

INTERVIEWEE: GERALD ESTRIN

INTERVIEWER: Robina Mapstone

DATE: 15 June 1973

RM: The date is June 15th, and this is Bobbi Mapstone, and I'm talking to Mr. Gerald Estrin, E-s-t-r-i-n, of UCLA. And this is an interview for the Smithsonian Computer History Project. [Recorder off]

Okay. So, perhaps start by going back and talking about your early beginnings first.

GE: I will start from my association with the computer field and backtrack a little bit from there.

RM: Okay. All right. Let me just check this. [Recorder off]
Okay.

GE: Actually, I completed my doctorate work in electrical engineering in microwave propagation at Wisconsin and I had just about accepted a teaching post on the faculty at Northwestern when, fortuitously, because there was no background in things related to computers, somebody told me about the project at the Institute for Advanced Study. Before making my decision, I sent off a letter to Von Neumann expressing my interest, and telling him that I was completing my Ph.D. work. A response did come back, asking both myself and my wife, who also was just completing her doctorate studies in electrical engineering, to come out for an interview. We made a trip to Princeton and were interviewed by Julian Bigelow, who was the chief engineer there, and of course, Von Neumann, and it just sounded very exciting. It was new. We

had good backgrounds and we both apparently looked like good candidates. There was a vacancy, at that particular point in history, in the group at the Institute, and it was a coincidence of interests. At any rate, we were both offered jobs for the summer with the proposal that I stay on permanently--it was a year to year contract--and that, as far as far as my wife was concerned, we'd see what sort of a matching of interests there was. And so it was, this was the summer of 1950, that we came to the Institute. It almost ended prematurely because on the way back by plane we got into an incredible storm in the connection from Milwaukee to Madison; a violent electrical storm; it was curling around the wings, and it was my wife's first trip of that kind, and first storm. I had experienced storms in the service before and I just kept saying, "This is nothing, you know. Safest place to be in the world during an electrical storm."

RM: [Laughter]

GE: We arrived over the airport at Madison and just then a bolt of lighting knocked the airport out, and we found ourselves in Minnesota, coming back by train the next day.

RM: Oh.

GE: That almost ended then. But at any rate, we came out to the Institute in the summer of 1950.

And the--all right now, I'll flash back a little bit. My own earlier education had been in liberal arts; in the beginning I had started at City College in New York, and first majored in

accounting and a number of things like that, but eventually majored in American history, and had almost completed my Bachelor's work in American history when the Second World War came. That motivated me to go into something technical; originally when I'd started college I had wanted to go into engineering mathematics, but I had been warned that there was a great deal of anti-Semitism in the engineering field at that time; there wasn't a particularly healthy job outlook and my family was deeply impacted by the depression, so I had changed my career path. As a result of manpower needs in World War II, I was put into radio repair in the service. Just before the war, Thelma Austern and I were married and she actually went into electrical engineering first at Stevens Institute and CCNY while I was in the service. Our interest in engineering and reenforced aptitudes were positive therefore, at the end of the war we both headed into engineering, undergraduate work. I went from senior back to a lowly sophomore

RM: [Laugh]

GE: and got started then. Things moved rapidly in that post-war rush of veterans returning to school. There were teaching and research opportunities, and so we went straight through to the doctorate. Starting in the spring of '46, my doctorate was finished at the end of 1950 and Thelma's in 1951.

RM: That was fast.

GE: It was quick. I had learned how to read and write in my previous years, so that part was easy. But there was a

complete shift in fields after my PhD. I had been working in microwave propagation and this accident of fate took me into this new, completely new, computer field with essentially no background except electronics and basic, general background--no knowledge in that particular field.

RM: Were you aware of the state-of-the-art at the time?

GE: No. I'd done some--a little bit of reading, but there wasn't very much literature, really, at the time.

RM: Was Von Neumann already sort of a figure-head in the project?

GE: No, no, Von Neumann was heavily involved. I hadn't really known anything about Von Neumann or anything to do with the computer field. My knowledge had been about engineering based on mathematics and physics. It had really just been on the basis of minimal knowledge that I became interested in the Von Neumann project. Well, I found the work very attractive--and in fact, people usually divide into two classes. Even given the group of people who have a rather strong interest in science and mathematics, there are some people who find the complexity of the details involved in computer systems either very attractive or not attractive. Well, I found it to be a great deal of fun. The Institute itself was attractive. The computer group was composed of a relatively small group of people, all of whom I got to like very much very quickly. It took a while for me to recognize that there was a bit of turmoil going on in that group. It was a small group; they had been struggling together under rather strong pressures and you

could feel some of the personal conflicts. That state of affairs may have enabled me to play an important constructive role in the Institute team, because characteristically my personality is not one to get highly involved in some of the interesting minutia that cause personal conflicts between people; not that I am generally disinterested, but I rarely jump into little frays. I really hit it off very well with Julian Bigelow, and quickly developed a strong respect for him and the extremely high level of creativity that had been introduced into the Institute project. He's a person who is a perfectionist in many ways and therefore it was difficult for him to draw a line and say, you know, "This is it. This is our trade-off. The design is frozen."

RM: Aha, yes.

GE: This, I'm sure, has come out a number of times.

RM: The Charles Babbage syndrome, as I call it. [Chuckle]

GE: However, there was a really fine aspect of his personality. If you had a disagreement, you could stand up and argue and he had a wonderfully strong capability of not falling into some trade lingo and into terminology that snows people. Exposition was a natural part of Bigelow's thinking; he would stick to basic ideas; and he enjoyed a good argument; and he enjoyed people standing up to him. And so I learned rather quickly under that kind of fire because that is what I liked doing; Julian Bigelow was very patient and led me into the kind of work that let me get quickly into what this computing machine project was all

about. ...

GE: And so who were the people in the IAS Computer Group when I joined them. There was Julian Bigelow; Jim Pomerene, who had primary responsibility for the memory system, and all-around, top-notch, creative engineer; Willis Ware; and Jack Rosenberg. Morris Rubinoff and I overlapped a very short period; I came as a replacement for Rubinoff in the group. He went to the University of Pennsylvania. Those people made up the key computer engineering staff. Due to the informality of the group, we quickly got to know Herman Goldstine, Jules Charney, the group of meteorologists who were working around there, and Von Neumann. In many ways, a perfect kind of group set up to carry out a project like this. The strength of Von Neumann behind it, a group of engineers, mathematicians, and a key set of principal users who wanted that machine completed to do calculations that had never been done before; you couldn't put together a mosaic that's really much stronger than that.

RM: So user and builder were together and feeding each other.

GE: Feeding each other and sometimes arguing, but just in the living together there was a chance to get to know each other and to know where these people wanted to go.

RM: What was the relationship with Von Neumann?

GE: Von Neumann did the hiring. Von Neumann was very active during his visits, participated in group planning and knew just where the development stood. He was around regularly; he knew what was going on, participated in the discussions, raised questions

about the design, the algorithms for carrying out arithmetic--everything was of interest to him. And, of course, he had this property that any area which he delved into, his mind was calculating away. There was always a numerical example testing a conjecture that had been put forth. And over the few years, we got to know Von Neumann pretty well, him and his wife, through social activities, which were carried on at a pretty good pace at the Institute. There were a lot of group activities. In fact, the rate of cocktail parties at that place was extremely high. There was a temporary housing project; mathematicians from all over the world; and it was really a close-knit community; well, even more close-knit than normal because it was sort of looked down upon by more permanent residents of Princeton. They brought down wooden barracks which had been World War II surplus and set them up on the grounds to save money, and this is where most of the visitors lived. It was just around the corner from the computer project itself.

RM: Did the university sort of look on this group as a maverick bunch?

GE: Not the university, the townspeople.

RM: Oh, the townspeople.

GE: I think there were really pretty fine relationships between the University and the Institute, and there was a lot of flow between them. Many people confused the two entirely. It was hard to tell where the Mathematics Department of the University

ended and the Institute began.

It was a very free atmosphere and, we got involved in this project day and night to get it done.

RM: Among the things that Von Neumann was famous for, I hear he made great martinis.

GE: Oh yeah. Superb.

GE & RM: [Laughter]

GE: And his wife, Clara, was a marvelous person, really full of life.

RM: What was your role?

GE: When we arrived we just became acquainted with the design of the machine; the arithmetic unit had just been assembled. At the time that I joined, it was being tested; not all of the control had been designed or been implemented. And so I made my entree into it by working on input-output gear, tying teletype tape punching equipment into the computer to produce the first input-output devices. It's a good way to become acquainted with the "animal" that you have to talk to. My wife, Thelma, worked mostly on the documentation of the arithmetic unit whose design had been completed and had been assembled, but one didn't know how closely what had been built actually compared to what was wanted. After working on the input-output I also worked with Jim Pomerene on the memory, additional memory developments, and got into the design of the control of the arithmetic unit and design and implementation of the control system. I came in the middle of the project, in

the final stages of decisions and development. In arithmetic, we hadn't yet settled on how the machine was going to do division, for example; multiplication methods probably had just about gotten settled when I arrived. And so I was involved in the division algorithms and the implementation and the debugging to make sure that they did what we wanted.

I recall Von Neuman coming in one time; we were having a lot of difficulty in making it work, and he saw all the work that was going on in our attempt to understand how the computer itself was doing division, working from his report to the actual implementation. We were in a period of real difficulty, and we couldn't understand what wasn't happening. He came by and he said, "Let me just tell you a little story." He said, "There's an old story about the centipede. It was once asked how it walks, and it never walked again."

GE & RM: [Laughter]

GE: That certainly came back many times. So, [I was] involved in these last important stages of design, arithmetic control, input-output, and then proceeded through the system debugging, and through the stages of seeing it work and executing its first programs. There were long periods of difficulty with electro-static memory, and there were times when the whole group would get involved in tracking problems down for periods that went into months. And documentation--describing what this machine did. So it was, I guess, late in '52 that, we progressed into a prolonged period of solving computational

problems, finding out areas of unreliability, finding out operations that the machine didn't do correctly, and repairing logical errors and errors in fabrication. Once we even moved the machine, took it apart; and were surprised to see that three or four years after being put together you still found wires that were unsoldered, and just lying against the other connection.

RM & GE: [Laughter]

RM: Did your wife stay on the project?

GE: No, she worked through the summer, finished her task; and then she returned to completing her doctorate dissertation at Wisconsin until beginning of and during '51. Then she took a position at Columbia working in biomedical engineering, which was her main interest. She's now director of the data processing lab at the Brain Research Institute at UCLA.

Now, as far as that period, some of the high points, I mean the things during those years that were important, for example, were the visits of people from outside and the feeling of what the early days of computer development were like. It's well known that people like Aiken, people like Bigelow, other leaders of the few initial groups distributed over the country, were very strong personalities. They convinced themselves about their ideas and their directions and followed them through to a conclusion. And there were many arguments among these people about "this is the only way to go; but historically, that's what has to happen during such a period;

otherwise you find that when there's some new development and the people are very malleable, you can never clearly identify what was intended--it becomes a folklore. But it's these kind of determined people who make strong impacts. Other people then extract the good out of all of the projects and move science-technology ahead.

RM: Right. Say, what's needed is a generation zero to establish all the various aspects.

GE: At least you need some groups which stick to their guns and go to completion, especially on any kind of a development that requires years to complete. You have to have good people who believe "what I am doing is the right way to do it" and carry that through long enough and go to completion so you can find out what was wrong.

RM: Yea, mhm.

GE: Aiken would come down through the labs and--you could feel the tension,

GE & RM: [Laughter]

GE: you know, you could cut it with a knife--and we would visit people up at Whirlwind at MIT. And there were sharp differences--at Princeton we were pursuing development of asynchronous machines and they were pursuing use of a clock to synchronize events. The arguments that would go on were incredible. You know, fun.

RM: And then some of the West Coast people must have been coming, too--Rand guys and --

GE: Yeah, at the Institute, yeah. There were a few groups around the country who were keeping very close contact, starting off by tracking the development at the Institute, and everyone of them at some point taking off on their own in some direction. There was a group at Argonne; there was a group at Oak Ridge; there was a group at Los Alamos; and at Rand.

RM: NBS.

GE: Yes. And NBS of course had their own philosophy; Sam Alexander established his own direction, and then there was also a group out here at the Institute for Numerical Analysis.

RM: Harry Huskey.

GE: Huskey. The contributions of all of these, these few engineers and the core of really dedicated technicians and people who made this group run--every one of them were very important during this period. And I'm sure some of the struggles that went on--all of those--were also of importance; they'd be hard to do without. And there should be no question about the really major contributions that Bigelow made to this technology--very, very strong.

RM: In a way he's not got as much credit as he should.

GE: No, he should get a lot more. ... He was just totally unconcerned with material things like patents and publication and--as a matter of fact, he was a combination, of somebody who is a perfectionist and doesn't like to get things down in writing because there's a new idea he wanted to explore and get on the record. He was less concerned with that record than

with pursuing new ideas. But the fact is he does deserve a great deal more recognition. ... He was a very unusual man. So unconcerned with material things like patents. And when we spoke later, in retrospect it became clear that he might have done great things for the Institute for Advanced Study and for himself personally by taking the kind of steps that would have had IBM really paying off for what they got out of the developments at the Institute.

Of course, I don't think there's any question that the work that was going on there created a turning point for IBM; and Von Neumann's direct involvement with IBM during that period was absolutely critical. I know from living through that period that IBM can never give enough to publicly supported research environments to pay off the returns that they and their stockholders have gotten. Every time I hear squeamishness on their part about support of research, [laugh] I check it off. It's improper. [Laughter]

We went through some rather tough times. It came to a point later in the game where there was obviously very serious competition with other machine projects, and Von Neumann needed the computing power of the Institute machine. And we worked like dogs, but it was with relatively unreliable components, sensitive elements, and there were, as I say, periods on the order of months where we'd get into difficulties in the control, and it would take far too long between problems in the control and the memory units until things got sifted out. But

there were also rather lengthy periods of reliable computation and any time one looks at the history of computation, you can pick out the major leaps ahead. The Williams tube memory, by its very nature, gave visual feedback to the user that I think we're still struggling for in graphic stations at great expense. Von Neumann and Herman Goldstine and others were able to get a good deal of intuitive information about just what was happening in the memory, which they could see on the slave monitor, besides having the fun of betting on what iteration a relaxation process would terminate on or which would be the last memory cell to go out.

RM: [Laugh]

GE: We also had some unusual visitors. Von Neumann's--one of his dreams, of course, was to have a computer make some significant impact on mathematics itself, and that really didn't come in his lifetime. But, as a result, there were many people in pure mathematics who were attracted to use the computer as a test bed; and aside from the problems that Von Neumann himself was interested in, people worked on theories of evolution and stellar theories. ...

All right. Are there any other questions that you have about that period?

RM: No, you pretty much clear them up as you go along.

GE: All right. Now, one of the, of our most frequent visitors to the Institute was Professor Chaim Pekeris of the Weizmann Institute of Science. His main area of interest is in

geophysics. At that time, I think he had come from a meteorology background, had come as a young man to MIT from a small town in eastern Europe, had gone through meteorology, but his main area of activity was in geophysics. And he had joined the Weizmann Institute of Science in their initial cadre of faculty members as a department head; he was head of the department of applied mathematics in 1948. He made his first visit a little sooner than that.

RM: Right after it was a full state.

GE: After or before. I think before. [Laughter]

RM: Was it?

GE: And he would then spend the summers at the Institute and sit quietly and get some new work under way. Back in Israel at that time he was heading up a geophysical survey of the country to discover sources of water, oil, and just to get through soundings throughout the country and to know what its resources are like. His goal was to set up a center or applied mathematics. He had been very strongly affected by some of the struggles between pure and applied mathematicians. He really didn't believe that these two forms could live well at all in the same family, and [believed] that it was essential that a few key centers in the world be set up whose concentration was in applied mathematics, primarily classical applied mathematics. Well apparently, since he had first come across computer development--and he is a man who worked with numerical tools, desk calculators--and since he'd come across the first

developments, for example, up at Harvard, he'd made up his mind--and he knew then he was going, apparently to be going to Isreal, he'd been having discussions with Chaim Weizmann who was looking for a cadre of people to establish the Institute in Isreal--and he decided he wanted to have a computer in Isreal.

RM: Just like that. [Laugh]

GE: That's right. [Laugh] And he made several attempts rather carefully to work with people who might go to Isreal and bring the technology there, and getting fellowships set up, and I think Rubinoﬀ was one person that he had hoped would come. It was in 1949, I guess that he met with Oppenheimer, Von Neumann, Einstein, to establish a plan to track Institute work and to get a computer built in Isreal. I mean, he was going to stick to it.

GE & RM: [Laughter]

GE: And they came to an agreement, they were perfectly agreeable that the fruits of the work that had developed here might be carried over. And then, I guess it was in 1951 that they formulated the plan for a man who was in electronics in Israel by the name of Ephraim Frei who Pekeris arranged to have brought over to the Institute and work with Bigelow with the hope that he would then come back and lead the development of the machine. He was also a fine guy; he's now head of the electronics department at the Weizmann Institute. We worked together; I was able to show him what computer development was like and to try and transfer information that would help him

carry out that development. And then--this was in 1952--he raised the issue with me of my possibly coming over there to the Weizmann Institute. And it was at the end of 1952, when we discussed this, and I thought it would be interesting adventure. I knew nothing very much about Israel, even though both my wife and I are Jewish; our education had been totally secular. My own parents were very much against any kind of religious establishment --I mean their culture was very strongly anti-religious--and this had kept me away from being interested and knowledgeable about what was going on in the world; I was certainly not a Zionist in any sense, but this looked like an interesting adventure to us with the chance to take part in the development elsewhere and use the fruits of our knowledge. The development at the Institute itself was in a state where it wasn't yet clear what the future of the computer group would be. And so we applied for a fellowship, I was awarded something called a Lipsky Fellowship set up by a philanthropist in the United States, to go to Israel for a year to help in the development of the machine. Frei actually stayed with us for two years. He was very knowledgeable, a very creative guy. At that time my wife had completed her doctorate (in 1951) and then, having been married since 1941, we heard something about it being difficult to have children as we grew older--she was twenty-nine or something like that

RM: [Laughter]

GE: and we decided, well, it's about time. It was right at that

same time that I remember picking up an article in Scientific American and reading the article and I remember it saying that estrin inhibits ovulation,

RM: [Laughter]

GE: the female hormone. So anyway, our first daughter was in 1953, in February. We were originally thinking of going over to Israel at the end of '52 or the beginning of '53, but then it was delayed until September actually, of '53. Our daughter, Margo, was eight months old when we left in October. We were in Europe for a couple of months.

RM: She missed being a Sabra by eight months.

GE: [Laugh]. She was ten months old when we arrived. When we arrived in Israel, it was still the tail-end of the long period of scarcity which really lasted from the time the state was created; people just had very little there through '51. I hadn't really the vaguest idea of what Israel would be like. First of all, the Weizmann Institute itself is a true oasis. Have you ever been to Israel?

RM: No. I want to go.

GE: Yeah. A really beautiful gem; at that time it sat in the midst of all the orange orchards, many of which have since gone down, with industry and buildings or homes replacing them. But it had been built in a really inspired way; a man by the name of Meier Weisgal who, after Weizmann's death, became, for a period, chairman and president, of the Weizmann Institute; he almost singlehandedly raised the money for the Weizmann

Institute. He took it upon himself to carry out Weizmann's dream, with whom he had worked very closely. On the surface, he was a very coarse man --my first reactions were incredible. [Laugh] I mean--but he had a marvelous heart and soul, just a superb person that way. Then I was taken and Pekeris and Frei showed me there was an applied mathematics building and it was shared by the polymer chemistry department and had some other biology research and there was a basement and in that basement were [phone rings]

RM: Excuse me. [Recorder off]

GE: By the way, I was on leave from the Institute for Advanced Study in this fellowship. It was clear scientific collaboration at its best level; any information that was necessary for transfer was just very free.

RM: You were describing the building.

GE: Yeah. It was shared by chemistry, biology, mathematics, and the new electronics department; and I was brought down to this basement and "this is where we propose to build a computer." This whole basement was occupied by two or three departments and I said, "Like where, you know?" And they said, "Well, you'll have to make your --"

GE & RM: [Laughter]

GE: Well, it became rather clear after two or three weeks, maybe a month, that Frei had decided it was too big a thing for Israel to build a computer like that; that he was willing to help and use all his training, but he just felt that, here was this

little country, no resources [laughter] and he had seen the six, seven years of struggle that had gone on in Princeton; he was totally aware of that, that history. He had lived in Israel; he knew you couldn't get any materials, parts; there was nobody that knew anything about it; and he also knew of many people who said, "This is the most ridiculous thing in the world, for an indebted country like this, an agricultural country, to have a computer which is something of the order of a million dollars or something like that. I just doesn't make any sense. Who knows how to use it?"

RM: He was talking good sense, too.

GE: Yeah, yeah.

GE & RM: [Laughter]

GE: And I'm going to flash back for a moment, because before I left the Institute at Princeton I--we had also received visits from a number of people, and Frei had told me that many people were opposed to building a machine like that in Israel. And I also had my qualms; the first time I took a look at that dot on the map. And I remember having a talk with Von Neumann and I asked him what did he think; I said, "We'll go through the struggle to build a machine like that," I said, "will it get used?" And he said, "Listen. This guy Pekeris," he said, "is an extremely versatile, hard-working guy." He says, "I guarantee you that if nobody else in that country does anything to use that machine, he will keep it busy full-time by himself." And it was a fortunate thing that I had that discussion with Johnny;

it came back many times because, see, the first cocktail party I was at at the Institute in Israel there were some of the most prominent scientists there; and one of the first things that came out in discussion was everybody there, except for Pekeris, was saying, "This is an absolutely ridiculous thing to do for our country. Go use the resources someplace else."

RM: You think if Johnny hadn't said that to you, you would have been right in there agreeing with them?

GE: Well, I don't know if it's a matter of agreeing, but I certainly would have been swayed; and during difficult periods you don't need much more than that. Why break your neck on something that is really not going to be used? But I had taken the pains to get to know Pekeris a little better, to read some of his work, and have a little feeling for what was going on.

RM: I'm going to turn the tape before we get carried away.

[END OF SIDE I]

[SIDE II]

RM: This is side two of my interview with Gerry Estrin on June 15, 1973. [Recorder off] Okay.

GE: We came to the Weizmann Institute. There was a background of, except for Pekeris, singular lack of enthusiasm about the prospects for this machine. My wife and I were set up in this little private house just off the grounds of the Institute--unpaved street. When we walked up and down to the house there was a little band of geese always trailing after us; it's a very threatening thing--have you ever been followed by geese? [Laughter]

RM: Yes. [Laughter] They scare the devil out of me.

GE: The next-door neighbors were the postmasters of the town of Rehovot. The main streets of the town of Rehovot still had bomb holes in them because they weren't very aggressive about repair and things like that; it's a sleepy, little middle-eastern town. But I had, of course, come there with very few preconceptions except to do anything I can to help them. And then it became clear, you know, from Frei that after strong consideration on his part he really felt that this was too big a project to do, as I had said. I then had a discussion with Pekeris. He says, "Yeah." He says, "I know." He says, "You're in charge. You're going to build the machine."

RM: [Chuckle]

GE: Well, it didn't take very long to decide as far as I was concerned; I was going to make maximum use out of my year

there; I found it possible to fall in love with the place pretty quickly; no, I didn't come with any conceptions of a panacea which was going to save the world nor conceptions about the importance of building up a Zionist state or anything like that. But everything that I did come in touch with and felt had very deep effects on me. There were things in my culture that had come through my father's sense of humor or little stories about his or my mother's traditions. And it was in a way quite a thing coming through England and Europe for the first time, moving back into history, then coming into this place. Really a thrilling experience. We had a very young child, and so we spent a month in London and a month in Paris and three weeks in Italy, because both of us had been brought up in New York; we were city people, not country people, and we always liked the feel of the soul of the city; just to walk around in it, downtown amongst people and buildings. So, it was very nice. And my whole tendency was "Okay, I'm not helping, I'm in charge. I'll try to give it a stab." And to go into it hot and heavy; and the year extended into eighteen months--a little less than that--until the time we left in the spring of '55. But it probably began the most intense period of my life. The Institute for Advanced Study had been pretty intense, it was day and night work back at Princeton; but I had not, until that point, had this level of responsibility thrown in my lap. And here's the way things went: my wife, fortunately, had that period of experience in Princeton, in

addition to her doctoral education and she joined as part of the group. What I was given there, aside from Frei, was one technician in the electronics department, and even that was half time.

GE & RM: [Laughter]

GE: And I started to plan the kind of materials and equipment we would need and to, first, even get to know what was available in Israel at all. What was available in Israel was dedicated people, and that's about it. I made some rapid decisions, as to what modifications we were going to introduce compared to the Princeton machine and decided to first build up a partial work-equivalent of its arithmetic unit so that we got all of everything in production under control. And so one of the first things was--with thousands of vacuum tubes coming in here--what kind of testing capability we had. Because reliability was a critical thing--and there were no tube testers. So we had to design and build our first tube tester. I said, "Okay." and then in order to get to know what was available we spent a period of time in Tel Aviv seeing what industry had to offer us.

GE & RM: [Laughter]

GE: And that was a lovely period of searching through little stores where they had gotten some used material; one place had a potentiometer that had been gotten--Lord knows where the stuff came from; but going through these alleys and little stores until you found some semblance of the set of parts that you

of them you may have encountered on the West Coast, George Eisler.

RM: No, I haven't.

GE: Oh, I see. I understand. He's been on the West Coast for quite some time and was involved in a good deal of the early frontier work and moved into computer test of automobiles; but it would probably be a worthwhile name to communicate with.

RM: Do you know where he is now?

GE: No. In the area. He may still be in the phone book. I haven't been him for years. But he and his wife, at that point, came through for a visit. I don't really remember now how long they stayed, but he worked on a magnetic drum development; and that didn't mean buying the drum, you know: that meant making the drum and we had difficulty with it. Peter Wagner, who is now a professor at Brown, happened to drop through the Institute; he was a very young man at that time, and he says our meeting together, just this impact, led to his decision to go into the computer field. First, of course, there was a small cadre of technicians to be trained and do the important kind of work in the development. And there was one interesting thing that I learned very quickly about Israelis; it's highly unlikely that they would ever come under the rule of a single dictator. There's a very fundamental kind of democracy that exists there, and it's in the tradition of: you don't look up to anybody or take anybody's word for anything; you convince yourself and you argue. That's it. And for

example, the key--this first technician whom I had, who then went on and almost put the machine together single-handedly: The first thing I was going to show this guy was how to solder components because the traditional ways of making radios and things like that were totally unreliable for computers; taking resistors and taking your leads and winding them around did the kind of things that were terrible for reliability in a digital system. So I put him through a training period, and argue--it was incredible!

GE & RM: [Laughter]

GE: "Was it going to be good enough?" And after three weeks I said, "Okay. That's it. We've been all through this and now you're going to do it the way I showed you." Well, he sulked.

GE & RM: [Laughter]

GE: It was terrible. But he really went on. A very, very fine person. He convinced himself that what I said was okay. But that was a characteristic that stayed; I found that over and over again. I went through extensive interviews to pick out a group of people and they came from all backgrounds, all records destroyed, they didn't have anything except their most recent histories; we set up tests to find out what they were capable of and to also get some feeling for reliability of the impossible stories you heard of people who came into that area from Germany, Europe, all the others. But a small group of people were brought together by a man who knew no Hebrew, didn't know the alphabet.

RM: I was going to ask you that.

GE: I think I studied the alphabet just before I left. My wife and I did spend time with a woman from Holland who gave us lessons two or three times a week. And what I decided--I was on the search, at that time, early in the game, for a person who I would conjecture to take over when I left--a very critical aspect; and found some exceptional people. I mean, they had excellent experience; there was one thing that was very characteristic there at that time there was still a pioneering spirit. There was the desire to build something out of whatever junk you could get ahold of; I mean, you saw some amazing things because everything was of value. It was sort of a refresher for us and the kind of things we had just taken for granted. Life in the States, where there would be paper, water, all those things, developed a new meaning for us during that time. But these people, that was their second nature; I mean, they were used to making things work out of the best stuff they had around. You had to be careful about that sometimes because you'd find, sometimes, innovation going on in the middle of the computer.

GE & RM: [Laughter]

GE: But rather early I did find a man who had been head of a group in the Signal Corps--no computer background, of course--but a serious guy, capable of management; I decided early that he was the guy who would take over and also did some things while we were waiting for flow of materials. I started training this

group of people and after going part way through with them, I began to have discussions with him and he would lecture in Hebrew to them to assure better transmission; and I had learned enough by then to be able to catch errors as he went along in his discussion. I very rarely spoke in Hebrew because it's my personality not to want to make errors and particularly in an environment like that. So instead I used that mechanism that I was always much better at, detecting errors in this new language.

RM: I take it then that you, until you had got this other man partly trained, you were doing all the teaching yourself.

GE: Oh, yes. And his name is Zvi Riesel. He's now on the faculty of the Institute.

All right. So there were six months in the flow of materials; it also turned out that for putting together, for fabricating this system, we needed things like filament strips for vacuum tubes. You needed some rather elementary sheet metal production work. The Weizmann Institute had a magnificent machine shop, the kind you would expect at a really top-notch research institute; but all precision, Swiss equipment to do sheet metal work. [Laughter] You know, you had to use the finest machine tool equipment to do that. And so we did some searching around, and half-way between Tel Aviv and Rehovot we came across a machine shop run by two Bulgarians--refugees. Chickens running around. They made bicycle parts and fan parts, but they also had some equipment that could stamp out

copper and aluminum strips and things like that. This was our machine shop. ...And so we had to do all the drawings, we had to convert, of course, to the system, European systems, as opposed to U.S. systems. And all our drawings were set up and so we were--as material came in, as plastic came in, as copper, as aluminum came in, we were set up for production of components and everything was fabricated there. All resistors had to be tested, all vacuum tubes had to be tested. We started off in the development--you know, there was nothing; no frames, anything around. There was a fair amount of chemical equipment around, so we tended to use chemical stands and equipment to hold together the first components. Here you can see some of the things. [Showing pictures] That was in that machine shop. That was the valuable hunk of equipment.

RM: Oh, it's lovely!

GE: And here you can see the chemical clamps and stuff, you know. Those were the kind of things that we used in building all this up. That was what was around.

RM: Isn't that great! Your air conditioning unit was a fan.

GE: Uh-huh.

RM: [Laughter]

GE: That's right. And it gets warm there.

GE & RM: [Laughter]

GE: And here we're in a group of people; this is Zvi Riesel, who is the one that trained to take over; that's Professor Frei; this young man came through Germany and was in on of the Rothschild

camps in France, you know, as a result of the Nazi period, and he came in rather early as a technician --meticulous. He now has some reasonably senior job; it's at Ampex or Electronic Memories here in the States.

RM: Oh, in the States.

GE: He migrated to here. Yeah. This is the man who was my first technician, Micha Kedem and after finishing the Institute machine--and he kept it going for a long time--he then moved to the Ministry of Defense and was in charge of all maintenance for their Philco computers and other computers there. And now he has transferred actually to IBM, Israel. This man was a visitor from Philadelphia, stayed for a reasonably short period and worked on arithmetic design; he's an interesting guy. Aviezri Fraenkel, who is now on the faculty there and at Bar Elan. One time we were having lessons with our Hebrew tutor and she told us that there was a young man, Aviezri Fraenkel, who was going to be a member of our group. I'd never heard his name before and I said, "He is?" because it was just a small group of people and I knew everyone, and she said, "Yes, he's going out with our daughter." Well, sure enough, about three weeks later, I got a request for an interview, a letter, a man getting out of the military service at a certain time and he would like to work for us free; he had sort of a leave until he was actually dismissed, and would like to be working with us. I interviewed him; he was an intelligent guy and, sure enough, he began working with us and he had it figured out in his

mind that he was going to continue as part of this group, he liked this field, and he was an exceptional guy. So there was just no question; but these were the kind of things that went on. Very religious young man--orthodox--and taught me a lot. I myself, before I went, had very little tolerance for the ritual orthodoxy. I learned a lot and appreciated a lot about people's feelings for traditions during the time I was there. He helped me in that. After the building of the machine, he later--he came to UCLA, and worked first under me, and then went on to get his doctorate in mathematics at UCLA and returned to Israel. It was an interesting sidelight that his main interest led him into pure mathematics. Pekeris had supported him to come to the States for training after his work there, with the expectation that Frankel would come back and lead other computer development. Well, here he went off in the pure mathematics direction, which to Pekeris was incompatible, you know, with the dreams for his department. So there were conflicts, plenty of conflicts that went on.

All right, so six months for training, flow of materials, the machine started production--really, when--I say flow when things would start, and that was a rather short period for Israel. And the machine, central machine, was finished in nine months; which was a very remarkable period by any standards.

RM: Even considering the fact that you were using the basic IAS design.

GE: With a difference. It was day and night work; it would have

been seven days a week if Frankel and others didn't raise objections.

GE & RM: [Laughter]

GE: So it was very remarkable. And then when we left--also, one of the big modifications we made in this machine was to make the decision to depart from the electro-static memory and get one of the first commercial magnetic core memories that was out; it was like the one that was being put into the Rand machine from--Telemeter Magnetics. And that was followed up--the arrangements had all been made for that before I left and then followed up after I left.

RM: Did you start with an electro-static memory or did you go straight to a core memory?

GE: Went straight to a core memory.

RM: So, in other words, when you left, the machine was done.

GE: When I left, the CPU was finished, a drum--as I remember here--that was working, input-output was working, and arrangements had been made for the specifications, the delivery of the core memory; but that was going to occur--I don't remember when the actual delivery on that was made; I'll have to think about it. [Laugh]

RM: Was it the same size drum as the Johnniac?

GE: No.

RM: Or [inaudible]?

GE: I don't even remember the numbers on that. But the core memory, that was four thousand words, forty bit words. Let me

think. Yeah. But the--and the central machine had all been checked out as far as its arithmetic control, adding, was concerned, and this rudimentary input-output.

RM: What were some other changes from the IAS?

GE: The basic vacuum tube that was used, and some modifications--we had learned a certain difficulty in our using--as far as the control design, and we simplified some of the areas of control design. But I'd say the basic choice of the vacuum tube, core memory, and some modification of control. Those were the things which I decided would show more cultivation than in my --

RM: What was the I/O?

GE: And the I/O was just teletype. And then they went on later from--I don't know if they ever got cards; I think they ought to have; I think they did. But they had--see, there was one important thing: Pekeris's problems were very heavy computational problems. I/O was not of very strong consideration, except for your interaction with reliability, you know, all that was going on. He was always formulating problems that were at the edge of the capability of the existing machines--the solution--and he was always prepared for the next one; had established his computational algorithms so that they were ready for the next machine to make another dent in problems that would be solved. And so he got about 46,000 hours of computation out of that machine. It was quite hard work. It put to bed --

RM: How did the machine get funded?

GE: Oh, from the Weizmann Institute itself and funds that they raised to support the Institute. I mean, most of the support of the Institute was from private donors; they had some support from the government, generally less than the other institutions. And some of the other geniuses and Weisgal, too, 'd go out and get money. When I came and made up my mind about the Telemeter magnetics memory, that this was the way to do, and I went and spoke to him: He was a man who was capable of coming to decisions quickly; at a private institution that's a little easier than at a public one. And he made some phone calls in an attempt to get some support from people, and did it; I knew rather shortly that this was able to be carried out, and could proceed. The cost of the machine, aside from the memory, was something under a hundred thousand dollars. These were really probably the most important things covering that period. I'm going to have to call it quits sometime.

RM: Can I just pop a question?

GE: Sure.

RM: While the machine was getting built, did the attitudes start to change of the people around you?

GE: The attitude certainly, within the group, the morale, you know, reached a very, very high--high level. The attitudes of scientists around only towards the very end, and I think only in the period following our departure, when the first applications--when it was seen that this thing would be really kept at work; and then, in fact, you found the inverse taking

place because Pekeris had worked for a lot of years for this result and he had problems backlogged, and it was a struggle for some of the people to get onto the machine and get time. It actually was the first training ground where a rather large flow of people, after awhile, did come through and became users for the first time on that machine, and then went out to other places and other countries. But whenever there's a shortage of resources you always find people saying, "that guy will never let me get onto the machine," and he was one of those strong personalities in the beginning of all those computer projects and so those kind of stories still stick around. But, yeah, I'd say there had begun to be a change, but that wasn't really true until some time after the machine was done, after Pekeris's first results were around and, in fact, I'm sure that in donations they got back their costs many times over because visitors coming through the Institute and seeing this capability, the possibility of bring to Israel this high level of technology and what it was doing, had to have a significant effect; couldn't help it.

RM: What about the training programs? How was that handled?

GE: That was done from within the group. Pekeris himself had developed a cadre around him of people who were interested in the solutions of these problems and who learned numerical analysis and who got on and learned to program the WEIZAC while it was being built. So he wasn't really short-handed, and in fact the demand for the solution of problems kept up so heavy

that it took a long time before compilers, you know, were worked on. There just was--with this backlog and with people being able to work in machine and assembly language and keep it busy. The incentive for opening up to a larger user group was not very strong. People came to help from Hebrew University in a number of different departments and got their first training on computers there. And, after a while, the Ministry of Defense made use of Pekeris's machine whenever they could get on it, but then that gave them the spur to get their own first machine and they became the key place that programmers got trained.

RM: Did they build or buy?

GE: They bought. They bought Philco S-2000 TRANSAC.

RM: Were their other WEIZACs?

GE: No.

RM: There was just one.

GE: There was just one, and then in 1960 or '61 the plan came up for the next generation. There had been a stab at--one of the machines you'll come across on the West Coast the Wenner-Gren--

RM: ALWAC.

GE: ALWAC, yeah. And its equivalent had been developed in Sweden and they made a gift of one of those to the Weizmann Institute and it was never gotten to work properly. There were a number of people who came over on fellowships like Wieselmann of Dataproducts you know, kept bringing advances to the group. Wieselmann, Goldberg, who was up at Stanford Research Institute,

and a chain of people. And then Pekeris began to press the plan for a new machine, the GOLEM machine.

RM: Oh, that's right.

GE: And Smil Ruhman, who was with Parkard-Bell at the time, working with Palevsky's group there, was the man I had come in contact with professionally out here and brought him together with Pekeris and Pekeris had also been working with people at Illinois and their plan was to go through this exercise again it had worked so well;

GE & RM: [Laughter]

GE: to do this with the next Illinois machine. His whole effort was to get much more scientific computing power. And also his dream was that a computer industry would be built up in Israel, too; that was part of Pekeris's own Zionist drive. So this thing came to fruition. Ruhman was an excellent computer engineer and he essentially took the design that came out of the University of Illinois laboratories on the ILLIAC II and re-engineered that so that it was something that was far superior to the machine that had been done in those laboratories and built two copies of what he call GOLEM A which have been working together for a long time now. And then in 1966 came up a plan for another yet more advanced machine called the GOLEM B which is--the hardware has just come to completion in this past week after a long stretch. He was trying to make a breakthrough in some new technology of ultra high speed work and didn't quite succeed; so the early years

went into that and it dragged on so long that the real difficulties almost came to a crash because of, you know, financial support dying out and not enough people that are interested in universities building machines.

RM: That's right.

GE: Not in these days.

RM: That's right.

GE: But they have just, let's say, come to completion of their hardware, and they're now in their final software development; presumably the machine will be on the air with reasonable operating systems, FORTRAN compiler, by the end of this year.

RM: Do you know, what was the date that WEIZAC went on the air? Do you know?

GE: WEIZAC--of course, that's an evolution. The date--it was doing its basic computations, you know, and working with input-output equipment and stuff before I left, with the drum memory in very weak shape. The core memory itself was delivered--let's see, I came back in April of '55 and we did have a [inaudible] going [laughter].

RM: Wow! [Laugh]

GE: And I went back to the Institute and came out here in October of '56. And Zvi Riesel came out for the check-out of the Telemeter Magnetics memory before it went there--I'm just trying to reconstruct--and my guess is that it was in 1956 that they intergrated the machine with its core memory. It was put together.

RM: Just sort of as a summary, what would you--can you sort of just pinpoint some of this in real significant highlights that have been caused by the WEIZAC?

GE: First of all, the WEIZAC was probably like, you know, the first modern electronic computer outside of Western Europe, and it initiated the whole development of computers certainly in Israel, which is rather strong now. Most of them have come from IBM computers and commercial computers in Israel, but there's very widespread use; you know, maybe the highest per capita [laugh] in the, in the world. So there was a heavy impact on scientific computation as far as its direct [inaudible] is concerned within Israel itself and a number of important problems were solved in geophysics using it, just as far as its use as a tool. The training of the first users of computers in Israel went on there; both programmers and scientists and people who eventually went into business data processing. Because it was a very different thing, you know, having touch with something in the United States or coming to use and try and test a problem in some area of logistics or inventory control, you know, that was locally available to you.

RM: Mhm.

GE: And so people got caught up in that and then went on to develop a computer industry there. So one has to say that WEIZAC was the spark of that. As far as its own impact on computer development, we will see, looking at that machine and its successors, a concentration on scientific computation in the

hands of people who were concerned with getting the most work out of this workhorse in their particular field of interest. And it's a rather nice, clean development. They didn't, you know, get into time sharing, except with the commercial machines, or into the somewhat sophisticated development of languages and operating systems, which are so critical to our computer development. But there was a reason for that; it was that they were unwilling to have the overhead. Okay? And they had people who worked very hard getting the last epsilon of work out of that machine on particular problems which were just beyond the capability of machines to solve them.

RM: Mhm.

GE: And there was historic reason, studying some of those things that will turn out to be, you know, quite good pieces of work to look back on. And that's--and aside, also was a general thing, in establishing the self confidence of a country to go into advanced technology; and this is the thing that, it's a step that's very hard to--people within the country admit it, it's a farm culture, to know that they can do that.

RM: Mhm. Yes. So this really was the spawning ground.

GE: That's right. And it undoubtedly had effect on many other things that went on in the industrialization in that country.

RM: May I--I see you've brought a file.

GE: Well, I thought there may be some things that'll be helpful.

RM: If you've got to dash. I'm going to turn this off for a minute. [Recorder off]

GE: I had a press conference. Twenty-three foreign language journalists. [Laughter] Now, Pekeris has a natural worry, aversion to press conferences. I guess he hasn't liked what has come out. But this was something that was unique. I unfortunately wasn't able to get the result of the articles, but people read some of the things to me, and the interpretations of what went on--they were just out of this world. You can just imagine the translation into twenty-three different languages at that time. But that gives you a write-up that at that time was a profile.

RM: Okay. Israel Speakes.

[END OF INTERVIEW]

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