Dr. Franz Alt

Oral History by Atsushi Akera

Session One, January 23, 2006

[Begin Tape 1A]

AKERA: I'm speaking today with Dr. Franz Alt, a former member of the National Bureau of Standards and a past president of the Association for Computing Machinery. If I am correct, you served as the president of ACM between 1950 and 1952?

ALT: Yes.

AKERA: I understand that you have participated in a number of earlier oral history interviews, as well as publishing several pieces on the early history of the ACM yourself. While I understand that some of the information about your early life appears in these earlier accounts, I'd like to begin this interview by filling in some of this early history, primarily as background to your involvement with the ACM.

Part I: Early Life & Career

AKERA: So, could you first tell us something about your early life—where you were born and how you became involved with mathematical research?

ALT: I was born in Vienna, Austria. My family all came from Moravia, which is now part of the Czech Republic. Both my parents, and as far as I can tell, all my ancestors, came from a very small area in Moravia. They were a Jewish family that immigrated to Vienna because that was the way to move up. That was still the time of the Austro-Hungarian Empire. Franz Joseph was the Great Emperor.

AKERA: And Vienna was a somewhat more permissive for the Jewish community?

ALT: Vienna was the capitol. Sure. That was the place to go and to grow. My father, who was born in a small town in Moravia, had to migrate first of all to the capitol of Moravia where there were German high schools. Then he had to migrate to Vienna to go to the university. There was no university then in Bruno. There might be now. Prague is older than Vienna and has a university. But he didn't get there. I didn't get to Prague until just a few years ago.

AKERA: [Laughs] That's wonderful. What was your father's occupation?

ALT: My father was a lawyer. It was assumed that I would grow up to be a lawyer. (Oh, I should say I had one younger brother.) But I didn't want to be a lawyer. I rebelled. Somehow, children

don't get along with their parents. I didn't want to be a lawyer. My brother took over law. I, instead, decided to study something else. Somewhere in high school, we had a very inspiring teacher of mathematics, and that inspired me to get into mathematics.

AKERA: Were you the eldest son in your family?

ALT: I'm the older of the two. My brother died of cancer in '69, almost the same time as my first wife, whom I lost to cancer at that time within a few months.

AKERA: I'm sorry. Vienna was, of course, a very important center for mathematics—actually, not just a center for mathematics, but a cultural center and intellectual center.

ALT: I want to correct your written question here. Schlick and Carnap were not mathematicians. Gödel, yes. Gödel I knew very well. For a full doctorate at the university in mathematics, it was required to have two examiners from the philosophical faculty, and I chose Schlick and a psychologist, Karl Bühler, and two in my main subject, mathematics. I chose Hans Hahn, who was an outstanding mathematician in his own right. And the other one was my mentor, my thesis advisor, Karl Menger, who was an associate professor. So those were my examiners.

AKERA: What kind of work in mathematics did you do at the time?

ALT: At that time, there were three full professors of mathematics, Wirtlinger, Furtwängler, and Hahn. The way it went, it was required in one's first year to take a yearlong course in calculus. Any mathematician, any physicist, most medical doctors, most pharmacists had to take that course, and the three full professors alternated in providing that course. It happened that in my first year at the university (that was '28 to '29) Wirtlinger was the one to give that course, and his field of interest was complex function theory. Furtwängler was algebra and number theory, and I knew nothing about that and never have. Hahn was the one who was most interesting to me, but because of conflicts—some other lecture given at the same time—I never heard Hahn present a lecture. It was unfortunate but true that some lectures were required and other lectures were interesting.

Each of the professors had to furnish one year of calculus and then two years in his own interests; in addition, they taught a seminar two hours a week. Of the two professors, Wirtlinger and Furtwängler, had seminars that were like very advanced lectures in their own fields. But Hahn had a different kind of seminar. In his seminar, he would select a book that had just appeared and would divide it up among the participating students. Each student had to report on one chapter. That was the way he ran his seminar. I thought I would take Hahn's seminar, even if I couldn't hear any of his classes. There was an announcement, "If you want to enroll in Hahn's seminar, see Prof. Menger at such and such time." Why Menger? Because the other two professors, the older ones, had assistants, but for Hahn there was no more money for an assistant, so he used Associate Professor Menger as a sort of assistant. Menger helped him around his seminar. That's how I met Menger at the beginning of my second year and enrolled in the seminar and had to give two seminar meetings. That's how I became known in Vienna. That's how I was one of the students that the professors knew about.

AKERA: In what area of mathematics, did you do your work in then?

ALT: Hahn was interested in the modern real functions. That means point-set theory, a lot of symbolic logic. Carnap, incidentally, gave the course on the philosophical foundations of mathematics, which was something introduced by Bertrand Russell, Russell and Whitehead--

AKERA: Yes. It was a very active topic at the time.

ALT: Yes. I took that course, and was fortunately able to hear Carnap, who went away the following year and never came back to Vienna. He spent a long time on the West Coast here in the United States. But his main contribution was certainly not mathematics. It became semantics. That's what he became most famous for, Carnap.

Just shortly before I left Austria in '38, I was asked to teach a course in symbolic logic, because by that time Hahn and Schlick were gone. I'd continued. A little later, Schlick was murdered, incidentally. Schlick was murdered in 1934. Hahn died, very unfortunately, in '36. Both these were times when I was out of the university already. Hahn had been in the first World War in the Austrian Army, had been shot through the lungs but somehow was saved and stayed alive. But in '36, he suffered a sudden emboli that killed him. It was tragic, really. Menger cried when he heard that. There was no one left in fields like symbolic logic, but the university would certainly not have hired anyone else. The university was dominated by a faculty that was mostly, politically, right wing—extremely right wing.

AKERA: So they were political appointees?

ALT: They wouldn't have hired anyone like that. Hahn had just barely gotten in at the end of the first World War. They didn't want to give up. There were some students who didn't want to give up on symbolic logic, but they couldn't do it within the university. So they asked me to take over a beginner's course privately to educate a—I can't think of the German word [chuckles]—a following.

AKERA: So by 1934 or '36 or so--

ALT: That was '38 now. Yes. Hahn had died in '36, otherwise they would have asked him, naturally.

AKERA: So then, you had really advanced into becoming part of the group of instructors at the university?

ALT: I couldn't do that at the university. I was not even a privatdozent. But in somebody's apartment we assembled something like a dozen students, and I began to teach the beginnings of symbolic logic.

AKERA: When did you receive your doctorate?

ALT: That was '32. The course which I gave in '38 was interrupted by the Nazi invasion of Austria. Now my memory really fails me. I remember it was the 15th, I'm not sure February or March of '38, the Nazi invasion of Austria.

AKERA: That was, in fact, my next question, which was whether you could tell us something about the specific circumstances surrounding your decision to leave Vienna?

ALT: Yes. The Nazis had come to power in Germany in '33, and Austria was infected by that. There was a growing pro-Nazi movement in Austria, but it wasn't there yet. They weren't in control. In fact, a strong Catholic movement that was opposed to the Nazis took power in Austria in '34 and lasted until '38 when the Germans marched in. We had some advance warning on that through a British friend of my late wife's, then my girlfriend, who had been in London, had spent a couple years there, and knew people there. One of them warned us. They knew long before we did that the Nazis were planning on invading Austria. Having this advance notice, we both could apply for an American immigration visa before it really happened. There was only a limited quota at that time for immigration, but being early on the application list, we were both able to emigrate within a few months.

AKERA: After the invasion?

ALT: After the Anschluss. That's how I got in right quickly.

AKERA: Did you come directly to the United States?

ALT: Practically. I mean, we spent two weeks, I think, in Switzerland with my wife's friends. We had gotten married just before we left Austria. Thereby hangs a story.

Vienna was now under control of the Nazis, of the Germans, and nobody knew who was in charge, who could even issue a marriage certificate. I was still in my old office. I was working for an insurance company, for an Italian company, by the way—their Austrian office. One day, my future wife called me at the office on the phone and said, "I have just heard from Rabbi Schwartz." He was the father of one of her best friends in school. "He's still permitted to perform marriages, and they would be recognized. So if you want to, we could get married this afternoon." I said, "All right." I hadn't given it any thought before then. "I'll meet you at three o'clock after your office hours are over at such-and-such corner in Vienna," and we walked over to see him at his synagogue.

AKERA: May I ask the name of your first wife, just for the record?

ALT: My first wife, Alice Modern. She was from a family that was much more interesting than mine. I would like to spend the next half-hour—I could, but I won't—telling you about her. That was a family that had emigrated from Vienna shortly after 1848. I say *1848*, when there was a string of revolutions running across Europe, first in France, then in Austria. They failed. They were suppressed with the aid of Russian troops to everyone's unhappiness. There was a woman in that family, Hirschl, who was unhappy. She said she didn't want her sons to grow up as cannon fodder for the Emperor, and she wanted to emigrate to America. She persuaded her husband to move. So they went to America. But after about 20 years, that was then after the American Civil War, it wasn't so nice here in America anymore. Meanwhile, Europe had grown more liberal, and so they returned to Vienna.

AKERA: Were they a fairly well-to-do family?

ALT: They had been an old Viennese family, yes, up to then. Oh, my goodness. They had been all over the monarchy, wherever they are of German-speaking islands within that monarchy—Agram in Yugoslavia, Aradin Romania, Budweis in Czechoslovakia. God knows...

AKERA: They were associated with all those families?

ALT: Yes. They were all intermarried, but they had settled in Vienna. That's that family 150 years ago.

AKERA: And were they also of Jewish descent?

ALT: Oh, yes. At that time, there wasn't so much intermarriage, but--

AKERA: Dr. Alt, you mentioned to me that they may be another interviewers coming here, who will be interviewing you more extensively on these aspects of your life. Actually, I would also be very happy to come back and interview this part of your life in much greater detail. But for the purpose of today's interview, I might move us a little bit more quickly to your life in the United States--

ALT: Yes. But I just wanted to finish... Because my future wife had relatives half of which were still in America, and half of them in Europe, it was much easier for us to get settled here (in America), and we had much better information. They were a half-English-speaking family still, 100 years later.

AKERA: I am very interested in asking more about that. In a sense, part of her extended family was already in the United States and in New York City?

ALT: Yes.

AKERA: Were they in New York?

ALT: Right. One was. Well, the family center had been in Davenport, Iowa. But by that time, over 100 years, they had spread over to Chicago, New York, Boston, various places. When we arrived by boat in New York (there were no flights at that time), one of my wife's cousins was on the pier to receive us. We waved "hello" to him.

AKERA: That's so reassuring, to be able to come to somewhere where there is a familiar face.

ALT: Now I have gotten ahead of myself. Let's go back.

AKERA: If we can go back to one question in particular, you mentioned that you were working for the life insurance industry in Vienna. Can you tell us how it was that you wound up with a job more in the business end things—in applied mathematics?

ALT: Thank you. I will come back to that. Partly under the influence from neighboring Germany, there was so much anti-Semitism in Austria at that time, and especially at the university where the student organizations were all right-wing, that I was warned if I aspired to

an academic job, that was hopeless for me, that they would never hire a Jew. [Chuckles] I don't consider myself a Jew.

AKERA: You were not a particularly active, practicing Jew at the time?

ALT: No. I'm not a practicing Jew, just ethnically by German definitions, by Nazi definitions. I had been warned not to expect an academic appointment. I'd have been satisfied to become a high school teacher, but even that was impossible, not so much because of anti-Semitism, but because we were in the middle of an economic depression worldwide that hit Austria much harder than almost any other country. Austria had lost the First World War, and that meant an economic depression. Austria had become a remnant, a very small country, but all the center of government functions of that whole empire were concentrated in Vienna, so there was enormous unemployment now, suddenly. All this contributed. Vienna was hit much harder than anyone else by the world economic depression. So I couldn't even get a job as a mathematics high school teacher.

The mentor of my doctorate, Menger, felt a bit guilty. He felt that he should be doing something for me. He had a good friend, Oskar Morgenstern, who was an economist—not a mathematical economist, but an economist. At that time, economics at the University of Vienna was taught in the Faculty of Law, strangely. *Rechts-und-Staats Wissenschaftliche* was the title—Faculty of Law and State, Public Sciences. That was where Oskar Morgenstern was. He had obtained a Rockefeller Grant from here in America. The Rockefellers and other people were so interested in the Depression and its unique aspects here, and they donated a lot of research grants for research in economics—on the sources, the reasons, for the Depression and more generally on economic cycles. He had received a grant from the Rockefellers for establishing an institute for research in economics and business cycles.

Branching off for a moment, Menger had other very outstanding students. Menger didn't have a seminar. He was only an associate professor. But he assembled something that he called a colloquium. That plus Hahn's seminar were important centers of modern mathematics. For instance, Kurt Gödel, probably the outstanding logician of the century, was a member of Menger's colloquium. The colloquium met about once every two weeks or so for an evening. In my second year at the university, Menger invited me to join the colloquium just because he had met me through Hahn's seminar. I had done a creditable job in reporting there, so he thought I... The other outstanding man to Gödel, Gustaf Beer, who got killed during the second world war. He remained in Vienna, but was killed in war with the bombing attacks on Vienna, very unfortunately. He would have made an outstanding mathematician. Olga Taussky, who survived, became Mrs. Olga Todd. She ended up here on the West Coast at Caltech. And, oh yes, Abraham Wald. Those were the outstanding members.

Abraham Wald was much better than I, and Menger first of all. Also, Wald was an immigrant in Austria. He had come from Romania, and Menger persuaded his friend, Morgenstern, to give Wald an appointment at that research institute. And that's how Wald got into mathematical economics, statistics. Wald did an outstanding job in whatever he touched. He very quickly became a professor at Columbia in statistics as soon as he came here. Wald was killed early in 1952. He had been invited to India to give a lecture tour on statistics and was killed in an airplane accident with his wife. He left two children behind here. And then the strangest coincidence of all, my first wife, when we moved back to Vienna after having been in Washington for many years, we looked around for a place to live, settled in an apartment

building a few blocks from here down the hill, and Wald's children had grown up there with Mrs. Wald's parents. [Laughs] I met them both here.

Back to where I ended—I'm unfortunately tending to branch off.

AKERA: No, this is very fascinating material.

ALT: Menger had persuaded Oskar Morgenstern to give Wald a full time consulting position. Morgenstern couldn't do more than that. But Menger still felt something should be done for me, and he persuaded Morgenstern—Morgenstern knew nothing about mathematics up to that point—to hire me personally to tutor him in mathematics a couple of hours a week. We began that by reading some books in mathematical economics. I remember Bowley was the author, but I don't remember the title. We'd both read a chapter ahead and then discussed it. That was a way to instruct Morgenstern in the mathematical aspects of economics.

AKERA: Did that work change your mathematical interests at the time?

ALT: That's how I got into it.

AKERA: But in terms of the mathematics, would the core mathematical work then be in statistics? Or were there certain ways in which your background in logic entered into the picture?

ALT: Logic would have been closest to the work in modern mathematics, but I had gotten away from that. I couldn't have gotten any paying job there. I'd gotten into statistics. I'd written something in statistics hoping maybe to find a job there. But I certainly wasn't interested in insurance mathematics. Still, I got a job in that somehow.

AKERA: Did you work in any other capacity while back in Vienna? You were a tutor to this faculty member and economist. But did you also, then, begin to work for an insurance company?

ALT: Yes, I had this job in an insurance company, which paid a salary. But I was really interested, still, in pure mathematics, still in symbolic logic. But then, more and more, mathematical economics became really interesting to me. Oh, I remember now an afternoon tea at Morgenstern's house to which I was invited. I met a whole lot of people there, among others, an American called Sweezy, a mathematical economist. His brother, Alan Sweezy, was a full professor of mathematics someplace here. But Paul Sweezy had a conversation with me for a while about something I had written for Morgenstern before that time. Paul Sweezy showed me a reprint that he was carrying from a Polish mathematician, an interesting person, Oscar Lange, about measuring the value of economic commodities. How does one assign a number to the value of this, or something such as a cup of coffee.

AKERA: Am I correct that this is the same Oskar Morgenstern that then eventually coauthored a book with John von Neumann?

ALT: Yes. And I knew John von Neumann quite well, but that comes later.

AKERA: Did Morgenstern's interest in mathematical approaches to economics—did it originate with your tutoring work?

ALT: Because I tutored him, yes, he got into it because I tutored him. That's right. It went from me to him, not the other way.

AKERA: Of course. And the work in economics that Morgenstern and von Neumann worked on, did it draw very much on logic? Perhaps not directly on mathematical logic, but did you introduce aspects of logic to Morgenstern at the time?

ALT: Yes, but the central problem of that book—what's it called, now? It's worth quoting. What's the title of that book?¹ Oskar Morgenstern and John von Neumann—that's one book by these two authors you can probably look up. The problem originated with Morgenstern. He had long wondered about something. It irked him. And as soon as he met me, he asked me, "How could we go about approaching this problem?" I didn't quite appreciate it. It didn't sound like a problem to me. Morgenstern consulted Abraham Wald on that. Wald, in turn, consulted a man who was a banker, a man named Schlesinger in Vienna who appreciated that that was a problem. They discussed it mutually, and Wald did have a solution. He had a meeting.

AKERA: Now, neither the Viennese nor the German mathematical research tradition was particularly recognized for applied mathematics, with the major exception of work on quantum mechanics and such at, Göttingen. But is this incorrect, historically? Was there the beginnings of an interest in applied mathematics in Vienna before World War II? Or was it an accident just because many mathematicians were forced out of the more abstract topics?

ALT: There really wasn't much going on in applied mathematics. But the very word "applied mathematics" had a different meaning then. At that time, when someone talked about applied mathematics—remember, I'm now way back in '38 or '37—it meant mathematics applied to physics—nothing else.

AKERA: Or astronomy, perhaps.

ALT: Fluid dynamics, things like that was applied mathematics. Nowadays it's much broader.

Oh, now, I mentioned Oscar Lange. He showed me that reprint that, if you want to assign a number to the value of an economic commodity, it has to have some properties. You must be able to say, "This is more valuable than that." And more than that, you have to be able to say, "If I exchange Commodity A for Commodity B, that exchange means more to me than if I could only exchange C for D." Now, Lange said if you had these two conditions, if these two things were ordered, both the value of something and the exchange—the difference between two—then that's sufficient to assign a number to every commodity by itself. He wrote that. I read that, and I was a very theoretical mathematician. That's not mathematics. That's not a proof, I thought. I began to write a letter to Oscar Lange. "That's no way to do it, but you have to specify, you have to stipulate, that the relation of greater, more, or less value is intransitive. If A is more valuable than B, then B cannot be more valuable than A. It's irreflexive." It must be transitive. A is more valuable than B, and B is more valuable than C, then A is more valuable than C. And a few

¹ John von Neumann and Oskar Morgenstern, *Theory of Games and Economic Behavior*, (Princeton, NJ: Princeton University Press, 1944).

conditions like that. I wrote this in my letter to Lange, and the letter grew to be 10 pages long. [Chuckles] And I realized I was writing a paper.

AKERA: Did you ever publish that?

ALT: Not only that—I was writing it in English because the original reprint had been in English, but I translated it into German and gave it to Morgenstern. Morgenstern was also the editor of an economics journal, the *Austrian Journal of Economics*, which was widespread. Oh, we had a famous Austrian economist who was then a professor at Harvard, Joseph Schumpeter. He found my paper in the *Austrian Journal of Economics*, wrote a letter to me, was I still doing anything on that? I said no, I thought my work settled the problem. I was naïve.

AKERA: When did he write to you? When did Joseph Schumpeter write to you?

ALT: '39, something like that.

AKERA: So this was after you had emigrated to the United States?

ALT: I was naïve. I now realize, in retrospect, that Schumpeter was fishing to see whether he should offer me a job at Harvard! I wrote, “No, I’m not doing anything anymore.” [Chuckles] Well, I already had a job here.

AKERA: [Laughs heartily] Oh, no! Oh, what a shame! Actually, Dr. Alt, I truly apologize, but I think we do need to move on...

ALT: Yes, go on. But you don’t have to have all the questions answered.

AKERA: No, I don’t have to address all of the questions. I think, ACM, in particular, is interested in covering some of the later aspects of your career. What I can do is come back and fill in some of the early history. I’d ideally, I’d like move on to the United States, while also maintaining a lot of the very interesting connections that you’ve raised for us.

Now, from what you’re telling me, abstract mathematics was a very powerful set of ideas that took shape in Europe throughout the first half of the 20th century. So there must have been the transfer of... historians have written about the transfer of mathematical techniques—how to handle partial differential equations by mapping certain topics in abstract geometry onto other subject areas, for instance. This is not my area of expertise, but if I understand it correctly, it seems that mathematics itself was a productive discipline that affected many other fields, and that perhaps because of unusual historical circumstances, some of this transfer went outside of the physical sciences, to economics and other realms shortly before World War II. Is that correct, by your description? Or were you very unique in doing this kind of work even before World War II.

ALT: Applied mathematics—especially in the narrower sense of mathematics as applied to physics—the great place for it in Germany, before the Nazis came in (that’s before ’33), was Göttingen. That was the center of mathematics in Germany. The head of that institute had been a very famous mathematician, David Hilbert, but then after him, Richard Courant. Courant was

driven out by the Nazis in '33. In '34, he came to New York. Luckily, at New York University, he was able to establish his own institute with Navy money. I remember somebody saying here, "There was nothing like that here before Courant built up his institute." I knew Courant. I applied there. Much later, I was in Berlin for an International Congress on Mathematics. The Springer-Verlag (1998) publishers had a display there. I stood there, saw a picture of Richard Courant, whom I knew. A lady standing next to me, I said to her, "I once applied for a job with him, but I didn't get it." She said back to me, "That was my father!" "Then you must be the sister of Lori?" "Yes, I am!" [Chuckles] I had played chamber music with Lori. Lori is the daughter of Richard Courant who lives here in New York. Lori is a musician. Lori got married to one of Courant's mathematicians, Jerry (Jerome) Berkowitz. And she looked like a "Model of the Year." [Laughs]

AKERA: Quite beautiful? This points to such an interesting side of the cultural history of mathematics that few people know about.

ALT: Courant brought applied mathematics from Göttingen to America.

AKERA: But almost all of their work was with the physical sciences, if I'm correct. Right?

ALT: Yes.

AKERA: Very little applied into other areas, such as economics.

ALT: Yes.

[Begin Tape 1B]

ALT: I have had an interesting life. [Laughs]

AKERA: I wonder if you could shift more to your own move to the United States. What was the specific-- There was a political situation which made it so you had to emigrate to the United States. But did you have a job waiting here?

ALT: No, certainly not. For three months, I was unemployed. I had letters of introduction to a number of people. I think I had 60 letters of introduction, and I wrote letters to all of these people, "So-and-so recommends, and I applied to you."

AKERA: Did you write to Dr. Courant as well back then? Or not then?

ALT: Yes. That was one recommendation. And I got 58 declinations. One of them was through Harold Hotelling of Columbia, who was very nice. He referred me on to a man named Charles Roos, originally a mathematician, a clean, pure mathematician from down South, New Orleans possibly, Louisiana I think. There had been so much interest in mathematical economics, all economics in general—economic research—so there was money for that. There was a man who gave money to establish an economic research institute. His name was Cowles—a wealthy man who thought he should use his money for economical research, to find the reasons for economic

depressions. In his mind, and in their minds, economic depressions were so horrible that they had to be avoided. We had to find the reasons for them.

It happens that Alfred Cowles was consumptive—he suffered from tuberculosis, and he had been told he should live at high altitudes. So he established a research institute in Boulder, Colorado, which is a mile high or something like that. 5,000 feet high. He lived there, partly just to see it. They had hired Roos because they thought they should have one mathematician on their staff. Otherwise they were economists. Roos had been there. Hotelling at Columbia knew of him.

Roos, after a while, after a few years, had been dissatisfied there. So he decided to set himself up in New York, migrated here, and established his own institute, which he called the Econometric Institute. Econometrics was the word for the kind of economics that deals with numbers. Originally, he had a longer title, but it boiled down eventually to the Econometric Institute. And Roos hired me. Hotelling had recommended me to Roos, and Roos said, “Ha! I can get a mathematician who’s interested in economics! That’s what I need!” He was a mathematician. He appreciated my work. I do remember that on my first evening at home, I drew some graphs and showed them to him on the next morning, and he was happy.

AKERA: So your exposure to the fact that economics might be conducive to analysis by formal mathematical means really paid off, in a sense.

ALT: I never finished the story about the paper of Oscar Lange that I wrote that Morgenstern published. It later on got translated into English. Somebody came into my office years later and said they would like to have my paper in Morgenstern’s journal translated into English and publish it here. I didn’t tell him that I had written it in English before [laughs], because he had gotten a grant and he wanted to have that. So they translated it, republished it here in a volume of collected papers on, oh I don’t know; basic papers in economics or something like that, by the University of Minnesota.²

AKERA: I can probably look that up...

ALT: Annice will know exactly, because the editor there is married to her cousin [laughs].

AKERA: The social connections are just astounding.

If I may move on, what kind of work did you do under Roos?

ALT: Oh, at the Econometric Institute, we had a standard procedure that I didn’t think of telling you, and that was the method of multiple correlations. When they would record a time series—time series analysis is a word for what we did—we got figures year-by-year or maybe month-by-month, on sales of something. One of our clients—The Econometric Institute needed money and sold its services to businesses. General Motors was my first customer, if you please. There were others of similar importance. Then I had a group of retail stores, Federated. They still exist.

AKERA: Harold Hotelling was also involved with multivariate statistics. Is that correct?

² John Chipman, Leonid Hurwicz, Marcel Richter, and Hugh Sonnenschein, eds., *Preferences, Utility, and Demand: A Minnesota Symposium* (Harcourt Brace Jovanovich, 1971). Dr. Alt has a paper in that volume entitled, “On the Measurability of Utility.”

ALT: For that sort of thing, we need annual figures. I got figures on automobile sales year-by-year, and correlated that with the national income places, of course. Economists think that sales depend on price, inversely. No, no, no. It's wrong. Prices go up and sales go up, but sales go up first, and then prices are raised. That's the way it goes.

AKERA: So it was sort of time series correlational analysis?

ALT: Right. That's sort of what we did. I wrote some papers on that, too. I should show you...
[Sorts through papers] Now we're getting serious.

AKERA: Indeed! [Laughs] Would it be possible for me to get a copy of some of this material?

ALT: We'll copy that, I'm sure. I'll give it to my wife, and suggest to her to come in. That will speed it up, because she'll remember the things that I keep forgetting.

[Pause in Recording]

Where were we?

AKERA: I don't know! I think we were now talking about your work in the United States, about how you got to the United States, and your first job here.

ALT: Oh, at that point. Yes. I was anxious. Come '41, Pearl Harbor, December 7th, that's a date I remember. December 7, '41, and the American entry into World War II. I was anxious to get my revenge at the Nazis. I volunteered for the American Army early '42 and was turned down because I was an enemy alien, theoretically. They didn't do any harm to me. They didn't intern me as they did the Japanese. But they didn't allow me to enter the Army until I was drafted the following year. In '43, I was drafted. Then while I was in the Army, I got my American citizenship, a little early being a soldier,.

AKERA: I'm a little bit confused. Why were you drafted if you were not yet a U.S. citizen?

ALT: Alien, non-citizen residents were subject to the draft. But volunteers were not accepted.
[Laughs] God knows why, but that was the way it was done.

AKERA: Had to wait for the lottery!

ALT: Well, I can see that. Volunteers were suspected of being spies setting out to infiltrate.

AKERA: I understand. And governments have to make such decisions, for sure. But then, you were drafted, and you were not drawn in to the Army for any of your mathematical abilities?

ALT: Right. As soon as I was drafted, I volunteered for the ski troops because I felt that me, coming from Austria—Austria, if you don't know, is a country in the high mountains, and we were all skiers. I wasn't even a terribly good skier, but by American standards, I thought I would be able to do a good job.

AKERA: And that was 1943?

ALT: In '43, I was drafted into the Army. For a while, I became a ski instructor. There, I could volunteer for the ski troops. This they allowed me to do, once I was in.

AKERA: But then you went into officer training not shortly afterwards. Is that correct?

ALT: That's right, yes. Army training, basic training, marching, and so on. Let's see. I was drafted in the summer of '43 and immediately sent to Colorado, almost immediately training—oh, and stiff training in the high mountains, spending times out and spending nights out in the snow in winter. I enjoyed that. Goodness, we trained strenuously for five and a half days a week, and then the weekends I walked out on my own on my skis, going up and down mountains, enjoying it [chuckles].

AKERA: Did you ever go to Europe into combat?

ALT: No. There came an announcement that one could apply for officer's training, so I applied. That was selfish. I knew that officer's training would be here in the East, near New York. Meanwhile, I had a little daughter in New York and I could visit my family on the weekends. That was tempting. So I applied for officer's training. From September to December, I was here. Aberdeen Proving Ground (Aberdeen, Md.) was the location of the training.

AKERA: This was the location of the officer training program?

ALT: Yes. That's the way I knew the location. No, maybe I'm wrong. Chemical Warfare Service...why was I in the Chemical Warfare Service? I don't remember. The training was someplace here in the East, not far away. But I knew about Aberdeen. At least it was one of the places that we inspected as part of the training.

AKERA: And you also knew that mathematical work was done at Aberdeen?

ALT: Yes. Chemical Warfare Service...why...?

AKERA: My guess is it might be somewhere down towards Maryland or thereabouts.

ALT: Okay. I was taken into officer's training in the Chemical Warfare Service for some reason, because my application still looked fairly good. I had had some little chemistry in college. It still looked fairly good to be accepted.

AKERA: So at that point, your scientific talents were recognized to some extent in deciding to enter you into officer training?

ALT: Yes. And the chemical warfare service is located at Edgewood Arsenal, that's right, which is halfway between Aberdeen and Washington on the Pennsylvania Railroad. So I spent three months in Edgewood (Md.). But on one day, we visited Aberdeen and were impressed. It's a great research installation.

AKERA: Did you speak with any of their mathematicians then?

ALT: No, I couldn't speak to them. I was in a company of recruits. I couldn't get away, nor did I know any of them. Later on I did. But then, we were in officer's training. Late '44, December '44, I got my commission as Second Lieutenant. And then in January, I was supposed to be deployed. The Army, in its great wisdom, focused on my knowledge of French. They were going to take me to Europe and drop me behind enemy lines in France. They asked me whether I would do that. "Sure. Why not?" Some people might be hesitant to be dropped on a parachute, but I would learn.

AKERA: [Laughs] You were quite willing in many ways. That is incredible.

ALT: [Laughs] But I didn't. I never got to it. They put me into a company that was waiting for the next shipment to Europe. And by that time, France had surrendered, so I never got there. I remember one day, they called me to Washington to the headquarters of the officer's training school and said, "We have been sort of unfair to you. We would like to do a thing that's unusual for the Army and give you a choice of where you want to go." Unreasonable. I said Aberdeen.

AKERA: And you knew that they did interesting work in military mathematics?

ALT: Yes, because I knew there was mathematics there and I could do something useful there. By that time, the war was only with Japan. I wasn't terribly keen on going to Japan, far away from my family. I would rather be here and do mathematics at Aberdeen.

AKERA: This phase of your life has been well documented in the interviews on file at the Smithsonian Institution. But nonetheless, I wonder if you could summarize for us briefly a little bit of your work at the US Army's Ballistics Research Laboratory in Aberdeen, Md., and particularly your involvement with the Computation Committee there.

ALT: I reported to Aberdeen—it must have been April '45.

AKERA: April '45, yes.

ALT: I was dumped into computing. I knew nothing about it. The subject had never occurred to me as something to study. The *word* had never come up. "I got some reading to do, first of all," I thought. There was a lot of computing going on at Aberdeen, namely of firing tables.

AKERA: Computing ballistics trajectories, including exterior and interior ballistics?

ALT: Yest, and that's differential equations in mathematics. There were a lot of new things to fire off at that time. Rockets were beginning to come in, a lot of firing tables to compute.

AKERA: Also bombing tables as well, I believe.

ALT: Going ahead of time, a year later was the first time I saw the trajectory of a rocket. It was, a fascination to me how that thing slowly, slowly starts from the ground and then slowly speeds

up. I thought, of course, the thing gets fired like that. No. A rocket starts slow and... the big rockets that are being fired now around the world...

Anyway, at that time, firing tables were being calculated. I had to quickly learn how to do differential equations in the American way. There were a few methods that I learned. Runge-Kutta is a famous method for differential equations. Somehow, that name came up in the last few weeks [chuckles]. Somebody by the name Runge showed up here. I don't know why—just because things keep turning up [chuckles].

They had the best available equipment of the day, namely they had an order out for the first real electronic computer that was being built, constructed, at the University of Pennsylvania, which is not too far from Aberdeen, in Pennsylvania. They also had on order, and was being built, two computers at Bell Telephone Laboratories in New York.

AKERA: Using relays, electrical relays.

ALT: They were enormously large computers. The ENIAC was a room full of computing devices, just for that one machine. We can't imagine that today. Nowadays, a computer fits on a table. They were typically a roomful. I remember von Neumann once explaining ENIAC and saying every time you turned it on, you'd blow two tubes.

But similarly with the Bell Laboratory's machines, except that nothing was electronic. That didn't occur to that group. They were older. In fact, the big, new thing was not the electronics, but serial computation—the thing that you perform a long list of operations, one after the other, just on one case, before you go to the next. The older way of computing was mass production. You do one addition on a thousand cases. And then you do the next step on all the cases. That was the way IBM punched cards used machines. Astronomers, I learned there, had been using computing machines--

AKERA: Yes. Using IBM equipment more in the sequential mode.

ALT: Even ordinary IBM equipment -- Well, come to think of it, at the Cowles Commission once in Colorado Springs, I had seen some computing machines, some IBM machines of that vintages, but they were sorters and tabulators. That's all they had. I remember a fellow, just for fun, picked up a handful of cards—see, these all represent something—dumped them on the floor, picked them up like that, shoved them in together, and now they are all out of order. You put them through a sorter a few times. Then they were in order. [Laughs] That was the way one computed at that time. That was all I knew about computing.

But by the time I came to Aberdeen in '45, they had the sequence-controlled calculator, and they had two relatively small machines of the size of a big cabinet, maybe, that could be set up to perform a list of instructions on one card before going onto the next card. That means a fairly complicated mathematical routine on one card before going onto the next card—much more powerful that way. They had two of these relay calculators, and I was naturally anxious to do something that produced results, not just sitting and dreaming about the future. So I got control of the relay calculators. There was, when I arrived, a Computing Section (I think it was called), the chief was Dr. Dederick, a competent mathematician—Louis Serles Dederick—and a handful of girls who would use desk calculators. I got hold of the sequence-controlled relay calculators built by IBM that had just arrived.

AKERA: The modified IBM equipment, yes.

ALT: Nobody knew what to do with them. I got the instruction book and read it and started to write programs for them, punch the cards myself...

AKERA: And then plugged the wires also, I believe, at the time.

ALT: Yes.

AKERA: What kind of problems did you run on that machine? Was it just the ballistics problems?

ALT: It was still trajectories, I think. Oh, I tried to go around the laboratory to see what computations people needed apart from trajectories. Some were fairly simple. I remember something about something chemical. I don't remember--

AKERA: There were multiple divisions at the BRL at the time?

ALT: It may have been, yes. God knows what. Optical perhaps. Fairly easy to do—maybe no more than half a dozen operations. But we printed it out neatly, and it turns out that when I returned the results to these people, what made them happy was that here they got it printed in neat columns. Until then, they had girls do them and they could hardly read the results

AKERA: So in other words, you basically became the computational service for other researchers at BRL? And you could solve various equations for these researchers?

ALT: To other people—to chemists, physicists. Basically speaking, that's the way I lived from there on through the Bureau of Standards. The Bureau of Standards was the same thing. The Mathematics Division provided mathematical services to the other divisions.

AKERA: Exactly. But let me ask about that later, because it does become very important to your work at NBS.

ALT: Right.

AKERA: Now, you didn't remain at Aberdeen very long initially. I believe you were there from April to November of 1945, and then left to accept a civilian job. Is that correct?

ALT: I wanted to get out of the Army as quickly as I could. I knew that my exit from the Army would be considerably sped up if I had a job waiting for me, and this time I did. Because my former employer was anxious to get me back, offered me a salary, immediately. Then he doubled that offer because he thought I was dragging my feet, though I wasn't! I was anxious to get out!

AKERA: Well, that was convenient, wasn't it! So you went back to the Econometric Institute?

ALT: I went back to the Econometric Institute just to get out of the Army. I never liked it very much.

AKERA: To be in the Army?

ALT: The Econometric Institute—I liked it less now. Because why? The work there was multiple correlations, and to do that efficiently requires some mathematical understanding. But that was way overshadowed by salesmanship. It became necessary to overstate our capabilities. We had to forecast business conditions, and that's guessing. We pretended to...

AKERA: ...so there was a little bit of hocus-pocus in the method, yes?

ALT: Right. A great deal. I disliked it for that reason.

AKERA: Is that what made you return to Aberdeen in October of '46?

ALT: That made me re-apply to Aberdeen, yes. And I chose Aberdeen because it was the easiest way. I could have applied some place else in the government. In fact, for a while I dickered with the Census Bureau.

AKERA: Yes, which was getting involved with computers at the time.

ALT: Yes. But there, they didn't know me. At the National Bureau of Standards, I had the inside track.

AKERA: But that was later?

ALT: Yes. For now, we are in '46, at the time I got out of the Econometric Institute. Now, why did I have the inside track? Some time while I was at Aberdeen the first time... No, it can't be. I'm sorry. I must be wrong. I had met John Curtiss somewhere along the line. Curtiss was an empire builder. He was in the Navy. He was a Navy officer.

AKERA: He was a liaison to the Harvard Computation Laboratory, I believe, under Howard Aiken.

ALT: John Curtiss was...let me see. What's the second highest rank in the Navy? Captain is the highest rank. Captain, and the next thing is...?

AKERA: Commander?

ALT: Commander. Curtiss was a Commander, or maybe he was a Vice Commander.

AKERA: Lieutenant Commander.

ALT: A Lieutenant Commander. And his boss was Cannon. Curtiss had succeeded in persuading the Director of the Bureau of Standards, Ed[ward] Condon at that time, to establish a mathematics unit at the Bureau of Standards.

AKERA: To create the National Applied Mathematics Laboratories?

ALT: Yes. Well, that was a great name that Curtiss invented. It didn't really exist. But he persuaded Condon to establish a mathematics unit at the Bureau. And Curtiss managed to get some money for that, namely from Mina Rees, who was in...

AKERA: the Office of Naval Research.

ALT: ONR didn't exist at that time. No, at that time, Mina Rees was in... the Navy had an Office of Research and Inventions, ORI, of which Alan Waterman was the Director and Mina Rees was under him, in mathematics. These names are all very significant. Later on, a few years later, the National Science Foundation was established by Eisenhower, I believe. No, Truman? It's established a few years after the end of the war. The Navy had the best existing research establishment, so it was taken over into the National Science Foundation with Waterman in charge, and Mina Rees in charge of mathematics under him. Mina Rees knew Curtiss very well. Curtiss had endeared himself to Condon and established a mathematics unit at the Bureau of Standards. There was also a name worth mentioning, Joe Weyl, under Mina Rees. Mina Rees pretty soon branched out and had a whole Mathematics Sciences division in ONR, with pure mathematics, statistics, I'm not sure, a couple more things. Joe Weyl is the son of the really famous mathematician, Hermann Weyl, and has a Swiss heritage. These are all good friends, and I became one of that group.

AKERA: Dr. Alt, if we could go back to 1946, you mentioned in one of your prior accounts that although you formally returned to Aberdeen, that you were able to stay close to the New York City area by being placed first on assignment to Bell Telephone Laboratories. Can you tell us a bit about this period and the work you did back then? I think you were already living in New York City.

ALT: Of course, I wanted to be in New York. I didn't want to live in Aberdeen. New York was fine. I loved it. But I don't remember who took the initiative or why. By that time, I knew Aberdeen pretty well... [Pause] During the war when I was in uniform and assigned to the Computation Committee...

AKERA: Yes, you were a member of the Computation Committee, and you specialized in machine computation?

ALT: In 1946, I knew the director of BRL, Colonel Simon, and it seemed natural to return there. I wanted to get out of econometrics. I enjoyed the idea of going back to the government. If I had to look for a job somewhere, it seemed natural for me to want to go back to the government. The easiest way into the government was to go back to the people whom I knew and who knew me. When somebody knows me already, it's easier to get the job! I also dickered with the Census Bureau a little bit, but they were slow. They didn't know me, and Aberdeen was happy to get me back. But I don't remember who had the idea to leave me in New York for a while, for six months.

AKERA: I know that the Bell Relay Computer—I forget which model, but it was a substantial one—was scheduled to be delivered to Aberdeen. And even while you were still in uniform, you were a member of the Computation Committee, where, because others were already working on the ENIAC and other projects, you sort of took up the work of managing the computing group...

ALT: ...of the machines that were already there.

AKERA: Yes. But then once it became close to the time for receiving the new relay computer, I think they said that they wanted you to work with Samuel Williams and E. G. Andrews at Bell Telephone Laboratories so that you could get to know this machine before it was delivered. This was so that once it was delivered, it could be put immediately into productive use.

ALT: Yes. Now, I don't think I had seen the Bell Laboratories establishment before I left the Army. I don't know who took the initiative, but it sounded like a very good arrangement for me to be stationed in New York for a while, to grow up with these machines. And then there were two enormous roomfuls of machines that I had to oversee. My job was, again, something that I knew well, namely, programming for a computer that wasn't quite in existence, but it was far enough along to do this work. At that time, the work on the Bell Telephone Laboratories's enormous relay computers was to put some test problems on them, and find out what went wrong and to correct the mistakes.

AKERA: This was because they were still doing engineering work on the computer to the point that it worked perfectly, and reliably?

ALT: Right. I remember one morning, we came into the office and it looked in shambles. There were papers all over the floor. Mainly that machine worked on paper tape, and unexpectedly, it had run all night—that had never happened before—and had produced an enormous amount of tape. We hadn't foreseen that. If it runs 12 hours without supervision, it produces so much tape that it fills the floor! [Chuckles] That had happened that time. Only once in six months! Later on, we got the machines to work. But it was only used for testing, because in those 12 hours, it didn't get as much work as ENIAC would have done in 12 minutes. ENIAC wasn't working yet.

You asked another question where I want to correct you on the written question. Once, during my second time at Aberdeen, I looked into some old files and found a carbon copy of a memorandum that Dick Lehmer, who had become a good friend, had written to the then Chief of the whole Research Laboratory, Colonel Simon. His name should be mentioned, by the way.

AKERA: That would be Colonel Leslie Simon?

ALT: Yes. It told him that, over the last weekend, John Mauchly and I (Lehmer) tried out the ENIAC and performed a problem on it. The problem was of no great interest to anyone—something about recognizing large prime numbers. But it was the best test for the ENIAC. It ran for two days and nights over the weekend without stopping. That's how we learned about Lehmer's problem. He didn't spell out in that memo—because it wouldn't have been understood back then by Simon—what method they used, but I could read between the lines that it was about—you mentioned the name—the Lesser Fermat Theorem. This is something that's characteristic of prime numbers, but not quite. There are exceptions. And what Lehmer was doing was listing the exceptions to the Lesser Fermat Theorem on prime numbers.

AKERA: By doing this in a brute force way, checking them one at a time...?

ALT: If I can still quote it. Something A is a prime if and only if two to the power A minus one is congruent to one. Yes, two to the power A is congruent to A minus... It must be something like that. It was fairly easy to compute on the ENIAC if you said "and equals to." For instance, you keep doubling numbers. That's fairly easy. You add them to itself. Even the ENIAC could do that fast, even though the machine is a slow machine by modern standards. You keep adding, and if it's too large, then you subtract. You reduce it and so on. And you can find out all the numbers applied that way or whether it's an exception.

By the way, related to this discussion, we also built our own computer, later. I didn't see this in your questions. When I came to the Bureau of Standards, we built our own computer. Not I, but our electronics section, Sam Alexander, built a machine, and I wrote a test problem for it that was for finding prime numbers—not by Fermat's Lesser Theorem, but just by decomposing a number. That was my first test program. And one evening, after hours, I buttonholed Sam Alexander's second-in-command, Ralph Slutz, who had become a very good friend of mine, and he sat next to that big machine, the SEAC,³ and watched it.

AKERA: It worked through the problem?

ALT: We put in the number 15, and it came back very quickly "3 x 5." We put in 36 and it came back with "2 x 2 x 3 x 3." Then, we said, "Let's put a big number in it." Well, the machine could hold a number with ten decimal places, or ten places. Ten nines would be the largest number that it'd hold, but that's obviously not a prime. 9999999997. We put this in and the machine stopped! It sat there for 15 minutes. I said, "What's wrong?" Ralph said, "I think maybe it's still working. I can't tell. We don't know." After 15 minutes, it came back with the answer that this is a prime! [Laughs]

Wonderful, great experience!

AKERA: I can imagine! Just moving back to Aberdeen for a moment, is it right that after you finished your stay at Bell Laboratories, you moved down to Aberdeen to become the Deputy Chief of the Computation Laboratory?

ALT: They hired me back with that title.

AKERA: So that was your title to begin with?

ALT: Yes.

AKERA: And you basically accompanied the machine back to Aberdeen?

ALT: Yes, that was the deal. And I came back to Aberdeen as Deputy Chief, under Dederick.

AKERA: Once the Bell Laboratories machine was delivered to Aberdeen, did you begin offering a computational service using the machine, like you had before? Is that what you did at Aberdeen?

³ This was the Standards Eastern Automatic Computer, completed in June of 1950.

ALT: Once the machine got delivered to Aberdeen, yes. It became part of the Computation Laboratory. By that time, it became necessary to subdivide the Computation Laboratory into a Bell Telephone Machines section, an ENIAC section, and...

AKERA: ...and probably still an IBM section. Is that right? Not based on its later electronic computers, but their older tabulating machines.

ALT: Well, I'm not sure now. I should say-- We are still at Aberdeen in the Computation Laboratory, and not yet the Bureau of Standards?

AKERA: No, not the Bureau of Standards.

ALT: Yes...

[Begin Tape 2A]

ALT: As a service to the rest of the laboratories, I was something of a salesman, to go out, meet people, and ask them whether they needed any computing. Some were very uncooperative. They didn't need anything like that. They do their own computing. Others were very happy when they got something printed out rather than scribbled by hand.

AKERA: That's right. There had to be a little bit of a selling job in computing's early history, but then once it became a more familiar service...

ALT: We still had a hand-computing section. I even had some work selling their services.

AKERA: Yes, because you were the Deputy Chief of the entire laboratory, so you really supported all of these different functions, I guess. It was around this time that John Curtiss, J. H. Curtiss, who was also the first President of ACM, was setting up the National Laboratories for Applied Mathematics at the Bureau of Standards.

ALT: He persuaded Condon to get some mathematics there. There were some roots there before Curtiss and some things that he had to take over.

AKERA: Well, there was the whole Applied Mathematics panel under National Defense Research Committee (NDRC). This included the Mathematical Tables Project, which was located at New York University.

ALT: The Mathematical Tables Project had been put under the Bureau of Standards because that was the only technically competent part of the government, which served as the supervisor. The Mathematical Tables Project was there at NYU. But also, there had been during the war a mathematics unit in the National Academy of Sciences. In the NAS' National Research Council, there had been a computing group.⁴ I don't know what title they had. Dick Lehmer, George

⁴ This would have probably been the NRC Committee on Mathematical Tables and Other Aids to Computation, established during the early 1930s. However, there seems to be some confusion here between this committee, and

Stibitz, and Howard Aiken at the Harvard Computation Laboratory were involved. Howard Aiken was an old man, had impressed IBM (he was working with IBM) into building a large, electromechanical computer to his specifications.

AKERA: I just spoke with Dr. Anthony Oettinger up at Harvard.

ALT: How did you get to know of him?

AKERA: He was an ACM president from 1966-68.

ALT: A long time ago, that's right.

AKERA: And because ACM wants these interviews for the presidencies, I spoke to him. So I've heard a lot about the work at Harvard from Tony Oettinger. But going back to the Bureau of Standards and the National Laboratories for Applied Mathematics, I guess you transferred there in October of 1948.

ALT: To the Bureau of Standards.

AKERA: Yes. Were you hired to head up the Computation Laboratory there at that point?

ALT: Yes. Well, the story there was that by the time I was at Aberdeen the second time, computing had become a familiar word. And people came from all over the country to see us at Aberdeen.⁵ John Curtiss visited Aberdeen because he felt that if he was trying to set up something at the Bureau of Standards, he ought to know something about computing. He also realized that it would help greatly enhance his position if he had a computer.

AKERA: He wanted to create an entire laboratory for the Applied Mathematics, and one part of it had to be the computation services. If I'm correct, the Mathematical Tables Project, which had been active through the war at New York University, was one of the units that was brought down to Washington and folded into the National Laboratories for Applied Mathematics. Isn't that correct?

ALT: I don't know where the Tables Project was located, here in New York. I knew Arnold Lowan. In fact, Lowan would have hired me after the war, but his group didn't sound attractive. There was only a small group of human computers under him, and even if they had ever managed to get a computing machine, that wouldn't have been where I wanted to be. Yes, Arnold Lowan is one of the people with whom I might have gotten away from the Econometric Institute, but that was less attractive than going back to Aberdeen. My wife didn't like that. She would have liked to stay in New York.

the NRC Committee on High Speed Computation, headed by John von Neumann, which was established after World War II. Curtiss, who served as an alternate member of the committee, had a run-in with von Neumann and the rest of their committee over NBS' contract to the Eckert-Mauchly Computer Corporation, who was building an electronic computer (the Univac I) for the Census Bureau and several other federal agencies, under Curtiss and his staff's supervision.

⁵ Because they had the ENIAC, a Bell relay computer, along with substantial computing operations on these and other machines.

AKERA: Yes. But then it would have brought you down to the same place in the end, anyway?

ALT: Oh, I should say, when I went to officer's training in Aberdeen, or shortly after I got my commission to be in Aberdeen, my wife moved there with our little baby daughter, and didn't like life down there. It's a pretty miserable place.

AKERA: Quite isolated, yes.

ALT: Not like New York. My wife, Alice, at that time, liked to be politically active in peace, human rights—the kind of things that interest us now. She was in the same field as I, but she liked to be politically active. In New York, she could be. Things meet again in unexpected ways. Alice tied up with some people very quickly who were in to that kind of work. And my oldest two friends, two married friends, Zaslavsky, who live a few blocks from here in this neighborhood and are amateur musicians—Later when we came back and I looked for musicians to play with, my wife said, “Well, there are the Zaslavskys. Tie up with them.” Three days ago last week, we read in *The Times* that Mrs. Zaslavsky had just died. They had been my chamber music partners for years after we came back to New York. We stopped a few years ago because they were getting quite old and didn't play anymore. But Claudia Zaslavsky... such a blow.

AKERA: Let me go back to your move to the National Bureau of Standards. Was it because you knew John Curtiss that you got hired into NBS?

ALT: I'm sorry. I didn't get the question.

AKERA: How was it that you were hired to head up the Computation Laboratory at NBS, National Bureau of Standards?

ALT: Oh! I wasn't. [Laughs]

AKERA: I'm sorry. Okay, I must have this wrong.

ALT: John Curtiss had first met me at Aberdeen because he was interested in computers and knew me. He offered me the position of Director of the Computation Laboratory. He, himself, was a statistician. In fact, I think his father was a professor of statistics some place in the Midwest. I would know the name if I thought hard enough. But then John Curtiss met the Todds, Jack Todd and Olga Todd, who had been Olga Taussky, from Vienna, who were both competent mathematicians. He tried to attract them, so he offered Jack the position of Chief of the Computation Laboratory.

Now, he had nothing for me. So he lowered his offer to me to the position of Deputy Chief of the Computation Laboratory. But because my wife wanted to get back to a place more like New York, I accepted it. My wife almost talked me out of it: “You shouldn't,” she said. “What the hell, I don't care!” I said. So I came to the Computation Laboratory as, I don't know, Deputy Chief or Assistant Chief, I don't remember what the title was, exactly, and stayed there. But there came a time--

Oh, the Mathematics Division that John Curtiss set up was another trick that he played. He hired his former chief in the Navy, the Naval officer who had been his superior. He hired him as his assistant. That was Edward Cannon. I think Cannon had been Commander in the Navy and John Curtiss was Lieutenant Commander. And now Curtiss was the head of the Mathematics Division.

AKERA: And Edward Cannon served as the chief of its Machine Development Laboratory. Is that correct?

ALT: Okay. And so he hired Ed Cannon as one of his section chiefs. Cannon, along with Jack Todd in Computation.

AKERA: Yes, but would that have been for the Statistical Engineering Laboratory?

ALT: Because Curtiss was a statistician. He hired a youngster, Churchill Eisenhardt, who was also a statistician. Now, a statistician is clearly needed at the Bureau of Standards. That was the only thing that was really needed and hadn't been there that needed to be put in. And it was as small as all of the smaller sections. He set up Cannon with one assistant, Ida Rhodes. Ida Rhodes is a name to remember.

AKERA: Her name has come up already in an interview with Tony Oettinger.

ALT: An old woman who is very competent in computer programming. I think she came from the Mathematical Tables Project.

AKERA: I believe that's correct.

ALT: I believe she must have been. I don't know where else. But she was willing to go to Washington. The others with the Mathematical Tables Project were reluctant to move. Didn't want to.

AKERA: They may have had family, too. They would have to move from New York to Washington to continue working with the Mathematical Tables Project.

ALT: So he put Ida Rhodes into what he called the Machine Development Laboratory. It was a word. It wasn't anything; it was just these people--

AKERA: There really wasn't a very formal organization, then? These were just concocted titles?

ALT: Right. And then Curtiss set up a section in California because he liked the idea.

AKERA: Ida Rhodes went out to the Institute for Numerical Analysis (located at UCLA)?

ALT: No. Ida Rhodes was here. But a separate section⁶ was created in California, which at the moment had an open position for a section chief. But Curtiss left that open so he could go out

⁶ NBS's organization basically consists of the NBS Director, divisions (with division head), section (with section chiefs), and groups (with group leaders). NAML was organized at the divisional level, but there is some confusion

there every so often. He'd spend half his time in California. Cannon left after a while. I don't know why. Maybe he didn't like it because he saw there was nothing to do.

AKERA: But then he came back for a while, also. Is that correct?

ALT: Yes, then he came back. Cannon joined some friends whom he knew from the Navy, Engineering Research Associates, a private company that had been set up to build special computers, and he joined them as a consultant (I don't know what he did there) for two or three years. And when he accepted that job, in '52 or so, I became Acting Chief of the Mathematics Division. Curtiss was out in California. Cannon was away on paid leave. Suddenly, I was above Jack Todd. He disliked that, so he left...

AKERA: ...I see. So you were elevated above the head of your own section?

ALT: Yes, I was.

AKERA: I'd like to move on to the ACM, at least to have some of that history covered in today's transaction. But before I do, just one more question. In your early oral history interviews, you speak favorably about the seminars at Aberdeen, you called them mathematical seminars in an earlier interview.

ALT: I don't remember anything about that. I don't know what that would have been...

AKERA: Really? Let me ask the underlying question I had in mind more directly. Did you aspire to do mathematical research in applied mathematics at the Computation Laboratory while at NBS—did you or your staff?

ALT: The way Curtiss set it up, the research was in California and the computation services here.

AKERA: But this was a boundary that became a little blurred, wasn't it?

ALT: Yes. I wanted to hire some good mathematicians away from Aberdeen whom I met at Aberdeen, and we needed them here. Oh, yes. We were a service not only to the Bureau of Standards. We had the only computer in Washington. This was the machine built by Sam Alexander. It was ready one day—it was ready to operate. And I had the staff to operate it.

AKERA: And your staff had the necessary applied mathematics and computing knowledge as well.

ALT: Not only was I able to write computer programs, but I had meanwhile educated several people from the New York Mathematical Tables Project, who had come to Washington—those who had been willing to move to Washington—to program.

here with regards to whether NAML existed as a formal entity, or whether it was just another name for the Mathematics Division of NBS. My research suggests that NAML did become an official entity in January of 1947, and was renamed the Mathematics Division in 1953 or 1954 during a major period of contraction and reorganization for NBS.

AKERA: Were they both men and women?

ALT: Why do you ask that question? It's a good question. I've never asked myself that question. There were four of them, at least two very good women whom I remember, at least two men. But there were more women than men—maybe four against two, something like that. I don't know exactly.

AKERA: But there was a core group of those already trained in computational approaches to mathematics that were already providing mathematical services to the various scientific divisions of National Bureau of Standards, and to some government laboratories, and eventually beyond that as well, I think.

ALT: Oh, yes. Most of our work-- We had very little money from the Bureau of Standards. I do remember the number \$20,000, to rent a big IBM computer, the 704—a big machine. We needed money from elsewhere. We had a budget of maybe \$200,000, of which \$20,000 came from the Bureau's direct allocation.

AKERA: And everything else was transferred military funds?

ALT: The rest was transferred, especially in my section. I never wrote that down. I forgot that. I don't think you'll find it in my earlier reminiscences. The Air Force, the Office of the Air Comptroller...

AKERA: ...The Office of the Air Comptroller—was this the office that had a project called Project SCOOP, for work in linear programming.

ALT: George B. Dantzig, he later became a Nobel Prize winner. He knew how to solve linear equations and set up something called linear programming, and he had our computer solve large systems of linear equations for him. We became well acquainted, George Dantzig and I. He had a sidekick named Emil Schell who worked with him. And I did mathematics for both of them. I learned to understand linear programming, which wasn't quite obvious; you had to understand it. But we hired (from Aberdeen or from elsewhere, I don't remember now) Alan Hoffman, Morris Newman—several competent mathematicians, doctorates to do research, for instance in linear programming, which they didn't do on the West Coast with anybody.

AKERA: That's right. Because if I'm correct, just for the historical record, the National Laboratories for Applied Mathematics had four sections: the Computational Laboratory, the Machine Development Laboratory, the Statistical Engineering Laboratory. And then the fourth section was the Institute for Numerical Analysis, set up on the West Coast at UCLA. INA, Institute for Numerical Analysis, was really set up as a research arm of the National Laboratories for Applied Mathematics. But if I understand it, numerical analysis in particular, and the larger genre of applied mathematics, was still very much new, and research-oriented, and it was very hard to distinguish between the applied work and the research-oriented aspects of this "applied" field.

ALT: Wait! Not only that, the people on the West Coast had the ambition of setting up their own computer.⁷ At that time, IBM was still not in the running, still not in existence. They still had only...I don't think they had the 701 yet.

AKERA: No. I believe they would have had the IBM 604 and then the Card Programmed Calculator.

ALT: I know there was a talk among the computing people. "IBM is missing the boat. They don't know electronics. They don't get in." It was years later before IBM entered. And then almost at once IBM controlled the field. But at the time, Mauchly and Eckert, those two names should go together, had created their own company.

AKERA: Yes. They were the ones at the University of Pennsylvania who built the ENIAC, and who created their own company after the war to build the first commercial electronic computers.

ALT: Right. Now, the West Coast section of the Bureau of Standards at UCLA had the ambition to build their computer. They hired Harry Huskey to be in charge of that. And they designed and they built it, eventually.

AKERA: The Standards Western Automatic Computer.

ALT: Right. There was never any real competition, I think, between the western and the eastern computer. I think they were parallel efforts. We didn't--

AKERA: But your group did wind up doing mathematical research in some way.

ALT: Back to the computer, SEAC: We wanted to be first, and we were. Clearly we were. I mean, we had started much earlier. Sam Alexander had started...

AKERA: Yes. His computer was running before Harry Huskey's. Isn't that correct?

ALT: Oh, what we forget! At that time, in building a computer, the big question was memory. What sort of memory do you have? By that time, the British had come in and their--

AKERA: The Williams tubes memory.

ALT: The Williams tubes. God! You know these things!

AKERA: Well, this part of the history I do know! [Laughs]

ALT: Yes. The Williams tubes at Manchester. And Forrester at MIT with magnetic cores. That work finally, finally...

⁷ This was the Standards Western Automatic Computer, a project headed up by the applied mathematician and ENIAC veteran, Harry Huskey. Huskey had also worked with Alan Turning on the Pilot ACE project in England.

AKERA: ...finally saved the field. But he was working on “electrostatic storage” at the time, which was very similar in its principle of operation.

ALT: But we here at SEAC used acoustic delay lines.

AKERA: Which is also the route that Eckert and Mauchly chose to do for the Univac I.

ALT: Yes. Oh, did the Univac I have acoustic delay lines?

AKERA: Yes. The Univac I was not randomly accessible memory.

ALT: What did SWAC do? You tell me that. I don’t remember. Williams tubes or cores?

AKERA: Williams tubes. But this created tremendous reliability problems for that machine. So early on, even though the Huskey’s computer came online not long after the SEAC--

ALT: Huskey spent a year in England.

AKERA: Yes, that’s right. And he learned--

ALT: Oh, Huskey came back. Huskey had meant to be assigned here in Washington. I didn’t hire him. I didn’t have any place for him, and he was very angry with me. But I couldn’t do anything; I really didn’t have an opening. So he went to the West Coast.

[Being Tape 2B]

AKERA: With all this as background, let me now move on to the ACM. I believe we can refer our readers to a very early article you wrote, “Fifteen Years ACM,” which appeared in the *Communications of ACM* (June 1962, an reprinted in the *Communications* in 1987). This account provides a very good general history of the early history of the ACM. What I’d like to do is to use this opportunity to dig just a little bit deeper into the early history of the organization.

First of all, one thing that struck me was the particular composition of ACM’s founders. There was John Curtiss, Harry Huskey, and yourself NBS. There was Samuel Williams and Richard Hamming from Bell Telephone Laboratories. Derrick Lehmer, who had a background at Aberdeen (I guess he was at Berkeley at the time), and then Alston Householder from Oak Ridge. And John Mauchly, of course, representing the Electronic Controls Company. Was this diversity reflective of the early character of the ACM? In a sense, this was an organization that spanned many different kinds of interests. What made such a diverse group get together?

ALT: We knew that there were mathematicians and computer builders—engineers who designed and built computers—and different degrees (disciplines) represented on both sides. I mean among mathematicians, several were pure mathematicians who only reluctantly got closer to computers. On the engineers’ side, there were general electrical engineers. I remember at UCLA once, I talked to one of the people of the engineering schools who said, “Students should study engineering, not electrical engineering.” Engineering! And the same happens in mathematics. You shouldn’t study computing, just applied mathematics.

AKERA: So diversity was very important to the early development of the field? ACM was a place where there were a lot of conversations between engineers and mathematics?

ALT: Well, it came from us—it came from us, and from university mathematicians. During the war, long before computers, there was talk about computers. At that time, IBM was not in the field, except through its contract with Howard Aiken. They built a machine⁸ to Aiken’s specifications. George Stibitz of Bell Laboratories. Aiken, Stibitz, and Lehmer was the first committee [in computing], created during the war, as an advisory committee to the National Research Council.⁹ And in this respect, Lehmer was still an amateur. Lehmer—although a mathematician, and one of the best theoretical mathematicians and number theorists—Lehmer had put together little circuits on which he did computing. That was during the war. But these was this group three, a committee of the National Research Council, that advised the government.

⁸ The Harvard Mark I, also known (by IBM) as the IBM Automatic Sequence Controlled Calculator, completed in 1944.

⁹ This is possible. However, see note 3 above.

In Aiken's laboratory, there was Ed Berkeley. I must bring him up early here because of his role in the early history of the ACM.

AKERA: I would love to hear more about Berkeley, yes.

ALT: Berkeley was way early. Berkeley was the first one who once called me up at Bell Laboratories or maybe wrote a letter to me, saying that, "Oh, I'm no expert in computing, but..." I was at Bell Laboratories then. This was only my second time at the Laboratories, running the computers from there, but I knew something about computing. I can't really be sure about the details, actually. Somebody called up, wanted a question, and my answer was that we ought to exchange information more frequently.

Howard Aiken wanted nothing to do with such a thing. I asked Howard Aiken. He said, "Ah! Oh, if you want to, we can phone each other once in a while." I thought we should inform each other about the programs we were writing. For instance...

AKERA: ...you wanted to inform each other, to try to coordinate the national research program in computer programming techniques?

ALT: In programming software, all right, to express it in modern terms. Aiken wanted nothing to do with it because he thought he was the only one active in the area. Berkeley came out of Aiken's laboratory.

AKERA: He was there during World War II, if I'm correct.

ALT: Yes. Then Berkeley had a group of people after the war. I think all of them came out of Aiken's, or most of them, out of Aiken's lab.

AKERA: That's right. Actually, why don't we talk about Edmund Berkeley. I think that's a very good place to start. But now, to fill in some of that history, Edmund Berkeley was working under Aiken during World War II. But afterwards, he left to go back to the company he was working for before, which was Prudential Life Insurance.

ALT: Here in New Jersey, across the river.

AKERA: Yes. But because he had worked in Aiken's laboratory, he became very excited about digital computers. It's worth remembering, and perhaps I have the timing here wrong, but a caricature of Aiken's computer appeared on the cover of one of *Time* magazine, didn't it?¹⁰

In any event, Edmund Berkeley became very interested in computers. And working as a kind of in-house consultant, a methods consultant within Prudential, he began advising his own company about possible uses of the computer. And then other groups, through an organization called (Life Office Management Association) an informal association of sorts that already existed to talk

¹⁰ This was a painting by Artzybasheff of the Harvard Mark III, which appeared in a 1951 issue of *Time Magazine*. Given the date of this publication, the connection I made here is incorrect.

about how you would use advanced methods and machinery to improve operations in a life insurance office...

ALT: Within Prudential?

AKERA: Yes. Berkeley was really one of the actuaries that got himself redesignated as an in-house consultant on matters related to computing. And that's where he really began having conversations with John Mauchly of the Electronic Controls Company, probably to try to both convince Prudential to buy an electronic computer and to convince the Electronic Controls Company to take an interest in the specific needs of the insurance sector. He really tried to play this role of an intermediary.

But it was around this time that Harvard had its symposium, in January of 1947, which was one of the earliest gatherings of people with an interest in computers. (There were several other gatherings.)

ALT: Question. Do you know where C. V. Tompkins came from?

AKERA: Not precisely. I know that he landed up at Engineering Research Associates. Was it NCR? He might have come from NCR, which did a lot of the work for the Navy that then became the basis for ERA.

ALT: Okay. That's the one I missed.

AKERA: Sure. But in any case, Edmund Berkeley attends this meeting at Harvard, and that's when Samuel Caldwell makes his sort of famous remark that there should be more open exchange of information in the computing field, that this field, in being...

ALT: I have something that should go into the transcript.

AKERA: Actually, may I read out this question as written? Because I think it's an important twist on this early history.

I know that you yourself cited Samuel Caldwell's speech at the January 1947 Harvard Symposium as a stimulus for creating the ACM. However, in going back to the historical records, it seems that Caldwell was using this occasion as much to speak up against the military secrecy surrounding Jay Forrester's Project Whirlwind.

ALT: I didn't know that. It would have been quite natural for him to do that without others noticing his motives. Everyone knew that the military was funding computing projects. But he was analog computers, wasn't he?

AKERA: Yes. I think it was true that he still remained partly wedded to analog computers. But I think it can be said that he was simply advocating for the open exchange of information in the computing field at large. In many ways, it was Edmund Berkeley, then still an actuary at Prudential Life Insurance, who took Caldwell's statement and used it for his own purposes to

really create a group of experts which, in a way, he could tap into in his in-house consulting work for Prudential. Is this how you remember the origins of ACM?

ALT: That could quite well be. For most of us, Caldwell represented the analog field, and that was the big distinction. Nowadays, nobody thinks of analog as even being computers. But at that time, it was almost a competitor to digital computing.

AKERA: And even in mathematical terms, there are certain classes of equations that could be solved more efficiently using analog techniques than digital techniques?

ALT: Yes. We felt it incumbent on us to invite Caldwell to give a major talk at one of our meetings. Caldwell started, "I have come to praise the analog computer, not to bury it," quoting Shakespeare very nicely. So that, I think, should be placed in the historical record.

AKERA: That's wonderful. His speech, in fact, appeared in the *Proceedings* from that conference, and your recollections are very much consistent with these proceedings as I remember it. He still was very committed to the analog tradition. Except he was also willing to integrate—to try combining the two technologies—and had a project funded by the Rockefeller Foundation to try this. But this project was cancelled around this time because Project Whirlwind had become the major computer development project at MIT. This is just one part of the history, but the local conflicts at MIT brought Caldwell to promote the open exchange of information, because Whirlwind remained a classified project. There were secret aspects to it. They could, and did publish openly on much of their work, but nonetheless, there were aspects that Caldwell, in particular, felt that the military sponsorship was leading to the closing down of the field. Given the youthful status of the field, he was especially concerned that academics remain a commitment to openness—of course this was one of the early struggles repeated throughout academe during the early postwar period.

But going back to Edmund Berkeley, can you tell us a little bit more about Berkeley? Why was he so interested in computers?

ALT: Berkeley chafed a little bit under the fact that he was not technically competent. He knew nothing about computers, really. He could play around with them. He got to the point where he could put little problems on the computer. He had learned to do that at Aiken's laboratory, and he learned to do it on the ones we had. He wasn't with any of the big organizations, nor was he scientifically anywhere or technically.

AKERA: Right. He was an actuary, so he didn't have an advanced mathematics degree? He didn't have--

ALT: Right. He didn't have any degrees, and he didn't have any great technical knowledge in either field.

AKERA: But then why do you suppose he was so interested in computers?

ALT: He was really a good salesman for computing. He was at every meeting. He wrote the [ACM Secretary's] reports for a long time. He was the secretary of the Association.

AKERA: He worked very hard in the early history of the organization, didn't he?

ALT: He was Secretary of the organization almost from the start, or directly from the start. Yes, exactly from the start. He seemed ideal for that purpose.

AKERA: Because he had a lot of enthusiasm and energy? Perhaps also because he worked at Prudential, and could regularly mimeograph his "reports" and distribute it to the membership?

ALT: I wish there was something I could locate. When he retired, when Ed Berkeley finally decided to resign from secretary-ship, we in the [ACM] Council passed a vote of thanks that E. G. Andrews at that time formulated, recognizing his enthusiasm and the zeal with which he pursued this work. I wish I could locate that. It must be in the Minutes of a Council meeting of '53, drafted by Andrews, voted by the council. A thank you note for his seed work.

AKERA: If I can ask the question this way, I think when you wrote the piece "Fifteen Years ACM," you mention Berkeley but you didn't speak very much about him. And then later on, I think it's in your piece from 1996, you actually speak a bit more about Berkeley and the work he did for the organization.

ALT: That was my fault. That was as much my fault as anyone's. I should have given him more credit than I did. But I don't know. I should say I didn't know what to say about him. Yes, if you have that impression, you're right. And it may have been my personal fault as much as anyone's—Caldwell's, all of us.

AKERA: No, I don't think it's anybody's fault, per se, but there was something about the growing up of the organization. I think you mentioned yourself that ACM began very much as a grassroots organization. I think part of that was due to Berkeley's personal enthusiasm for computing.

ALT: The job began to be too big for him. It's true. I mean, he couldn't keep up with it.

AKERA: And I think there were some differences of opinion at the time. But without asking this so directly, can you tell me...

ALT: ...he said he would need some secretarial help. Now that we are there, that reminds me of the next step. When did we move to the New York Academy of Sciences? We had a succession of Secretaries after Berkeley resigned.

AKERA: Yes. Bromberg was the first, I think.

ALT: Good. And then about the fourth or so, who was he?¹¹ From the West Coast. He said we shouldn't be doing this work by ourselves, by volunteers. We should...

¹¹ Actually, the decision to contract out the membership services of ACM to the NY Academy of Sciences occurred under Bromberg, who immediately recognized the volume of work that Berkeley had been undertaking as the ACM Secretary. Bromberg, who was himself an academic, convinced the ACM Council that more formal arrangements to handle such things as the membership list had to be transferred out of the Secretary's duties and to an organization

AKERA: ...create a whole national office?

ALT: And I don't know how he found the New York Academy, but it was sold to me. Somebody came to me and said, "Now we think we ought to go to the New York Academy." And I went in to see their office. I spoke to their director, whose husband had been a technical editor, and she was doing the business side of it. The New York Academy at that time was principally a publishing organization. They made money, lived that way—made money on the publications.

AKERA: But as a consequence, they were good at handling paperwork and mailing lists and such, and could really support the administrative functions that Edmund Berkeley had been doing by himself up until that point?

ALT: Yes. Then the New York Academy saw that this was a big job—saw that they would have to hire one or two secretarial people. They hired somebody whose name I can't remember, a woman. And then I persuaded them to hire Myrtle Kellington. She had been at the Bureau of Standards. She had been John Curtiss' assistant—not secretary as much as mathematical assistant. She was something of a mathematician.

AKERA: And later on hired her as the first, staff editor for the *Journal of ACM*, is that right? Not editor in chief, of course, but as senior editorial staff.

ALT: Yes. She was mathematically competent. [Pause] She was John Curtiss' mathematical assistant. Then she became mine when I was at the Bureau of Standards. Myrtle Kellington worked directly for me on the editorial work. And I persuaded ACM to take her over. A West Coast man—was it Forsythe?

AKERA: George Forsythe played a major role in putting together the Editorial Board. There was also Richard Canning, some other names that come up later—Don Parker, for instance.

ALT: Let me see a copy of my "Fifteen Years" paper. The Appendix is very neat there.

AKERA: Very useful, yes. These are the officers.

ALT: Now, what am I looking for? Secretaries. Bromberg. Gilchrist was the one.

AKERA: Yes, Bruce Gilchrist.

ALT: Who sold us to the New York Academy, I think.

AKERA: Actually, I think the New York Academy was 1954. And then ACM decided to create its own offices in 1960 under Bruce Gilchrist. But those are both very similar developments, and both have to do with the growth of the organization.

like the NY Academy of Sciences. In the reference to the fourth ACM secretary, Dr. Alt is probably remembering the second administrative transition, where ACM decided to set up its own offices in New York.

ALT: It could have been Bromberg. That could be that, then. My earlier recollection is wrong.

AKERA: I've actually looked at some of the primary sources from that time. Bromberg takes over from Berkeley first, just as Secretary.

ALT: Bromberg took over when Berkeley realized that he didn't want to do it all by himself.

AKERA: Realized how much work there was to do, I think. And Berkeley had been complaining to the president, who was Samuel B. Williams at the time, saying that...

ALT: Would you happen to know, I think Bromberg is still at the Courant Institute here in New York.

AKERA: That I don't know. I'm sorry.

ALT: I think so. I was at the Courant Institute recently, a year or so ago, because they had a memorial for somebody who had died. We went there, and to my amazement, there was Bromberg. I wouldn't have recognized him. But he introduced himself. I don't recognize people. My eyesight is very poor now. I can never recognize people. I might mention, If I meet you in the street tomorrow, I won't know who you are because I have glaucoma in the right eye, and I don't see details of faces.

Well, there was Bromberg there, and we exchanged reminiscences.

[Pause in Recording]

AKERA: Good. Let me return to Edmund Berkeley. We were talking about how Ed Berkeley is somebody that had a certain kind of reputation inside ACM. I think he was a wonderful, enthusiastic person. But I just wanted to get this for the historical record: what did other people say about Ed Berkeley, at the time? What did people regard to be his strengths as well as his weaknesses?

ALT: Oh, the same thing. We all agreed. As far as I know, there was no disagreement about Ed Berkeley—a very competent secretary, and glad to have him. We were glad we could rely on him.

AKERA: It seems to me that Ed Berkeley did have some sort of run-ins with your successor, Samuel Williams. Do you recall any of the specific differences? Something led to his resignation in 1953, and I don't think it was just that he...

ALT: He found, in the end, that the job was so big that he would need some secretarial help, and he wanted ACM to pay for that. The ACM Council didn't want to agree to that, because by the time you put some money into it, we wanted to have somebody more technically competent. So we either voted it down or dragged our heels. I'm not sure which. That led to his resignation.

AKERA: Because by that point, ACM was growing as an organization and really needed more official staff support... not just a voluntary Secretary doing it on the side.

ALT: Yes. You know, he set up his own publication. I wish I could remember the title. He had a periodical.

AKERA: I see. But if I can just stay with this other line of questions, some of the things in the historical record that came up as a disagreement, of sorts, was that Berkeley wanted to keep the membership dues at \$2. At least I think so. At that point, a proposal had been put forward to increase the membership dues so that ACM could start publishing its own journal. I know there was a whole related set of issues. Can you comment about this a little bit?

ALT: Some place along the line you asked, what were some of the challenges for the ACM Council, say, at the time when I was President and after? One of the big questions to be debated was whether we should have our own publication. On that, I was negative in the beginning. I remember walking home from the Council meeting with somebody, I don't know who, and he said, "Look, you know you're going to lose that. They're going to set up a publication sooner or later. You might as well go along." And that impressed me. I followed his advice.

AKERA: And became the first editor in chief, as a consequence?

ALT: Yes. When it was voted, I said, in that case, if it's accepted, I'll volunteer to be the first editor. That way I'll be at least convinced that it's done the way I think is right, the way I think it should be done.

AKERA: That's right. I understand. And these were closely related issues, since here you would have fallen on the same side as Ed Berkeley, who really felt that ACM should not issue a journal because to issue a journal meant that the membership dues had to go up. And to increase the membership dues would have meant that fewer people would be able to join the ACM.

ALT: I don't know that Ed Berkeley was on that side.

AKERA: I'm pretty sure he was. I mean, this is based on...

ALT: Are you sure it would have been? Because he wanted to issue his own publication. I don't remember that [what you say was his motive].

AKERA: It's hard to say. My recollection from the historical documents is that he wanted ACM to be as broad of an organization as possible. He had started ACM that way, which is to say, he set out to get all the different people who were interested in computing to come together and speak with each other. He wanted this inclusivity; he wanted ACM to be very inclusive. And if you raised the dues, and made it more academic, that this might seem like constraining the mission of the organization.

ALT: Well, if that's the way you remember it or you see it, I'll give in.

AKERA: No, you don't have to give in! And I am asking about what your recollections are in this regard.

ALT: Well, that's not my recollection. That was not where he opposed us.

AKERA: But leaving Berkeley aside, was that an issue that the organization was contending with?

ALT: Whether we should have a publication—that was the big issue.

AKERA: How about in terms of how academic the organization should be? Should it be primarily academic? Or should it be as inclusive and include a lot of different people?

ALT: The next step, after we had started the journal, there was a strong movement within the Council, and more even within the membership at large, for something that people could understand. The journal was an academic publication, a technical scientific publication [and people wanted something else].

AKERA: There were scientific articles in it.

ALT: Yes, refereed.

AKERA: And the articles tended to lean towards mathematics more than anything else; less on programming.

ALT: Yes, probably. But there was certainly a demand for a publication that was more popular, more widely understood. My recollection is that by that time, I knew I wouldn't resist it. But I looked around, asking who would be a good editor? Who would be in charge of it? I didn't want to be in charge of that. I looked and looked until I found at one meeting, going around, looking at people, "Hello. How are you?" Greeting people, talking to people, I happened to run into John Carr, I think, who was the first editor of the *Communications of the ACM*. And that's how *Communications* was started, with John Carr as the editor.

AKERA: Editor-in-Chief, yes. Does the name Alan Perlis also come up in this context?

ALT: I don't recognize the name.

AKERA: That's okay. I somehow thought that name was also associated with the early history of the *Communications*. I may have it wrong. He might have been involved with the *Computing Reviews*.

So the *Communications of the ACM* was started in 1958.

ALT: '58, yes. And [it was a monthly publication].

AKERA: That's right. It starts as a bimonthly publication, and becomes monthly after a while.

Now, we've had quite a long session, and I'm thinking that this would be an appropriate place to break. So this will be the official end of Session One of this interview. In Session Two, I will

come back and chat much more directly about the history of the ACM organization, if that will work for you.

ALT: All right.

Dr. Franz Alt

Oral History by Atsushi Akera

Session Two, February 2, 2006

AKERA: Just off the tape, I had reminded Dr. Alt of the beginnings of the conversation we had about the ACM at the end of the first session, and how most of today's interview will be focused on the ACM—various aspects of its history and its organization. Is there anywhere in particular that you would like to begin?

ALT: Well, the way ACM came about, there were many people interested in computing. But ACM, at the time, was a side issue. There wasn't quite a feeling that such a thing was necessary. During the war, or even before the Second World War, it was known that the IBM machines, which was used for accounting, could be used for scientific purposes. One of the early things that I found when I was here in America, I visited an economics research organization, and they had IBM machines there and showed me how they were used for record keeping.

AKERA: Is that to say that there was already a good deal of computing going on, and it wasn't clear that ACM would be the most important organization for those involved with computing?

ALT: ACM wasn't in existence yet before the war—certainly not. Early in the war, I think, the National Research Council established a committee on computing. It consisted of three people, essentially: Derrick Lehmer, Howard Aiken, and George Stibitz. He's been forgotten, George Stibitz. He was very important.

AKERA: I actually know something about his work. Yes, that's correct.

ALT: He was the one who had a real big project [at Bell Telephone Laboratories].

Now, just to get a sense of the kind of things we were interested in during the months immediately after World War II, I again remember the story Derrick Lehmer told us, much later. We had invited Lehmer to be one main speaker at one of our later ACM national meetings, and he chose to speak about the use of computers for number theory. He said number theory is a small part of mathematics, but it's one that seems to have some appeal to laymen. [This then led him to recount his story about Fermat's Lesser Theorem and its use by Lehmer and John Mauchly to test the ENIAC after its completion.]

AKERA: I believe that Dr. Lehmer's problem ran on the ENIAC during the summer of 1946. Is that right?

ALT: Yes.

AKERA: So that was one of the biggest, very well known calculations of the period?

ALT: Yes, and again, this was something indicated in a carbon copy of a memorandum that Dick Lehmer had written to the director of the laboratory, Col. Simon, about a test program that they had run on the ENIAC for a whole long weekend.

AKERA: That's right, because Lehmer was at the Ballistics Research Laboratory then, and as such, he and Mauchly worked together to figure out what an appropriate mathematical problem might be to run on the machine. I think what's particularly interesting about the number theory problem they ran was that this was a difficult enough problem that it attracted the attention of some mathematicians who could say, yes, an electronic computer could actually do an interesting problem in number theory—something as sophisticated in number theory—and produce useful results. There were many people who speculated about this—von Neumann among them—but to actually do it, to demonstrate it, was, I think, important to the post-war reputation of electronic computers among mathematicians.

Dr. Alt, if I can ask directly, in January of 1947 when the first idea about a national association for computer people was brought up by Dr. Caldwell, who would you have considered to be the major individuals or major centers for computing-related work in the United States?

ALT: Howard Aiken at Harvard had a computing laboratory. He worked with IBM. That was one.

AKERA: Bell Laboratories was also important.

ALT: Bell Laboratories. George Stibitz had actually done some work by 1933.

AKERA: But it was during the war that he built quite a few machines, or had designed it in any case, and had the Bell Laboratories engineers, Williams and Andrews, build them.

ALT: When I came to Aberdeen during the war in my first stay there—I was a Second Lieutenant—Col. Simon said, “The Army does the most incredible thing. They have just cut our allotment of officers by five, and we have to send five of our officers away to other organizations in the Army. And here they send us another one! What are we going to do with you?” He said, “We are establishing a committee on automatic computing, and [this committee would consist of] yourself and three civilians: Derrick Lehmer; Haskell Curry, a logician from upstate New York; and a very young astronomer from Boston.”

AKERA: If I can continue, it seems like there were quite a few established groups. There was also Samuel Caldwell and his group doing analog / digital electronic computing at MIT. Then there was, of course, the ENIAC at the University of Pennsylvania. And by 1947, weren't there also several new groups that already started to work in the computing field?

ALT: That sounds too early to me.

AKERA: C. V. Thompkins—is it Charles Thompkins?—was involved, even in '47, if I recall, as you mentioned in “Fifteen Years ACM.” He's listed as one of the original signatories to the letter

that Edmund Berkeley circulated. So Engineering Research Associates was already active in computing...

ALT: He must have been at Aiken's laboratory. Berkeley himself was at Harvard during the war as a Navy officer; he was at Howard Aiken's laboratory.

In any event, Berkeley was the one who really thought that there should be an organization. It didn't find any great resonance with people at the time. I thought programming was the bottleneck. We had these big machines coming. We had four young women sitting in Pennsylvania, where ENIAC was being constructed, preparing problems. Four young ladies--

AKERA: But by the same token, weren't there some concrete concerns about how you would go about preparing for the new machines, in preparing to use and basically learning to program the new machines? On the other hand, I supposed Berkeley's interests were much broader. Specifically, Berkeley's reasons for creating the ACM were broader. You said you didn't think there was a need for ACM at the time.

ALT: On the contrary! I remember once talking to Howard Aiken on the phone, I think, on a phone call, or maybe when I met him, asking whether there shouldn't be an organization for the exchange of information. It was Aiken who shrugged it off. He said, "Well, you can have a phone call once in a while, but there's no need, really."

AKERA: At this point, you were still at Aberdeen? Not yet at the National Bureau of Standards?

ALT: No, that's right. The Bureau of Standards came much later, in '48.

AKERA: How did you feel about...

ALT: But I'm talking now about '45, during the war. What we had at Aberdeen was two special machines built by IBM that had to be programmed. It was the first time that we thought about programming. When I came there, I was put on this so-called Computations Committee that Col. Simon set up, but we didn't do a real agenda except in preparing and deciding what had to be done. [While some worked on machines that had yet to be delivered] I liked to have immediate results, so I took on these [IBM] relay calculators. They were as big as a desk, each of them, and had to be programmed by plugging in wires.

AKERA: So these were IBM's Pluggable Sequence Relay Calculators?

ALT: Yes, that's right. So I did some real computing at Aberdeen for other sections of the Ballistic Research Laboratory. They were more enthusiastic than I about the results! They said, "So nice to see these things printed out in columns in a way that we can understand them. When we get them from our computers there, they are hand-written and hard to read and messed up and God knows what."

AKERA: So in other words, you were involved in breaking new ground in figuring out how to use such early machines.

ALT: How the machines worked, yes.

AKERA: Yes. So perhaps you yourself felt, unlike Aiken, that it would be valuable to exchange knowledge with other people about how to do this effectively. Would that be fair to say? Were you curious about how people programmed Aiken's machine, for instance?

ALT: Yes. Well, we had four programmers. But when these IBM machines came in with their greater speed, we needed much more. Then after the war, my main interest was to get out of the Army quickly. My work then had nothing to do with computing... but then I was interested in getting back into the government and doing something useful there.

AKERA: Sure. Dr. Alt, I'd like to just move to the ACM itself. So Berkeley circulates the memo in June of 1947 notifying people that there seems to be interest in creating some kind of association for computing machinery.

ALT: I had just come back to Aberdeen when I got that note.

AKERA: What was your reaction to the note?

ALT: Well, I was glad that this came up. Berkeley invited a number of us to come to New York. He had organized the kind of meeting in which there was going to be one technical paper so that we would have a reason for being there. That was the talk by [Tom Kite] Sharpless.

AKERA: That's right. And Sharpless was working on the EDVAC at the University of Pennsylvania, which meant that it could have been about the first stored program computer that would come on line—it was not, but a lot of people perceived it as such at the time...

ALT: I didn't think so.

AKERA: Is that right? I guess von Neumann would have been the obvious other person to speak before that crowd, right? And as it turns out, he was selected as the keynote speaker for the second meeting, which in fact took place at Aberdeen.

ALT: No, I was just thinking about when we started to actually program computers. When we had the ENIAC at Aberdeen, but we aren't there yet. In 1946, I applied to Aberdeen for a position. They didn't have any use for me at Aberdeen, but they said, "We have these two Bell Telephone relay machines being built in New York. We want somebody to learn about how to use them. Will you take that on?" So I was happy to do that. It gave me another half-year to stay in New York where I could be. And I learned all about how to program the Bell Telephone relay computers. They were a roomful of equipment. They were much larger. That was the kind of computing machines we were looking forward to using—what is now being called a mainframe computer. We were thinking about the two relay computers Bell Telephone Laboratories was building, and the ENIAC at the University of Pennsylvania.

And Aiken's IBM machine was also a mainframe, as we would call them. He was, by this time, calling his original computer the Mark I. And he was beginning to think about the Mark II. I don't know how far along that was.

AKERA: The January '47 meeting, actually, was based on a discussion about the design of the Harvard Mark III, actually. So I imagine it was fairly far along.

ALT: But no electronics?

AKERA: Hmm... Actually, let me stay focused on the ACM. So can you tell me something about the meeting at Columbia University in September of 1947—I guess that was the first time that the people to whom Berkeley sent a letter actually gathered.

ALT: Yes. We got together here in New York. I don't know how many... a roomful of people.

AKERA: It was 78 people, actually.

ALT: It was something like that. And Sharpless read a paper, and Berkeley felt that we should *pro forma* have a vote to establish an organization. So we all dutifully voted, "Yes. We are in favor."

AKERA: In other words, everybody gathered more or less just to have a conversation about computers. They knew that Berkeley might be interested in doing something more, and it was Berkeley who called the vote and formally organized what, at the time, he referred to as the Eastern Association for Computing Machinery.

ALT: Right. Oh, that's right.

AKERA: But the adjective, "Eastern" was dropped in early 1948. That wasn't that wasn't much later, but I think that's correct. I guess it says here also that you had elected officers and also created an executive council.

ALT: We went through the formalities.

AKERA: Actually, can you tell me a bit about these formalities? What kind of discussions did you have during that first meeting?

ALT: About the organization, at that meeting? No.

AKERA: There wasn't too much discussion?

ALT: I don't think so. We felt that one had to have a council and one had to have officers, and they had to be elected. Who put forward the names? Well, I as much as anyone else there felt we ought to have a balance between mathematicians, who would be users—that was the only kind of user we knew of then because it was so complicated—and the producers, the designers, let's say. We ought to have a balance between those two. Therefore, I put forward the name of Curtiss, who was my boss, and John Mauchly to balance him off.

AKERA: You proposed both of these individuals?¹²

¹² Who were ACM's first President and Vice President, respectively.

ALT: I guess so. I think so. I'm not sure. It was in the conversation.

AKERA: This certainly makes some sense because, of the people attending, John Mauchly would have represented the machine designer. And he had the cachet of being the inventor of the electronic computer?

ALT: Yes, but with regards to "inventor," I have heard people to say that it would be an overstatement to give that attribution to him. It was Mauchly and Eckert, J. Presper Eckert was the other one. Those two should always be named together. Perhaps. I can't tell how they divided the work between the two.¹³

AKERA: If I'm right, you were also a member of the original Council.

ALT: Because I was there.

AKERA: Because you were there, I see. Do you recall any of the other early members of the council? And who was considered important?

ALT: I can remember only by reading back the paper that I wrote 15 years after the founding. The early things were, apart from balance—that was always an important question, important decision, should we have all kinds of members? The American Institute of Electrical Engineers had, by this time, a section on...

AKERA: They had a special section on computers.

ALT: They had a section on computers. The Institute of Radio Engineers also had a committee...

AKERA: IRE had their Professional Group on Electronic Computers.

ALT: Right. So there were other committees active in the field.

AKERA: Actually, I was asking just about the ACM Council—what was called the Executive Council at the time. Do you remember any of the individuals on it—Robert Campbell?

ALT: I remember him. He was on the West Coast somewhere.

AKERA: Do you know why he was chosen to be the first treasurer?

ALT: Probably volunteered, but I don't know; I may be wrong.

¹³ The confusion here is due to my phrasing of the question. I should have probably said, "cachet of being one of the inventors." However, Dr. Alt's reply also hints at the controversy surrounding the origins of the electronic computer, and whether John Vincent Atanasoff at Iowa State College could be regarded as the "inventor" of the electronic computer. This historical dispute, as well as the precise nature of the relationship between Eckert and Mauchly, is discussed elsewhere in the history of computing literature.

AKERA: I assume Berkeley was selected to be Secretary because he was already doing all the groundwork, so this made sense. Now, some of the other people—Mina Rees?

ALT: Mina Rees was very important on a much higher level. And I should go back to John Curtiss: why was he important? Computing was not important to him. He was a mathematician. His father had been a professor of mathematics. His background was really in statistics. He dreamed of a mathematical organization, and he persuaded Ed Condon, the Director of the Bureau of Standards, to establish a Mathematics Division.¹⁴ The immediate need there was obviously statistics, namely on the experimental side. An organization like the Bureau of Standards that performs experiments—that's its lifeblood, experiments in physics and chemistry. You need quality control, and you need good experiment design. Experiment design is a word that just came up at that time, but we now know how important it is. So that was a very strong reason for establishing a statistics section somewhere in the Bureau of Standards.

And as we spoke about before, there had been in existence, for a while, the Mathematical Tables Project in New York... So John Curtiss had the idea to take these things and put them together into a Mathematics Division--

AKERA: And again, it was the former Mathematical Tables Project, which when it came down from New York City to Washington, became the Computation Laboratory of NBS?

ALT: Somewhat to their unhappiness. They were not glad to go, but they were persuaded. Some of them, anyway. And Curtiss needed money, needed it for financing for that operation. The Bureau of Standards could not put so much of its own money. One has to do what they must.

AKERA: No, I understand. And the National Bureau of Standards' situation, at the time, was that a very large percentage of its budget, something like 70%, came from external sources.

ALT: Mina Rees was important in this respect. Oh, there was no National Science Foundation at that time. That was established, I think, if my recollection is right, in '48.

AKERA: 1950 is the first year NSF received a budget. So there's actually quite a bit of time before...

ALT: the Science Foundation?

AKERA: Yes. National Science Foundation doesn't get set up until 1950.

ALT: That late?

AKERA: Yes.

ALT: That surprises me even more.

¹⁴ See note 5 above on the name of this unit.

AKERA: But that's why the Office of Naval Research remained so important, as a source of basic research funding, during the first five years after World War II.

ALT: Okay. But the Navy was one organization that had an Office of Research and Invention, as it was first called. And Alan Waterman was its director—a really important physicist. Mina Rees was under Waterman in charge of mathematics. She found the money—Navy money, that is—to finance John Curtiss by transferring money from the Office of Research and Invention to the Bureau of Standards.¹⁵

AKERA: In fact, if I know this history correctly, Mina Rees first makes the arrangement so that the Office of Research and Invention would support the Mathematical Tables Project. The Mathematical Tables Project had been supported during the war as a wartime project through the National Defense Research Committee (and its parent organization, the Office of Scientific Research and Development). And with the termination of the OSRD, they were desperate for new sources of funds. So I believe that's how she became involved with the Mathematical Tables Project.

ALT: Okay. That's very interesting to me. I didn't know that.

AKERA: Yes. And then this provided a sustained connection so that when John Curtiss became interested in creating an applied mathematics division, there was already an institutional connection there, and these components all started to come together.

ALT: That's interesting to hear.

AKERA: Now, I imagine, though, that the reason you nominated John Curtiss wasn't just because of his standing as a mathematician. Was it because you were trying to help him with his project.

ALT: It was entirely the latter. He was not a famous mathematician. Statistics is a denigrated part of mathematics, mathematical science, actually. No, I pushed him into the Council of the newly formed ACM because...

AKERA: ...you thought it would be useful for him?

ALT: No, it was useful for ACM to have him; not so much useful to him. ACM was a very small thing at that time.

AKERA: Well, yes. He was the person who was putting together a major national laboratory in applied mathematics.

¹⁵ Rees initially negotiated this in her role as the Technical Aide to the NDRC's Applied Mathematics Panel. She secured support from the Navy's Coordinator of Research and Development (who then became the head of the ORI, I believe), who agreed to transfer unexpended naval research funds to tide the MTP over until a more stable source of funding could be found. The decision to move the MTP to Washington was tied to the transfer of funds to either ORI or ONR (established only in August 1946), which presumably preferred to support this group in the context of its support for NAML.

ALT: It gave ACM some standing to have somebody like John Curtiss as President.

AKERA: That's great. And of course, having Mina Rees on the council also made sense. It probably didn't make sense to have her as the president because she was a sponsor, and you don't want that kind of arrangement in a professional organization.

ALT: She could keep her fingers on the initiative that way. She knew what was going on. Something I missed in your written questions... About three years later, ACM, by that time was already a more well-established organization, and it held a meeting in Washington. That was me! [Chuckles] I suggested this to the Council. Then, I was at the Bureau of Standards.

AKERA: That's why you organized two meetings (as mentioned elsewhere in a different publication). I missed that connection. That's right.

ALT: And so I urged the Council to set ACM's next, annual national meeting to Washington. At first I thought we could have it at the Bureau, on the Bureau grounds, but it grew too big for us.

AKERA: So Dr. Alt, can we go back to the *first* meeting, just to keep things in chronological order?

ALT: Yes, right.

AKERA: Because, just one item for clarification first—when you nominated Dr. Curtiss, you were still at Aberdeen? Not yet at NBS, obviously, because the group there had not been created yet.

ALT: Curtiss was not at Aberdeen. He was probably at the Bureau of Standards at that time, I think.

AKERA: No, but at the time, in [September of] '47, I think you were at Aberdeen.

ALT: Wait a moment. Not until... I got out of the Army at the end of '45...

AKERA: ...You returned at the end of '46...

ALT: And I stayed at the Econometric Institute until late '46. Then somewhat before that, I applied to Aberdeen for a position. It was accepted as of November '46 or something, but with the provision that I could stay in New York for a while until the Bell Telephone Laboratories machines were finished. I went down to West Street every day and just wrote little programs for the machines. I remember that we wrote one program for launching a rocket. It was one of the early programs for that machine.

AKERA: Aberdeen was beginning to get into that direction. But by September of '47, then, you were certainly already in Aberdeen, right? Because then--

ALT: I moved back to Aberdeen sometime in '47. And for a year, I was at Aberdeen as a civilian.

ANNICE ALT: But for the first meeting of ACM, you were at the Bureau or Aberdeen?

ALT: At Aberdeen.

AKERA: Okay. In any case, I was just establishing the timing, for the record.

But so one of the outgrowths of this meeting at Columbia University is that the first real national meeting takes place, with an actual technical program and everything. And this happens at Aberdeen. If I'm correct, you were the one who proposed this meeting and organized it?

ALT: Oh, yes.

AKERA: What was your reason for having everybody come down to Aberdeen?

ALT: [Chuckles] I don't know what answer I should respond with.

AKERA: It's easier to have people come to you than to go elsewhere, I suppose [laughs].

ALT: Actually, I hadn't expected that many people. I hadn't expected as many as 100 to come. And as it was, 300 registered in advance, and I didn't know where to put them up, where to quarter them. There are no hotels down there.

AKERA: No, there are no hotels in Aberdeen, Maryland...

ALT: I went to Col. Simon and asked him, "What should I do now?" He deputized another officer to help with that, and he found some barracks. And in the Mess Hall, we had a dinner meeting. It was rather primitive. It was so close to the war, it still seemed alright.

AKERA: I see. I imagine the contrast with the Washington meeting will become apparent, as it was held at a hotel. So the Aberdeen meeting, what was its purpose? This was really the second major meeting of the ACM, right?

ALT: Really the first, because the one in New York had been small and we hadn't known how to advertise it.

AKERA: Yes, I guess that was really a planning meeting. It wasn't a conference, per se.

ALT: Yes. But at Aberdeen, we had some publicity ahead of time. And as I said, more people responded than we had expected.

AKERA: So why do you suppose so many people decided to come?

ALT: Well, it was in the air at that time that this was an up-and-coming field of interest.

AKERA: Were those attending mostly computer designers? Or were they mostly people who were interested in using computers?

ALT: There were all kinds of people. There were certainly both kinds of people, if you want to label the two. But there were also all kinds of prospective users.

AKERA: Sure. Please elaborate.

ALT: There had been some people, for instance, who had been using IBM machines—their sorters and tabulators—for computing. There was a man named Herbert Grosh, an astronomer, who should be remembered as an early member. I don't know why we didn't make him an officer.

AKERA: He doesn't show up as an officer.

ALT: No, he doesn't. But he was important.

AKERA: Yes, I guess he was around from very early. Wasn't he helping to run the Astronomical Computing Bureau in New York City? Is that right?

ALT: Yes. At that time, the first time I came to Aberdeen, before ACM existed, there had been a book on the use of IBM machines--

AKERA: *Punch Card Methods of Tabulation* by Wallace Eckert, is that right?

ALT: Wallace Eckert, right. Now, the second meeting: that was a big meeting. It had John von Neumann's famous opening address. One has to give instructions to machines. It turns out that about a dozen or so instructions are quite adequate. Instructions like: add, multiply, and transfer control from here to here, things like that—if-then an instruction of that kind. There's about a dozen instructions that would be adequate to program all mathematical problems. It may be surprising to people that this complicated field of mathematics was...

AKERA: ...was reducible to such a simple...

ALT: --was expressible in so few words. But, on the other hand, he said "It is known that about 800 English words would be enough to express all situations of life. [Chuckles] And life is much more complicated than mathematics! Mathematics is a very simple part of life." There was some laughter at that. Then, he added, famously, that "People don't think mathematics is simple because they don't realize how complicated life is."

[Begin Tape 3A]

AKERA: You were just speaking about Dr. von Neumann's famous address.

ALT: Yes. But the big problem with computers at the time was down time.

AKERA: Yes. But if I can ask, were people at the time intrigued by Dr. von Neumann's comments partly because it had some significant implications for computer design? In other

words, if all mathematical problems could be solved using a very limited instruction set, the implication was that a relatively simple computer design could suffice to serve all of applied mathematics. This meant that computer design was, our ought to be a fairly simple engineering challenge. Is this correct?

ALT: Yes. But to put a machine into reliable operation—that was a different matter. We didn't have reliable components. [This complicated the machine design.] Again, von Neumann himself noted how computers had become so elaborate that every time you turned the ENIAC on, it blew two tubes.

AKERA: And in fact, if I'm correct, the earliest computers—this gets a little ahead of the story—had fairly predictable failures rates. That machines would tend to run, on average, say 8 hours, or 12 hours. So you would try to create programs that had self-checking procedures, and would periodically dump out any intermediate results, so you could restart from a reliable breaking point. I imagine those were some of the programming tricks that you had to use back then.

ALT: Yes, and here's that story once again about the Bell Telephone machines down here on West Street, when I came into the office one morning, and there were papers all over the floor. [This was what was so surprising about that machine's failure to fail.]

AKERA: You didn't expect it to happen.

ALT: It was unusual. We didn't believe that it could happen.

AKERA: But the mechanical computers at the time were more reliable than the early electronic machines. Is that correct?

ALT: Right.

AKERA: I can imagine that in—this would be 1948, is it, when you met in Aberdeen?

ALT: It was still late '47.

AKERA: Oh, still '47 when you met in... [Looking through notes.] Yes, that would be December of '47 when you met at the Ballistics Research Laboratory in Aberdeen. I imagine these were the kinds of technical conversations that you were having about computer operations and its design.

ALT: But that didn't concern the ACM.

AKERA: What didn't concern the ACM?

ALT: The technical problems.

AKERA: Why not?

ALT: Well, at ACM, we read papers and had conversations. It was not our job to design machines or build machines or use machines.

AKERA: Wait. What kinds of papers were presented at the ACM? Were they mathematical research papers on applied mathematics?

ALT: No. They were usually a description of problems from other fields that were being put into mathematical formulation so that they could be solved by computing.

AKERA: So you were taking general (scientific and engineering) problems and converting them into computable form, which required some effort in terms of this translation...

ALT: Yes. Now, among the early problems that ACM had to deal with in the Council was to keep a balance between different kinds of people.

AKERA: And different interests, yes.

ALT: And then whether we should have a publication. That was discussed from the beginning.

AKERA: From the beginning, I see.

Let me shift to this. How were publications handled in the early days. There was a Publications Committee created quite early on. In fact, I believe you were the first chair of the Publications Committee, in 1948?

ALT: Yes. But I don't even remember who else was on the committee.

AKERA: So this really wasn't yet an editorial board? It was just a regular committee within ACM?

ALT: Of course. Sure.

AKERA: Please go ahead. Can you tell us something more about the Publications Committee?

ALT: I think we discussed this before, but in the beginning, I really was dragging my feet. I remember once walking home from a Council meeting and talking about this: should we have a publication? And somebody was walking along and said to me, "You know, it's going to be voted anyway that we should have a publication. You might as well go along with it." And it impressed me; in modern day language, saying, "You should get on the bandwagon."

Yes, he was probably right. The general sentiment was more and more for having our own publication. It seemed unnecessary to me at first. There were so many technical publications in all fields, many more than one could read. There was one specifically, MTOAC, *Mathematical Tables and Other Aids to Computation*. At one time, I suggested in the Council, I remember, that this be made the official publications outlet for the ACM. Well, MTOAC was edited by Prof. Archibald at, at...

AKERA: Brown University.

ALT: Brown University. I knew it was some place up there, but couldn't remember. Well, finally, the vote was taken that ACM should have its own publication, its own journal. At that point, I volunteered to serve as editor because I felt if we have to have one, at least it should be done according to standards.

AKERA: I meant to ask a question on that. When you say "standards," what did you really have in mind? Academic standards? Standards in mathematics?

ALT: Just that papers should be refereed. Mainly that. Papers should be refereed.

AKERA: And perhaps that they should be fairly scientific, as recognized by mathematics as a discipline?

ALT: In order for a paper to be accepted, it should say something new. It should not merely tell a story of what's happening.

AKERA: Not so much techniques, but really something that could be considered an academic finding?

ALT: The author should have made a new contribution. It should be, of course, technically correct. That is the point of refereeing. It should not have been published someplace else before. And that was not obvious to everyone. I remember an author saying, "I have published this in such-and-such place, but you are welcome to publish it again." [Chuckles] No, we don't want to do that. We don't want to duplicate publications, even less so.

AKERA: Did you feel that a journal at this point—this would be 1953—was also an important means for establishing computing as an academic discipline? Was this other kind of motive at work, where things went in the other direction: to say that you wanted to do this to help build up ACM's image as a professional society?

ALT: Maybe you are right, in retrospect. I certainly didn't feel that way at the time! I felt that if we have a publication, it must meet the standards.

AKERA: What I know from the historical record is that this conversation took some time to resolve. It was really between... The Publications Committee was created in 1948, but there really was active conversation until 1953. Some felt it was expensive to produce a publication, and ACM didn't have a lot of money early on. It was still, really, a grassroots organization. It was only collecting \$2 dollars from each member.

ALT: Yes. Well, that was one argument. But as it turned out, it didn't make any difference. People were just as glad to pay \$6 dollars instead of \$2.

AKERA: Yes. But it seemed to me that this issue took some time to resolve, partly because the emphasis of ACM was on the national meetings, and everybody was already freely exchanging ideas there. The field was moving very rapidly, and in a very fast-moving field, publications may

not seem as important because of the time it takes to get something in print. By then, new things would have already happened. Moreover, I suppose many of the early findings were considered quite small, isn't that right? That much of the early improvements were kind of incremental: new techniques for reducing this particular equation to a computable algorithm, or whatnot. And the academic merits of this kind of work wasn't so apparent those with disciplinary interests, at least until the field came into its own in some ways.

I guess, separately, it's also worth noting that Edmund Berkeley himself had tried circulating all of the abstracts from the conference in lieu of a *Proceedings*, so that I think he might have felt that this was a sufficient way of conveying technical information, and perhaps didn't fully understand academic tradition in this respect.

ALT: And he was in Aiken's laboratory before he returned to Prudential. I think he had been at Prudential before the war, and then returned. Aiken also felt it was not necessary to publish information...

AKERA: ...to have a journal? I think it may also be relevant, in some ways, that Berkeley was still at Prudential, and perhaps even using the resources and facilities of Prudential Life Insurance to produce the ACM's reports and other informal publications. And at a certain point, this became a budgetary concern. He needed money to do some of this, whether inside, or outside of Prudential. We spoke about this last time, a little bit.

Let me ask, can you tell us anything else about the early character of the national meetings? Say, the first four or five meetings leading up to the meeting in Detroit. D.C. was the fourth one, wasn't? Given that I want to ask you about this separately, let's say the first three meetings, up through Rutgers.

ALT: Generally, we were always hoping that somebody would invite us to come to a meeting at their institution, because we realized it was a lot of work to organize a meeting. There had to be an organization behind it. Had to be somebody who worked for a sizeable organization in order to be able to...

AKERA: ...put on an ACM national meeting, yes?

ALT: So we were always waiting for somebody to volunteer. Alston Householder, Oak Ridge National Laboratory, that was a good possibility.

AKERA: That was the second national meeting. And why was Oak Ridge interested in computing?

ALT: Well, by that time, I think any group in that area... Many of the early computing problems that came up had to do with nuclear physics.

AKERA: Statistical analyses of nuclear material processing, for instance?

ALT: There was, for one thing, an impossible problem: A second order partial differential equation describes the orbiting of electron particles. On ENIAC, some group, not from Oak

Ridge, but from Los Alamos, tried to put the problem on our machine, and tried to get time on the ENIAC to solve the problem. That group was Robert Richtmyer and Leslie Peck—two physicists from Los Alamos, who came over to Washington. And they had high priority for their job. So why wouldn't Oak Ridge be interested in having its own computer? Quite right.

AKERA: So Alston Householder was very involved with the computing work there?

ALT: He was one who felt that he could carry the load.

AKERA: And he was a member of the Council, is that correct? From the beginning or at least early on.

ALT: Yes. I think you got on the Council, again, mostly by volunteering.

AKERA: Yes. And he served as the fourth Vice President, and fifth President, of ACM.

ALT: It sounds like they were "elected." It was mostly about finding somebody who was willing to serve.

AKERA: That's great. So back to the national meetings. So the goal really was to have one of the big centers that were doing computing research—or at least wanting to get involved—who had a reason to pull others to come to their site, so that they could either launch their own computing initiative, or else advertise their program and its latest efforts?

ALT: Yes.

AKERA: Actually, going back to the first national meeting, was this part of the reason for bringing everybody together at Aberdeen as well? Was it to really to draw attention to, and make people aware of the services offered by the Computation Laboratory at Aberdeen?

ALT: Well, it was really more that Aberdeen was almost the only place where computers were actually in operation. It was worth going to Aberdeen just to see them, just to talk to people doing this work.

AKERA: So there was a lot of equipment and activity there. People wanted to come to Aberdeen to see the ENIAC there. Is that correct? The ENIAC was moved there earlier in '47?

ALT: ENIAC was moved in '46, I thought. Maybe I'm wrong. '47, yes.

AKERA: So in any case, it was already at Aberdeen.

ALT: By the time of that meeting, ENIAC was there, and running.

AKERA: And the Bell machine was there, and the modified IBM machines were there. So there was a lot of computation capabilities at BRL, which made it quite unique at the time.

ALT: ENIAC was running if you kept your fingers crossed [laughs]. It was certainly being programmed, and there were people there.

AKERA: Now, can you tell us something about the tenor of the early ACM Council meetings? You know, what kind of things did you have to talk about as a Council? Not the national meetings, but the Council itself, as an executive body.

ALT: Well, in the first place, we asked, “where is the next meeting going to be?” Who would like to volunteer? Secondly: “should we have a publication?” That was a big problem, discussed each time.

AKERA: Was there much discussion about the constitution or bylaws?

ALT: Oh, yes.

AKERA: And very early on, I guess?

ALT: That’s right. E. G. Andrews got into that quite strongly. He must have been on a subcommittee that was writing the constitution. I’m not sure.

AKERA: That’s right. A subcommittee made up of E. G. Andrews, Berkeley, and J. B. Roosll, whose name I don’t recognize, submitted a constitution to the membership at the Aberdeen meeting.

ALT: That’s right. We had a little group working on a constitution. And then it failed, namely it made the unwise provision that at least half the members should vote for it, and we fell short by a few votes.

AKERA: 177 votes were cast, and this was 8 votes short of the quorum, I believe.

ALT: But finally, we got it passed somehow.

AKERA: Yes, finally it was passed. Was there anything substantial in the early constitution? Anything that you and others spent some time discussing? Or was it perceived just as a generic document?

ALT: I don’t remember any important discussion about it.

AKERA: Only that you weren’t able to get it passed [chuckles].

ALT: Let’s see... Yes, there was one important issue that doesn’t show up anywhere; namely, if there was a national meeting, how would the speakers be selected? How would papers be accepted? Should papers submitted for presentation at the national meeting have to be refereed? Or how would they be selected? There was a lot of discussion on that.

Should they be published, necessarily? That was also a big thing, because people who attended the national meeting always wanted to have some written record of papers that they had heard

but hadn't quite followed. The attendants always wanted a written record. On the other hand, that would have been a publication much bigger than [the *Journal*]

AKERA: Quite large, yes, especially as the program became so large.

ALT: We finally settled on something, which I think is worth for emulation. We specified that anyone wanting to present a paper, offering to present a paper, should submit not an abstract, but something more. There was a better word for it. The word will come back to me.¹⁶ And they'll insert it with the program. It was more elaborate than an abstract.

AKERA: Like a synopsis or something, of the paper?

ALT: Something like that, yes. I'll find the word, because it took me a while to find it that time! And these should be published so that there would not necessarily be a publication of full papers. The papers might then be submitted to the *Journal* or to a publication someplace, or not. But at least these synopses would be short.

AKERA: I imagine there was the dual concern about the cost of publishing an entire *Proceedings*, plus concerns about quality—that if you were going to create a publication, you wanted them to be high-quality academic publications. Is that to say that in the early national meetings, there was a great deal of variation in the quality of papers presented?

ALT: I don't mean to say that.

AKERA: But some were more academically worthy than others? Although they may be important papers, some of the papers were based on some aspect of design or some technique that's not necessarily considered "academic."

ALT: You're dealing with so many people, many of them not academic, many of them who have never before presented a paper, others who have been at numerous meetings and know what's being done and can compare. Some are commercially interested—all kinds of things. And you have to weed them out. I mean, you *should* weed them out.

AKERA: Yes, if you wind up with a more valuable publication as a result of doing that. Now, ACM did actually experiment once with a *Proceedings*. I believe the Toronto meeting--

ALT: Twice. I think we had two volumes of *Proceedings* and then we decided on a compromise.

AKERA: And the compromise was that, by then the *Journal* was in existence, and the compromise was to say that the best papers will be published. Some would go into the journal. Some, if it's appropriate, would be recommended to other journals. Is that correct?

ALT: Yes.

¹⁶ Note *pro domo*: The word was "digest"

AKERA: Okay. Let me move on, if I may, to a slightly different question. We've spoken about this a little bit already, but perhaps on a more personal level, can you tell me why you feel you, yourself, became drawn into the ACM? How did you become so involved quickly with ACM?

ALT: Oh. Because there was a gap, because I happened to be there. That was really all.

AKERA: What gap, in particular, did you fill? You had Mauchly representing the computer manufacturers. You had Curtiss representing applied mathematics, but more, as you say, on the statistical side and not particularly research methods.

ALT: No, Curtiss did not get in at all. I don't know that he attended Council meetings. Yes, in the beginning, just until things got going.

AKERA: But he dropped out? And Berkeley represented more of the commercial end of things.

ALT: Berkeley was there, and I was concerned that Berkeley was not a technical person. I was concerned ACM would slip too much in that direction. You said in one of your questions last time that I seemed to talk Berkeley down. No, that was not it. I was trying to keep ACM from becoming too much non-technical. I wanted it to be a competent technical organization.

AKERA: But with very diverse interests. Is that correct? Or did you, yourself, want to see ACM go in certain directions, like more to applied mathematics? Or were you also wanting to see the organization go into many different directions, but simply be of a high caliber?

ALT: Well, I thought it should be a technical organization, but of both users and designers and builders—both engineers and maybe mathematicians, but more generally users. In the beginning... I may have been wrong at that time. I may have overestimated the need to understand mathematics for designing programs. Maybe in my own mind, I overstated that. But I learned.

AKERA: So what you wanted to focus on was developing mathematical techniques suitable for solving a range of problems in the physical sciences in particular?

ALT: Yes.

AKERA: I understand. Very good. Let me ask, then, since the subject is on the table: did this perspective guide your emphasis for the *Journal of the ACM* initially? Did this attitude carry over into the *Journal* in its earliest years?

ALT: Very much, yes. As long as I had the *Journal*, as long as I was running the *Journal*, I tried to run it like a technical publication.

AKERA: What was the breadth? What topics did you cover, and what topics did you tend not to cover in the *Journal*?

ALT: Oh, it wasn't the subject areas. We considered whatever would come in. It wasn't the subject areas that I tried to sift or use as a sieve, but with the standards that they should represent contributions by the author.

AKERA: So you took engineering articles as well, sometimes?

ALT: Indeed, if we got them, yes. There were very early papers, I think, on miniaturization. I don't know whether that came in still in the first three years or even later. But at that time, that was something very interesting.

AKERA: Did you have an editorial board that consisted of different kinds of people?

ALT: Yes. When the *Journal* was started, I tried to have a group of editors, I think we called it the Board of Editors, that represented different aspects. I remember Morris Rubinoff helped a great deal. He was at the University of Pennsylvania, and he taught engineering, I believe. I think so.

AKERA: So you had an engineering representative. You certainly must have had somebody in mathematics, right? You would have been able to serve in that capacity as well, though.

ALT: Yes. Well, I don't remember now. I don't remember who the members were. That would be nice to know. But I don't remember whether...

AKERA: But it was a diverse group, so that the various subfields were represented. Perhaps about four people covered different aspects of computing?

ALT: Yes, about four people.

AKERA: Yes, that makes sense. Did you accept submissions on programming techniques?

ALT: Definitely. A man whose name might well appear, Herman Goldstine, who wrote a paper on flow charts—it was something everyone did. I'm not sure that I would even have accepted a paper on flow charts. Yes probably; just barely, but borderline. It was not a great...

AKERA: ...finding, but it was important to document because everybody did them. I guess that makes sense.

I'm sorry, I've jumped forward a little bit, since this is more about your tenure as the editor of the *Journal of the ACM*. Let me move back to the earliest years and ask the question—why do you suppose you were nominated to be Vice President in 1948?

ALT: When would that have happened?

AKERA: Let's see. Curtiss and Mauchly were the first President and Vice President. It would have been during early 1948. You had just had the meeting in Aberdeen.

ALT: It may have been that I volunteered. It could be.

AKERA: Yes. I guess Curtiss started to become less involved. And by then, I think you were just...

ALT: Yes, that's right.

AKERA: ...moving over to NBS, so you might have been seen as an appropriate substitute for Curtiss. This, plus all of the work that you had done as well, of course.

ALT: Yes. I read one of your questions. Was it customary to have people advance from Vice President to President? Yes, I think so.

AKERA: In the beginning, I believe.

ALT: Usually done unless the incumbent refused. Lehmer, for instance, was Vice President somewhere and didn't want to become President. He didn't think it was worth his time. But in general, we thought it was a good system. Everybody thought so. And we are not the only organization that actually does that.

AKERA: Regarding why you were nominated, was it partly, do you suppose, because you had a PhD in mathematics and that people admired that?

ALT: I don't think anyone cared.

AKERA: That didn't matter? No? So it was probably more your involvement in the organization.

ALT: Yes, because I was there.

AKERA: And your position as the Deputy Chief, or whatever, of the Computation Laboratory.

ALT: I could do something with the computers.

AKERA: So you were certainly a very active and visible member. Yes. Okay. That makes sense. Let's see.

ALT: You had to look for people at that time.

AKERA: So it was really more about finding volunteers?

ALT: Right. I remember when somebody asked me at one of the meetings, just walking around the aisles, you know, "Hey, Franz, don't you think you could find somebody who would be good for such-and-such?" And I said, "Oh, yes. I just spoke to so-and-so. He would be the right person." That's the way it went.

AKERA: That's right, so things were done much more on an ad hoc basis until you got a nominating committee together. But I imagine other people must have encouraged you, or put you forward as well.

Let me move to the substance of some of the issues you might have been working with at the time as an officer of the ACM. Part of the work, I think, was just simply about growing the organization, so I think there was a lot of conversations of that nature. But this was also a time when the first electronic computers were taking some time to come into existence. In 1949, for instance—so while you were Vice President—there's a major study by the Defense Department's Research and Development Board. It's was a fairly critical study, this may be overstating it a bit, but they felt that there was a lot of duplication in computer development research, and they explicitly criticized some of the projects for being too large and expensive. Do you remember that particular report and the repercussions of it?

ALT: I'm not sure which report that was. But one had a general feeling that deserves to be said now, which was that IBM was not keeping up. IBM had missed the boat. They were not working on electronics, really. They didn't know anything about electronic components. Then suddenly, IBM came up with the 701? Yes? That was a surprise. "Oh, IBM is in again."

By that time, there were many computers already in existence. Now, Michigan had an early one. It was John Carr, I think. And Oak Ridge? The Institute for Advanced Study...

AKERA: ...with John von Neumann. Jay Forrester at MIT was also working on the Whirlwind computer. Do you recall that machine?

ALT: Yes.

AKERA: So that's some of the major computers.

ALT: But the Bureau of Standards was one of the early organizations.

AKERA: I'm actually going to ask about NBS' computer later on during the interview. I understand. You were in fact...

ALT: ...we had the second working electronic computer.¹⁷ After ENIAC, the first working computer was ours.

AKERA: And it was the first stored-program computer to operate reliably in the United States?

ALT: Yes.

AKERA: The first "in the United States" is an important qualification there, because the British build the first two. The first one, the Mark I at Manchester, was not really a full-fledged computer. But the EDSAC, under Maurice Wilkes at Cambridge University, was a real stored-program computer and entered operation in 1949, roughly a year before the SEAC.

¹⁷ This would have been in the United States. The Manchester Mark I and Cambridge University's EDSAC were in operation in 1948 and 1949, respectively. The Standards Eastern Automatic Computer entered operation in May of 1950. (The dedication was held in June of 1950.)

ALT: Oh, yes.

AKERA: Was that something that the ACM discussed?

ALT: Of course. The big problem in computer design at that time was memory. How much memory, how much you need, and what do you use for it, what—

AKERA: And how do you make it reliable, right?

ALT: Right. Now, ENIAC had only vacuum tubes, and an enormous number—10,000,¹⁸ I believe. I forget, but certainly thousands. Then, at the Bureau of Standards, we had acoustic delay lines. And they had been used in...

AKERA: Radar systems. But they were not “digital.”

ALT: ...anti-aircraft gun directors, special-purpose computers.

AKERA: And the UNIVAC was designed based on mercury delay lines?

ALT: Did they have acoustic delay lines?

AKERA: Yes. And Mercury.

ALT: All right. That was an early one. But they fell out of favor. Then there were the Williams tubes that came from England, which we copied.

AKERA: And these were reasonably successful—still a little fickle, but they were the first CRT-based memories¹⁹ to enter reliable operation, as opposed to the RCA Selectron, which took longer to really get to a point of being reliable.

ALT: But the really successful technology in the end was the magnetic core.

AKERA: Which came out of Forrester’s work at MIT. So these were all issues that you must have been discussing in ACM at the time.

ALT: That was discussed. That was certainly discussed, but privately, as persons; not by the Council as a function of the Council of ACM. ACM’s function was, by that time, to distribute the reports, and the early proceedings...

AKERA: Yes. And run the organization and have the national meetings...

ALT: ...to present papers and the...

¹⁸ Nearly 18,000 tubes.

¹⁹ Cathode Ray Tube based memories. CRTs were like television screens, where the phosphorous coating on the surface of the tube could be used to store an electrical signal.

AKERA: But I'm still curious. The ACM, both the national meetings, but especially the Council meetings, become an occasion for many of the leading people in the field to meet and talk about the serious issues of the field. So I am interested in the kind of conversations you had, officially or "offline." I mean, in 1949, before the SEAC came online, do you remember the kind of conversations you had? Were you all concerned that these computer development projects were taking so long? Concerned that these computers were very difficult to complete?

ALT: Of course, we knew it was difficult, and especially reliability was a big concern. Everyone was talking about it. We felt that the national meetings of the ACM were important just so we could communicate.

AKERA: And try to figure out, help each other, and try to find solutions.

ALT: Yes.

AKERA: So did you have sessions on, for instance, the Williams tube memory, or about memory systems? During the national meetings, did you have sessions devoted to talking about memory systems and their reliability?

ALT: I think we must have been talking about that. That was on everyone's mind.

AKERA: Then, to ask the question, when the SEAC—I'll ask more about the background to the SEAC later—but when the Standard Eastern Automatic Computer, SEAC, came online in June of 1950, was everybody relieved that there was now finally a working electronic computer? After the ENIAC, that is.

ALT: Well, ENIAC was already there, of course.

AKERA: A working stored-program computer in the United States.

ALT: But also, we really knew that ENIAC was too big to ever be reliable.

AKERA: And you also couldn't run certain kinds of programs on it, it not being a stored program computer.

ALT: Also, it had only a small memory.

AKERA: Yes, a tiny memory.

ALT: You couldn't have put more vacuum tubes together. As it was, it had 20 words, I think. And SEAC had, what, 500, 1000? Something like that; 500 I think. That was substantial memory for back then.

AKERA: It was 500 words originally, and then 2000 or more—500 as designed, I think. But so was there a collective sigh of relief when the SEAC came online?

ALT: Yes, oh, yes.

AKERA: Certainly at NBS, but also in the field at large?

ALT: At large, right. Everyone knew then that it was possible to have large computers.

AKERA: It's noted here that the September 1950 meeting of the ACM, the fourth national meeting, happened in Washington, D.C. This was two to three months after the SEAC came online. Was this related? Can you tell us something about that meeting and the reason for bringing ACM to Washington? And there must have been a demonstration of the SEAC as well. Is that right?

ALT: Yes. That was just what we were talking about when you came in today. I was at the Bureau of Standards then, and I told the director that I was going to run this meeting. He had a technical assistant, a very competent physicist in his own right, named Hugh Odishaw who later became a leading figure, though I don't know where. I think he was a leading figure in the Antarctic expeditions for research.

Anyway, at that time, he was at Bureau of Standards. He told me that it would be smart for me to announce in my publicity that Dr. Condon would give a leading address. I would be delighted if he did, and Hugh Odishaw said, "Well, just announce it. He may be prevented from actually giving it by some unforeseen circumstances. But you can use his name right now. That will attract other people to submit papers." You see, it was still a matter of asking people to submit papers at that time.

And Yes, I had originally hoped to use university dormitories to quarter people, but that became impossible. We had outgrown that. ACM outgrew that. I think it must have been the first meeting that we were held at a hotel.

AKERA: Yes, that's right. And part of it may have been that other early hosts were universities, and universities had such resources. But NBS isn't a campus that has student housing. So you would have had to borrow somebody else's dormitory quarters, so it perhaps wasn't as easy to do that either, I would imagine.

But please tell me the rest of the story behind the D.C. meeting. Was there anything else to that story? You were saying you invited Dr. Condon. Did he present?

ALT: No.

AKERA: Did that get you in trouble?

ALT: No. But we had good technical papers!

AKERA: Oh, good! And there must have been a demonstration of the SEAC as well. Is that right?

ALT: Probably. I can't even remember it. We must have had guided tours.

AKERA: In fact, was the reason that the national meeting was held in Washington, D.C. primarily because of the SEAC? Or, I'm just realizing: was it primarily because you were ACM President then?

ALT: Well, SEAC was an attraction, something to show. It helped make the meeting desirable. It also helped put *ACM* on the map.

AKERA: Yes, I see. It does sound like it was partly your “volunteering” work once again. This is sort of emerging as a theme, namely that you did a lot of things to help the organization early on.

ALT: Yes, certainly.

AKERA: If I'm correct, ACM at that point was still looking for people to volunteer to host the meeting?

ALT: I think so.

AKERA: And you had the third meeting at Rutgers in March of 1950. So by late 1950, it was about time to have another meeting.

ALT: Yes. I don't know who did that one [in Rutgers]... I don't know who were the local people. But every time, there was a local group, small group of people who did the actual work [of organizing the meeting].

AKERA: That's great.

So during this time period, during your tenure as President from between 1950 and 1952, were there any tensions or growing pains... things that you had to face and deal with inside ACM? Was the organization growing too quickly? Not growing quickly enough? Were there different interests that were hard to keep together?

ALT: No. I don't recall any. I don't know. I may be wrong, but as far as I know, it all went smoothly. I can't remember any fights or dissensions. Once we had decided to have annual meetings—it was the real first decision that was made, to meet annually—and once we had decided to have a journal, and once we had decided to issue the *Communications*. That was a big step. When we added *Communications* in addition to the *Journal*, that really changed things. For one thing, it was a monthly publication. It was much more work. That practically all happened within the same few years that Ed Berkeley resigned.²⁰ It was too much work for him.

AKERA: So this led you to create what was sort of ACM's first national office.

ALT: We went to enroll the New York Academy of Sciences to help, first as a publications office, because that was their main job, doing that work.

²⁰ The workload was increasing, but both the *Journal* (1954) and the *Communications* (1958) started after Berkeley's resignation as ACM Secretary.

AKERA: Their main job, yes. That and maintaining the membership list, which, in turn, is tied to the publications.

ALT: They were publishing journals for many other societies and organizations, so they knew how to do that. That was the first reason for going to them. And then they hired a staff just for us. So really, Ed Berkeley's work was for a short time taken over by Eleazer Bromberg, but very soon, somebody whose name I don't remember, a lady who very competently became something like the head of the office.²¹

AKERA: Myrtle Kellington?

ALT: No, that wasn't her. Kellington is good. Kellington I wanted to bring up next. But who was the other one? The administrative head?

AKERA: I apologize I don't have that information with me right now. Yes, there was another woman at the New York Academy.

ALT: There was the administrative head hired by the New York Academy to take care of all ACM business, and nothing else. And ACM paid for that. She came to annual meetings and reported finances and things like that. She did the real paperwork. It'll come up.

AKERA: Okay. I was looking quickly to see if I could find her name. But that's right.

ALT: Myrtle Kellington—I really want to talk about her. She had been John Curtiss' mathematical assistant. She was a mathematician. Not a creative one, but she knew mathematics. She was certainly a very good editor. She helped me with the editing of the *Journal*. And when ACM established its own office, she became the editorial assistant who did the paperwork, negotiating with publishers, with printers.

AKERA: Yes. And that would have been 1960. Did that mean she was the editorial assistant for all three of ACM's publications back then?

ALT: Yes. But as late as that? I thought it was earlier [when the national office was created].

AKERA: Yes, I believe so. *Journal of ACM* was created in 1954. *Communications of the ACM* starts in 1958 and *Computing Reviews* starts in 1960. And I think it's right at 1960 that ACM creates its own national office. Myrtle Kellington... did ACM actually move elsewhere? No, the organization stayed in New York City, so she simply moves offices and became part of ACM's office staff.

ALT: Earlier, she moved from Washington to New York. She quit her job at the Bureau of Standards. Even earlier, I had taken her over from Curtiss. Oh, and so what happened to the Bureau of Standards back then—that was another thing.

AKERA: I'll ask about that later. Yes, that's a big story.

²¹ This might have been Irene Hollister, who was the Executive Secretary of ACM later on (in 1966).

ALT: But that would be the wrong year.

AKERA: But as a consequence of the turmoil at NBS, she became your mathematical assistant. And then when the opportunity opened up, she moved to the New York Academy of Sciences.

ALT: Right.

AKERA: And then became folded into the ACM national organization when that was created in 1960. I've heard several references to her work and efforts, and everybody speaks very well of her.

ALT: She stayed on for many years, and was really a backbone of the organization.

AKERA: And really helped you, then, to launch the *Journal*, I would imagine, in the practical sense—made sure that all the production process went well.

Let me just check my notes and see where I'm at. Okay, actually, we were exactly where we were in the conversation. I was going to ask about the *Communications* and *Computing Reviews* with you. Just one question on *Journal of the ACM*, first. You became its first Editor-in-Chief, is that right? Editor of the *Journal of the ACM*. I think, actually, you were the Chair of the Editorial Board or Committee.

ALT: Well, we called it the Editorial Board as soon as we had another publication.

AKERA: So when you didn't, it was still the Publications Committee? Or when you started the *Journal*, did it become the Editorial Committee? You were the Chair of the Publications Committee, I believe. And I think it's in 1958—same year that you created the *Communications of the ACM*—that then you also passed on your position; not to one person, but to two people, right? Walter Bauer steps up as the Chair of the Editorial Board. And M. L. Juncosa becomes the Editor-in-Chief, now officially known as such, of the *Journal of ACM*. Can you tell us something about this transition? Why was it decided to reorganize a bit? Also, why did you decide to step down as the de facto Editor-in-Chief of the *Journal* in '58?

ALT: [Laughs] I was trying to remember, myself, and I couldn't. Namely, I was trying to remember what else happened in my life in 1958, and nothing specific happened [that I can recall]. I don't know. As long as it was only the *Journal*, I could cope with it. But once we decided to add the *Communications*, that would have been too much for me. That's why I resigned from the Editorial Board, I think.

AKERA: And actually resigned.

ALT: Yes, I had somebody else take over the Editorial Board.

AKERA: Walter Bauer, who, if I'm correct, is a West Coast individual—is that right? Do you remember him? Do you know who he was?

ALT: Barely. How did I...? Where was he?

AKERA: He was involved with a company called Infomatics. I don't know if that firm was already in existence then.

ALT: If you read the drama, the real challenge of starting the *Journal* was to find somebody who would be the editor, and since I volunteered for that, that took care of that. But the trouble with adding a new publication-- I knew that we had to add to what we had-- the membership demanded that we added a monthly publication that would be not entirely technical, but more for news and information. That was too much for me to handle. And we could only approve it by the Board. I mean, the ACM Council had to vote for establishing the *Communications*. But it would have been a meaningless thing unless we had somebody to take charge of it. The thing was to first find somebody, and I had to find him, in fact. I had to find him, and once I did, he had to accept it.

AKERA: I looked this up—John Carr was the first Editor-in-Chief of *Computing Reviews*. Alan Perlis, who was at Purdue University, was very involved in computer languages work, and he became the first Editor-in-Chief of the *Communications of the ACM*.

ALT: Perlis was a good man. I remember him very clearly. Yes, I was always finding somebody who was willing to take over. Then you could vote to establish the position.

AKERA: Right. I'm going to go back and a bit about why the *Communications* came into being. Could you first tell me something about Alan Perlis? You said he was a good person. Was this because of his technical work?

ALT: Very competent mathematician.

[Begin Tape 3B]

AKERA: You know, ACM increasingly had people on the programming side. They were mathematicians, but they were not research mathematicians in the numerical analysis tradition. They had started to branch out.

ALT: Yes.

AKERA: I think, Alan Perlis, his first work, or at least his early recognized work, was on a high-level language compiler called IT—something "I" Translator.

ALT: That was a great thing.

AKERA: I think this was an attempt to create a language like Fortran, but with a much smaller object code, so it could be loaded on an IBM 650, a smaller magnetic drum-based machine. And it really required a great deal of clever tricks about how you would construct the language.

ALT: Fortran was designed by an Englishman.

AKERA: Perhaps it was John Backus at IBM that came up with Fortran, but I don't know what his origins were.

ALT: I'm sure we'll come back to this topic. Fortran was a very practical language. We used it to develop standards. My staff used Fortran almost exclusively, but then we did a lot of scientific problems. There was a fight between Fortran and Algol. There was also a really good general programming language, designed in Europe somewhere. It was used by the Europeans, by the Dutch. Oh, wait a minute. Who was it, who was... He won a Nobel prize later on. That will come back to me.²²

AKERA: Let me take one step back for a second. I've asked things in the wrong order. I apologize.

ALT: Yes, you make sure that I have all your questions answered.

AKERA: Yes. The one step back is just to go back to the origins of the *Communications of the ACM*. You mentioned that the membership demanded this other publication, something other than the *Journal*. Can you give us a little bit more about that history? Why was it that a new publication was necessary?

ALT: Because in the *Journal*, there are technical papers from different fields. Nobody could read all of them. Few people could even read any. The members were programmers, some non-technical people.

AKERA: Certainly by the mid- to late-fifties, because by then there are already a lot of computers, mostly IBM computers out there, and people are already programming them, right?

ALT: Yes.

AKERA: So there are a larger number of programmers around.

ALT: Yes. A lot of non-technical people in ACM, and they wanted to have a publication that could serve them.

AKERA: What were they interested in seeing? What did they want to have in the *Communications*?

ALT: Well, to find out what's going on in computers, what new computers appear, and what the problems were.

AKERA: So discussion of machines, the latest machines. Discussing the application areas, which are not necessarily research articles, but they are sort of summaries of current techniques?

ALT: Right, yes. Yes.

²² Dr. Alt later recalls this as being Cobol.

AKERA: And didn't more of the programming techniques get put into the *Communications*, rather than the *journal*?

ALT: Yes, that was common. But also the *Communications* had news and information.

AKERA: Yes. In fact, all of the regular proceedings of the Council started being reported in the *Communications* as, "Official ACM," is that correct?

ALT: Yes. That's correct, yes. Oh, you could mention, before ACM had any publications, the Office of Naval Research published a computing newsletter, maybe a four-page thing or possibly an eight-page thing, with a small circulation. And when we started the *Journal*, we took that over.

AKERA: That was the ONR's Digital Computer Newsletter, right?

ALT: Right. Digital Computer Newsletter, right, right. We took that over. It was just as easy to reprint it in the *Journal* once a quarter. And then that went into *Communications*, therefore the *Journal* became a smaller publication.

AKERA: Though actually, you reached your maximum page length—the publications kept growing again, so by 1961, you met and exceeded your past peak, and there were 650 pages in an volume. So moving the DCN didn't hurt the *Journal* per se, but I think it did give the *Journal* a more scientific focus whereas *Communications* was then free to be a publication open to a much larger audience. Do you remember who in particular pushed for the *Communications* the most? Are there any individuals who stand out in your memory?

ALT: A push for the publication of *Communications*. I don't remember. There must, I don't know that it was one person or just the general feelings of...

AKERA: It was discussed in the Council?

ALT: There was a lot of discussion. There was a lot of discussion.

AKERA: Now, was there any distinction between the East Coast and the West Coast representatives at the time? A sense that there's a slight difference in interests?

ALT: No, at least not in the sense of disagreement. I don't remember any disagreements. You have questioned me about that.

AKERA: I apologize. It's just that these things are often important for the growth of an organization, you know.

ALT: I don't remember any disagreements with the Council, just some different people stressing different things. But nobody came out objecting.

AKERA: The historical reason that I ask some of those questions is that organizations on the West Coast acquire IBM computers more rapidly than the East Coast.

ALT: Oh, yes?

AKERA: So proportionately, there are a lot more programmers on the West Coast than on the East Coast during this early period. This difference doesn't last very long, but I think it contributes to a shift inside ACM. At least as I understand the history, because there are suddenly a very large number of programmers who are not primarily academic, and who would not, by background, be able to publish something in the journal. But at the same time, they wanted—needed—to be involved in the organization. One of the things that I was wondering is whether this group, and their need to publish, lead to the publication of the *Communications of the ACM*? There are individuals such as Paul Armer. This is right when Share²³ as an organization is coming into being. And they start appearing at ACM, and have an identity that is somewhat different from the more “longstanding” members from the East Coast.

ALT: I don't remember that impression of things, but it might have been the case.

AKERA: Now, this is all after your tenure as President. This is a bit later on, more during the time that you were involved with the *Journal*.

In fact, let me go back to your tenure as President. Do you remember anything in particular that you worked on as President of the ACM that was important to you at the time? This was '50 to '52.

ALT: The major problem was about publications at that time. The big problem that we discussed over and over again, and not so much a serious disagreement, but talking pros and cons.

AKERA: Right. And deciding when to do it.

ALT: And deciding when, yes, yes.

AKERA: And I think one of the other big issues was that you simply had to keep the organization going. This meant that you had to keep finding somebody to host the national meetings...

ALT: Yes. Oh, another thing. Several questions that I read had to do with committees. I don't remember anything about that. I must admit that they must have been there, because I wrote about them,²⁴ but I don't remember if they ever came up, or what they did. I have no recollection.

AKERA: Good, good. We've actually covered quite a bit of the questions. And so we can move forward in time a bit. But just one thing that I don't think we've covered. There was a lot of

²³ The first computer user's group. Share was a national organization established in 1955, through the primary initiative of the West Coast IBM installations.

²⁴ Several committees are specifically named in Dr. Alt's piece, “Fifteen Years ACM” (1962).

discussion about the constitution in 1953. There was some discussion about changing the constitution and by-laws in 1953. And then there was another round in 1960 and '61, when there was a thorough rewriting of the constitution and by-laws. Do you remember either of those two moments, and the reasons behind the changes?

ALT: No, no. No idea. But maybe it's my old memory that's fading now. It was too long ago.

AKERA: That's okay. There's a concrete change that's worth noting, and this might sort of jog your memory. I think it's in 1964 that ACM decides to create local chapters. Do you recall that conversation?

ALT: I know it was done. Again, it happened almost before it was acted on. It was almost just acknowledging an accomplished fact.

AKERA: Yes.

ALT: It had already happened, and we gave it official standing.

AKERA: In other words, ACM was originally organized by sections, regional sections.

ALT: Yes. By geographically defined sections, which were quite large. This wasn't practical. It was too broad. It didn't serve a purpose. It turns out that the sections had almost no functions.

AKERA: Instead, people met in their particular cities. There were local chapters, where people were working and could easily get together and talk about their particular problem. And this became an aspect of ACM that really began to take off. These groups had their own local meetings. Some even started their own publications and even hosting dinner speakers.

ALT: Oh, yes. New York was one of the most active chapters.

AKERA: Do you recall what some of the other active chapters were, just by chance?

ALT: Why did I recall New York? Because at that early time, I wasn't there. But I knew what they were doing. Later on I got their publications, of course. And at Washington, we didn't have a local chapter.

AKERA: No. And the other active group was the one in Los Angeles. Los Angeles had a very active chapter.

ALT: Oh, probably, yes.

AKERA: Yes. But the first local chapter that was actually recognized was Ft. Worth.

ALT: Oh, yes?

AKERA: You don't recall why?

ALT: I don't remember, no.

AKERA: Now, the other decision that's made is to create a category of student memberships... this was perhaps later. Do you recall that?

ALT: Oh, I don't remember anything about that. No.

AKERA: Okay. I thought I'd ask, just in case.

Now, one other line of questions that I'd like to ask—one I think that you would be able to illuminate us on—has to do with specific individuals. So your immediate successor was Samuel Williams, who I guess, was originally at Bell Telephone Laboratories. Do you remember Mr. Williams?

ALT: I remember Sam Williams.

AKERA: Can you tell us anything about him?

ALT: He was an interesting, very competent technical person. An engineer. He and E. G. Andrews jointly designed their Bell Laboratories relay computers.

AKERA: Now, George Stibitz did the initial design?

ALT: He was earlier.

AKERA: But so you feel the actual engineering design [for the machine delivered to Aberdeen] was done by these two individuals?

ALT: I think so. Right.

AKERA: Even the mathematical design would have been by them?²⁵

ALT: Right.

AKERA: Now, how did Samuel Williams become president of ACM?²⁶

ALT: Oh, I had problems in asking him. He was an old man. He didn't take the initiative, but I asked him.

AKERA: Because you knew him from your work at Bell Laboratories there?

²⁵ Although I have not traced this in detail, my sense is that George Stibitz did contribute substantially to the design of all of the early Bell Relay Computers (model II-V at least), as part of the work contracted for, or supervised by the NDRC. Williams and Andrews were charged with their execution, and after Stibitz left for the University of Vermont, they may have continued to reengineer the machine. When Alt was placed on assignment to Bell Labs shortly after the war, Williams and/or Andrews would have been in charge of the project.

²⁶ Fourth President of ACM, from 1952-1954. (Curtiss was President 1947-48; Mauchly from 1948-1950, and Alt from 1950-1952)

ALT: I knew him from my work there, right.

AKERA: Do you recall anything that he did before he was the President of ACM?

ALT: On the work at Bell Laboratories? Here's a story. When he came in one morning, the machine was stopped. Why had it stopped? You look all over to locate the trouble. "I see no problem." By running test problems here and there, it meant you could tell which relay had failed, had gotten dirty somewhere. We did that with toothpicks. Stopping relays in various places, that was a standard technique. They knew that from their telephone work. They would do it all the time, stick toothpicks in various relays, here and there, to stop them and see where you could duplicate a malfunction.

AKERA: Quite clever. So he was a clever engineer. He knew how to work with complex systems.

ALT: Yes.

AKERA: Now do you recall anything he did as ACM President?

ALT: No, no.

AKERA: He just continued the work you started?

ALT: For him as well as for me, being President was a formality, almost. You had to call meetings, and preside over meetings. He also wrote some of the early articles.

AKERA: He did. That's right. And I hasn't mentioned it yet, but he also wrote a piece on the early history of the ACM. The piece is on only three or four pages long, but it was in the first issue of the *Journal of ACM*. Is that correct?

ALT: Right.

AKERA: I will have to go look for that. Now how about Alston Householder?²⁷ Do you remember him?

ALT: I knew him very well, right. Yes.

AKERA: Can you tell me something about Dr. Householder?

ALT: Well, what's there to tell? He wasn't a man of many words. He visited us in Washington, repeatedly. He was very competent. He understood what was going on. He knew what needed to be done. He was a good man to have. He ran the computing unit at Oak Ridge.

AKERA: He made considerable use of computing. He was an applied mathematician, right?

²⁷ Fifth President of ACM, from 1954-1956.

ALT: I think so, but I'm not sure.

AKERA: And he did a lot of computing work at Oak Ridge. And there was a lot of computing work to be done there because of the physical processes associated with nuclear materials processing. I would imagine that was his major interest in computing.

Now is there anything else from the early history of the ACM Council that you care to mention? You spoke about Mina Rees already. Some of the other names that appear early on in the Council minutes are Frank Verzuh, Charles Tompkins...

ALT: I remember Verzuh briefly. He was up at MIT. He wrote the programs for weather forecasting. Oh, that's right. Verzuh. It was something I could understand—one of the few things that I could really understand. He had long records of weather data and ran multiple correlations between those and other factors which would then lead to forecasting the weather.²⁸

AKERA: Now, was this based on the IBM 602, on using the early IBM punched card based machines?

ALT: He would have, yes. He would have run that.

AKERA: Yes. I think he ran the Office of Statistical Services there.

ALT: But what you could do with such equipment was too small scale. What you need for weather forecasting is one the large computers.

AKERA: Right. So that's why he would have been interested in the electronics computer.

ALT: That's why he was interested in computers, in large scale computing.

AKERA: Now how about James McPherson (Census Bureau), and Jan Rajchman (RCA)?

ALT: The Census Bureau. He tried to get a computer for the Census Bureau, where it might really belong. He must have gotten one of the Univacs.

AKERA: Yes, they received the first Univac I. So is there anyone else on this list, and we're looking at the list of ACM Council members from your "Fifteen Years ACM" article. Does anyone else stand out as somebody interesting that may be worth talking about?

ALT: I am sure that there is. Arnold Lowan, who commanded the mathematics tables project.

²⁸ This was actually the work done by John Mauchly, so there may be some confusion here. Verzuh ran the Office of Statistical Services, where he had a bank of IBM machines; he was known for developing techniques for using this equipment for scientific calculation after World War II. Verzuh also operated the Whirlwind I, after the machine was opened up for general scientific and engineering use. He may have helped perform weather-related calculations using these, or subsequent facilities. (Jules Charney transferred from IAS to MIT around 1956, and did numerical meteorology work there, although their work was based primarily on hydrodynamic models, and not, primarily, based on correlational analysis, I believe.)

AKERA: He was also an applied mathematician?

ALT: He did not get into machine computing. On the contrary, he would have been against this-- The mathematical tables project originally was created to make work for people.

AKERA: Yes. It was part of a Works Progress Administration.

ALT: Yes. It was a public WPA project.

AKERA: Now, was he near retirement, or did he move on to a different field after the war?

ALT: He was close to retirement. He was very sick, I remember. Well, he had run the New York part of the Tables Project to the end, which was a few years after the end of World War II, after which they were dissolved. He must have retired then, but I don't really know.

Now, you haven't mentioned Eckert and Mauchly. I think you should.

AKERA: Oh, yes.

ALT: It was always Eckert and Mauchly.

AKERA: I know. I actually know something about [J.] Presper Eckert and John Mauchly. But I didn't want to make it a focus of this interview, since you are asked about them quite a bit in the Smithsonian interview, and I wanted this interview to be focused around your career.

But it's worth integrating some of that into this transcript. Do you know why Pres Eckert never became involved with the ACM? At least he seemed not to show up in any of the early records.

ALT: No, I don't think he got into the ACM.

AKERA: So Mauchly was involved for a little bit, but Eckert wasn't?

ALT: Sure. He was president, and he came to meetings. He played an active part.

AKERA: Yes. I think Eckert was simply too busy, and Mauchly represented their company within ACM. Their company was very busy with the engineering work. On the other hand, Mauchly was involved more on the marketing and applications side, and he had an interest in marketing the Univac computers. I think one of the reasons he came to ACM was that it was an opportunity to make connections so he could find people to sell the Univac to.

ALT: Yes, yes. Quite right. Oh, you mentioned Caldwell before. We had Caldwell representing analog computers at the early meetings. We thought we should have a session on analog computers too, and invite Caldwell. And his introducing words were, "I have come to praise the analog computer, not to bury it." [Chuckles] So Caldwell should be mentioned.

AKERA: Yes, absolutely. I can close the ACM segment of the interview with just two fairly simple questions, I think. Actually, one is a bit more substantial. So this first one is about the relationship between ACM and other organizations at the time. Can you tell us a little bit about this?

ALT: Yes. Well, we mentioned a little while ago that there were active committees within AIEE and IRE.

AKERA: Yes, the AIEE Committee on Computing Devices, and the IRE Professional Group on Electronic Computers, yes.

ALT: Right. They went ahead with big meetings before ACM did, and they were sort of competing. We finally decided on having joint responsibilities.

AKERA: These were the really big meetings, which included manufacturers, trade shows and so on.

ALT: Yes, right. They were originally called the Joint Computer Conferences.

AKERA: Yes. After awhile, they started to have one on the East Coast and one on the West Coast, each year.

ALT: Yes, right. There were two per year; two large meetings per year. One of ACM's major activity was to contribute to those meetings, in addition to holding its own meetings.

AKERA: Yes. In fact, by 1953 or 1954, you reach a compromise where ACM agrees to hold its national meetings nominally during the summer, so that the JCC could have their meetings in the spring and fall.

ALT: Yes. Now, AFIPS, American Federation of Information Processes Society, was a joint organization created between these other organizations, plus ACM. It was a roof organization. Not a membership organization, but it was a sort of joint committee of the three organizations.

AKERA: Yes. What was the reason for creating AFIPS? It basically grew out of the Joint Computing Conferences, I believe.

ALT: It was set up just to operate the joint meetings.²⁹ That was their only reason for being. What should be mentioned somewhere, even though it's way later, is that ACM became a member of the National Research Council.

²⁹ Actually, there was at first just a committee to organize the Joint Computer Conferences. AFIPS, as recorded in either the Oettinger or Galler interview, was created in an attempt to broaden membership and to create a US information processing society that could represent the computing field in international meetings. This was of limited success, initially, and a major constitutional revision to AFIPS was made during Galler's tenure at ACM President.

AKERA: Yes, in the Division of Mathematics. Or actually, wasn't that the American Association for Advancement of Science, the AAAS. It became affiliated with Section A and Section X for Mathematics and General Science, respectively.

ALT: The AAAS was another organization we became affiliated with. But the National Research Council was the more important one.

AKERA: Oh, you're right. That was in 1954. Then in 1958, ACM becomes represented in the Mathematical Sciences Division of the National Academy of Sciences' National Research Council.

ALT: Yes. I know that because at one time, I was the delegate there.

AKERA: How about the Share, and the Joint Users Group? Do you recall anything about these organizations?

ALT: I wish I knew how ACM related to that. It didn't come up while I was very active in ACM, but I know it became a problem later on, how to relate to the Joint Users Group.

AKERA: What was the issue or problem?

ALT: I don't know how it was.

AKERA: My recollection is that it was not the Joint Users Group, but the Share which presented something of a problem for ACM, because Share had become very active but remained tied to one manufacturer. And ACM had qualms about developing an affiliation with an organization tied to a specific manufacturer. So the Joint Users Group was a solution which said that here was an umbrella organization, sort of like the AFIPS, that brought together all of the different user's groups. And then ACM could have a relationship with the Joint Users Group because that entity was not tied to any specific manufacturer, and then there could be coordination between ACM and the activities of the Joint Users Group.

ALT: Yes.

AKERA: There's a lot of talk about programming that becomes very important by the late '50s and then certainly by the '60s. As a consequence, ACM's national meetings, the Joint Computing Conferences, and Share and Guide (a user's group for IBM's business data processing machines), the Univac Scientific Exchange were all talking about programming, and presenting similar papers. And the desire was to try to coordinate some of that exchange and work across platforms, I think.

ALT: Yest, that was a problem; should we participate in the designing of programming languages. That became one important issue.

AKERA: Now, this was actually an activity you were directly involved with, right?

ALT: Yes. Cobol was the language that I couldn't think of before. Fortran and Cobol, and Algol was the European language. I don't know if Cobol is still used; Fortran, I'm sure, is.

Another little sideline: At one of the ACM meetings, some people—including Bob Bemer—predicted that Fortran would soon be a dead language. But Fortran is still alive. It has survived.

AKERA: Yes. I recognize the name, Bob Bemer...

ALT: Fortran is still doing well, has survived... and it may yet survive Bob Bemer.

AKERA: That's right.

Finally, I'd like to ask you about the changing nature and character of the organization. How did the tenor of the organization change as its membership expanded from, I guess it was 300 members in 1947, to 1,100 by 1951, and at the end of the next ten years, 8,000 by 1961. How did the feel of ACM change?

ALT: It must now be 50,000 or something like that.

AKERA: Yes. Or perhaps twice that. I think it's more like 90,000 people.

ALT: Well, I can't say. Most of the really significant growth happened after my time, after I was active in it. I don't know what kind of an organization it is now. I don't know whether I still belong in ACM, but I still...

AKERA: Well, they certainly remember you! But ACM did certainly grow. It kept growing and growing, didn't it, during the early years?

ALT: Yes.

AKERA: Even in the earliest years?

ALT: Yes, yes.

AKERA: You said it was around 1958 when you became less involved—started to become less involved. After that, you didn't know the folks as much any more?

ALT: After that, I still took an interest for the next decade, say up until in '67. That was when I resigned from the Bureau of Standards and moved to the American Institute of Physics. And there I had less to do with computing machines. No, not even that is true. I was at the American Institute of Physics because they were doing the publications for a number of organizations that related to physics research. What I was working on was a project to develop a computerized index for their publications. If you go into a library, you'd find a card file of all the books. We tried to have a computerized card file of journal articles in physics and related sciences. That was our project back then.

AKERA: That's great. Finally, is there anything else about the history of ACM—from the early period, the period when you were active, that you'd care to relate to us? Perhaps some anecdote?

ALT: Well, I think we have exhausted that!

AKERA: Okay. Then let me just get to the final stage of the interview, which is about your subsequent career. So prior to your involvement with AFIPS, you were still down at the National Bureau of Standards. I'd like to fill in some of that history. Any recollections you have is fine.

I know that there was another group that was actively involved with computers during the 1950-1952 time frame, and that would have been the Electronic Computers Laboratory run by Samuel Alexander. Can you tell us anything about Sam Alexander and the relationship that NBS' Computation Laboratory had with him and his group?

ALT: I met him when I was still at Aberdeen. He was one of the people who came to the meeting there. Was he actually in ACM? I doubt it.

AKERA: No. I don't think so.

ALT: It was not his field. He was strictly an engineer. Yes. Of course I came to know him well at the Bureau of Standards.

AKERA: Yes. What was the work that his group was doing?

ALT: I think I was probably a bit mistaken about that, too. I was good friends with two of his immediate assistants. Ralph Slutz was a very close friend of mine. We worked together very closely, and with Margaret Fox. Ralph Slutz, unforgettable. We sat together at lunch every day. This was at the Bureau of Standards. It was an institution—a serious staff lunch club where all the people met. And one day, I think that was before the SEAC was finished, one day he said, “I think we are at the stage where we could try and put a real problem on.” Well, I had already, in expectation of that, written a little program, a few hundred words, and had punched it out on the paper tape. At that time, we had input on paper tape; it was the only input we had. “All right. But let's try that out after hours, so nobody would know.” So we got together at five o'clock, or whatever the ending hours were, and sat down at the computer. Ralph turned it on—I wouldn't have known which switch to turn on, and I put the tape on the reader and it clicked away.

The first problem that I had put in was to factor numbers, to factor integers, and I put in the number 15, to see what the factor was, and it promptly printed out, “3 times 5.” I tried a few more numbers, and then Ralph said, “Let's try a real big one.” Well, the biggest number that we could put on was 10 digits. The biggest capacity of the machine was a string of ten “9s.” But that obviously wasn't interesting. Nine “9's” followed by a “7.” We put that on, and the machine stopped. I sat there. I said, “What's wrong?” And Ralph said, “Well, I don't know. Maybe it's working but very quietly.” You couldn't hear anything. I sat there and sat there for 15 minutes. Ralph insisted. He wouldn't let me go. At 15 minutes, it printed out the same number. That meant it was a prime. At that size! The biggest prime number known at that time.

AKERA: That's amazing! So Sam Alexander was the head the laboratory, and Ralph was the Chief Engineer. You, in turn, were sort of the de facto head of the Computation Laboratory, right?

ALT: Yes.

AKERA: And so you really were working together to figure out how you could actually use a stored program computer. Now then is it true that your staff was then able to start using the SEAC once it came on line?

ALT: From then on?

AKERA: Yes.

ALT: It began, I mean, very soon after that. The soonest the engineering were really confident about the machine. What we did was just a test. And up until then they had been building it.

AKERA: Then they started using it for the regular computation service that you provided to other NBS divisions, and to other organizations?

ALT: Yes. And we had our own staff of programmers.

AKERA: Yes, some of whom came from the Mathematical Tables Project, is that right?

ALT: Yes. As many as we could bring over, and a few we hired afresh. We hired a few in Washington and trained them.

AKERA: What were some of their names?

ALT: Well, Milton Abramowitz was the head of the computing laboratory, but he died not long after. Irene Stegun was in charge of our programming group. We still had a group of manual computers, I mean, desktop computers. We couldn't quite do without them. There were many small computations...

AKERA: ...that you didn't want to load onto an electronic computer?

ALT: We did work for everyone in Washington. They gave us a problem on the design of a basket—a shopping basket. It must have been some part of the government procurement supervising unit. “What's the volume of this?” according to its shape. So we gave that to the hand computer. They gave it back to us and said, “We have to know how much, what's the radius of this curvature,” and so on. We measured that, and then they did it in a day's work. That was faster than writing a program.

AKERA: But some of the more complicated problems were put on the computer. There were much more complex problems in fields like optics, geophysics, and so on.

ALT: Oh yes. Routine problems in optics and design, lens design, design the focus of the system of lenses.

AKERA: It must have been some work related to electromagnetic radiation...

ALT: I wish I could identify the people from Los Alamos. Richtmeyer and others brought down the early programs. They had written their own-- Oh, I remember Leslie Peck complained that our program couldn't convert from decimal to binary or vice-versa. They took this for granted, but we didn't have them.

AKERA: Good. Now wasn't it around 1953 that you become Acting Director of the National Applied Mathematics Laboratories?

ALT: Ah!

AKERA: Could you talk a little bit about that?

ALT: The situation there was as follows: John Curtiss established the mathematics division, with one section located in California at UCLA. The idea was to have research in numerical analysis out there. We had some in New York, especially later. He hired a man who had been his supervisor in the Navy. Curtiss had been a Navy officer during the War. His supervisor there was Ed Cannon. He hired him as the Assistant Chief of the Mathematics Division, with reason. Then Cannon took a leave of absence, for a year first, and then for two or three years, and went to work for ERA, Engineering Research Associates.

Now, I'm not sure why, but the years after '53, when Eisenhower became President, there was something that had a big impact on the Bureau of Standards as a whole, and in the mathematics division especially. Very severe budget cuts.

AKERA: Yes. Now why was it that Curtiss wasn't there to handle this?

ALT: In 1953, why was he away? He resigned. As I recall, to a University. And that was why Cannon was away. So, for a year or so, I was Acting Chief of the Mathematics Division. There wasn't anyone more senior there. The mathematics division in Washington had a Statistics section that was really important, headed by Churchill Eisenhardt. A computer section, the so-called Machine Development Laboratory, was created for Ed Cannon to keep him—to give him something to do. And he had one of the former New York tables people there, Ida Rhodes who was willing to go to Washington, but wasn't really a desk computer type.

AKERA: Right. So Ida Rhodes worked under Ed Cannon?

ALT: Ed Cannon and Ida Rhodes were in the Machine Development Laboratory.

AKERA: And they did not create a machine?

ALT: They didn't do any machine development, but they just kept track of what was going on.

AKERA: Yes. They were actually responsible for NBS' computer procurement service, in effect. They advised other agencies on what computers to purchase, and supervised the contractors who built computers for these other agencies, starting with the Census Bureau's contract for the Univac I.

ALT: Yes, that's right. And then Ed Cannon went to ERA and extended his leave for a year or two after Curtiss left.

AKERA: I see. So that left nobody in the top position.

ALT: That left nobody more senior than me, so I took over the division.

AKERA: Yes. But didn't you take over the division at a time when there was all this turmoil at NBS, with regard to its budget?

ALT: Yes, yes. Our budget was also cut. We had to live on the work that we did for other organizations in Washington, other government organizations.

AKERA: So it nearly all of your work had to be supported through funds transfers?

ALT: For a while I became a salesman, practically, going around meeting people from all the laboratories. The National Institutes of Health (NIH) was a big customer who put a lot of problems on our machine.

AKERA: Can you tell us the circumstances behind the budgetary pressures? You mentioned Eisenhower, but was there something more specific than that?

ALT: [Chuckles] Yes, but I don't know the names. There was a businessman, who felt that the Bureau of Standards had done him an injustice. He was engaged in the production or selling of battery additives.

AKERA: Yes, the AD-X2 battery additive controversy. I remember.

ALT: Something, when the battery gets dead, in an alkaline battery, you can put something in to make it live again. And the Bureau of Standards found that this was nothing. It was no better than water.

AKERA: Right. And NBS had a traditional responsibility in the field of product testing. Is that right? In other words, NBS was part of the US Department of Commerce, and as an organization, one of its responsibilities was to test the products made by commercial manufacturers.

ALT: Yes. For some reason, we had to do that. We experimented with it, and I don't know who, but the Electricity Division of the Bureau found that the product was no good. So when Eisenhower came in as President, that fellow managed to get a big job in the government somewhere, where he had influence through friends. These friends, in turn, had enough influence to cut the budget of the Bureau of Standards, just out of malice. I don't remember him.

AKERA: So really, this one incident resulted in a tremendous reduction in the size and budget of NBS?

ALT: Well, yes. Well, the effects were partly organizational. That was when we turned over our West Coast section to UCLA. But it was still the Navy giving us money. That fellow didn't manage to cut the Navy's budget, and the Navy continued to give us money.

AKERA: In other words, because of this controversy, there emerged general conversations about reorganization, and in that process, the decision was made to dissolve the National Applied Mathematics Laboratories. The Laboratories as a whole became the Applied Mathematics Division at NBS?

ALT: No, no. That had nothing to do with the change in name. It was never officially the National Applied Mathematics Laboratory. It was always the Mathematics Division of the Bureau of Standards. It wasn't anything else. But we cut off the Institute of Numerical Analysis, on the West Coast, and transferred it to UCLA under the same financing they had before. The Navy simply gave money to UCLA instead of to the Bureau of Standards.

But amid the larger budget cuts, we had to cut our own staff, too. I had to fire a few people, especially research mathematicians whom I didn't need badly. I didn't have to have a researcher in numerical analysis. There were some very good mathematicians who went on to IBM or someplace else.

AKERA: So you were getting paid for computational services, which could raise its own revenue, but others who weren't directly providing services had to be cut?

ALT: Yes. The computing services within the Bureau continued.

AKERA: But then didn't Edward Cannon come back? Is that what happened?

ALT: Then after a few years, Ed Cannon came back and took over the Mathematics Division.

AKERA: So then you just went back to being the head of the Computation Laboratory?

ALT: Oh yes, and I went back into the Mathematics Division. But then after awhile, I was moved out of the Mathematics Division. The Bureau had an office of international relations, and the head of that went away on what's called annual leave, or something like that. So there was an opening there, and the NBS Director put me into that—I knew foreign languages. For three years, I think, I headed the Office of International Relations. A very interesting time for me.

AKERA: What years was that?

ALT: It was 1961 to 1964. I remember it precisely. In 1961, I went out of Mathematics. In 1961, I moved physically one floor down to an office next to the director's office, where foreign visitors came to me and I had to take them around. I met some Russians and I invited them for dinner at home. I met Germans who came in to visit the Bureau of Standards.

AKERA: That must have been pleasant.

ALT: It was quite interesting. Until 1964. Oh, yes, not only that, but I had the responsibility—the Bureau of Standards had some money that the American government owned in foreign places in foreign currency.

AKERA: Yes, and it had to be spent?

ALT: And I had to take care of that. Issue research grants, mainly in India. Foreign students had traveled to India to work with physicists there, and I handed out grants to support their research, research that was very interesting to the Bureau of Standards. Now, that was for three years, until 1964.

AKERA: And what did you do then after 1964?

ALT: In 1964, the original inhabitant of that office came back. He had originally come from Belgium, I think. Where had he gone? I don't know why he was away for three years. But he came back. Then in name, I reverted to the Mathematics Division. But in 1967, I retired from the Bureau of Standards.

AKERA: Now you actually spoke a little about this during our first session. The other day I was here, you told me a little bit about your decision to retire from NBS. Can you repeat some of that story for us? Why did you choose to leave NBS in 1967?

ALT: Well, before I left, I had become more interested, and really it was more interesting, to do technical work. Generally among scientists, there is widespread complaint about too much overburden of administrative duties and not enough time for creative research. I felt this, too. I had a system that I can recommend for you. I spent mornings at the library. I went away from my office. That meant if somebody called me with some stupid administrative thing-- I was Chief of the Mathematics Division, but I didn't want to be. If somebody called—now, if it's somebody like the Director of the Bureau, then they can forward the phone call to me—but if it's anyone else, someone would say, "I'm showing he's away in the library. You can reach him there." But the library doesn't like this, and people know it. So they'd call somebody else with their question. So at the library, I could reasonably work.

Now my work, at that time—and I was really interested in this—was computer programs for translation of human languages, especially translating from Russian to English. Now the famous Ida Rhodes, whom I mentioned once, she was originally Russian. And she had decided to work on machine translation—wanted to work on that. So she had persuaded somebody to give the Bureau some money for this work so that we could pay her. And since my salary was already taken care of as Deputy Chief of the Mathematics Division, I assisted her in her work. I did find this very interesting.

AKERA: That's wonderful. And, I understand. So you did some very important work there.

ALT: More generally, information processing.

AKERA: Yes. Which NBS itself had started working on in earnest?

ALT: Yes. But quite specifically, language translation is something good to have.

I did this work until I got to the American Institute of Physics. One day a visitor came to the Bureau, Bill Koch, a nuclear physicist who had left the Bureau and taken over the Directorship of the American Institute of Physics. He sat down at lunch with us, and said, "How would you like to join us. I need to establish some computers at the at the AIP." That's what he said.

AKERA: So he gave you an open invitation to come up to New York to do that work?

ALT: Yes. I wasn't the only one. Richmond. Where did he come from? I don't know. But what I did at AIP was citation research. Are you familiar with that line of work?

AKERA: Yes, yes.

ALT: I found it very interesting. Some people don't care, aren't interested, but I find citation research a very useful thing to do.

AKERA: It sounds like it goes back to some of your early interest in statistics and econometrics. Is it, in some sense, a similar kind of activity?

ALT: There is some statistics in it. You can see what papers are cited, and how frequently, and how long ago. One paper that I wrote way back was not only cited but also requested for translation after 30 years. There are not a lot of papers that would...

AKERA: ...that would fall into that category.

ALT: ... be requested 30 years after they are published. That's one thing.

AKERA: Which paper was that?

ALT: Something in econometrics. The measurability of utility— mathematical conditions for assigning numbers to the value of... how valuable something is. What should be the price of it.

AKERA: Yes. During the last session, you also mentioned that there were other reasons for moving to New York City. Something about the Vietnam War, and the larger political context. And perhaps there was also something about wanting to return to New York, instead of living in Washington. Was there either motive, or both?

ALT: Vietnam. That was one reason for quitting the government in general, but I was disgusted with them more. Another reason was that my wife was anxious to get back to New York. She had never liked it in Washington, so I was glad finally to have a chance to get her back to New York.

AKERA: What was it about New York City that your wife, and perhaps you as well, really found attractive?

ALT: I don't know. She just liked friends. To make them here.

AKERA: Family, as well?

ALT: We didn't have family. But more about interesting people.

AKERA: Yes. But Washington is also a very cosmopolitan place, an international culture. But it wasn't the same?

ALT: I don't know. She, unfortunately...

[Begin Tape 4A]

AKERA: We're very close to the end of the interview. But I just wanted to learn a bit about this stage of your career, because have had such a fascinating life. I would like to document all of it.

So you came to the American Institute of Physics and worked on computers there, and especially on citations research and such. How long did you do this work for the American Institute of Physics?

ALT: I was at the AIP for six years. I went there in order to establish the citation indexing system. The AIP started the project, with government support, to establish a computer file of journal articles, which no other library had. See, libraries had a file only of the journal volumes. We made it so a physicist could look up specific papers on any given subject.

AKERA: Was that successful? Did that system come into being?

ALT: Well, I don't know, because by the time I left, it was being sold to the libraries, but I don't know how much it was being used. Really, I don't know. I was getting out of it. Then when I retired from there, after six years...

AKERA: ...that would have been 1973?...

ALT: ...that project was financed by the government, some grant. And I didn't feel-- I was almost ready for retiring anyway. I was well over 60 years old. So I decided, I might as well retire now. My wife had died. My children were independent. I didn't see any reason for making more money. I thought of going into teaching, but that was not interesting.

AKERA: So you retired.

ALT: I just took it easy for a while.

AKERA: That's wonderful. Now can you tell us something about your retirement? I know that you've done a lot of things since your "retirement." I wonder if you can tell us something about this.

ALT: For a couple of years, I did nothing. That is, I took a course in automobile mechanics for example, because I wanted to fix up my own car. But I did essentially nothing. But then I felt that I should do something useful, and I joined as a volunteer in a peace and justice organization. Why did I join them? Because I happened to see an ad that they were looking for workers to be their staff, and I called them up and said, "If you want to have a cheap staff member, of course without any expense, I'll be glad to work for you half-time or something." And I said, "Now I know that with some things, it takes more of your time to keep a volunteer busy than to do the work yourself. I know that from experience, and I don't want you to land up with that. But let's see how it works out." And it did work out very well. They said, "Well, it happens that our own bookkeeper is leaving. Would you mind being a bookkeeper?" "Ooh, I have never been a bookkeeper, but I could learn," I said. I remained there, and was actually their organization and money person, for 15 years.

AKERA: Wow. And then...??

ALT: I stopped doing that partly because I was really getting old. I was 81 years old, and I had poor eyesight. I found the paperwork hard. And they moved; they left New York. They went to Georgia, and I didn't feel like moving to Georgia. They needed me too. By that time, I had become their chief representative. So for a while, they kept the office there, just with myself and one person. But the main work was being done down in Georgia, and the necessary information was sent to us by phone. I wrote their paychecks. But eventually, they took that over too.

AKERA: Dr. Alt, you've had a tremendously fascinating career, starting with your life in abstract mathematics in Vienna, and then your emigration, your career at NBS, and then your work at AIP, and then as a volunteer in New York City...

ALT: ...and by that time, I had married a second time.

AKERA: Yes, with Annice, who was very kind enough to host our conversations both days. Is there anything else from your life in retirement that you would care to relate to us?

ALT: My retirement? No. I spend my time now outdoors, or playing music. Well, I'm still interested in reading, but I am not really active. At age 95, you have had enough.

AKERA: Actually, I was very impressed that, when I called you the first time, you said that you had two different string quartets to play in that day.

ALT: Oh, yes, yes. I have about five different groups with whom I am playing.

AKERA: You do? Oh, my gosh!

ALT: I pull the groups together. I do the formatting for them.

AKERA: And which instrument do you play?

ALT: The violin or the viola. And yes, they are playing music quite a bit, both here and in other people's houses. But never performances. It's only for our own recreation.

AKERA: I think you have a very wonderful community, here in the Upper West Side of Manhattan.

ALT: People ask why it is that the interest in mathematics and in music so often coincide. Well, one answer is that both are abstract. Well, if this is the case, I don't think much of programmed music, though this is my own feeling, my fondness. I frankly don't like programmed music, especially if it's too pointed. I like the abstractness. Then the format between the written part of music and the performance is very close to geometry, to spatial geometry. There is some overlap here.

AKERA: ... well, Dr. Alt, thank you so much for a very wonderful interview.

ALT: Okay. I thank you, also.