holes in the card were used to complete a circuit. You opened a specific door so you could drop the marker in.

TROPP:

Or else they had a particular counter move one notch. It's interesting, his daughters are still alive and I've spoken to them about the origins. And they are absolutely insistent that his inspiration was not the Jacquard loom, but was instead, a railroad conductor punching holes with a ticket. But at the same time, there are members of the family who are in the manufacturing business of using Jacquard looms, so it's hard … again, this is one of those stories that will never be completed.

COHEN:

Well, he might have seen the principle, you know, of the Jacquard loom and had it in his mind, and seen a conductor punching a card, and suddenly in … The process of the genesis of invention is something we don't know very much about.

TROPP:

There is a rather tenuous link then, between Jacquard, whose is clearly a giant in this chain, and Hollerith, whose ideas eventually come back into this.

COHEN:

But if you look at a Jacquard loom, as I say, I don't know the model you've seen, but I've seen a real one working at Rawside.

TROPP:

This one in the museum works.

COHEN:

Does it? And when you see it there… Hollerith collects data, for example, by states and occupations and so forth and this goes through, and the electric circuit closes through mercury and through these fingers that go through it. The clock turns one for that state, the box opens and you put it in by hand for sorting these two operations. Now that's a means of collating data, but you'd think of the control mechanism of the Jacquard loom what we think of as a punched card doing today, it isn't just the sorting. I mean, it seems to me that Hollerith was really very elementary. I think it's important because it worked, yes, but I don't know. Maybe you feel differently.

AIKEN:

No, I agree with you.

COHEN:

Whereas Jacquard is a real breakthrough, and I'd like to know how he got it. You were saying earlier, I've never known how that idea came to him.

TROPP:

Well, it didn't come to him essentially. It's a synthesis of ideas that were there first. I think you can identify the genesis of the Jacquard loom about 1750ish, and that when Jacquard build his first loom in about 1803, he was synthesizing ideas that were not original with him and he didn't claim them. But what he did was something that no one had yet been able to do, and that was to build a loom with this kind of programming that was commercially feasible. The ideas were about a half century old. You know, if there had been a patent fight, Jacquard would have lost on all counts, in terms of the individual elements that went into the Jacquard loom. Yet, he was able to bring them together; without Jacquard, there would not have been a loom of this sort, I don't think.

AIKEN:

Oh, there's no question about it.

TROPP:

I should chase down the history of that for you, but I think you're right in your suggestion in the car, about doing a study of Jacquard the person. It would really be fascinating.

AIKEN:

How romantic it is. He did his work in a shed back of his house. His children were in the house running the loom when he was out loafing in the back. He was almost kicked out of the guild of weavers because he didn't contribute. Then he completed this thing and it operated and for just a short time, was an extremely famous individual. And he was driven out of town with rocks because he was stealing the livelihood away from the weavers.

TROPP:

Right. All the Jacquard looms were destroyed, burned, broken apart.

AIKEN:

Now I don't know where you can find a more romantic story.

TROPP:

The Galois of the weaving industry.

I want to get back to the Comrie story, but I think maybe I should put a fresh tape on, because we heard these marvelous stories about Comrie and I would like to get back to them.

AIKEN:

I don't remember much about them.

TROPP:

Well, you were talking about Comrie and his habits in the evening after he arrived home, and I think that triggers a story.

AIKEN:

Oh, you want me to tell that?

TROPP:

Yes, I'd like that, because there aren't many stories around about Comrie because there aren't many people left who knew him well.

AIKEN:

Well, Comrie wore a wooden leg and it grew very heavy and tiresome, and when he got home in the evening, the first thing he did was to go upstairs and get rid of it. And he put on a pair of pants that had the one leg with safety pins pinned up. Then he would proceed to come downstairs hopping from step to step, taking three steps at a time with his wife standing at the foot of the steps, praying that he would make it one more time. She told me sometimes, "I don't know whether he'll make it."

Another amusing instance was that Comrie belonged to a driving pool that left Bedford Square every night at five thirty to six. When I went there, I rode out to Comrie's house in the car, and the London traffic at about 6:00 in the evening is such that you move ten feet and then wait. There was an elderly gentleman in the driving pool whose name I can't remember, and when I was there, he enjoyed asking questions about things and the situation in the United States, largely so that he could go on and comment about how crass and crude that was compared to England. Then one night, he said, "Is it true that in the United States, you lay out the streets of your cities in rectangles, checkerboard fashion?" And I said, "Yes, that's true."________________ Then, he says, "Well, it's monotonous, isn't it?" (Laughter.)

COHEN:

How right he was.

TROPP:

I don't know. After Washington, D.C., I'm ready to go back to monotony. The downtown area is more than I've been able to fathom. I keep losing streets.

COHEN:

There were about two people in connection with the Harvard situation you were talking this afternoon after lunch about how only bit by bit people became interested and were then concerned with the fact that there was not time for them. What about Van Vlick, or was he beyond the stage of active work by the time you were having free time?

AIKEN:

Van Vlick's only association with that laboratory was at one of the symposia at which he, as Dean of the Engineering School, made a speech welcoming people to the University. That was about the only contact he ever had with it. There was one other thing that bothered the man very greatly. We were publishing these reports that you see bound up here for different agencies, and Van Vlick could never quite understand why we didn't publish those in journals, and in his capacity as Dean, we had quite a few discussions. He didn't feel that those were publications and so he didn't quite see how we were really holding up our end on publications in the computer laboratory at that time.

COHEN:

Yes. And I suppose they weren't publications in the sense that if it was a journal, they would get into every laboratory and they would have a copy of it. As a separate volume of this kind, it had to be purchased separately and they might or might not.

AIKEN:

Well, we sent out hundreds of copies of these things. No, I think his feeling was that we were not subjected to the process of refereeing, and that bothered him. We produced the stuff, and we published it, and we mailed it out, and the government paid for it, and everybody was satisfied with what was going on except Van Vlick. As you probably well know, this has become a mode of operation now— government contracted research.

TROPP:

Well, the mathematical tables group did similar…

AIKEN:

They did the same thing.

TROPP:

Was DeRack at Harvard during that period, because he had computational interests? I can't remember where he was.

COHEN:

He came to Princeton in the Institute, but I think he never came to Harvard even to give a colloquium. I've never seen him.

AIKEN:

Not at Harvard, no.

COHEN:

I mean at Harvard. I once met him at the Institute.

No, as a matter of fact, the thing that interests me, apart from people who were philosophically concerned more and more, you know, like Drebbin and Quine, and Filipe LaCourvier and so on, the number of consumers, real consumers, whose work had either always needed it or who saw a capacity for developing work that they hadn't been able to do before. There were very few. Now Leontief, as I mentioned, was one but he had been looking for such a capability from the work go, and he proceeded until he found it.

TROPP:

One area of mathematics that used machines early on, but didn't the Mark I were the number theorists.

AIKEN:

On the other hand, Lehmer, who was interested in machines in California…

TROPP:

Back in the thirties, had built a prime number sieve.

AIKEN:

Yes. At one time, the Bureau of Standards operated a computing facility in Los Angeles.

TROPP:

Institute for Numerical Analysis— SWAC.

AIKEN:

Right. And Lehmer was in and out of that place all the time.

TROPP:

Yes. I was thinking, though, of the Mark I era, that that was the one area where mathematicians might have wanted to come in and use the machine at a university like Harvard.

COHEN:

But you know, the Harvard math people had no particular interest in number theory.

TROPP:

Oh, okay.

COHEN:

I was a mathematician, and as you were saying it, I was running down the list and there was analysis, differential geometry, topology and so on, but not number theory. The Hardy-Littlewood-Polya kind of person we just didn't have.

TROPP:

Well, that explains it. I think Lehmer was doing some prime number generating on ENIAC later on.

COHEN:

And looked for Mersenne numbers.

TROPP:

Yes. Well, Mersenne and some prime numbers generating functions were the first things done on EDSAC in England too.

AIKEN:

I was just looking to see if Lehmer came to our first symposium, but he didn't. He came to the second symposium.

TROPP:

You've kept such beautiful records. Maybe you have a list that so far I have been unable to turn up, and that's a list of the participants at that 1946 Moore School.

AIKEN:

No, I don't have that.

TROPP:

You kept the list for your two symposia, and all the participants are there as well as the lecturers. At the Moore School, we know who the lecturers were but we don't know who the participants were.

AIKEN:

Well, I guess I have to say that I was selfish and meticulously kept the records for our laboratory and didn't bother with anybody else. But, here's every participants at these meetings.

TROPP:

And as far as I can find, there were no such records were kept at the Moore School.

AIKEN:

You know, this was tough work, organizing these things.

COHEN:

But particular records were kept at Harvard. I mean there are the log books for example, of the day-by-day activity.

TROPP:

The log books are marvelous.

AIKEN:

Of Mark I. Oh yes, including all the personal bets that were made between the members of the staff.

TROPP:

Where does the legend go back that St. Patrick is the patron saint?

AIKEN:

Of computers?

TROPP:

Of computers. (Laughter)

AIKEN:

I don't know. I believe you could find the answer to that question in the log books.

TROPP:

Well in the log book, Bobby Burns showed me how on St. Patrick's Day, as long as the machine was there, it was always turned on. If it had been down for a while, it was always turned on and run on St. Patrick's Day.

AIKEN:

Everybody knew that St. Patrick was the patron saint of computers, but I'm sure I don't know why or who started it.

COHEN:

Unless driving the snakes out of Ireland was considered a form of de-bugging or something.

TROPP:

Yes, but de-bugging at that point was an unknown term.

COHEN:

Was it?

TROPP:

Yes.

COHEN:

Well, I'm told that that was invented in your laboratory.

AIKEN:

De-bugging?

COHEN:

Yes.

AIKEN:

I'm not conscious that it was. I'm not conscious if that's true. I know that everybody talked about getting bugs out of the computing machine, including myself, but the fact that when you located errors in an electric circuit, you said that you'd caught the bug, it is not clear to me that that was an original terminology in our laboratory.

COHEN:

But it was certainly used in your laboratory.

AIKEN:

It was widely used, very widely used.

TROPP:

I would like to get back to that question about Von Neumann and his introduction to computation on Mark I. Were there any other people like Von Neumann, who got their first introduction on Mark I?

AIKEN:

Yes. Let's see. There was Stephen Bergmann, who was a mathematician. There was a distinguished mathematician who I believe ended up at Syracuse… what's his name? He, for a while, was at Stanford. What's the name of the man I'm thinking of? I can't think of it right at this moment, but I'll think of it. You see, this is going back thirty years. The fact that I have to fish for it once in a while is not too surprising. This man that I'm thinking of is a very distinguished mathematician.

COHEN:

Yes, and I know who he was. He was brought in to Syracuse as a star professor at a tremendous salary from Stanford. He was one of the first people into this star system, you know.

AIKEN:

Yes, yes, that's the man.

COHEN:

Because I remember that I went there to give a lecture, and I remember meeting him, and I said to someone in the Math Department, "Aren't you jealous of having this man brought in at such a huge salary, when you are getting relatively small ones." And they said, "No, he lends such distinction and it lends honor to the whole department." And I just happen to remember that. But I didn't know that he had been at Harvard.

AIKEN:

Oh, yes, he was there.

Vanderpoel was another person. Vanderpoel was interested in number theory and discrete variable processes of all kinds, and he trotted around that laboratory for years.

COHEN:

Were there any people from MIT?

AIKEN:

Wiener and his associates, Forrester and his associates. Forrester was headed right straight down the analog computer path until he started coming up to our place. I've never known him to acknowledge that fact.

COHEN:

No, I've never known him to. In fact, he has a biography based on tapes at the Smithsonian as I understand it.

TROPP:

Not that I know of. I mean I have a taped interview with Jay.

COHEN:

No, it was a man named Mertz, who was doing a history of Project Whirlwind, and as I understand it, he made hours and hours and hours of tapes with Forrester on the genesis of his ideas, not only concentrating on the magnetic-core but his whole career.

TROPP:

In the one conversation that I had with Jay, he talked about the original analog direction. What's hard to find is the shift from analog to digital.

COHEN:

He didn't mention his Mark I experience?

TROPP:

No.

AIKEN:

Well, that shift took place in our laboratory, when he came up to see what it was all about, he re-evaluated his position in analog computing. The whole purpose of the discussion was "which is the right way to do it? Should you go analog or should you go digital?" And under the influence of the Institute, you can see why he was going towards analog. My position to him was very clear— that digital was the way to go.

TROPP:

Well, I think that once you look at the problem, it was clear that it wasn't going to be solved by analog methods at the level that they wanted to solve it. But of course, again, that's hindsight.

AIKEN:

But I have never known Jay to refer to the fact that he came to Harvard and shifted his course of direction.

TROPP:

Well, he does acknowledge visiting Harvard. I just can't remember the other acknowledgement.

AIKEN:

As a matter of fact, I've never known Eckert and Mauchly to acknowledge that they shifted their entire path and development because of Mark I. You see, one reason that the ENIAC had such primitive programming, was that the ENIAC, as its name implies, was an electronic numeric integrator and calculator ____________________, and that was what they set out to build.

Then when Mark I came along, they became interested in general purpose calculation. Whether they would have made that transition without us or not, that's not known.

TROPP:

Of course, there are a variety of stories. I haven't talked to Eckert about this, but I have talked to Mauchly. Mauchly says that they were always talking about a general purpose machine, but they were building a special machine to do a job. There was a war on and there was a feeling of pressure to get this job done, so a lot of things that they might have wanted to do got put off until the war ended, or until they got this machine going. This is along after the fact, because again, we're talking thirty years later.

AIKEN:

I guess I have to say that it has been demonstrated over and over and over again that you never win, building a special machine. Never. There have been special purpose computers talked about ever since this field got going, and nobody is _______________.

TROPP:

And the last computer conference I went to, I heard an argument all over again about special versus general purpose computers.

AIKEN:

Just as soon as you put a program in a general purpose computer, you have made a special purpose computer. It's so much cheaper to write the program and put it in the machine than to design the confounded thing.

COHEN:

Yes. Well, this question is a very interesting and unsolvable one, because one has to say, for example, this kind of thing: if there had not been the Mark I, whatever their own thoughts about what they might do or might not have done, even if they had talked about it, would they have done it or wouldn't they? Now you can't make it run back wards. I mean Mark I came along.

In the work that I was doing, I had a discussion once with Brainerd. Did you know Brainerd?

AIKEN:

I knew him, yes.

COHEN:

Brainerd was quite annoyed at me because I said that the Mark I was conceived from the outset as a general purpose computer, and while it's true that it had limitations, we all may know what the limitations are, the conception was general. And the Moore School was building a very special purpose machine and these are its purposes. Its name tells you what its purposes were. "And the Mark I, if you think back to its name", I said to him, "tells you what its function was. It was automatic, it was to do things in sequences which were to be pre-determined, or as we would say today, programmed, and it was a calculator more than computer. There's no doubt about it."

And he said, "Yes, but that's stating a very narrow view." And I said, "Well, sir, I'm not an advocate, you know, because I happen to be at Harvard. I'd like to know the truth, so let's take a wider view." He said, "Well, it's very clear. For example, not only were there ballistics tables, but there was some computations done for other work in the government."

And I said, "Well, yes, you know, this is very interesting, and I'd be very glad to know that they are, and it's clear that even a special purpose machine, if there's some other problem for which that purpose will serve you can use it, "But he insisted, you know, that it was broad, but I couldn't get him to …

TROPP:

Well, Mauchly would insist the same thing and he would insist it because of one device that apparently everybody overlooked for a couple of years of operation. That's what he called the master programmer.

COHEN:

The plug board system.

TROPP:

Right. And he said that if you stopped and looked at it hard enough, they didn't write it up, they didn't tell people about it, they didn't really go into the expanded concepts of the machine, and so to all practical purposes, it looked just like what you described. Yet in retrospect, he said that if you look at this master programmer, you'll see that we were really thinking along broader lines.

AIKEN:

Well, the question I always ask is, "Where do I go to find ____________?"

TROPP:

That's right. The documentation…

AIKEN:

Where do I go to find this?

TROPP:

The documentation is just awful on that project, and they're the first to admit that they did not keep records.

Another interesting by-product commentary, and again, I'm just recounting …

COHEN:

No, I think it's very interesting, because you see, we had the same problem this morning. I quoted Wiener's statement in which he says that the stored programs, you see, routines and so forth, that this is something he was talking about, but again, you say, where's the bibliography, where's the program, where's the plan. And I must say, if I can give you two examples of this which exist in the literature, who discovered the negative proton or the positive electron? Well, it turns out that the first person … lots of people said symmetry in nature you've got to have it, that's worth nothing. The first person who said scientifically that there has to be a positive electron, and this we can document, was Donald Menzel, but he gets no credit for it, because it was just his thinking as he was studying problems in electrodynamics and quantum theory. It's not Derack's kind of prediction, you see. Now when you get to Derack's statement, you've got something else again. Then you have to decide whether it's Derack or Anderson, the experiment or that kind of real prediction.

It's the same as the discovery of the planet Neptune. Who discovers it? Is Adams the discoverer because he wrote the paper saying it's there, and nothing came of it. Now this is a very ticklish thing. But when you come to the other statements, they are of the category of all the people who say, "It’s possible to send rays out into space and the rays will bring back information". Now, did they invent radar? Do they get any credit for it? In a sense, they do. They get this credit, and they are part of an atmosphere where people are thinking that radiation will bring back information, but there is no discovery to it. Even if some of those people were more advanced, you see, if you don't have the thing real, it's sad, but I just don't see how you can really say to someone, "Yes, you were thinking of making this and that but you didn't really do it and it wasn't clear." It's ticklish. It's very ticklish.

TROPP:

Oh, yes. Well, as I say, this is all in retrospect, and only by going back, if you had the machine in its original state—unfortunately it was altered continuously—you'd have to get the machine back in its original state.

COHEN:

Well, isn't there a statement that they made though, I've never looked into that, didn't they make a prospectus? They must have made a prospectus to get the go-ahead.

AIKEN:

I've never been able to get my hands on it.

TROPP:

Neither have I. There is a very short description of a conceptual device that John Mauchly wrote in 1942 and passed on to Brainerd and then it vanished.

Then in 1943 when Goldstine joined the project, it was revived and a secretary's notes were used to reconstruct it. That was the original thing and now that's around. The thing that I can't find is the detailed proposal that was written on the train going up and that night while they were waiting outside of then Colonel Simon and Veblen's office for a decision, and they were writing like mad. That I haven't seen.

AIKEN:

Nor have I ever been able to get hold of a contract with the Army Ordnance Department.

COHEN:

Have you found that?

TROPP:

No.

AIKEN:

Nor have I ever been able to get anybody at Army Ordnance to tell me what they thought was going to be done by the University of Pennsylvania.

TROPP:

Oh, I think it's clear that they were to build a machine to do ballistics calculations. I don't think anybody would argue with that.

AIKEN:

Integrate ballistic differential equations, period. I think that's all they were supposed to do.

TROPP:

Oh, yes. I think that's perfectly clear from the standpoint of Aberdeen and the standpoint of the charge to the Moore School. And I think that Mauchly and Eckert placed all their energies in achieving that goal and did not, no matter what their conceptual ideas were at the time, attempt to do anything with ENIAC, because that would have been against the contract.

AIKEN:

Well, they came over and visited the computer laboratory at Harvard way back in the days when it was down in the Battery Room, and they were always… they never stayed at what their original objectives were as regards the ENIAC. Now when we start talking about the EDVAC, it was very clear that…

TROPP:

Yes, but that's something else. But as regards ENIAC, you have to be right. The name is accurate because, unless I'm wrong, Goldstine was the person who pushed that proposal. His job there is very nebulous to me, but it seems to me that he was primarily there to get ballistics tables out. They weren't getting them out fast enough. He was sent there to help. You know, "what holding up these tables? Can you find a better way of doing tables? Do something to get more and more tables out." So I think the pressure was there to do precisely that job. Now what else was done at this point is a matter of conjecture.

COHEN:

It's a pity that it's so murky, because heaven knows, since this is a tape for posterity, I'd like it to be made absolutely clear that I am not one who wants to deny them any credit for what they did. In recording my conversation with Brainerd, I told him as I say now and put on this tape for if anyone ever listens to it, I was interested in documents, even documents of someone who heard a lecture or a conversation, and went down and wrote it up. I've never even seen that. And I could find no reference to a contract, no reference to a specification that was brought up to Brainerd who was Dean, was he, then?

TROPP:

He was Dean or head of the School of Engineering. Dean of the School of Engineering, I think he was.

COHEN:

I don't think he was yet Dean. I think he was the head of that section of the engineering part of the electrical engineering within the school and became Dean later.

I talked to him but there was no memorandum that this was what was to be done and there was no documentation. Now it certainly is true that the machine did develop certain capabilities and that this idea of a plug-in board, but for example I tried to find out whether they were part of the original conception.

TROPP:

Yes, the master programmer was. But it wasn't used for about two years after the machine became operative, so it would have been 1946 before people started to use that plug board section. Because it had nothing to do with the job of just getting ballistics calculations.

COHEN:

I see. But it had been put there as a capability.

TROPP:

I have seen some of the blueprints, but the only blueprints that I've gone over are individual circuits to look at the synchronization or asynchronous operation of certain elements, and I really haven't seen the whole machine in its blueprint form yet.

COHEN:

Of course, I suppose there must be somewhere in the National Archives a classified set of documents which might have information.

TROPP:

All of this has been de-classified. Nothing on this project is classified.

AIKEN:

I've never been able to find it.

TROPP:

I'll keep looking. They will turn up eventually.

COHEN:

It's inconceivable to me that they could have built a project; how much did it cost?

TROPP:

I have no idea.

COHEN:

A half million dollars?

TROPP:

Oh, at least that, before they were through.

COHEN:

How could they have a contract for half a million dollars without specifications?

TROPP:

No, no. Those all existed, but how many copies there were and what's happened to them…

AIKEN:

Where they are and why it is that you can't put your hands on them.

TROPP:

It may be that one of these days in the stacks of boxes in John Mauchly's house I'll find some of these documents. They may be stored in these large files that the legal firms have put together but not exposed because they didn't need them in their litigation. They are liable to be any place. But I don't think they have vanished and I do think they existed. There was an original proposal, there were a series of contracts that are referred to in order to get payments of sums, at various points they had to write progress reports. Those progress reports are around. Then, more money would be given to go on with the project. So I'm sure these things you're looking for do exist. Maybe I'll find them, just like Professor Stibitz found that lecture.

But it is interesting, and the controversy is further clouded by the incredibly strong personality of John von Neumann, who had this marvelous ability, apparently, to absorb and make big jumps, and when he talked about things, talk about concepts without worrying about where they came from. So again, you know, you have further clouding of this whole intellectual development.

But I think in terms of what Dr. Aiken said earlier and you said: this century between Babbage and Mark I. By the time of the era of Mark I, I think the time was ripe. If you hadn't built Mark I and Eckert and Mauchly hadn't built ENIAC, eventually both events, I'm convinced, were going to occur. The time was ripe and …

AIKEN:

The technology was here.

TROPP:

I mean, it might have taken a little bit longer, it might have been a post-war development and we might not be where we are today, but the air was there, the atmosphere was there.

COHEN:

On the other hand, you know, the interesting thing to me is the great difference between Mark I and the ENIAC. I mean, the ENIAC was built for a job for the Army, who in every war has needed tables. You can go back to the invention of artillery. I've seen some of the first tables of Elizabethan days. There's always been a table. If you fire something, where's it going to go? And it's accelerated in wartime, there's no doubt about it, so that pressure would do that kind of thing.

But you know, there isn't any economic pressure on the Mark I, and that's what I find so interesting. The Mark I apparently came out of the fact that the technology was available. You had an idea and Watson was convinced that he ought to put some money and some engineering time into realizing it. And as we brought out today, and this is highly significant, it did no work for IBM as a condition and IBM had no work for it to do, nor was it a first step into a computer business, because there wasn't any. In fact, I saw — I don't know but that you might remember better than I— an estimate that was made in 1945 or 1947 as to how many computers the country could absorb. Do you know that?

TROPP:

Yes— three.

COHEN:

Whose statement was that?

TROPP:

Wasn't that von Neumann?

AIKEN:

Von Neumann felt that there should be one national computing laboratory to take care of the needs of the country as a whole.

COHEN:

I've heard that statement. I've heard of somebody who said we could absorb three and someone a little later on who had raised it to twelve. But any such number would indicate that there was not the economic demand which produced it. Now whether you want to argue, as I take it you might, the technological capability assumes an invention will follow. Or do you say it would have come anyway?

TROPP:

No, I'm thinking of other pressures. Now there were pressures on Dr. Aiken in terms of computational needs. He saw it in his dissertation. And if you look at the mathematical environment, the differential equations were something you had that could handle. But suddenly, in physics, you were beginning to have problems in partial differential equations that had not been around, and these differential analyzers were not going to solve those problems.

COHEN:

And non-linear.

TROPP:

And non-linear, right. We were getting into a whole new environment, and of course, I'm convinced that with the talent that we had around, given those pressures that were going to build up and build up and build up, something was going to have to break or our whole intellectual development was going to stop short. It just had no place to go. Now this is my feeling, and I guess that's why I made the statement. I think it was more from the mathematical environment.

COHEN:

Yes, yes.

TROPP:

And this is the kind of thing, Dr. Aiken, that you were running into___________________. The need to solve larger systems of equations than in twelve by twelve, which that machine that you looked at solved.

COHEN:

Well, now how much mathematics, and again, I don't want to be pejorative about this because as you may have gathered from what I said earlier, I admired, liked, respected and still do admire, like, and respect Leon Chaffee, who was a very able man and a very able administrator and a wonderful person, but I never had the feeling from conversations with him that he got very high in mathematics. He was a practical engineer vacuum tube man, you know, and a genius at studying characteristics of vacuum tubes, and yet obviously he had come upon and encouraged you in your work on space charge and then encouraged the computer work. There is no doubt, all away along, but this was beyond his capability, and I'm sure that many people on that level would have just stopped, you see. I'm not so sure that they would have just gone on. I think they just would have been blocked.

AIKEN:

Chaffee was a very encouraging individual and a very sympathetic individual, quick to admit that there may be something to that. And I can hear him using those words: "There may be something to that." He was a good man to work with and to work for.

COHEN:

Did he suggest your problem to you or did you find that yourself?

AIKEN:

I took his course something or other, but I can't remember the number of it, in which he was concerned with the physics of what was going on in the inter-electron space. That course influenced me more than any other course I took. I found it extremely interesting and that's where I got my thesis in this area.

COHEN:

I see. Instead of you just don't know much about the nature of the space charge itself.

AIKEN:

So I climbed from there and found it extremely interesting. It still is. There are still problems kicking around there that are no longer important, but which I could still play with happily.

[End Of Side I, Tape II]

[Tape II - Side II, February 27, 1973]

TROPP:

This is the 27th of February, and Professor Cohen, Dr. Aiken and I are continuing our discussion of yesterday. Professor Cohen, you had some questions.

COHEN:

Yes. Let me start then, since I'm going to have to leave toward noon and you'll be here this afternoon.

There are two things I did want to mention or discuss and one question I wanted to ask. My mind flipped on something, and that was on your method of division, using the Newton- Raffson rule. I wonder if you'd just say it again, because I just missed one thing. I must be awfully stupid.

AIKEN:

Well, the method of division by reciprocals derived from Newton-Raffson to be sure, provides a means of computing the value of a reciprocal without division. It's accomplished by addition and multiplication. Perhaps the best thing to do is to take pencil and paper. Now let's see. I wonder if I can't derive this as a function of x as a zero here, and if I start at x0, and lay down that tangent and call that x1 and so on, I'm going to convert. Now this says that f of x is zero divided by x0 minus x1 is equal to the tangent of that angle, and that is equal to f prime of x0. That is Newton's rule, isn't it?

So, I then have x0 minus x1 is equal to f of x0 over f prime of x0, and so x1 is equal to x0 plus f of x0 over f prime of x0. Now that's Newton-Raffson.

TROPP:

That's the standard conventional Newton's rule.

AIKEN:

That's right. Now to do reciprocals. You say f of x is equal to 1 over x minus n. I think that's what you do because then at the zero, x is equal to 1 over n. If I plug that in, I have x1 equals x0, plus 1 over x0 minus n divided by minus 1 over x0 squared, and that is equal to x0 minus nx0 squared. So this is equal to x0 times 2 minus nx0. And that's the iteration rule.

So now I take that expression, x1 equals x0 times 2 minus nx0, and I say, let n equal 3. So I want 0.1333. So now I say, let x0 equal 3 tenths.

TROPP:

But you could pick five tenths or any number you want really.

AIKEN:

And now I plug in. I have x1 is equal to 0.3 times 2 minus 3 tenths times 3, which is 9 tenths minus 0.9, which is equal to 0.3 times 1.1, which is equal to 0.33. So x2 is equal to 0.33 times 2 minus 3 times 0.33, which is 0.99, which is 0.33 times 1.01 and that's equal to 0.3333 and so on and so on.

COHEN:

I see, I see.

TROPP:

And if you take a bad enough guess, it will vacillate a little bit up and down, but it converges so rapidly.

AIKEN:

You double the precision.

TROPP:

And once you see the procedure, you can see how easy it would be with the ability to multiply in the machine.

AIKEN:

And add.

TROPP:

And add, right. To add and multiply for division.

COHEN:

Okay, that's fine. I thank you very much. That's very good, because this is, of course, the method that Newton devised for the solution of Kepler's problem, where you couldn't make a solution because it got transcendental as he said, in a finite number of terms. So he devised this very elegant method which was then improved and made popular by Raffson in his book.

Now, what I don't understand is why was it that IBM could add and subtract— that they had to do for business— and multiply. I suppose multiply was the word to find percentages of things for business purposes. I was trying to ask myself why on earth did they never have to face the problem of division.

TROPP:

You could do that by continued subtraction.

AIKEN:

Well, in division, the frequency of occurrence is much lower than addition or multiplication. There is no question of it. And IBM did handle this by computing the reciprocals by hand, and then punching a reciprocal on a card and then multiplying all the arguments by that reciprocal.

TROPP:

Or they'd use a table of reciprocals and look it up.

AIKEN:

Or look it up in the tables. In fact, there were some computers and some mechanical calculators that published tables of reciprocals to sell to you right along with the machine. In fact, I believe that Comptometer did that.

COHEN:

They even re-printed a table of reciprocals of the 16th and 17th century that was done by Copernicus student ________________.

TROPP:

Oh, I didn't know that.

COHEN:

Because he had tables of reciprocals that computed, I think, ten places. They were incredible tables. Suddenly we had a use for them, and made a facsimile edition.

TROPP:

One of the earliest tables that the Math Tables Group did was a table of reciprocals.

COHEN:

Yes, and I can see now why. Because, of course, it's true that most of the work that a business company does is multiplication.

AIKEN:

Yes, So many at five cents a piece.

COHEN:

Yes, and if they have to compute profits, it's always a percentage and that's good enough.

TROPP:

Occasionally, they're looking for averages, which is one of the usual kind of number to have. The average cost of an item or the average cost of doing something.

AIKEN:

Let me make a comment. You said that they could add and subtract. They couldn't subtract, nor was it necessary, because of a great invention by Crumpton. The invention by Crumpton was called "end around carry."

COHEN:

Oh, yes.

AIKEN:

Do you remember that?

TROPP:

I don't. You'll have to explain that to me. What was that called again?

AIKEN:

End around carry. This, I believe is one of the most beautiful inventions that I've ever seen. And if you read Mr. Crumpton's patent, it was pretty clear that he knew what to do, but he didn't understand how it worked. It was a very amusing thing. Now, when you build an adding machine, you have a number of column bar positions like this. And you must arrange carry like that, so that you have carry over from this column to this, to this and the carry over is always zero or one.

What Mr. Crumpton did was to say: and we'll carry like that.

TROPP:

Oh, I see. That why the …

AIKEN:

Yes. End around carry. Now let's just see what happens. Suppose I take… this is a one, two, three, four, five digit computer, and suppose I take the number five nines and add to that any integer, which I'll take for the moment to be 37 for purposes of illustration. So 7 and 9 are 16 and 10, 3000, but there's an end around carry to come over here, so it's 00037.

Now you ask yourself: to what number can I add to an integer, and have that integer for a sum? And the answer is zero. So you must conclude then that five nines are the same as a zero. Now if five nines is the same as zero, what's five nines, is the first question you ask. You say, "Well, there won't be any five nines, because we'll use that highest order column for only zero or nine." And so five nines are the same as four zeros. Well, if five nines are the same as four zeros, let's take any integer of less than four zeros and subtract it from five nines. Now that is to say that that integer is 106363989, and if I put a zero here, you see that all these digits here are the nines complements of these, so you really don't even have to do subtraction. You just arrange for nines complements.

Now, as soon as you see a number with a nine in front of it here, you immediately invert the digits on nine's complements and prefix a minus sign. If you see a number that begins with a zero, you know that that a positive number and you take the digits exactly as they are. So, 999389996101035, now just add those up by positive integers, and the signs will take care of themselves and away you go.

So take the domain of positive integers under addition, and provide it with a one-digit lesser domain in both positive and negative numbers. With Crumpton and with the reciprocal technique for division, you really whittled a computing machine down quite a ways. In fact, you only had to deal with positive numbers and you only had to deal with addition and multiplication, and now you have a facility that takes in negative numbers and provides the division as well. It's a very interesting device.

TROPP:

Now was your method of multiplication with reciprocals rejected because the circuit needs were going to be difficult?

AIKEN:

Why, it was just rejected out of hand. Then, Bryce devised his dividing machine, which they wanted to put in Mark I. I wanted to argue about it. Then, later, we took up when we built the relay multiplier which is much faster, and then we yanked out the divider and the old multiplier too, for that matter. And that greatly increased the speed and the flexibility of Mark I.

COHEN:

Well, that's very interesting, I must say. It clears up that point for me completely.

Now another kind of question I'd like to put out as a general one, and this is related, I guess, to something that he wants to ask you. Then when I leave, he has certain problems he'd like to pursue a little further, on the details of the actual operations of Mark I, II, III, and IV. Whereas, I suppose my major concern was with Mark I, and what I like to think of rightly or wrongly as the establishment of a really operative multiple purpose computer. Now, you have to use the word operative, because I mean Babbage did conceive one which didn't operate, heaven only knows what Atanasoff was doing, heaven only knows what at any stage people had been talking about with regard to ENIAC or what it might have done. I mean, one has to really think of possibilities. If they hadn't been financed by the Ordnance people to make those tables, but by a more enlightened division of the government, for example, if OSRD said, "We'll do that," in one of the jobs to do with these tables. But there must be others around and they might have done something else. One has to do with operative things that were done.

TROPP:

Well, I really share your interest. The others were just part of the total picture.

COHEN:

Right. And one must always be extremely clear about this, because my own work in the history of science and in studying the scientific revolution of Newton, obviously he wasn't the only man around. It happened that he was a lot better than the others and first. Laplace once said: "Fortunate Newton. There was only one real basic law of the universe and he lived at a time when it hadn't been discovered." Now, Laplace meant, "If he hadn't, I would have", and having studied a bit of Laplace, I think he was right, but it's irrelevant at that point. So, I mean I really conceive of this, and it seems to me as I look at the computer history, I have a special point of view here that not only was it in that sense, the first, but it was first in another sense. That is, and we were talking a little bit about this yesterday at lunch and in the afternoon, it seems to me that from the very beginning, every bit of information, all the surrounding intellectual environment of Mark I was about the future of multi-purpose computers. And I remember, personally, even a discussion which I heard you give in which you were talking about such things that would seem shocking in those days as the completely automatic factory. One of them that you talked about, you will probably remember, was the steel foundry, because there after all, to make an automated factory for turning out gun stocks, there was no problem. You just feed in the blocks of wood and Olive Evans in the eighteenth century had an automated factory for grinding flour.

But your point was that in a steel mill, there are constant controls, and when you have a whole variety of controls the moment at which you dump into the Bessemer converter, and when you dump out, the stages of hardening depends on constantly making tests and having information brought in and …

AIKEN:

Feed-back.

COHEN:

And you have to have kind of a complete feed-back system. And for that to keep track of all this information and then direct it, this was one of your examples of a complex process that maybe one could have done. But the point is that it was there, you see. So I think of this as really in all of its aspects, a tremendous revolution.

I was telling him last night, and it may amuse you, you ought to know that among the high points of the interview for me yesterday were first, the revelations to us of the contacts you had not only with Europeans and others all over the world, but notably, with von Neumann here, and the elucidation of that mystery. The fact that as early as 1944 he had come up to the Mark I, is a very important bit of data which I think exists no where in the literature.

TROPP:

No place.

COHEN:

Another question which I'd come prepared to ask you was whether you had ever gone to South Kensington, because the number of people who would have heard of Babbage in the thirties in terms of computers—I mean they knew about him in terms of his work in mathematics, his introduction of the Leibnitzian calculus into England and so forth; he was considered kind of an eccentric mathematical nut. In fact, the first thing that I ever heard about Babbage was that he used to chase organ grinders and drive them away, and this made a great show. So that was a real mystery that you cleared up for us yesterday.

AIKEN:

Well, there is another word or two to say about that, and I think you'll find it interesting. I have two copies of the Passages and the Life of _________________.

TROPP:

I noticed that yesterday.

COHEN:

Oh, I didn't notice that.

AIKEN:

And this one … Wow!

TROPP:

Would you read the inscription aloud so that we'll have it on the tape?

COHEN:

Oh, my. I will read this aloud, this is inscribed and it's inscribed by the way in a terribly interesting fashion. It says: "From one admirer of Babbage to another, L.J. Comrie to Howard H. Aiken, 1946, March 8." This is the logical way, of course, to do a date, where you have in any sorting process, first the biggest one which is the year, then the month and then the day. Isn't that amazing?

AIKEN:

Now here's the second copy of that Babbage.

COHEN:

You've got to be joking. (Laughter.) Isn't that amazing! Isn't that amazing! By the way, this is a very valuable copy, they usually don't have this paper cover bound in. But this one belonged to Charles H. Pillamar, "from W.M.C., February, 1889", and then underneath it, it says, "J.W. Bryce,"and then underneath it, "Howard H. Aiken." So it shows you what I always say you must never distinguish the occasion and the cause. Many men may read the same book or the same passage or encounter the same problem. It's just like the apple. I mean, how many people had ever seen an apple fall, but it didn't do them any good. Isn't that interesting?

AIKEN:

Have you ever seen the books that Babbage, Jr. gave to me at Harvard?

TROPP:

Yes, the ones that are on exhibit there.

AIKEN:

Yes. Are they properly being taken care of?

COHEN:

Oh, yes they are.

TROPP:

Yes. They are in a glass case and they are beautifully exhibited in the computation lab.

AIKEN:

You know, I don't want to sound crass, but those volumes are worth over $25,000.

COHEN:

Well I know they are.

AIKEN:

Because I had a book dealer come to me one time and this was in the late forties, and he wanted to buy them for $10,000.

COHEN:

No, they are well guarded and well protected and constantly admired.

TROPP:

Yes, and they are well displayed in that glass case.

AIKEN:

They were given to me by, I guess, the great-great-grandson of Babbage, who was the editor of a farm journal in Canada.

TROPP:

Saskatchewan.

AIKEN:

Yes. You know, I don't think I've ever written a letter giving those books to Harvard. Isn't that amazing?

COHEN:

Well, that's my next' point that I wanted to talk to you about.

TROPP:

Do you want this on tape or do you want me to turn it off?

COHEN:

Oh yes, but again, we can erase it later on. Anyway, it does seem to me to be of extreme importance that … I'm thinking of people's real interest in history, and in part, of our coming down here. And I think I told you the story of Newton and why I feel that you've just got to get things together. There are so many examples where history is not documented and people are left to wonder. I think what I've heard yesterday in confirmation of what I've guessed about, to me seems so preposterous about Mauchly and Eckert, not only that they shouldn't have been careless about this, but so obviously, there is an implication here that they weren't perhaps fully aware of their history making role.

TROPP:

Oh, I think that's a very recent sensitivity of theirs. I don't think they were aware.

COHEN:

And you know, this worries me a little bit because it seems to me that Howard Aiken, if I may speak for him, from the first day was aware of the history making role, and kept the record. And the problem that I was going to put to you is that among the archives and among the things that were left that were the official documents of the laboratory, which, by the way, are very well preserved, and as you may remember, those are all under restricted use. That was your condition. And that Tony Ottinger could give to people that he thought were absolutely qualified…

AIKEN:

That's right. He was my heir.

COHEN:

… permission to do so. Now I can tell you that he's given me permission and Owen Gingerich, who has worked with me, and very few other people. So he's been very, very good about that. But I happen, therefore, to know something about the contents.

The question I was concerned about was: you must surely have a personal archive that you've kept.

AIKEN:

I've left everything at Harvard.

COHEN:

There is nothing else?

AIKEN:

The only thing that I brought when I left Harvard was my books, and I tried to make sure that I had a complete set of publications in the laboratory. That was the only thing I look. As far as letters and correspondence and contracts and all that sort of thing, I left that as the property of the laboratory.

COHEN:

Oh, that is all there.

TROPP:

I think that part of the problem is that there are laboratory documents that Bobby Burns is really the only one who is sensitive to and has kept.

COHEN:

You know, they've moved, they've taken on a second building in supplement of the Aiken Laboratory, the old IBM building, as I was mentioning, in back of the fire station. They've been terribly concerned with the move.

I have arranged with Bobby Burns, and he likes the idea and Tony Ottinger agrees, that all of that material ought to be now moved to the archives. Laboratory directors are not archivists. As long as Tony and Bobby Burns were there, one could always be sure that things were well looked after, but let's face it, when Tony leaves, they're going to have an ad hoc committee and they're going to appoint the best man they can get. He may never have had any interest or contact.

TROPP:

In terms of the documents, I didn't know that you were going to move them this soon. When you talk to Bobby, he and I had talked about it earlier, he has the complete photographic record of every series of machines, and every photograph is dated and there are books of them. I would like a copy, I don't want the original, but I would like a copy of that collection in our Smithsonian collection.

COHEN:

Well, there's no reason why you can't have it.

TROPP:

I would also like copies of the log books.

AIKEN:

Yes, those log books…

TROPP:

Some of those entries are really funny.

AIKEN:

Have you read any of the bets?

TROPP:

Yes, yes, I've come across some of the bets.

COHEN:

My favorite one is about a rating who was sloppy, and that was recorded just as if it was on a ship. But all of those are being properly preserved. The only thing, as I say, I didn't know whether you had taken a certain amount of your personal correspondence out and kept it.

AIKEN:

No, the whole thing's at Harvard.

TROPP:

Yes, I would guess that the correspondence you would find also in Bobby Burn's holdings. I just don't think he got around to showing it to us.

COHEN:

Did you ever keep your student notebooks?

AIKEN:

When I was a graduate student, you mean? No.

TROPP:

How about lecture notes from some of the courses you gave?

AIKEN:

I had an iron clad rule that on the day that the final examination was graded, that the lecture notes were destroyed. This was self-discipline to eliminate the temptation to give the same course the next year. So that I never gave a course more than once to a student.

TROPP:

I do much the same thing, but I just thought you might have had them.

COHEN:

This does indicate that perhaps one should find people who took that switching course.

TROPP:

Well, as I said, I have the 1953, and I think I've located somebody who had it in 1954 or 1955.

COHEN:

Well with the class lists preserved, there's no problem about getting those.

AIKEN:

I believe that it is essential that you gentlemen talk to Professor Warren Siemen at Syracuse University, and to Professor Robert Menneck at Rice University. Now they were two of my students who even more than Tony Ottinger, have attempted to keep in touch with me and with the developments.

TROPP:

Now I have talked to Mennick on the telephone.

AIKEN:

Now Warren was assistant director of the laboratory during that time period before I retired. I think you'll find that they, together with Jacqueline Sill, will provide you with more details than anyone that you can find.

COHEN:

Good. The reason that I was asking you about these other papers and things is not only to find out about them, but it's very helpful often to know whether one should ever look for them or not.

AIKEN:

No. Anything that you do not find in the laboratory, just let me know.

TROPP:

I have talked to Professor Minneck on the phone, but I haven't had a chance to go to Rice to see him. We did have some conversations, and again, he was very gracious.

AIKEN:

He's a charming fellow— absolutely charming, and he's a man of very great ability. We are associated now in magnetic bubble development for Monsanto. So here it is, after all these years, still going on in collaboration in research at Monsanto.

COHEN:

Well, Tony is a fine man, and I must say that I like him very much and he's got an awful lot on the ball, but his primary concern is not in computer development. As you know, he's interested in linguistics, education, and programming. He now has a new project which I'm going to be working on with him with a number of other people, on information processing and its effects, and he wanted to get a couple of historians. Because, actually, you know, just to give you an aside, the real effect of any new technology is very hard to discern. You can't find in the literature what the real effect of printing was on thought; it's not possible. I've looked through all the literature and I've been asked to give some lectures at Pennsylvania, and I've been trying to find out and no one knows.

For example, it's often said that it made a more rapid dissemination of information, but the invention of printing did not; it slowed it down. Because before printing, when you had to depend on the written word or the oral word, the man would leave to come to Paris and everybody would come to hear him, and they would learn what had been going on in Madrid within a month of a man giving a lecture on a new subject in the fourteenth century. By the time of printing when you waited for the book which had to be composed, you didn't get any information about what was going on in another city for six to eight months and sometimes a year or more.

TROPP:

But there still was dissemination in that the books traveled to the places that some of the people didn't get. Now, in dissemination in terms of the spread, sheer special spread, there is that kind of dissemination.

COHEN:

But there is no evidence that… oh, recording and putting it away in the library, yes, but did people read new books that came into the library? We don't know about that.

I want to suggest to you one of the things that I discovered about printing. The one thing that was so marvelous about printing was the illustrations. Now, you see, when you have a manuscript, a scientific manuscript, the illustrations are sometimes left out. We have correspondence of mathematicians trying to figure out what the theorems mean because there is no diagram. They aren't sure what angles are being referred to. People are writing about biology and medicine where there is a bad, crude drawing. They just don't know what they are talking about.

Now, when you had the invention of printing, it went not only with printing and not with plates, but with the woodcut, and the woodcut was printed along with the type, you see, right there. And once you have it correct, and the author has looked at it and corrected it, where you get a really good artist to draw the anatomy as in Versalius, then you can just re-strike those pages in the impression of a thousand copies, and you have a thousand perfect scientific documents, you see. I mean, this is tremendous. Now this simple thing, which, when you say is obvious, you won't find anywhere in the literature. It has taken me years of research just to come on that one small facet of information.

AIKEN:

Have you ever looked at Leonardo?

COHEN:

Oh, yes, yes.

AIKEN:

He understood the value of the drawing.

COHEN:

He certainly did, and you know, he did research on how to record information. That is, how do you record the three-dimensional body? He did it by showing cross-sections of the leg going up and so forth. It's a terribly interesting problem. So I'm interested as an historian, in Tony's project because it seems to me that the problem of information retrieval and what it will do is worth looking at.

TROPP:

Of course, I think that's the major problem of the rest of the century.

COHEN:

Right.

AIKEN:

Information retrieval.

COHEN:

And I should say not just retrieval but the whole information processing.

AIKEN:

Have you talked to Jerry Salton?

COHEN:

No, I haven't

AIKEN:

You should talk to Jerry Salton then. Jerry Salton was the most distinguished of my students to go into the field of information retrieval, and he has become very distinguished— very distinguished in his field. He is professor and director of the operations at Cornell and rather easy for both of you to get to there.

TROPP:

I've heard of him and of his work.

AIKEN:

Yes, he's tops.

TROPP:

Of course, I've been interested in this problem with Ken May's work for a number of years. One of the problems is that so far people have shown a lack of willingness to do the intellectual input to make the system work.

AIKEN:

In information retrieval?

TROPP:

Yes.

AIKEN:

Well, may I make this comment? I think a thing that you should know about information retrieval ____ a storage medium. You've got to have a hard storage medium before a static ______________________one in which the information is not lost in the juice that goes off. After you've punched or otherwise recorded all that stuff, the danger of losing it with a loss of power would be terrible. And you can't use magnetic core because it's too expensive and there aren't enough of those finite girls to wire them all up either. You could use them to say, mechanize the
____
___________________ and I think that magnetic bubbles are going to supply the mechanization that has been absent in all of these enormous information retrieval problems________________________. Give us another two or three years and this thing is just going to start to go like that.

COHEN:

Well I have a feeling that people who are working on the computer in relation to information retrieval have generally done the wrong thing. Now I don't know enough to have my judgment be worth much, but it certainly seems to me that the reason they've done the wrong thing is, that unlike you and what you were saying as the guide yesterday, that what one ought to concentrate on is the logic, they're concentrating on hardware. One thing we know from the history of technology is, you can't guess what new hardware is going to be available tomorrow, it's impossible. And to say that you can't do this and we've got to do this, is wrong. Oh, of course, if you need a retrieval system today, then you must. But I don't know many people who are really thinking in terms of ideal logic and never mind the hardware.

TROPP:

Well, I do know some people who are.

COHEN:

Are there?

TROPP:

Or are thinking. But again, there are problems.

AIKEN:

This was the beauty of switching theory, you see. You had switches and you could take a relay or a vacuum tube or a transistor or anything else, a magnetic core, and make diagrams and draw pictures for a machine completely on the logic of it. Like those diagrams that you were looking at yesterday. They were symbols for switches and take the switches out____________________. I'm afraid __________________________________. Is the course on switching theory still being given at Harvard?

COHEN:

I don't know.

TROPP:

The theme that you were talking about, you know, the role of Mark I, I think that you have to look at two other things in evaluating it and estimating its impact. I think we talked about both of them lightly yesterday. One is the creation of an environment in terms of computation and computational needs that's a very different paradigm than the one that the community had been using up until that period, so you really have gone into a totally new model.

Then the second problem is the problem of training people, and begins the explosion. You mentioned printing and the idea of a lecturer going to some place and speaking to a group of people and you begin the dissemination of knowledge. We had the same thing in the early days of computing, and this is why some of the landmarks are not so much the hardware breakthroughs, but these symposia. These meetings where everybody who was involved was there, and you had an environment in which these things could happen. I think that's the important issue.

COHEN:

Well, I think so too, and that's why I introduced it. It seems to me that if nothing else had ever happened—I'm going to make a very bald statement in discussion— but I think one can argue that if nothing else had ever happened with regard to you, except to have a device of whether of your own invention or not being irrelevant, which would in the first place have been a center for these symposia, for visitors to come— I mean the fact that von Neumann came is not without significance— but people came from all over the world. Where you, yourself, went out to get people and said, "Look, linguistics should be brought in." Various kinds of business, manufacturing, scholarship of all kinds. I remember indexing and concordance, which was another great interest of yours. I think it was either the Bible or Thomas Aquinas, I don't remember which.

AIKEN:

It was the Bible.

TROPP:

The first one that was done was on the Bible, the first concordance.

COHEN:

And so on. And somebody had to go and say to these people who were doing those jobs, "Look, you can't do linguistics without it." Neurology. Now this is a very important role.

Now the second role is the role represented here, and I don't know, Hank, whether you have that complete list of all those particular theses.

TROPP:

I don't know. I've seen them.

COHEN:

Now the statement is yet generally made that the bulk of the big people who did go in came out of your stable, and if you look at that list, you see the kind of people that there were.

TROPP:

You don't even have to look at the dissertation topic, but only at the person's name.

COHEN:

That's what I mean. Have they ever been tabulated?

TROPP:

No.

COHEN:

Because you see, what these people do…

TROPP:

I should make a list of those.

COHEN:

Yes, because it's hard to get at otherwise. You ought to just read them off before you leave. But it's hard to get at these things. When you look at many of the people, they say, "What is the computer today? The computer is an electronic instrument. What is its lineage?" And they go down through electronic instruments. Well, this may be true, but it certainly doesn't tell you what produced the computer revolution.

TROPP:

That's right. I think you are making a very important point. I ran into this about a year or so ago with Brian Randall whom you corresponded with. His source book is a source book of electronic computers and I provided some original documents for that source book. And when I offered him Professor Aiken's 1937 document, the answer was: "That's an important document, but it's not in the realm of the source book that I'm doing. It's not in electronic computers." So that whole era is left out in that study, in terms of the reproduction of important documents. So one of the publications that I want to do is a publication of mostly unpublished important documents that span the whole field.

AIKEN:

____

TROPP:

But that's the only one that I know of that has the 1937 document published. Until you showed it to me, I had not seen it.

COHEN:

Well, I hope you go ahead and do such a documentary work. I think it would be very important.

TROPP:

Well I've run across some interesting unpublished documents, many of them in another environment that's related, in terms of what happened in the computer revolution, and had similar occurrences to Harvard, because people were starting clean. That's the aircraft industry primarily in the Los Angeles area. Communications were different in that period. The East Coast had its computing environment and the West Coast tended to be separate and distinct and they almost grew up by themselves. But there were links back and forth and one of the questions that I was going to ask you was who were some of the people from that aerospace industry who visited the Harvard Computational Lab?

AIKEN:

The first man that I can think of is Louis Ridenhauser, with whom I was very closely associated. Louis was the Vice-President of Lockheed and he almost clubbed the Board of Directors of Lockheed into starting electronics machinery. And I was associated with him in that venture and was a Lockheed consultant for many years. I was always hopping out there to Los Angeles for a week nearly every month.

Then, I was in and out of Los Angeles for the Bureau of Standards operation.

TROPP:

That's right. That was at UCLA. Did people come from Northrop and from Hughes in the late forties? Did any of that group come to Harvard?

AIKEN:

I saw Lehmer very frequently.

TROPP:

How about Harry Huskey? Did he come?

AIKEN:

Yes, yes. He was at our place. In fact, he attended one of these symposia.

TROPP:

I'm trying to think of some of the other people. Were there people from Rand in the early days that came to visit?

AIKEN:

George Brown. I saw quite a little it of George Brown. In fact, I went out to make a speech to dedicate his laboratory for him at UCLA when he was starting that laboratory there, after he left Rand.

Who was the director of Rand.

TROPP:

Of course, the people I keep running into are directors of… I keep thinking of Willis Ware.

AIKEN:

No.

COHEN:

The last one was Charlie Hitch.

AIKEN:

It was the one before. Well, it doesn't make any difference what his name was. When I went to Rand, he gave me a personally escorted tour, and he forgot that he took me around Rand. Then I happened to show up three months later and he laid on this same tour— the same path, the same place, the same comments, everything. He finished his tour the first time by sticking his head in the cafeteria, saying, "As you can see, we have an extremely able staff. As you can see, for recreation they play kriegspiel.

COHEN:

They play what?

AIKEN:

Kriegspiel. It is a form of chess.

TROPP:

It is chess with a barrier between the two and a referee; you can't see your opponents.

AIKEN:

Yes. You can't see your opponent's board. If you move, you try to move into a square and it's occupied, you told you can't do that.

COHEN:

But you're not told what's there.

AIKEN:

No.

Well, so the second time I took this grand tour, we ended up and I got the same crack about the kreigspiel as a recommendation for the brilliance of his staff. By that time, I was able to anticipate what might be happening, so when he told me about the kreigspiel, I said, "Yes, you know, I have an extremely able staff too. They amuse themselves by playing four-dimensional tic-tac-toe. (Laughter).

TROPP:

Going back to our earlier discussion, I think one of the most important things was to introduce people to computation in other ways. I mean, just as people have come to me and said, "What's going to happen in the classroom when every kid has one of those little, tiny calculators?" One of the problems is that the teachers have not been introduced to a computational environment, the youngsters have not been introduced to a computational environment, the school superintendents haven't, the school boards haven't, and nobody really knows how to use one of these devices and use it effectively to augment and to be a part of an educational process.

AIKEN:

Give yourself another five years. Every kid is going to have a Mark I.

TROPP:

That's right. And we're going through this…

COHEN:

Did you see the New Yorker cartoon of the three ladies finishing their lunch and each one had one of these and was a …

AIKEN:

Debating who was going to pay the check or what part of it?

COHEN:

Yes. But it is very interesting, and I think this is a tremendously interesting social problem of science. I happen to be looking on the shelf and I see the Mechanical Engineer's Handbook. Lionel Marks was an Englishman who was on the Harvard engineering faculty and a very charming man, as a matter of fact. I remember him saying once when we were talking about all of this, that it would have no relation to the engineering profession whatever, and the grounds on which he said this was not that he was a machine wrecker type, you know, and didn't believe in it, but he said that the engineers had been trained to use handbooks, and it would never occur to them to think differently and to explore new possibilities. He found that engineers generally were extremely conservative. Now I think this is extremely fascinating, because the group of engineers as I understand it who first did begin to think about computers, were people in the aerospace industry, who were not traditional, they were doing something entirely new, and they didn't have the tables. If they could have done it by tables…

TROPP:

Tables were out of date before they were published.

COHEN:

If they could have done it by tables and wind tunnels, I think they would have. But they couldn't.

TROPP:

Yes, they got the NACA tables and they were worthless before they got them in their hands.

COHEN:

Right. So they had no choice and they had to begin to think differently.

TROPP:

Well, they also had the problem of when you put … I mean, now we're going into the jet age… you put some shape into a wind tunnel and you had to get all this data and you had to find a way of handling it. They had data that the textbooks that they had studied didn't know how to handle. They had a whole new problem.

AIKEN:

Have you ever heard Louis Ridenhauer's definition of an airplane company? He says it's a place where well-informed brash old men sit around all day in conference discussing irrational things. I was very, very fond of Louis. I was with him the evening he died, in Washington. It was a very unusual thing.

There was a young man who was interested in very low temperature devices in computers— what was his name?

TROPP:

I don't know.

COHEN:

I do.

AIKEN:

Well at any rate, he and Ridenhauer and I talked ____, and we went to Washington. I met Ridenhauer and we bummed around Washington all evening with him. And the last thing he said to me before he left was, "You're going to the Cosmos Club. I wish I was going to the Cosmos Club but I can't go because I'm now Vice-President of Lockheed and I've got that two-room suite," and he shoved off and ____. And the next morning, I got up at a quarter to eight and Ridenhauer didn't show up and this other man didn't show up. Somebody wanted to know where they were and I said, "Well don't worry about Louis, he probably has a hangover this morning." And a couple hours later, the Manager of the Statler called up looking for me and telling me that Louis had died in bed. And almost immediately after that, we got a telegram that ________ was dead, so we just folded the meeting and everybody went home. It didn't seem very worthwhile to go on.

COHEN:

You know who he was, don't you?

TROPP:

Lou Ridenhauer? Of course. There's a very nice biography of him in one of the journals that I have a copy of. I'm trying to remember where it was published. This would have been in the mid or late fifties, so this was published about 1958. It's a very warm biography.

AIKEN:

Well, I believe the first sentence is: "Louis so and so Ridenhauer was especially known for the shortness of his temper___________________." That's the first sentence.

TROPP:

It's generally a very warm biography. Everybody that I've run into who knew him and worked with him said that he was one of the real important people in this whole revolution that we're talking about.

AIKEN:

He was Dean of the Graduate School at the University of Illinois when he was 28 or some thing like that, after having been Chief Scientist in the United States Air Force.

TROPP:

Arthur Samuel told me the story about how Louis Ridenhauer got a computer at the University of Illinois, and that's a marvelous story. It's a very long one, but it's indicative of how he operated— how he got word to some body who got word to the Board of

Trustees and the next thing you know, they've got money and they're going to get a machine. It took about three days.

AIKEN:

He was a real operator.

TROPP:

From the time somebody went to him and said, "We want a computer", until he came back and said, "by the way, I've got $100,000 for you." Apparently, Samuel was very close to him during this period.

You had some other questions that you wanted to ask.

COHEN:

No, I think I'm about exhausted.

It seems to me that now that we've got it, let me give you a proposal and then we all three might discuss this a moment. Whether you transcribe it or not, I mean you might just as well have it on the record, as long as you've got that machine here. It seems to me we've gotten a tremendous amount of information and insights yesterday and today. Now we have to digest these and think about them. I think there are some follow-ups that we want to do and I would like to propose to you that you and I might see if we can get hold of, for example, of Ted Brown. Even in his own right, he isn't going to last much longer. I think we ought to do that. I think we ought to go and see Jackie Still. Has she remarried?

AIKEN:

No, she was Miss Sandborn, and she married Still.

COHEN:

Yes, and she lives in Gloucester, you see, which is right nearby. And I think that that … and then later on, I do think that Minnick sounds like a very important person to do, but at least we ought to do those two, and perhaps then, get a sheaf together, taking out of theirs what is irrelevant and duplicated, and then see what questions we have that we want to put. Does that seem sensible?

TROPP:

That seems very good. I have some things that I want to do after you leave. I'm not in the hurry that you are, and as long as Professor Aiken is being so gracious, I'm going to take advantage of him.

COHEN:

Oh, I understand. Please do.

(Break in tape. I.B. Cohen has taken leave)

TROPP:

Why don't we start with your early upbringing in Indianapolis and your first job?

AIKEN:

Well, my first job was in the old Indianapolis Light and Heat building. I was a switchboard operator and I had the night shift twelve hours a day, thirty days a month. I went to high school in the daytime.

TROPP:

So you started working for them about the time you graduated or finished elementary or junior high school. You were fourteen.

AIKEN:

Yes, since the eighth grade. There was no junior high school in those days. With the aid of the Superintendent of Public Instruction, whose name was Milo Stewart, I had an examination set up for me so that I could get some credits and get out of high school and get off of this rather bitter twelve hour nights. He also assisted me by writing a letter of application for employment with a public utility operating in every <u>Midwestern</u> University town, and I was offered a job with the <u>Madison</u> Gas Company and that's the reason I went to the University of Wisconsin.

TROPP:

That was the job that you said started the eight hour day.

AIKEN:

Yes, it was responsible for the eight hour day____, so I had worked from four to midnight on that job and it was much easier.

TROPP:

Well, you graduated from there in 1923, and then you stayed on with Madison Gas.

AIKEN:

Yes, I did. I became Chief Engineer of that Company. I was promoted from switchboard operator to Chief Engineer overnight when I got my master's degree. (Laughter.)

TROPP:

The one thing I guess that I just remembered about the Arsenal Tech was that you said that you were a member of their first graduating class.

AIKEN:

Yes sir.

TROPP:

I well remember that high school from my living in Indiana.

AIKEN:

Well, after a few years of building powerhouses and gas works, I decided I better get back to school and get an education. I elected to go back to the University of Chicago to study physics. But upon discovering that the faculty were bootlegging grades at the behest of the newly appointed president, Mr. Hutchins, after two quarters I decided to go to Harvard.

I had an arrangement by which while I got a doctorate, I would go back to the University of Wisconsin, where I had an offer as an assistant professor of electrical engineering. But I never got away from Harvard, and when I returned to Wisconsin to go to commencement, when they gave me an honorary degree, I told the president that I guessed the best I could say was that I was still bucking for that job as assistant professor.

TROPP:

(Laughter.) That was in 1967, according to the vita.

AIKEN:

Yes, and before I got up from the luncheon table, a contract was put in front of me.

TROPP:

(Laughter.) Was it as an assistant professor? They really went along with the gag.

Well how did you happen to pick Harvard?

AIKEN:

Well, Kendle, who impressed me most at Chicago, told me he thought I'd like it at Harvard. I got some catalogues and found out who the men were who were there, and decided it looked pretty good____.

TROPP:

I think we mentioned yesterday that in physics, the three really prime places were Harvard and Columbia and Chicago, in terms of the people who were there.

AIKEN:

That's right.

TROPP:

In terms of the centers of study. I don't think that Berkeley or Cal Tech had yet reached the evidence they later achieved in physics. I think this marvelous story that you told me today about the Rochlitz Foundation was ____________.

AIKEN:

Yes, a rather comparative letter. They invited me to the Rochlitz Foundation, and I was amused when I got there— I was a little bit late, and that was made clear to me. But be that as it may, I was given the Rochlitz Prize ________

TROPP:

What did you find out about that Foundation afterwards? You said that you had tried to find out what that Foundation was.

AIKEN:

It was a creation of Jim Rand, and largely, it was his personal thing.

TROPP:

And people like Douglas Mac Arthur and others like that who were on his board were close friends of …

AIKEN:

And officers of these companies were also officers of the Rochlitz Foundation.

TROPP:

And what is its prime function?

AIKEN:

The Rochlitz Foundation?

TROPP:

Yes.

AIKEN:

Giving cash prizes to scholars chosen by the Foundation.

TROPP:

You said the year before, Lawrence had been awarded… this was in your case, a $10,000 award.

AIKEN:

That's right, and tax-free.

TROPP:

Tax-free. That's a very important consideration.

I was going through the vita and looking at the very long list of honorary degrees, and I guess the one that interests me most is the very first one, at the Technische Hochschule, which was in 1952, which indicates an early awareness in Darmstadt of the work you were doing.

AIKEN:

Well, Professor Alwin Walther, from Darmstadt, spent a good deal of time in the computer laboratory. He became very interested in automatic computation and went home and established courses and research programs that were very much centered on our computer. That gave me occasion to travel back and forth to Darmstadt and lecture there, ____.

TROPP:

You also mentioned the Swedish award, and I thought the manner … you mentioned the way of giving honorary degrees in Sweden, which I though was interesting.

AIKEN:

Yes. Not only was there the professor, but then there is another member of the faculty who assumes the quizzical role of doubting the desirability of giving the degree, so it is something like a trial.

TROPP:

Well, I guess you'll have to read this Swedish listing for me, and explain some of the distinguished awards on that.

AIKEN:

Well, I was appointed a Fellow of the Royal Society of Letters and Science, and I was given an award by the Royal Swedish Engineering Academy.

TROPP:

Oh, this is the 1950 award that's listed.

AIKEN:

If I can find it. Let's see which one it is.

TROPP:

Wow! That's quite a collection of medals and awards.

AIKEN:

Here it is, but I can't read it anymore.

TROPP:

I can't read it either.

AIKEN:

It's the Royal Swedish Engineering Society.

TROPP:

And that's 1949; is that what that says?

AIKEN:

That's what it says—1949. Here's the medal of the City of Paris.

TROPP:

Oh, this is the award that you mentioned in connection with the UNESCO Conference. Would you care to put the occasion of that award on the tape, the occasion for that gold medal?

AIKEN:

That was the UNESCO International Conference on data processing, of which I was the President.

TROPP:

And that was the very first.

AIKEN:

Yes, of the UNESCO International Conferences.

TROPP:

That is a beautiful gold medal.

AIKEN:

That's the Order of the Crown, from Belgium, and Legion of_________.

TROPP:

You know, we have an exhibit up now at the Smithsonian, of the history of medals and awards from all over the world that you might enjoy seeing the next time you're in Washington.

AIKEN:

Yes, I'd like to.

TROPP:

It just went up.

AIKEN:

I wonder how many people have them from both the Air Force and the Navy.

TROPP:

Both the Air Force and the Navy? I knew about the Navy medal from the Navy Department, and this is the Air Force one. Well, I guess I should ask… I knew about the Navy, but how come the Air Force? Is that because of the work that Mark I did?

AIKEN:

No, this one they gave me because of Mark IV.

Now, what else do we have here?.

TROPP:

That's the public service…

AIKEN:

Distinguished for public… I have the wrong notes in some of these packages. This is the Belgian award.

TROPP:

Have you ever had occasion to wear all those awards?

AIKEN:

I've never had occasion to wear one of them.

TROPP:

This looks like the Franklin Medal.

AIKEN:

That is the Edison Medal of the Institute of Electrical Engineers. I got an award from the Franklin Institute.

TROPP:

Yes, it's listed here.

AIKEN:

That's in the Harvard Archives.

TROPP:

It's the John Price Award.

AIKEN:

Yes. The night I was to go and get that award, I also had to fly away to Madrid, and so I went to St. Louis and stayed for a short time at the Franklin Institute, and then out to the airport, and my plane that I had as President of Aiken Industries at the time, flew to Boston to meet TWA to go on to Madrid. I changed out of my dinner clothes on the plane. We left Philadelphia in the aero commander after TWA left New York, and we got there and so I made the flight. So Tony Ottinger picked up the Franklin Institute Award in my place and took it to Harvard and I've never seen it myself.

TROPP:

I think it's on exhibit with the volumes that I mentioned.

AIKEN:

This is the Harry Good Award that the Data Processing Societies presented. And finally, this is the National Science…

TROPP:

Is this the Spanish?

AIKEN:

No, this isn't. This is Belgian, the National Science Foundation of Belgium.

TROPP:

Is that 1951?

AIKEN:

Let's see, 1968. Isn't that what it says? Something's wrong.

TROPP:

It comes out 1928.

AIKEN:

So that probably is not an error. This is the date at which …

TROPP:

… The medal was founded.

AIKEN:

That's right. And with this organization, the date makes sense now, because the President of this organization, Mr. Herbert Hoover, the honorary president. He was appointed honorary president of the National Foundation because of his work in aiding the Belgians.

TROPP:

That's right. So that's a post World War I…

AIKEN:

That's right. So that record is probably correct.

TROPP:

Right. So 1951 is the correct date of the award.

They're just beautiful. You'll have to add them to the Harvard collection.

AIKEN:

Oh, the whole thing goes to Harvard some day. I want to hold that off as long as possible, you know.

TROPP:

Oh, of course— indefinitely. But it's going to make quite a collection there, of awards, and some day, you ought to have a picture taken with all those medals on.

One of the other things I wanted to talk about were these various speeches. You mentioned the UNESCO meeting and you know, we've talked about the flow of information and passing things on.

AIKEN:

Well you see, in the earliest trips I made to Europe, what people wanted more than anything else was a landslide lecture of Mark I, followed by an another lecture about how you used it, and another as to what it had done. And it was that sequence that I gave a great many times in France and Spain and Italy, and in Italy, at the Institute for Numerical Analysis at Rome. And at the Engineering Department ____________________________, in Switzerland. Incidentally, when we were talking about all these countries…

TROPP:

Yes, because yesterday, we left out Switzerland.

AIKEN:

We left out Switzerland, where at the Technische Hochschule, I spent many visits with Eduard Steefel. He was quite a professor in mathematics there, and I had several of his students for different periods. The one who was with me the most was Ambrose Spizer.

TROPP:

Oh yes, he's a very famous man.

AIKEN:

And Steefel and his students built computers at the university there.

Well, to get back to the lectures, that was the first sequence. Then, the number of times that that sequence was given was governed largely by the time I had to travel.

TROPP:

When did you first start going abroad? When did you first begin to find the time to travel?

AIKEN:

Oh, shortly after the war.

TROPP:

So that was 1946 or 1947 time frame.

AIKEN:

Yes. The first time I went abroad to give lectures was for UNESCO at the so-called UNESCO month. Have you ever heard of it?

TROPP:

Yes.

AIKEN:

UNESCO arranged a month in which thirty odd people were invited to come one after another to give a lecture. It was when UNESCO was housed in the old _______ district. Who was the Secretary? I can't remember his name. Paris was still dark, the lights weren't on yet. So that was the first trip, and then after that, it just grew. And it still grows. I have an invitation in today's mail to go to Germany, to Guttingen. Let's see (he reads), "Central Data Processing proposes to organize a meeting on the second of November to delve into the development of digital computers, on the occasion of the 350ieth birthday of the machine by Professor Shepard." Do you know that machine?

TROPP:

Right, right. He was connected with the astronomer— well, with Copernicus and …

AIKEN:

Right. Well, let's see what else he says: "He constructed his really-working digital computer into a million, and at this place, Professor Freytag Luringhof rediscovered the device in a letter written from Shepard (Shickard) (S Hickler) to Johannes Kepler in 1693."

TROPP:

Kepler was the name that I was trying to think of, not Copernicus.

AIKEN:

"Several copies have been built and one will be at our disposal. The committee that prepares this meeting thinks that it will be suitable to read some lectures." Well…(Laughter.) "The proposal is to give a survey from Shepard (Schard) to the future of digital computers, beginning with Schicard, Pascal, Leibnitz and the age of the mechanical computers. The idea is to hear from Dr. Zuse on his relay device in 1941, especially to ask from you for your Mark I development." Do you know Zuse?

TROPP:

Yes, I know about his work.

AIKEN:

He's the German professor. "I would be very glad if you could follow an invitation to the meeting." Then they say that they pay all expenses, etc.

TROPP:

That's interesting. When is the meeting going to be held?

AIKEN:

In November.

TROPP:

November of this year.

AIKEN:

Yes. I haven't made up my mind whether I'm going or not.

TROPP:

Well, that would be real interesting, because Shicard's machine has been duplicated, but I think none of the originals are still around so nobody quite knows the size of it. There was no scale. It was just arithmetic.

AIKEN:

It was a four-rules-of-arithmetic machine?

TROPP:

I can't remember whether it was four or just two. I'm not sure whether it could do all four. My memory is that it could only do two. I'm almost positive that it could not divide. Now whether it could perform subtraction, I don't know either. Or maybe it could only do addition and subtraction, and multiplication only as continued addition, I just can't remember. But again, the interesting beginning of this is the needs of astronomy, which is the very same thing that …

AIKEN:

You can only get out of a computer.

TROPP:

That's right.

AIKEN:

I imagine that it would be interesting to hear from Zuse.

TROPP:

That’s right. That's a meeting that I would like to be at.

AIKEN:

Zuse, you know, feels that he hasn't been very much in the light.

TROPP:

He's just published a book, and I have a copy of it in German, and I'm trying to find a publisher who's interested enough for them to attempt an English translation of the book. The translation of the title, I guess, is "The Computer: My Life's Work." Of course, again, so much was lost because of the war, and so much was destroyed, it's hard to re-create. The same problems exist there as you mentioned with some of the other machines, the lack of documentation. The documents are just not available nor are the devices available. There are some pictures and there are a few fragments, I think, of documents, and then a lot of it is kind of hindsight and claims. So there's a problem there.

Now, Walther is no longer alive and he was one of the pioneers. The elder Piloty is no longer alive. Did you have contact with him?

AIKEN:

Yes, yes.

TROPP:

When did you first meet Zuse?

AIKEN:

I met Zuse in Switzerland. I met Zuse in Steefel's office in Zurich and that was after Spizner had been ______________________.

TROPP:

That would have been in the mid to the latter half of the fifties.

AIKEN:

No.

TROPP:

Not that late. Latter forties? That early?

TROPP:

Yes. It was around 1948 or so, but I can't remember. You see, I didn't know that the Technische Hochschule had a Zuse machine. I didn't know that. Even at that time, Zuse was pretty disgruntled. He felt that he hadn't been given recognition.

TROPP:

Of course, his work was pretty much ignored, I gather, by his own government during the war, as having any impact on the war effort. So really, if there is any recognition, it's just beginning; there has not been any until, I would say, within the last two years. I think Fritz Bauer published a recent article, or wrote it, in his Karnfrogle. That was published last July, and which is in a sense, an attempt to write a program for calculations on the early machines. Then, there is this book which he recently had published. I think a few people now have begun to take an interest and look at the developments that he was connected with, but it is very, very recent and I think your assessment is correct.

AIKEN:

He's quite bitter about it, too.

TROPP:

Well, I've had only correspondence contact with him. If I do get to Germany, I do want to visit him. He knew of our existence, so his publisher contacted me as soon as the book was published and I'm fairly hopeful that we'll get an English translation published. This is going to be difficult to do because of the way he writes, it is important that the translation sound like Zuse and that it not sound like somebody else, and that's going to take a great deal of skill. Because you know, it is an autobiographical work, and it's important to try not to lose the flavor of the man you're doing the translation of and there's a tendency to smooth or to do it literally, either one of which loses the man. So I was kind of hopeful that somebody would get interested in Germany, who has facility with English and who also knows Zuse or would spend some time with him so that the flavor of Zuse isn't lost.

How about Alvin Walther? What were some of the things that he was working on that were of interest? He was one of the early pioneers there.

AIKEN:

Yes. He had a differential analyzer. He started building a digital computer. He and his associates did a good deal of work on magnetic drums, and increased the density of bits per inch on the periphery of the drum well over what we did. He was interested in programming and had very large courses in programming. I guess it's safe to say that he did for Germany with his people in computers what we did at Harvard.

TROPP:

Who are some of the people that you remember who were associated with him then who would still be around?

AIKEN:

Goodness, I wish I could remember. There were two or three young men with whom I spent a good deal of time, but I can't remember their names.

TROPP:

Do you know where they would be likely to be located?

AIKEN:

Oh, I think there would be no trouble locating them by just going in to Darmstadt.

TROPP:

Just go to Darmstadt and to the Techniche Hochschule.

AIKEN:

You see, Darmstadt and _____________ are very close to one another, and they are both very close to Frankfurt. You can go to Frankfurt, get a car and drive to Darmstadt and from there to _________, and then come back.

TROPP:

How about the elder Piloty? Did you have contact with him?

AIKEN:

Yes. Let's see, he was at Munich, I believe, and I had contact with him. He told me a great deal about the university in which he was Professor of Mathematics. He described it one time as the only school in the world to give a _________________ degree in brewing. (Laughter.) Then you can understand why.

TROPP:

That's interesting when you think of the devastation in Germany in the post-war period, the fact that you had three men of this stature both very early in the post-war period and just off-hand, I can't think of anybody else of equal stature to Zuse, Piloty and Walther. Was there anybody else that you ran into?

AIKEN:

No. Also, Walther and Piloty were much bigger men than Zuse.

TROPP:

At that point in time, yes.

AIKEN:

Zuse was a man who had built a machine, but Walther and Piloty were teachers, scholars, interested in using machines as well as building the machines and developing young men to forward the art. Zuse kind of vacillated between going into business to make machines to sell for accounting. He never seemed to be quite fixed in purpose.

[End Of Tape II]

[Tape III Side I]

TROPP:

I'd like to have you tell that story on tape, about Vanderpoel.

AIKEN:

Well Vanderpoel, in World War II, was in Intelligence. He had always been an amateur weaver, and he transmitted intelligence in the form of dropped stitches and red and white tablecloths which he wove. The tablecloths without intelligence were sold to German Army officers and those with intelligence were delivered to the Royal Navy.

TROPP:

The one that you showed me was one without intelligence. This is one that he gave you.

AIKEN:

That's right. This is a tablecloth he gave me.

TROPP:

It's interesting because you mentioned that he was in Intelligence, but what you didn't mention was the fact that as for as the population knew, he was a German collaborator.

AIKEN:

They thought he was a German collaborator, and his reputation was ruined until after the war when he was decorated by the Queen. That straightened him out very quickly.

TROPP:

It was easily then, one of the best covers probably in Intelligence during that period. That's really a very romantic story. What was the occasion when he gave you the woven tablecloth that you just showed me?

AIKEN:

He told me this story and then he gave me the tablecloth.

TROPP:

Were you visiting him?

AIKEN:

No, he was visiting me at the time. It was at that time that he learned to read the digits in coding the musical notes as they were typed out in the hymns that Mark IV was composing. He liked to stand at the typewriters and watch the typewriters printing the codification for a hymn, and then he would sing the hymn. He had perfect pitch. What is it that musicians call it? He had absolute pitch, that's what he had.

TROPP:

Yes, there's a difference.

AIKEN:

He was quite a musician as well as being a mathematician and a weaver.

TROPP:

I had never heard about the musical or the weaving aspects. I should mention that that Tea Cloth that you showed me is really kind of a grid with the prime numbers and the complex domain.

AIKEN:

Did you ever know Harry Clark?

TROPP:

No.

AIKEN:

He's the fellow that Cohen mentions. He was a weaver too, but he was a more serious weaver. He wove his own cloth and made his own suits out of it.

TROPP:

You mean that he tailored his own suits out of the cloth that he wove.

AIKEN:

He wasn't a tailor; he only wove the cloth.

TROPP:

That's really fantastic.

Well, getting back to Europe, in the log book of Mark I it looks like representatives of almost every country in the world visited you, and Bobby Burns pointed out, before our cold war involvement, the number of Russian visitors and visitors from Eastern Europe. Did you have any contact with early computer developments in what became the other side of the Iron Curtain very shortly thereafter?

AIKEN:

Only with two men from Czechoslovakia— Antonin Szoboda and …

TROPP:

Well, Svoboda is now at UCLA here.

AIKEN:

… and his associate, Trnca, I believe. Undoubtedly, I do not pronounce it correctly. Both of those men were at the Staff Radiation Laboratory at Cambridge during the war. They went home as great patriots to assist in the re-building of Czechoslovakia and got there just in time to have the Iron Curtain come down _________________________.

At one of Walther's meetings, a symposium that Walther held in Darmstadt, he managed to get Svoboda out of Czechoslovakia to come to that meeting. Svoboda was followed by two members of the Czech police. Svoboda and I would go out for a walk and walk rather slowly, come to a corner, go around the corner and then run like Hell to get to the next corner and get around that before the cops got there. We led them a merry chase. It was on that occasion that he first told me about his work and that of his student, Pollack, in the attempt to use residue arithmetic in design of computers. Do you know about that?

TROPP:

No, I don't, and I should visit Svoboda because, as I mentioned, he's at UCLA now.

AIKEN:

Well, I did a good deal of work on this residue modular arithmetic later, and the way that this thing works is this: You choose some mutually prime module.

TROPP:

Let's say we pick three and five.

AIKEN:

We'll pick five, three and two. Now we write the integers modulo five and modulo three (writing on blackboard).

TROPP:

And modulo two.

AIKEN:

Now, let us take three two zeros being an encodification of eight and 401 as being an encodification of nine. So we add three and four modulo five which is two, and zero and two modulo three which is two, and zero and one modulo two which is one, and then we look at 17 and we find it to be, we coded this to two one.

Now, this works for multiplication as well: Four times three modulo five is two and zero and zero and 12 is to __________.

TROPP:

That's the Fermat Theorem on modular primes.

AIKEN:

Now here is a scheme for addition and multiplication where the phenomena of carry does not occur. Now since there is no carry, you can multiply and add in one impulse. Furthermore, you can add and multiply at the same time. Your instruction would be operate, and then pick whichever one of the results you wanted, that is, either the sum or the product. Now you can cheat on this thing by saying this is minus 14, minus 13, minus 12 and so this would be minus 1, and so this one would be…

TROPP:

Plus one.

AIKEN:

This would be minus 14. Wait just a minute. This would be minus 14 and this would be minus one, minus two, minus three, minus four, minus five. Now let's just check it out: That one plus minus one would be five zero, three which is zero, two is, so this is minus one. So now, by a device, we've extended this thing to take care of both plus and minus. Now, here's where the grief comes in. You cannot look at one integer and tell whether it's larger than another, and since you cannot tell which number is larger than another, you cannot tell whether a number is positive or negative. The amount of calculation that one has to do to tell whether he's in the second half or the first half is prohibitive.

But last of all, you cannot divide because suppose I wanted to divide 24 by 12. I'd have 400 over 200 and I've got to divide zero by zero which is indeterminate and from two different columns here. So both Pollack and Svoboda, in Czechoslovakia, and my students and I spent a great deal of time trying to see if we couldn't solve some of these problems, and _________Garner did at Michigan. Here's Warren Seamen and I in a discussion of the application of modular arithmetic in computer design.

Here's some work that was done at Lockheed along the same line.

TROPP:

__________, 1959–61.

AIKEN:

So Lockheed, Harvard and Michigan all tried to make sense out of this technique.

TROPP:

Well, it must have yielded something, and I can't be more illuminating than a chance comment at the Computer Conference in December. You know, it was one of these throw-away comments when you don't have a chance to say "what do you mean". The comment that I'll just throw at you was that the base three, which is modular arithmetic, modulo three, "has turned out to be valuable in circuit design", but that was the only base that was mentioned, there was not a set of modular modular primes.

AIKEN:

Yes. Now here are some observations on number systems.

TROPP:

Now this is the book for which you showed me the Russian translation the other day.

AIKEN:

Yes. Now sit down here for a minute and let's see if we can't find it. Here we are now. This is a discussion of all number systems from two to twelve and what they'd be good for, and the best representation of the erratic three systems. You don't use the digit zero, one, two, but you use zero plus and minus.

TROPP:

You use __________ addition, <u>multiplication</u>.

AIKEN:

Yes. Now you see, multiplication is extremely beautiful because it has no carries in it. And there are only two carries in this thing. Then negative and positive _____________ represent zero and one. It's possible to record more than a thousand digits per inch.

TROPP:

And this same central arithmetic unit could be employed for all arithmetic operations including input and output translations required by the external ________ system, by using a _______________.

AIKEN:

Yes, that's right, and this is a much more efficient system than the binary number system. I think, if I were going to build a machine that was not decimal, I'd pick a ____________.

TROPP:

Well, theoretically, e is the optimal phase and three is a little bit closer approximation to e than two is.

AIKEN:

Yes, and the proof of that statement are right here.

TROPP:

I think I've told you, I've seen about twenty of these proofs and everybody considers themselves to have done it for the first time. I think until you published this, and the date of the publication is 1951, I don't know of that proof showing up anyplace. Because I have seen proofs even later than 1951, because people said, "I haven't seen it in the literature, so I thought I'd see if I could prove it."

AIKEN:

Yes. This was mentioned in coding systems here. The way you choose a coding system for your digits has enormous effect on the amount of apparatus that's used. Here is a base three scheme: it is where you want to multiply by two and the rating is three. Let's take the digits zero, one, two and now we want to multiply by two. That goes zero, two, and one. So you multiply by two, and what you do is take that wire and that wire and interchange them. So if you get two wires coming down here like that representing the digits, zero, one, two, in radix three system, you cross the wires like that and now you can multiply it by two, and you tap off here, and that's the carry.

TROPP:

I have to look at it again. It's so simple I can't believe it.

AIKEN:

Zero, one, two. Multiply by two. Two times zero is zero. Two times one is this representation, one over two, and two times two is one to carry and one left over, which is odd one. Now that column is exactly like these ________________. So, here's the number x and here's the number two x modulo three, and there is only one carry and that's when there is a one here and so you tap off here and that's the carry.

TROPP:

And that's the whole multiplication table.

AIKEN:

Yes. And the amount of apparatus is up to a place where you can get an add in the carry, its zilch, it's zero. I mean in this representation of arithmetic with radix three, you can multiply by zero, one or two. And if you multiply by one, you don't switch the wires, if you multiply by two, you do switch them, and if you multiply by zero, you cut them off.

TROPP:

To make sure nothing happens.

AIKEN:

Yes.

TROPP:

Oh, the simplicity. I guess we do have a tendency to make simple things so complicated they are almost incomprehensible at times. But that is one of the first places I've run into print of the proof of the optimal number base to use. I've seen lots of proofs but they are all in the hands of individuals, and nobody has bothered to publish them.

When did Svoboda decide to leave and come to this country?

AIKEN:

Well, I guess you have to shut this off. **[break in tape]**

TROPP:

Okay, let's add Austria to our list.

AIKEN:

We have to add Austria to the list because of contacts with Professor Zemanek of the Technische Hochschule in Vienna, Austria. He, I believe, is now an IBM consultant.

TROPP:

Yes. He has some connection. He is also current President of IFIP. That's interesting in that there are no countries in Western Europe where you didn't have visitors and where you didn't, in return, go back. The Spanish environment is kind of a curious one because the name that you mentioned is the only name that I know of, and yet I can't think of any original work that occurred in Spain and I kind of had the feeling that the Spanish sponsored major meetings to find out what was going on, that outside of the gentleman that you mentioned, there weren't really any important people in that period working in Spain. Am I wrong?

AIKEN:

Well, let's see. Santos Massus did work in magnetic storage and logic with magnetic cores. He did things like ________ core. He used to stick three or four conductors through a ______________, and put a current on any one of those large _______ core. If the current was ________ saturated, it is an occurrence on all of them, why you don't magnetize them anymore. This is ore. Now, if the currents you stick through on wires through a core are roughly half of what it takes to saturate the core, you must have both so that you can add two variables. Now you can't add more than two variables _____________________________ because you're rapidly heading in the direction of analog computation. But of course, this business of adding two variables is what makes this core storage work.____ ________________. Students in Spain are not too well prepared and so graduate students very often are no better prepared than ___________________________, so he hasn't had too much to much to work with at his institute. But he's founded a magazine, a publication on the field of computers in automation. He invites people to Spain to lecture. He gives courses to his graduate students and he supplies people to Spanish industry. So, in the general level of development of the sciences in Spain at the present time, I think he's done very well.

TROPP:

Yes. His was the only name that surfaced, and with each of the other countries there's an early machine attached. In Spain, there aren't any of these.

AIKEN:

One of the things I did in Spain at Jose's behest, was to lecture on computers to the Spanish Naval Academy, and one of my students on that occasion was a young man named Christopher Columbus.

TROPP:

(Laughter) Oh, in the Naval Academy? That's marvelous.

AIKEN:

He is the appropriate number of great’s grandson of the Christopher Columbus, and he has the hereditary title of Admiral of the Ocean Seas.

TROPP:

Oh, that's beautiful. I didn't know that there was a direct descendent.

AIKEN:

Yes. Admiral of the Ocean Seas. That's the hereditary title for Columbus's descendents.

TROPP:

That's the marvelous title of Samuel Morison's two-volume work. That's quite a work by the way, but that's a totally different subject that we can get off into.

Well, we're going to follow up parts of your career after you retired from Harvard, in a sense, the two new careers that you began following retirement.

AIKEN:

Yes. First, was forming Aiken Industries beginning in 1961 and in 1967… where does it say when I …

TROPP:

Vice-Chairman of the Board in 1967. So that's when they decided that you were retired from there too.

AIKEN:

That's right. So I now go to board meetings, but essentially I'm not going at it the way I used to.

TROPP:

You were talking about your work with Professor Minnick in Monanto, and I see that your consulting with Monsanto began in the year of your retirement from Aiken Industries in 1967.

AIKEN:

Yes. When they kicked me out of Harvard, I had to find a new job and that was Aiken Industries. And when they kicked me out, I had to find a new job and went into the consulting business. So now I spend a good deal of time at Monsanto.

TROPP:

When I visited with Jack Nash about a year ago at Lockheed, or maybe it was longer than that, he said that you had been with Lockheed as a consultant.

AIKEN:

Yes, for many years.

TROPP:

And you still are as far as I know.

AIKEN:

No, I quit that this year.

TROPP:

Because Bill Main, when I saw him a little over a year ago, said that you were still active.

AIKEN:

That's right.

TROPP:

And one of the things they gave me was a …

AIKEN:

That was Bill Main that called me on the telephone today when we were sitting here.

TROPP:

Oh. Well, Bill got for me and I'm trying to remember the title of it now, a videotape of a speech that you made over the educational channel in the Bay Area. I think there were three or four panelists.

AIKEN:

Oh, my goodness.

TROPP:

So Bill mailed me his videotape copy of that speech.

AIKEN:

Do you know this volume?

TROPP:

No. (Laughter.) Bill didn't say a word about this: "Switching Theory and Space Technology", that you and he edited jointly, published by Stanford Press in 1963.

AIKEN:

Yes. This is the proceedings of a symposium which I organized and got everybody from all over the world to come and see if we couldn't get the space industry to made more sense in the equipment they were designing.

TROPP:

Oh, I see you had an introduction by George Polya on combinatorial problems.

AIKEN:

Yes. Well, you know why that was that? You see, if you interchange the variables and invert them, you convert one function into another usually but not always. There are some places where the symmetry is all right. So, there's roughly 65,000 functions in four variables. But under these input transformations, how many are there? Well, it turns out that there are 402 __________, and to count those, you use _________theorem _________. So, I invited Polya to …

TROPP:

He's the most fantastic lecturer I have ever heard. He's a beautiful man.

AIKEN:

Yes. It was at this time that Vito Bellovich, I believe Steve Eckhaul (I can't remember who all), went down to the educational T.V. station in San Francisco, and sat down and discussed switching theory.

TROPP:

Yes, for an hour, and I now have the videotape of that discussion.

I'm looking at the list of people in this book and it's incredible. You know, all the people we've mentioned: Of course Franz Hone I know from mathematics, and Heinz Zemanek, Warren Seaman, Robert Mennick, Jan Reichmann, Fandemasus, Spizer and I guess the rest I don't know.

AIKEN:

Do you know Speed Jack dress?

TROPP:

No, I don't.

AIKEN:

Speed Jackdress is a president of a company up in Oklahoma that makes disc _______? Do you know a company that makes that?

TROPP:

No.

AIKEN:

Well anyway, he was a Lockheed employee for _____________, and the night that Lockheed and the Air Force decided that they were going to _________________, Speed showed up and he said, "Well, how are you going to get all those fellows?" I got on the telephone and called ____, and asked him to come, and he said, "Well, what's the chances of if you ask him to come, he'll come." I said "Well, it's about 95 percent." Actually, it was 100 percent, because no one turned us down.

TROPP:

Yes, because of the ones that I know on that list, I can't imagine any of them turning you down. Of course, the others I don't know.

AIKEN:

So, I should say that by this time, Bill Mennick and David Willis, Bill Mandamus and (Speed Jackdress?) Steve Atkins and I were all sitting in the bar at the Hilton Inn, outside the airport in San Francisco. And this is where the talk of "how are you going to get these people." I said, "Well, we're going to call them up." (Steve?) Speed says, "Well, who are you going to call first?" And I said, "Well, I'm going to call Steve Eckhaul." Steve (Speed) called the bartender and asked him for a telephone.____, and with infinite gall, put in a call to Steve from the bar.

TROPP:

(Laughter.)

AIKEN:

And how he managed to be there I'll never know. But sure enough, we got Steve, and I said, "Steve, we're having a symposium on switching theory and space technology, and it won't be complete unless you come and make a speech. It is at such and such a time; will you come?" And he said, "Sure".

TROPP:

(Laughter.) It had to be midnight or later in Sweden when you reached him.

AIKEN:

Oh, yes, it was in the middle of the night. And I still don't understand how that telephone call ever got through the Hilton switchboard and I don't know who paid for it.

TROPP:

(Laughter.)

AIKEN:

Then the next day, we went to work calling all of them all over the world.

TROPP:

I was going to say, most of these people are from all over the world.

Your choice of Polya is interesting as a generalizer in a sense and in the introductory portion.

AIKEN:

No.

TROPP:

You mentioned the theorem and I wondered here about your contact with Polya.

AIKEN:

You see, here are the 402 distinct solutions for four variables; they start with zero. Now, we wanted to know these functions and we wanted to count them. So, since we both wanted to know the functions and wanted to count them, it seemed that the reasonable thing to do was to find the functions and them having found the functions and shown that they were generating ___________________, which we did. And then, having …

TROPP:

It's almost like looking for a particular group in a haystack.

AIKEN:

Then what we wanted to do was to prove that we had all. At that point, we found that we had to have a code. So I invited Polya to come to that because here was an application of his combine theory that I thought he ought to be aware of. So I went over to Stanford and saw him and told him how his work had influenced us in switching theory, and because he was distinguished, I would like him to come to the symposium and make the opening speech. He said, "Sure". That's one nice thing about people who are distinguished: when you ask them to do something, they say "yes."

TROPP:

Oh, Polya is one of the most delightful men I have ever known. You realize that his work as a mathematician-it's only been the last two or three years that people have recognized the impact, because Polya really had a number of careers. I might start off by telling you that he told me one day that von Neumann was a student of his in Hungary. He very early on established the reputation of being a very important mathematician and did a lot of work in inequalities. I guess inequalities are the one that is going to turn out to be among the most important things that he did.

Then, after he came to the United States he became world famous for his whole idea of teaching: the idea that a mathematician should be concerned with solving problems. In fact, in the recent issue of the *Mathematical Monthly*, I don't know whether it's January or February of this year, there's a long letter from George Polya in which the person he's writing to and the person he's writing about are omitted, it was a letter he wrote when he was very disturbed about what he thought was a very good assistant professor who had been denied tenure under the "publish or perish" concept. He had found this man's name very prominent among the list of problem solvers in problems through mathematical journals. He said, "After all, what is it that's important in undergraduate mathematics? To teach the students how to solve problems in mathematics." Of course, you know Polya's philosophy.

And he is still very active. He gave a week's lectures in Washington last summer. Polya has to be past 85. When I first had him up at Humboldt, he was about 80 and that was quite a few years ago.

But his mathematical contributions and his work are just beginning to be recognized. In a sense, he had these two major careers too— first, as a research mathematician and second, as he calls himself, a"teacher of teachers".

AIKEN:

Well, that's a story that important. His volume is listed in there somewhere.

TROPP:

Right. This is listed here in the publications. I think when you add to this vita; somehow you ought to add the various symposia and major lectures and so on.

AIKEN:

Yes. That would probably be a good idea. Of course, the lectures, I can't even remember anymore, as to where they were, but the symposia are in the volumes.

TROPP:

Yes, the Harvard symposia and as I say, you did publish the proceedings of the Lockheed Space symposia.

The one that I was going to ask you about is on this last page, of all the biographical dictionaries you're in, and there are about a dozen or more here. Is the one called "World Mobility and Courage" (reads in French)________________________, 1960. Now is that a result of the awards?

AIKEN:

Undoubtedly. You see, you'll also note that even last year, I went into the "National Who's Who" again. Undoubtedly, I've made so many of these international biographical dictionaries because my name crops up all over Europe from visiting here and there and giving lectures on electronic computers.

TROPP:

Well, this one about nobility and courage; that was the title that threw me.

AIKEN:

That just happens to be the title. That is just the name which is applied to that particular volume. That's another way of saying, who's who.

TROPP:

Oh, I see. It sounds like a collection of special titled people.

AIKEN:

A great many titled people are in there and peons like me are too.

TROPP:

Non-titled Americans.

This multiple career is interesting.

AIKEN:

Well, you can't quit you know. At the time you quit, you've had it.

TROPP:

People have talked about this in our contemporary society where everybody's worried about what we are going to do when we retire.

AIKEN:

You can't retire.

TROPP:

People are talking about maybe that most working men in terms of their life working span, say 20 to 65 or whatever it is, ought to think in terms of at least two major careers in that time span, with a third career possibly coming at the retirement age as a way to keep the juices going longer and stronger.

AIKEN:

If I were to quit work and sit here in this study, I'd think I'd be dead very soon. I don't think I'd last.

TROPP:

You also might mention, without going into any details, the current state of the company that's being formed on the cryptanalytic task, which is a brand new venture.

AIKEN:

Yes. That's Information Security Corporation, being formed by Dick Bloch to exploit a cryptographic invention of mine. I hope we will be in operation before too long.

TROPP:

The way Genesis operates, or the way Dick told me it operates at any rate, is that they look for ideas and then they find the financing, and then they get it started, and they provide the early management and then, as soon as the company is able to stand on its own feet, they, in a sense, get out and look for something else. Is that about right?

AIKEN:

Yes, except in this case, this is especially close to what Dick want to do that I doubt that he'll leave it. I think he'll stay in this company for quite a while.

TROPP:

That would be great. He's a very exciting person.

AIKEN:

Yes. One interesting thing is in all of these discussions, you see the same names keep cropping up. Dick was one of the first officers on my staff when the Mark I started operation. Now, that's all over and done but here's another association.

TROPP:

Right. Like Mennick that you mentioned earlier.

AIKEN:

Yes. So the men that I was associated with in the beginning at Harvard have not just run away like last winter's snow, at all. There has been a certain cohesiveness.

TROPP:

Of course, this has been true of what I would call most of the pioneers in the field. The majority of them that I have met have not rested on these early laurels, but are all out at some frontier today. Some of them have even left the computer field, but they are still at the frontier. I have not run into anyone who has stood still. Some of the individuals allowed the technology to pass them by, but in a sense, went off to frontiers in other aspects. The transistor made a lot of people who grew up on tube technology obsolete, and because they were no longer involved in the day-to-day engineering problems, it wasn't long before they were out of it, but that didn't seem to stop them.

I think if somebody just abstractly pulled out, say, the fifty to a hundred names of the first group of pioneers, and then looked at the ones today and looked at the role that they play, I think that they would still play relatively the same important roles.

You know, you asked me about George Stibitz. Well, here he is making important contributions in medicine computers. John Mauchly is still very intellectually involved in the same weather problems and statistical problems, although on a different level, that he was in the thirties. Pres Eckert is still apparently doing exciting things for …

AIKEN:

He's still an officer of Sperry Rand.

TROPP:

He's still Vice-President, I guess, of Sperry Rand. Atanasoff is intellectually involved in something else, although he since, I guess, sold off his company and is no longer connected with the company that he was head of and retained part of another company. But everyone of these people… nobody seems to stand still, and I think that's a general characteristic.

Now, on the Monsanto thing, you've been talking all day about the magnetic bubble. When did you start the research in that area with Monsanto?

AIKEN:

Oh, it was just over a year ago.

TROPP:

It was that recent?

AIKEN:

Yes. The bubble problem was originated at Bell Labs, and they're still the most important people in this field. But there are bubble projects that a number of other agencies are working on, of which IBM, Autonetics, ____, North American Aviation, or rather North American Rockwell as it is called now, and it was good for Monsanto to get into this area because Monsanto has a tremendous _________________________. They are the largest manufacturers of ________________________________. They developed the three____. So it was a natural.

TROPP:

And of that, as you say, in three or four years, we are going to begin to see the impact of that research.

AIKEN:

Well, in another year or two, you won't see any more hot pilots like that.____________________________, and a few years behind that, magnetic bubbles will begin to carry the load for storage. By that time computers will be yea big.

TROPP:

The size of that small calculator you pulled out earlier, and very, very cheap.

AIKEN:

Very cheap compared to ____.

TROPP:

Well, most of the first generation machines that the government funded… almost every one of those machines cost at least a million dollars. A million dollars could buy you one machine if you were frugal and clever. If you would run off into other directions, it could cost three or four or five million dollars.

AIKEN:

If you stuck to your last, the object was to make a machine with input and output and that would compute.

TROPP:

One story that I neglected to get on, and I promised Professor Cohen that I would, and those are the relationships with von Neumann. I just remembered you told us the AC story yesterday.

AIKEN:

Von Neumann was naturally one of the first people to hear my Mark I in its operations of the people that visited the laboratory. He wanted to see the machine and I told him how it worked and what it did, and he did a problem on partial differential equations ____. Dick Bloch, as you probably know, was assigned as programmer on the problem. During the days when this was being programmed, von Neumann was in and out quite frequently to see what the progress was and how it was coming along. He, for purely theoretical reasons, knew the values of functions defined at certain isolated values. _______________, and he would be sure to show up when the machine progressed near one of these so that he could compare them with his theoretical knowledge of the situation.

Then, that problem was written up in one of the progress reports, I believe.

TROPP:

Dick showed me the original handwritten program for that and promised me a copy of it— that famous von Neumann problem.

AIKEN:

Here it is.

TROPP:

(reading): "Second order nonlinear partial differential equation."

AIKEN:

Yes. And here are the results. Now, there is a date put on there and it's December, 1944.

TROPP:

In December, 1944, it was finished and published. The program was much earlier than this.

AIKEN:

Well, the work was done between August 7, 1944 and December. The programming was completed and the problem was run during that period, and that is the period in which von Neumann spent most of the time in our laboratory.

TROPP:

What were his reactions to the results?

AIKEN:

He believed that they were accurate. He had no reason to believe they were not.

TROPP:

They were obviously checking with his isolated values.

AIKEN:

And variables. Yes, he seemed to be delighted.

TROPP:

Well, it's the needs for the solutions for these kinds of problems that I was talking about yesterday when I said that you just couldn't keep these machines from happening; somebody was going to do it. If you didn't do it or Eckert and Mauchly didn't do it, somebody was going to come along, because those problems had begun to emerge about a decade earlier to the point where they couldn't be ignored any longer. You just couldn't get by with ordinary differential equations anymore.

AIKEN:

I don't know if Johnny ever referred to that in a publication or not.

TROPP:

I never ran into any reference on publication, because that was a classified problem at the time. It wasn't until much later that people knew about the problem and realized its relationship to the Atomic Energy research that was going on at Los Alamos. I think other people referred to it because of the continuing research that was done. I think this same problem, or a continuation of it, was then run on ENIAC, or if it wasn't, then at least they talked about it as to whether or not they could run it on ENIAC. I don't know whether they did or not; I can't find any record of it because it was classified, and unless records were kept, there would be no way of knowing what some of these classified problems were. They either only had a few copies or somebody had published them.

But you told a later story about your connection with von Neumann— the AEC and this whole problem of computation within atomic energy.

AIKEN:

Well, Tom Murray was one of the commissioners of AEC, and he was very disturbed because the Atomic Energy Commission did not have sufficient computing power and they were dependent upon von Neumann to provide that. Tom was very dissatisfied, and he asked me to go to Princeton and investigate what was being done by von Neumann, and report back to him, and I did. Shortly thereafter, the Commission did go out and buy commercially available machines. He had the notion that they could never satisfy the Commission's needs with the machines of the class that von Neumann was building.

TROPP:

There were a couple built at Argonne, one for themselves, one for Oak Ridge, the MANIAC at Los Alamos. They were all copies of the Princeton machine with minor modifications.

AIKEN:

That's right. But even if they had been completed and ____.

TROPP:

Well, they were also farming out problems. I know because I mentioned this group at Northrop that eventually built things like MADDIDA and went of into CRC (Computer Research Corporation). But in their computation laboratory, they were doing atomic energy problems that were being farmed out to them, and I'm sure that AEC was looking for anyone who had computational ability to do problems for them, because their needs were so great.

AIKEN:

Well, for years, they actually ran Mark I.

TROPP:

I talked to people at Argonne who were involved in building the AVIDAC and ORDVAC, their first machines, and I guess the pressures for computations in terms of nuclear reactors were fantastic. They just had to have this equipment, and there was nothing commercially at that time that they could buy. The Univac's were not yet available. All of the other machines were one of a kind, government funded. There was no place where you could go and buy a machine. If you had a commercial operation at one of the universities, which a university is not capable of doing, I guess you could have sold as many machines as you could build.

AIKEN:

Well, there were times when we had requests for about two or three machines at the same time.

TROPP:

Universities are just not set up for that kind of thing.

AIKEN:

No. You should never do in a university anything that can be done or will be done somewhere else.

TROPP:

I was talking to Professor Gottlieb at the University of Toronto. I mentioned that they had a pilot project machine that they eventually scrapped when the first Ferranti became available for purchase, which they then bought. There was a big battle between a number of people within the department building the machine, some people from Chalk River (their Atomic energy group) over whether they should go from this pilot model to a production model. Gottlieb says to this day he's convinced they made the right move, because universities should not be in the business of production model computers.

AIKEN:

That's right. Absolutely not.

TROPP:

And of course, you must have made this decision fairly early yourself when you said that Mark IV was going to be it.

AIKEN:

Yes, it was going to be the last machine we built. That's right.

TROPP:

I'd like to back off to the Marks I, II, III and IV. We've talked about Mark I at great length, and I think about the only remaining broad questions I have on Mark I, are the early problems that you had connected with Mark I, getting it running— in a sense, a shakedown cruise of Mark I. Were there any problems that stand out in your mind as being significant, or were they just the problems one would expect with a device that large?

AIKEN:

There is he one that stands out in my mind because of particular circumstances. We had a number of twelve by twelve sets of variables ___________________________.

TROPP:

Were these from Leontief?

AIKEN:

No, these were from a man named Freedman.

TROPP:

This was the NSA director, or is that another Freedman?

AIKEN:

This man was an economist.

TROPP:

Oh, that's right. I know that Freedman. There was a Freedman who was head of the NSA, and there was another Freedman, who was an economist.

AIKEN:

At Chicago. At about 2:00 in the morning, after it wouldn't work, and I called two or three other people, and they went to the laboratory, and they called,_______________________________, and it was a shambles still.

So later we found that when one of these sets of simultaneous equations ____, we went one from the other and the whole thing was a mess. Well, we started playing with these confounded things by hand, and discovered that there were at least two rows of _______________________, at least two and sometimes more, that were directly proportional by a constant 59.89. Now obviously, 59.89's were 60, they were represented by some 10 million computer machines, and to this day, I believe, maybe not to this day but up until a few years ago, that if you had gone into that laboratory and asked anybody what was the value of Freedman's constant, he would tell you, "59.89."

TROPP:

(Laughter.) So he put two equations in that were identical and proportional, and there was no way you could run it. That was like trying to divide by zero.

AIKEN:

Yes, _________________________________. Of course, when you divide by zero on a computing machine, you get a peculiar hash as a result. Then, when you attempt to perform check procedures, all you've got are big numbers ______________________.

TROPP:

I heard about dividing by zero. I heard a humorous story last week that Ida Rhodes told me about the SSEC, I think it is, demonstration, but I haven't verified it yet. I'll put it on tape because she didn't. At the demonstration, where she knew so many of the people there, when she came down to look at the machine one of the engineers was telling her about its characteristics and said. "We've even fixed this machine so that it can divide by zero".

She tried to get away from there, and there was nothing that he would do but that he had to demonstrate it. She kept trying to shut him up and go away, you know, "Don't prove anything to me," until finally they insisted on showing her. Nobody else had quite come down yet. That was the end of that machine for that demonstration. They tried to divide by zero and it just went puff! I guess from there on in, they just had to fake it for days, because that will do it every time.

I'm not sure if it was the SSEC or one of the other big dedications at IBM.

AIKEN:

I was just wondering if I could find these confounded simultaneous equations.

TROPP:

That's Milton Freedman, isn't it?

AIKEN:

Milton Freedman, yes, that was the guy. (Reads): "Fire control, third class."

TROPP:

(Reads): "Paul B, Toggins."

AIKEN:

"Relation of a certain integral". This is a report on <u>preparing</u> a code book to assist in using the machine. (Laughter.) And this is the code book. Oh, yes, that's where we program to take a square root. Now, here's one on multiple regression.

This is Bob Campbell: "Evaluation of a Certain Function. Rotation of coordinate axes in the Solution of fire control problems." Here are some more on that subject.

This is a joker I got when I asked for all the Ph.D. mathematicians and M.A. people. I got him and I got Hopper. This guy wasn't worth a damn, and after I got him, I couldn't get rid of him. I used to write letters saying, "___________ _________________, but a really good officer, I don't need." They'd say, "What's his name"? I'd say, "His name is Hubert Arnold."

"OH, yes, Arnold. Well, we don't need him."

Here's another integral, and there's Dick's problem__________.

And here for the first time, in November of 1944, we begin to hear about the kind of machine that is going to be built for … this is Mark II. So here is a description of Mark II as seen from a very early day.

TROPP:

And that date on that, again, is November of 1944.

AIKEN:

This is Copy #2, by the way.

TROPP:

The only names on it are yours and Bob Campbell's.

AIKEN:

That's right, yes. (reads): I can't even remember what that's all about. It's apparent that the pages were bound upside down. "Certain Functions of a ___________ a progression of _____________________". Now, who the hell?

TROPP:

"Bob Seeber and Hubert Arnold."

AIKEN:

Yes, but I can't remember anything about it.

TROPP:

"Ruth Treadwell".

AIKEN:

"Magnetic Use of Strength from a dipole." "Ten Place Tables of Certain Functions". "Fifteen Place Table of E to the Minus X. " God knows why, we were required to do that.

"Six Place Tables of Certain Besell Functions." "Add the computation reciprocals and the reciprocal square root and of some transcendental functions." Now what in the hell have we here?

TROPP:

It might say something in the first paragraph: "Involves some fundamentals involving design of proposed automatic…". So you may have been going into the feasibility of doing these computations in the base two.

AIKEN:

Well, it looks like it.

TROPP:

This is all preparatory to Mark II.

AIKEN:

Yes. "Solution of Heat Sensitivity equations in the Case of Spherical __________." Why had we wanted that? "Dick Bloch".

TROPP:

Now this problem is Grace Hopper's, because she gave me this one. ______________.

AIKEN:

Yes. You see, this thing later became a volume of the annals. "Certain Functions in the Design of Besells." "Bill Blick". Blick is now in a graduate school in Los Angeles.

TROPP:

B - O - B - I - C - K?

AIKEN:

Yes. Here's Albert Werthheimer… Grace Hopper.

TROPP:

She's given me a good bit of this material and all of her notes and hand work in its crude form and some of the first printouts that she saved some of.

You may not find this Freedman thing. The machine may not have ever recovered from it.

AIKEN:

"_______________Electronic Photographic Printer of ___________________"

TROPP:

This must have been for Mark II, or did you do that on Mark I?

AIKEN:

I believe this was an invention of mine.

TROPP:

The high-speed printer.

AIKEN:

Let's see. No, this was an invention of Harrison Fuller. This was one of the means of printing with cathode-ray tubes.

TROPP:

Now, was that intended for Mark II or had you planned to add it to Mark I, to replace the …

AIKEN:

It was started for Mark II and never used, and then reconsidered for Mark III, and also never used. These were circuits which generated the digits, by golly, multiple gate development. You see, people who say we didn't like electronics are in a little bit of trouble, because here it is, you see.

TROPP:

Yes, and the date on that is 1946, I think.

AIKEN:

No. Let me see, what is the date on this? "On the Meriscope". 1947.

TROPP:

January, 1947. So I was really only off by a month.

AIKEN:

Of course, this had to be going on for some months. So, here was an attempt to print electronically, and then there were multiple gates also in 1947. So you see, we were not so completely stupid.

TROPP:

It's really curious, when you stop to think about the misconceptions that have been prevalent for two and a half decades now, about whose positions were what, when, and where.

AIKEN:

Our position was that, "By god, we had to have complete machines and they had to compute". And within that framework, we didn't give a damn whether we did it with carpet tacks or electronics or relays or what. It didn't make any difference. (Break in tape.)

TROPP:

I've heard about a number of controversies with Norbert Wiener on a number of issues. One, and I think it was Dick Bloch though it may have been Bob Campbell, reminded me of or mentioned to me, was over input. Wiener said that punched cards were absolutely obsolete. This would have been about 1947. I think it's Bob Campbell who said that you had a major conflict with Wiener over this.

AIKEN:

Well, the statement that punched cards were obsolete then was even more ridiculous than to say that they are obsolete today, because they aren't. For input, you're either going to use cards or you're going to use tape. Cards, being discrete, had the advantage that you can separate them from the deck, you can mail them away, you can put them in a string and you can sort them and re-order them. You can't re-order a tape. You can, of course, take the data off of it and put it into a machine and rearrange it and make another tape, but you can't order or change a tape, per se. Now, cards also have the advantage that when they are in a drawer, they are subject to random manual access as well as serial machine access. Tapes are available for serial access regardless of whether it's manual or machine. So if you simply think it over a little bit, you'll find that because we've had bright, shiny magnetic tape, we had insolvable problems. So it's a good thing that we've had cards to do, and I suspect that there will be for a long time.

TROPP:

What was Wiener's position? Why was Wiener upset about the continued use of cards? Why did he feel that they were obsolete rather?

AIKEN:

He was upset because magnetic tape had been invented and people were still using cards, and people who wouldn't give up on an old-fashioned procedure and go to new ones were stupid. So this was the basis of the argument. Wiener was not very well informed on what he was talking about positive way when he made his assertions that cards were no longer any good.

TROPP:

This sort of prompts me into getting you into a philosophical discussion. We often talk about the positive things— the milestones that led to certain developments. Another way to look at computer developments are in terms of some of the mistakes we made, which ones we recovered from, and which one we are still living with.

AIKEN:

Well, cards are an excellent example of a mistake that we made. We had cards in ten positions here— zero through nine— and we punched one hole, which represented a decimal digit. Now, if we had used four holes, we would have needed four/tenths the amount of paper, and by reading combinations of those four holes, we could have read decimal digits. I don't know how many billion cards have been produced operating on the one-out-of-ten coding system in the ten decimal digits. I suspect that the amount of paper wasted was sufficient to sacrifice an entire forest because of the stupidity. And we're still doing it; you still don't see any coded cards other than using one or two holes for a representation of the alphabet, which really is twelve holes in the cards. So, it's with that exception, but we still represent the decimal digits with poking one hole out of ten.

TROPP:

So, we're still really using the 1890 Hollerith system, without any fundamental change. So, you indicate the mistake we made was not really changing the way in which we look at how information goes into, digital information on cards.

AIKEN:

Of course, I will note that in all the equipment that has been designed had to be supplied with cards, but that's no reason why we couldn't have changed the _______. That's one mistake.

One of the most amusing mistakes was an operating mistake. The way public utility bills were made, you punched the card with the last month's meter reading on it, and when meter reading were brought in for this month, you had to punch the meter reading for this month. You then took these cards and put them through a calculating punch that took the difference, and punched the difference between the two months of consumption on the same card.

You then sorted the cards onto consumptions so that all the one kilowatts were here and the two kilowatts and the three kilowatts and the four kilowatts and so on. You then knew, because you had pre-computed a table that you could read, the bill for one kilowatt and two kilowatts and three kilowatts, and so you took all the one kilowatt cards and the bill for one kilowatt, and you put those in a duplicating punch and punched the bill, and you did this all the way up to a hundred thousand kilowatts and so on.

Now, when programmable computers were available commercially to the utility industry, they initially programmed the machines to compute the electric in the same way. (Laughter.)

TROPP:

In exactly the same manner that you did them by hand?

AIKEN:

Oh, of course, they stored the table of the bill for n kilowatts, and the sorting was done on tape, but the programming procedure employed was exactly the same. It took a little while to convince them there was another way.

There was another amusing error. One wonders if couldn't have saved a good deal of magnetic tape if we'd gotten the trinary number system on tape, using a negative dipole for minus and a positive dipole for plus and no dipole for zero. In that way, we would have taken three digits to represent a decimal digit instead of four, and similar extensions to take care of letters _______. I believe that this search for speed was greatly overdone.

TROPP:

Don't you think that that's still an overdone hang-up that we have?

AIKEN:

Oh, yes.

TROPP:

I mean, up to a certain point, you know it's like the supersonic aircraft, we never said to ourselves, "Really, when we mean fast, how fast is fast enough?"

AIKEN:

Well, let's note that there are certain problems in defense and certain problems of nuclear physics in which the fastest computer we can build will never be fast enough.

TROPP:

Okay, we'll grant those special cases.

AIKEN:

As long as we grant those special cases.

Well now, as long as we are talking of commercial applications and the computational requirements of individual engineers and individual ventures, you don't need speed.

TROPP:

No, if you increase the speed so that I can get my two million computations in two seconds instead of one second, for me as an individual, you've changed nothing whatsoever. You know, two minutes probably would not have bothered me.

AIKEN:

Yes. Now, to that end, the electronic computers that I built were always serial machines. You have the same equipment, and the equipment that you don't put into a machine never fails. So there was a lot to say for the serial machine: less equipment, not quite so fast, cheaper, or simple. Of course, these little electronic hand boxes are all serial, so that's a throw back to the way we did it on Mark III.

TROPP:

Yes, but it is very fast.

AIKEN:

Well, a million megacycle is still pretty fast. I've always thought that building binary computers for commercial application was a mistake. You spend so much time translating from the decimal to binary number system and back that you would have been better off to do it in the decimal system. I argued that building binary computers is a concession to the designer to simplify his job, and the designer's job is done in a few months, but the user's job goes on forever after as long as the machine lasts, and really, it is his job that you are concerned with.

TROPP:

Would you still say that today— that for the commercial computers, the …

AIKEN:

Oh, yes. These little things are from the decimal number system, you see, and the designers managed to put the entire machine on two or three chips of ________________________ in each square, so …

TROPP:

Another interesting thing about those is that I remember looking at the early literature, the arguments over floating point and fixed point, and on many of these little compact devices, you have a choice. You can do your arithmetic either way.

AIKEN:

Yes. Well, amusingly, I designed a machine that would add and with end-around-carry, subtract, multiply and divide, and you needed only to push. You needed to respond to only one binary digit of information to tell the machine whether you wanted to operate in the binary number system or the decimal number system. I worked out the design of a machine to do that a year or so ago, more or less as an amusing exercise. Are you interested in that?

TROPP:

Yes, I am interested.

AIKEN:

Well, suppose I have a machine that can add in the binary number system, and let's suppose that's it's a serial machine and that I could provide it with quads of information— that's four binary digits of x and y representing something, you see. The sum of these four digits as a four-digit sum, is provided together with the carry, zero or one, for the next quads coming in. Now, I have now a binary serial machine operating under quad basis.

Now suppose the numbers coming in are decimal numbers. If there is carry from one quad to the next or if the sum of the quad is greater than nine, the sum of the two quads is greater than nine, recirculate the adder and add six, and this corrects the situation except now the sum is decimal form. So all you've got to do is provide the switch that says "operate by binary or by decimal", and provide for the recirculations of the adder for the second time or through a second, simplified adder. And now your machine will add binary quads or decimal in a four minor digital coding system.

Well, having learned how to add the way one multiplies it is very clear. Actually, one doesn't even need any specialized equipment. You use the scheme of multiplication in which you multiply the multiplicand by a number in which each quad digit is zero or one in the highest order that you can work out. And having effected that multiplication, you are multiplying only by zero or one now, you double the product. Now then, multiply that by another multiplicand in which the digits are all zero and one depending on the third digit. Having effected that multiplication, add that to the previous product and multiply by two. So the first product is now multiplied by four and the second group by two, and the next time around, you do it with a second digit. So the highest order of digits is now multiplied by eight, and the next by four, and the next by two, and now for the lowest order digit.

And this algorithm is equally satisfactory for decimal multiplication or for…

TROPP:

Well, it's really an expansion; that's all it is. The new math book is called "Expanded Notation."

AIKEN:

Yes. Now, since we have an adder that we can tell to add decimal or binary, we need only one algorithm for multiplication, and here we are.

Division is best done by the method of holding over subtraction. Subtract one in the divisor and then you keep on subtracting until you get to zero.

TROPP:

That's an interesting intellectual exercise.

AIKEN:

Yes, and you see, when people give me long arguments that the binary system is much better for scientific calculation, as I have been told by people whom I don't think know what they are talking about…

TROPP:

If I had to do it myself, it wouldn't be better because I'm not as vasselled with binary arithmetic as I am with decimal.

AIKEN:

But you see, if I can design a machine that will operate in the binary number system without almost no additional complications, then on what basis is one better than another?

TROPP:

Well, on the early machines, I think you could argue… for example, with relays, it was perfectly natural to think of a binary system as Stibitz did in his kitchen that day: "I can add one and one and one. I can add one-digit binary numbers with relays because of the on/off position." And I think in the early electronic machines, it's kind of a coin-flip whether you want to build all of this additional circuitry in to do it in the decimal system or whether you want to have all this additional stuff to convert it.

TROPP:

You don't want to take all the time to do it, programs to do it and all of that stuff.

TROPP:

Right. But once we got to the kinds of speeds and methods we have now, I guess it's not even a question of better or worse, I think it's just a question of the rut that we're in, and the programs for conversion are so standard and so easy that's it's just easier to keep going the way we are then, as you say, to move over and do it. Since everything is in the decimal, all the input and output anyway, just keep it decimal all the way through, would be out of a different kind of a rut than the one we're in.

AIKEN:

Well, my choice would be to make it decimal from beginning to end. For commercial machines, for the machines that have to be commercially or applied or for individual use, I would use serial. Throw away as much speed as possible to save the equipment and simplify it well, that's quite a list.

TROPP:

Well philosophically, how about some of the other areas where you think we went wrong. When I say where we went wrong, I guess I'm thinking of the industry in general in the last two and a half decades.

AIKEN:

Well, I think the place where I would change it if I could, I would express all programs algebraically, and I would extend what we ordinarily think of as algebra to include the manipulation of inequalities and signs of inequalities— lesser than, greater than— this sort of thing.

TROPP:

To give you the proper branching and choice functions.

AIKEN:

And instead of regarding all of this as being something inherently associated with machines, I would regard it as part of the province of algebra, and I would teach people to think in terms of these signals exactly in the way in which they think in terms of addition and subtraction and multiplication and division.

TROPP:

And also in the way that they think of the truth tables for and or disjunction.

AIKEN:

That's right, and with that all done, I think I could lay the groundwork to make programming ____ rather than manipulation of ___________________________ that happen to be on a certain IBM punch.

TROPP:

Well, I have seen one attempt to do this in terms of mathematical symbols, and it's in the system that's being designed and installed at the University of Santa Cruz. Harry Huskey gave a paper on this last fall, and at lunch, he sat down and described to me the way a student, say, taking a course in advanced calculus, what he would keyboard in order to solve certain standard kinds of problems, and it was almost totally algebraic. Now it had no name attached to it.

AIKEN:

Did you ever run into a coding machine of Mark IV?

TROPP:

No.

AIKEN:

Well, Mark IV had a coding machine, and it was a coding machine with a keyboard here, and there was a key here that said "plus" and another one that said, "minus", and "plus absolute" and "minus absolute", and then, "A", "B", "Z", and obviously, "zero" and "I" were omitted. And then, "Zero, nine" and then"plus H, plus C times H times C", and then "divide" and then "cosine", "sine" and "minus one into x","log x", and a few more. And then, "A to Z, zero, nine equals A, Z, zero, nine." Then, there was one here called "Sigma Pi" and there was another one over here called "Sigma Pi." Now, there were twenty of these _______________, and the registers in Mark IV were 200, and every register was designated by a letter and a decimal digit. So you could say, "Take the number out of the register designated by A and three, and add that to the number in the register designated by B and five and put the sum into the register designated by C and eight."

END OF SIDE I, TAPE III TROPP:

We are looking through some dissertations, and this is Warren Seaman's doctoral dissertation on let's see what the title of this is.

AIKEN:

"Application of Matrix Methods in the Theory of Switching, 1954." Seaman observed that Nagoshima, Petshule, Dusschick, Heish, Shannon, Montgomery, and Guthrie, all got interested in switching about the same time.

TROPP:

Yes, in the latter half of the thirties.

AIKEN:

Yes. And he goes on to say that to represent the two states of a switch, Shannon used zero and one, somebody else used zero and infinity, and someone else used plus and minus and so on. And that other than, the application of two-valued logic or two-valued algebra to switches, was more or less done by the people at the same time. Shannon did rather more about it than any of these other early people.

TROPP:

Yes, but nobody really picked it up until the forties.

AIKEN:

Until we started working on it pretty hard. This is as good a discussion of these matters as I know of, and he also put together a bibliography on the subject which, for the time, now Warren had complete. You see it. And not only do you see is there the entrance but he's got a …

TROPP:

He's annotated the bibliography. Yes, I'd like to get a copy of that, and I'm sure that I can get a microfilm copy from Harvard.

AIKEN:

It's a nice approach to Seaman for you.

TROPP:

Would you mind commenting on Herb Mitchell's next, which we mentioned earlier?

AIKEN:

Herbert Mitchell's thesis was the shakedown…

TROPP:

That was the very first, wasn't it?

AIKEN:

Yes. This was the shakedown problem for the Mark II calculator. What he did was deal with some matrices provided by Leontief, arising in his input/output theory of economic research. I suspect that this was really the first— I don't remember what the order of those matrices were, 35 or 40 or something like that— attempt made to invert and deal with fairly large matrices on an automatic computer.

TROPP:

Well, I think you're right. I think this is the largest system, at least it's the largest that I have run into, the date on this is 1938, and up until then, I don't think anybody had gotten to 20 by 20.

AIKEN:

That was Mitchell's work.

TROPP:

The other interesting thing is, and this is where I could check it with other material, is to compare it with the von Neumann 1936 paper which deals with the difficulty of doing this with the required precision, because of a high predictive round-off error, which, if I remember right, Mitchell's work shows will not occur.

AIKEN:

I believe that's right.

TROPP:

Do you want to pick another one?

AIKEN:

Sure.

TROPP:

There's Mennick's.

AIKEN:

Mennick was interested in using magnetic cores for performing logical operations, and he finds ands and ors and inverts, and writes out the conical form of switching functions in the same way you would do it if you were going to recognize the functions by more conventional circuit elements. He approached the possibility of building computing circuits out of magnetic cores alone. Now it's interesting to note that in his recent work on magnetic bubbles, he has considered doing logics with magnetic bubbles, and he gave a paper on the West Coast not very long ago suggesting the preliminary outline to build a whole computer using magnetic bubble techniques. So Mennick's thesis not only has been completed, but really, it's working in a field by magnetic logics is still going on and he's still leading that field.

TROPP:

What's the complete title and date of this thesis?

AIKEN:

This thesis is: "The Use of Magnetic Cores as Switching Devices", and the date is 1953.

TROPP:

And he is still pushing in that same direction, except now it is magnetic bubbles. That's fascinating.

AIKEN:

Yes, it was twenty years ago.

Robert Ashenhurst is a professor of mathematics in computers (or whatever it is called) at the University of Chicago. He worked almost exclusively in switching theory. He is an excellent theoretician, and his thesis deals with a number of problems of different kinds of circuit elements and solution of logical problems, using coincident circuit cores, __________ fires and so on.

TROPP:

And what is the date and title of his thesis?

AIKEN:

His thesis is entitled: "The Structure of Multiple Coincidents Selection Systems", and it is 1956.

TROPP:

1956. So this is early in the application of …

I ought to state for the record that I've asked you about when you first started teaching a course in switching theory, and I guess it must have been when I first saw it in the catalogue which is either 1946 or 1947.

AIKEN:

Yes, it was pretty early. I believe the course was first called, "The Organization of Large-scale Computing Machinery." That course began with and dealt with exactly what the course title had in mind. I put together how you organize computers, what the different parts were like and what you could do with them.

TROPP:

I think it shows up in the catalogue the first full academic year that you are back at Harvard in the Navy.

AIKEN:

Then, that course began to convert itself into switching theory. The first time the course was given, it was a description of what you did, and then as we became more proficient in the application of switching theoretical techniques of designing computer machinery, it became more of the switching theory nature until finally it dawned on me: "Well, this is really a course in switching theory". So the old courses on the organization of digital computing machinery was dropped and now it was given an honest name of switching theory. It continued on that basis until I retired.

Well, let's see. Salton wrote an automatic data processing system for public utility revenue accounting in 1958. I might say that the Edison Electric Institute with the Trade Association of Electric Utilities gave us a research contract to work on applications of computers to the public utility accounting problems. Salton was one of the principal investigators of that, and he got his thesis out of it. As a result, he became quite well known in the utility industry, applying computers to their problems.

Then, he graduated from this interest to problems of information retrieval.

TROPP:

Yes, and he is now one of the leading authorities in that area.

AIKEN:

At one time he concerned himself with computer programs for transcribing… what is this system that people use for recording dictation?

TROPP:

Oh, Dictaphone.

AIKEN:

No, not Dictaphone.

TROPP:

Oh, I know what you mean. The Gregg?

AIKEN:

This was a device used by court reporters.

TROPP:

Yes, I know what you mean, but I don't know what it's called.

AIKEN:

So, that was Salton.

This one is "Switching Functions Nautical Forms over Integer Fields", 1960, Martin Cohen. This is an advanced investigation in application of switching theory to a number of problems.

TROPP:

Where is he located now?

AIKEN:

I have no idea where he is.

"Logical Designs for Arithmetic Units", Henderson, 1960. Henderson came from South Africa and returned there. I don't know where he is or what he is doing there, but he concerned himself with a number of ways of approaching the problem of designing arithmetic units.

Richard Gould, 1957: "Application of Graph Theory to the Synthesis of Contact Networks." Gould was probably the best theoretician.

TROPP:

That's an almost abstract mathematical problem.

AIKEN:

He was probably the best theoretician I had as a student. His thesis is almost all graph theory. He was a man who I fully expected to distinguish himself greatly. Unfortunately, he went skiing in the Alps so we lost him.

TROPP:

Because graph theory right now is one of the hottest areas of research.

AIKEN:

Here is "Digital Calculators with Solution of Ordinary Differential Equations", Alonzo, 1957. I don't know where this boy is any longer.

TROPP:

What was the motivation of a thesis for that particular thesis because it's interesting for that particular time?

AIKEN:

You mean, why did he write it?

TROPP:

Yes. Was he interested in something at that particular time?

AIKEN:

Well, there are certain differential equations in physics, wave equations, heat equations, and these occur with great frequency and over and over again. And he was interested in building machines which would deal with these equations alone. I guess this was an attempt to build a special purpose computer. The work was good and the results were interesting, but I've already expressed myself on the special purpose computer.

TROPP:

Right. That's why I was curious about that, particularly at that time.

AIKEN:

Here is"An Investigation of Non-olnic Resistance Resistor Switching Network", by Hopkins in 1957. Hopkins in now in fact at MIT.

TROPP:

What's his first name?

AIKEN:

Albert. You can build a machine out of any non-linear device. Of course, the ideal one <u>is an electrical device</u> like this, but if it isn't like this, it's only like that, you can still do it. Al Hopkins was concerned with how you did logics with non-linear devices which were non-square wave and what this did to the reliability and accuracy.

"Linear Programming with Pattern Constraints." This is Eastman, 1958. Eastman, if I remember, went to work for Sperry Rand. I don't know where he is now.

TROPP:

What's his first name?

AIKEN:

Willard Lawrence. That's hard for me to remember because we always called him Bill.

TROPP:

This is an interesting spectrum of topics.

AIKEN:

"Numerical Solution of Differential Equations". This is Hayes and this is one of the earliest, 1950. Miles Hayes is professor of applied mathematics and director of the computing laboratory at Dartmouth.

TROPP:

At Dartmouth? That's right, you asked me if he was working with Stibitz and I didn't know, because I had not run into any of Stibitz's colleagues. I'm sure they know each other, but whether they are involved in similar areas or not, I don't know. When I next see Stibitz, I'll have to ask him.

AIKEN:

Here's Singer, 1954:"Class of Time Sequential Circuits." This is a thesis on switching theory, particularly dealing with circuits which vary their behavior in time. Certain functions generated which goes back to the input and the thing re-cycles itself. We were interested at the time in trying to find ways of expanding time to save equipment - to take longer and use less apparatus. That's what this thesis was concerned with.

Here is Charles Coolige, 1953. He got his thesis out of Mark III and in particular, out of the arithmetic unit. Charles Coolige is the son of the Coolige who started the Harvard Corporation. His father was also the member of a very distinguished law firm in Boston, called Coolige, Robes, Best and Grey, colloquially known in Boston as Coolige's best grey robe.

Ashenhurst, we've already had. Now, here's Blau: "Application of Cylinium Rectifiers as Switching Devices in Mark IV Calculator." We used cylinium devices in the magnetic core storage, and I guess this was in 1952, so Blau got his thesis out of the design of Mark IV.

TROPP:

When did he go to Europe?

AIKEN:

He went to IBM from our laboratory and he was with IBM for quite a few years. I can't tell you when it was but it was some time at the end of 1952, when he went back to the Netherlands to work at the university there.

Here's Brooks.

TROPP:

Oh, yes, Fred Brooks.

AIKEN:

"The Hellenenic Design of Automatic Processing Systems", 1956. Brooks got his thesis out of the utility building operation. This was the inspiration for his thesis. He had a lot of other interests too.

TROPP:

Yes, he is professor and I think chairman of the department at North Carolina. I talked to him on the phone.

AIKEN:

"Study for the Design of an Automatic Dictionary", 1954. That's the Russian/English dictionary that was published in translation.

TROPP:

I didn't know that's what he did his thesis on. How did you happen to suggest this study to him?

AIKEN:

Well, at the time, mechanical translation of language was already beginning to be a new industry in computing, because people spent so much money to accomplish so little. So I suggested the dictionary as something that a machine could do very, very simply, and the output of the machine would be an excellent input to a one-language individual who wanted to affect a translation. That is, with this technique, I can translate Russian into English and I don't know any Russian. Now to be sure, every once in a while, I get into a snag and I have to go to a linguistics to help me over this, but there it is.

Then, coming out of that was the notion that if everybody translated what he wrote into what I might call machine language, then translation from one language to another would be very greatly simplified. You see, if there are n languages, you need n squared translating procedures, but if you define machine language, then you need only two translating procedures; namely, into machine language and out machine language for every other language.

Note that there is only one sentence that you can write and that one sentence is the following:" The subject of this sentence is ___________(blank), modified by the following adjectives: _________, __________, _________,_________. (blanks) The action in this sentence is ____________(blank), modified by the adverbs _______,_______,_______,_______. (blanks) The object of this sentence is…" Now, why do I go on. You see, you can write a very long and awkward paragraph which tells you every sentence that you can put in to it. Now, if you knew how to do that for Russian, into this machine representation, then you get from the machine representation to English by simply translating this into a reasonable English. So then a translation is then effective, dealing with the idiosyncrasies and the rules of one language at a time and not in pairs. That was another part of this.

TROPP:

There is a marvelous quotation here from Warren Weaver that I've not seen before: "When I look at an article in Russian, I say 'this is really written in English but it has been coded in some strange symbols. I will now proceed to uncode it.'" That's exactly what you were saying.

AIKEN:

Warren is an awful lot of fun. I mentioned to you that he took my first course in calculus.

TROPP:

Yes, at Wisconsin.

AIKEN:

Warren was a punster, did you know that?

TROPP:

No, I didn't.

AIKEN:

According to Warren, there is only one triple pun in the English language.

Here's Iverson: "Machine Solution of Linear Differential Equations: Applications of a Dynamic Economic Model." Again, he's fiddling around with the idea, and he says that with Mark IV, he used Mark IV in the solution of a new, dynamic economic model and it continues the work of Dr. Herb Mitchell; that's the first one.

TROPP:

Mitchell's is 1948 and this is when?

AIKEN:

1954.

"Approximation of Ratios and Integers Chosen from a _______ Range," Martin. Martin is professor of computers at another North Carolina school. This thesis has some pretty high-powered major work in it, pertaining to the design of gear teeth.

TROPP:

Oh. I thought it was number theoretic, but they are closely connected.

AIKEN:

Gear teeth have an integral number of teeth, and you can build gears with 8, 10, 12 teeth, up to 120,130,140 teeth, or something like that of one design. And any gear ratio that you want has to be obtained by taking two of those gears and substitute your ratio. And with a two-gear ratio, all you do is limit all possible ratios of integers, arrange these in ascending or descending order, take the ratio you'd like to have and find the closest approximation and read off the two gears that give that. Now if that doesn't do it for you, what you do is put two gears on a shaft and you drive one of those gears on this shaft with a gear over here, and let the second gear on this shaft drive a fourth one. So you now have a two-geared range, each of them is a pair a gears but the number of distinct overall gear ratios that you can make is rather more complicated. You can't list all of these because there's just too god damn many.

What Martin is concerned with here the fine continued reactions.

Now let's see here. We've already got Mennick, Mitchell, Gallinger is multiple output…

TROPP:

Now for that, you're going to have to spell the last name on.

AIKEN:

C - A - L - I - N - G - A - E - R - T., 1955. That's "Multiple Output Relay Circuits."

TROPP:

(Laughter.) Did he draw that cartoon in here?

AIKEN:

Yes.

TROPP:

That's marvelous.

AIKEN:

This is the computer lab.

All right, that's it.

TROPP:

This covers the theses.

END OF INTERVIEW