

An Interview with
HERMAN GOLDSTINE

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Conducted by Nancy Stern

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Charles Babbage Institute
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Herman Goldstine Interview
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Abstract

Goldstine, associate director of the Institute for Advanced Study (IAS) computer project from 1945 to 1956, discusses his role in the project. He describes the acquisition of funding from the Office of Naval Research, the hiring of staff, and his relationship with John von Neumann. Goldstine explains that von Neumann was responsible for convincing the Institute to sponsor the computer project. Goldstine praises von Neumann's contributions, among which he counts the first logical design of a computer and the concept of stored programming.

Goldstine turns next to the relations between the project and one of its funders, the Atomic Energy Commission. He points out the conflict of interest of IAS director Robert Oppenheimer, who chaired the AEC General Advisory Committee, and von Neumann who sat on this committee, when other AEC officials discontinued funding for the project. Goldstine also recounts the problems that arose during the project over patent rights and their resolution. Goldstine concludes by discussing the many visitors to the project and the many computers (Whirlwind, ILLIAC, JOHNNIAC, IBM 701) modeled after the IAS computer.

HERMAN GOLDSTINE INTERVIEW

DATE: 11 August 1980

INTERVIEWER: Nancy Stern

LOCATION: Princeton, NJ

STERN: This is an interview with Herman Goldstine in his home; August 11, 1980. What I'd like to talk about, Herman, for the most part today, is your work with von Neumann at the Institute; that is, the computer project at the Institute; because we've spoken in the past about the Moore School work. First, according to my reading of the papers down at the Library of Congress, von Neumann seemed to decide that he wanted to build his own computer around the spring to the summer of 1945. Is that correct?

GOLDSTINE: Right. That's right.

STERN: And he sent out queries to various institutions like Harvard and Columbia and MIT to see if they would be interested in setting up a computing project.

GOLDSTINE: Yes. In fact, each one of us independently tried different sources.

STERN: What sources did you try?

GOLDSTINE: I tried the American Philosophical Society and the American Mathematical Society. I talked to the people at the University of Pennsylvania about it. Those are the ones I remember right now.

STERN: This was for funding now, that your referring to?

GOLDSTINE: That's correct. Right. Right. Now, von Neumann also approached the Rockefeller Foundation. He approached Warren Weaver.

STERN: And what happened with that?

GOLDSTINE: Weaver turned us down. He preferred actually to back the--I've forgotten what it's called--maybe it's called the Rockefeller Analyzer or something. This was a differential analyzer, an electric differential analyzer that was being built at MIT, and Weaver felt that that was a better bet than a digital computer.

STERN: Now, the documents that I read at the Library of Congress indicated that Weaver was a little bit concerned about a computer project that would be run by an academic institution in conjunction with a commercial institution. Had he said anything to you about that?

GOLDSTINE: Yes, well there were a lot of reasons. My own reading of the thing was that he really believed in the MIT people more than he believed in us. My feeling was that it was just poor taste on Weaver's part. But that's my own view of it.

STERN: Now, the project was eventually funded by Army Ordnance.

GOLDSTINE: Army Ordnance, correct.

STERN: Initially.

GOLDSTINE: That's correct.

STERN: That was the same branch of the Army Ordnance that had funded the Moore School project?

GOLDSTINE: That's correct. Right.

STERN: And according to the papers I read, they only provided about \$100,000 in funding to begin with?

GOLDSTINE: I don't remember the numbers, but if you say that, I believe you.

STERN: The Navy also provided some money?

GOLDSTINE: Well, concurrently with the Army project, we had a Navy contract to do numerical analysis, and that was the way things stood at the beginning. That was the Office of Naval Research. And that got expanded then so that there was a second Navy contract, which had to do dynamic meteorology, i.e., numerical forecasting.

STERN: This was in the 1945-46 period, or was this later?

GOLDSTINE: It was in--'46-'47, or '47-'48 that the numerical meteorology project started; again, you'd have to check the dates, because I don't remember them.

STERN: Okay. The office of Naval Research grew out of the Office of Research and Inventions?

GOLDSTINE: I think so. But again, if you really want to know the history of that, you should talk to Mina Rees, because you might as well know it correctly.

STERN: The correspondences I read were between von Neumann and a man named Admiral Bowen.

GOLDSTINE: That's correct. Admiral Bowen was, I think, the first head of the Office of Naval Research.

STERN: Okay. Now, did the Institute fund this project in any way?

GOLDSTINE: Yes, they put up \$300,000. And there is correspondence on that. In fact, I think in my book there's a

statement about this funding, about this amount of money that the Institute put into it. But at any rate von Neumann tried to get the Navy to put up money for this project initially, and the Navy refused on the grounds that it wanted title to the machine. So then I turned to Colonel Gillon, and he put up the money for the project.

STERN: And the Army was not that interested in having title, as long as it had royalty free rights.

GOLDSTINE: That's correct.

STERN: Okay. Now, what sort of funding did RCA provide for this.

GOLDSTINE: It didn't provide funding at all. It provided work on the Selectron. That's what its contribution was. The Selectron, if you remember, was a storage tube.

STERN: I was going to ask you some questions about that later. Princeton University indicated that it would be willing to offer its services in terms of laboratory facilities.

GOLDSTINE: Well, it didn't, in fact. What it did was provide John Tukey as an occasional consultant. That's what it amounted to.

STERN: How come that didn't materialize--any further assistance?

GOLDSTINE: I don't know. But that's the way it was.

STERN: Initially then, this project--once it was approved by the Institute, which I believe was around October of '45--

GOLDSTINE: Yes.

STERN: --the main problems, as I say them, were to find the funding for a building and then to get a staff assembled.

GOLDSTINE: Right.

STERN: Were there any other immediate concerns that you can recall?

GOLDSTINE: Yes--there was the whole problem--I mean, you've said it really. The main problem was that the Institute had never done any experimental work so there was no equipment of any kind. So it was a matter of not just getting a staff, but also building laboratories. And that was really a big problem.

STERN: Now the Institute from your initial involvement--was it averse to the concept of having a laboratory and experimental facilities?

GOLDSTINE: Not at that time. Later it got more and more averse. The Institute had a policy--has a policy still--that it really wants no experimental work. And so this was a departure. And it was willing in those days to make that departure and it got less and less willing as time went on.

STERN: It was willing to make a departure because of von Neumann?

GOLDSTINE: I think probably that's the main reason.

STERN: What were some of the other reasons?

GOLDSTINE: Well, if you read that letter from Aydelotte to the Board, it's in my book--or part of it's in my book--you'll see that Aydelotte was convinced it was really a ground-breaker, and that it was so exciting a thing that the Institute should depart from its policy. And as it got nearer and nearer--as it got finished, the opposition got more and more. And part of the opposition, I think a major portion--well, there was a whole build-up of opposition by

mathematicians to computers in that period. That was Item 1. Item 2 was that the budget for the project was as big or bigger than the budget for the School of Mathematics. And so I think the mathematicians were more and more worried about what would happen in case the government cut the budget; then the Institute would be left with something they couldn't support--so it was a question of who shared the money. And those were all the bases for the opposition. So I think you can say that there was this feeling on the Institute's part, the School of Mathematics, that it wanted to do research in mathematics of the sort that it felt was proper. And secondly, it was worried about money. But anyway, those were the concerns.

STERN: Now, when you talk about mathematicians, does it include [Marston] Morse

GOLDSTINE: Sure.

STERN: [Oswald] Veblen?

GOLDSTINE: Right.

STERN: And who else?

GOLDSTINE: Well, there was a whole group: Alexander, Veblen, Montgomery, Morse, Einstein, Godel...

STERN: The fact that Oppenheimer took over soon after this project started as Director at the Institute, did that have any effect on the attitude of people at the Institute?

GOLDSTINE: No. No.

STERN: He was as firmly committed to this as Aydelotte?

GOLDSTINE: Well, that's very complicated. He certainly was very helpful to me. How much he was committed to it, that's hard to know. He and von Neumann were not very good friends, so his attitude was one of keeping his hands off and letting us do what we wanted.

STERN: They were not very good friends on the basis of their political differences, or it went deeper than that?

GOLDSTINE: I don't know. It was a deep difference between the men. I think it had to do with a lot of antagonisms that grew up between them when they were students together in Gottingen. I think they were two very bright boys who didn't get along too well.

STERN: At the point that the project was funded, there was a question of-first, getting a building, before getting even the actual facilities that a laboratory would need.

GOLDSTINE: Well, we actually built laboratories in the basement of the Institute, before we built a building.

STERN: In the basement of the Institute?

GOLDSTINE: Yes.

STERN: Where or what was Baron's garage that I read something about?

GOLDSTINE: There was a building on the grounds of the Institute--it was a little house, in which a man named Baron lived. It was, I think, a used for another house. It was just one of those little buildings that the Institute used for people to live. I don't think we ever built a lab in that building, but we may have had some stuff in there. I've just forgotten about that.

STERN: But you initially worked in the basement of the Institute, and then you built your own building?

GOLDSTINE: That's right.

STERN: Now, it took about a year before that building was completed. Is that correct?

GOLDSTINE: Oh, I'm sure. I'm sure. In fact, we not only built a building, but we later enlarged the building.

STERN: During that first year, there was development work going on at the same time that the building was under construction.

GOLDSTINE: Sure.

STERN: Now, in terms of getting a staff, after you, Bigelow was the first person to be hired.

GOLDSTINE: No. No. I think the first person after me was Art Burks.

STERN: Now, Burks only stayed for a summer.

GOLDSTINE: No, he stayed for, I think, a year, and then we brought him back for a summer. That's my remembrance.

STERN: I thought he came in March and left in September, and then came back the following summer.

GOLDSTINE: That probably is true. That may be true.

STERN: That struck me as odd that he would make a commitment to come to the Institute and then stay only six months or so.

GOLDSTINE: Well, he got an offer from the University of Michigan.

STERN: You did too, didn't you?

GOLDSTINE: I was on leave from the University of Michigan at that point. But he got an offer from the University, and he took it, which I think was wise.

STERN: Why do you say that?

GOLDSTINE: Well, it was a permanent position for him.

STERN: And no one had any way of knowing how long the position at the Institute would last?

GOLDSTINE: No.

STERN: It's just--you're working on a computer project; you know it's got to be for several years, I would think.

GOLDSTINE: That's correct.

STERN: But he just felt that that wasn't permanent enough?

GOLDSTINE: Right. Right.

STERN: And then after Burks, who was hired?

GOLDSTINE: Well, I suppose the most important one was Jim Pomerene. And Jim I guess brought Willis Ware.

STERN: They were both from Hazeltine, is that right?

GOLDSTINE: That's correct, yes. I don't know how I got to know about Jim Pomerene, really. The thing you've got to realize is, that before we hired Bigelow, we tried to hire Eckert and not Mauchly, and that started a big hassle. And Eckert procrastinated, and finally turned it down. And then we talked to [Norbert] Wiener, and Wiener recommended, in my remembrance, Julian Bigelow, and Bigelow came then.

STERN: So Bigelow was hired specifically to Chief Engineer, as Eckert would have been had he decided to come.

GOLDSTINE: That's correct.

STERN: And Pomerene and Ware were not--

GOLDSTINE: They were staff. They were engineers, whatever, you know.

STERN: Then [Robert] Shaw and [Jim] Weaver were hired from the Moore School. Is that correct?

GOLDSTINE: I guess that's right. I'd forgotten this, but if it's true, it's true.

STERN: You've forgotten that they were hired?

GOLDSTINE: Yes.

STERN: They weren't there very long then, obviously.

GOLDSTINE: I don't think so.

STERN: Okay--because I did see some correspondence that indicated that [Irvin] Travis was a bit upset at the fact that these two were hired from the Moore School.

GOLDSTINE: Oh, really? I think--well, I'm sure you're right. It's not clear in my mind. But there are lots and lots of these things that are not clear, Nancy. I mean--had you asked me on the witness stand whether Shaw and Davis worked for the Institute, I would have said no. That's how my remembrance is.

STERN: What about [John] Sims?

GOLDSTINE: Oh Sims. I forgot Sims. He was hired very early. He was Eckert's brother-in-law, yes.

STERN: Had he worked on the Moore School Project?

GOLDSTINE: No.

STERN: How did you find out about him?

GOLDSTINE: When Eckert was planning to come, he mentioned him. Eckert was very ambivalent about whether to come or not, and at first he wanted to come, and so he foisted, if you will, on us his brother-in-law. And that's how we got Sims. I forget how long Sims lasted, but maybe a year.

STERN: So that these people I'm mentioning are not people who obviously made very much in the way of contributions to the project.

GOLDSTINE: That's right. But Shaw and Davis were very excellent people. I'm not belittling them in any way. I want to make that very clear.

STERN: Now, I'm just going through a list that I have developed, and if you can give me some information on some names on this list. It has Ralph Slutz as having started in June of '46.

GOLDSTINE: I think so, and he stayed for quite a while.

STERN: Where was he before he come to the Institute?

GOLDSTINE: Well, I'm not positive, but afterward he went with the National Bureau of Standards, and maybe he had come to us from the Bureau. I'll tell you a person, Nancy, you might want to talk to at some point is Arthur Burks.

STERN: I had a four-hour interview with him in June.

GOLDSTINE: Oh, you did. Art may know some of these things which I don't remember. Slutz, as far as I know, is still with the National Bureau of Standards out in Colorado.

STERN: I've tried to reach him but I've gotten no response to my letter. It's the summertime so it might be that he's away. I also have a name here--Jack Rosenberg.

GOLDSTINE: Jack Rosenberg, right. He was very useful.

STERN: And he was there for some period of time.

GOLDSTINE: For a considerable period of time. There was also, talking of these things, for a while there was a man named Dick Snyder, I think, from RCA--who came to us from RCA. He was less useful. Rosenberg--well the most useful of all the people was Pomerene. It took Pomerene to finish the machine. He became Chief Engineer, as you know. So the most useful, in my opinion, was Pomerene, and Bigelow. And then Rosenberg, Ware, Slutz were all very good, hard-working people.

STERN: How about a man named Theodore Hildebrandt?

GOLDSTINE: Ted Hildebrandt. He was useful. He was a useful man.

STERN: He was the son of...

GOLDSTINE: Ted Hildebrandt of T. H. Hildebrandt who was the Chairman of the Math Department at Michigan.
Right.

STERN: And the Ted Hildebrandt that worked for you was a student at Princeton at the time?

GOLDSTINE: I don't know where he was a student.

STERN: Art said he didn't remember either.

GOLDSTINE: No, I think he already had his engineering degree, and I think he'd been a student at Michigan. I mean, if I were betting, I'd bet he had been a student at Michigan, not at Princeton.

STERN: So, the contact with him was with his father, then.

GOLDSTINE: That's correct. Sure.

STERN: Okay. Peter Panagos?

GOLDSTINE: He was a mechanical engineer. He did the mechanical drawings and whatever you call that. And he was very useful.

STERN: Okay. Lieutenant Thompson.

GOLDSTINE: He was with the meteorology group. Philip Duncan Thompson. P. D. Thompson. I think it's Duncan.

STERN: And Morris Rubinoff.

GOLDSTINE: Yes. He was very useful. He went to the Moore School where he is now, as far as I know. And there was Dick Melville. You don't want to forget him. He was just the guy who did almost everything. Purchasing--and you name it.

STERN: I have him down here. Gordon Kent came late on.

GOLDSTINE: Right. The only thing I can remember about him is --he was Rockwell Kent's son. And I have very little remembrance of him. I think he was a minor member of the group.

STERN: He didn't stay very long.

GOLDSTINE: I don't think so. There was also Ames Bliss who was there for a while.

STERN: I have him down as business manager.

GOLDSTINE: That's right.

STERN: And just a few other names. Ephraim Frye?

GOLDSTINE: Oh yes. He came on loan from the Weizmann Institute in Rehovoth in Israel.

STERN: Gerald Estrin--did he--?

GOLDSTINE: Oh, Gerry Estrin was one of the most useful people. Yes. Gerald Estrin was very useful, and so was his wife, Thelma. She was also very good.

STERN: Norman Emslie.

GOLDSTINE: Yes--Norman Emslie was, I think, a mechanical engineer like Panagos.

STERN: Okay--and Hewitt Crane?

GOLDSTINE: Hewitt Crane, yes. He was an electrical engineer. He came rather later in the project and was very useful.

STERN: How, there's a 1945 letter in which von Neumann indicated he wanted to add Stibitz to the project.

GOLDSTINE: Well, nothing came of that, in any case.

STERN: Did he make the effort, or did he decide not to.

GOLDSTINE: I don't know. I don't remember.

STERN: Okay. Now, Bigelow began as chief engineer and remained as chief engineer until '51.

GOLDSTINE: I don't know when he got off as chief engineer, but if you say '51, I'm willing to believe you.

STERN: I think that's when he got his Guggenheim--

GOLDSTINE: Yes, when he got his Guggenheim. That's right.

STERN: Did he really direct the engineers? What sort of relationship was there at first, between him and the engineers--the other engineers?

GOLDSTINE: Well, he was the chief engineer to them. There's no question.

STERN: So that he really gave them an assignment and had full direction.

GOLDSTINE: Yes. That's right.

STERN: What was his relationship to von Neumann? I'm not clear on this. There are no documents in von Neumann's records that have any correspondences between him and Bigelow.

GOLDSTINE: Well, it was very complicated, largely because Bigelow was a perfectionist and had difficulty getting anything into the condition he really liked. He's been working for a Ph.D. for a long time but as far as I know he still hasn't finished it. I'm serious.

STERN: I didn't realize that. I thought he did have a Ph.D.

GOLDSTINE: No.

STERN: But it seemed from the correspondences--there's a lot of correspondences between you and von Neumann in which you relayed, when he was away, some of the things that were happening. And my sense of the situation was that Bigelow constantly wanted to perfect something.

GOLDSTINE: He constantly wanted to avoid finishing it! He never wanted to get done with anything, and he would constantly drive me up the wall. That's why I finally got von Neumann to agree to this mechanism of giving him a Guggenheim; urging him to take a Guggenheim so that we would then displace him and put in somebody who would actually finish the thing.

STERN: But that took five years.

GOLDSTINE: That's right. It did.

STERN: It must have been a little disconcerting for the rest of the engineers to be in a situation like that.

GOLDSTINE: Well, I suppose so, but I can't put myself into their shoes, because I wasn't in their shoes. It was disconcerting to me. I felt that we were frittering away time like crazy, but I couldn't help it. Bigelow had a lot of pluses. He had lots and lots of very fine qualities. He really understood what he was supposed to be doing. But he just couldn't get finished with anything. I could drive you over and you could see a house that he started in the 1950s which is still standing, incomplete. I'm serious. It stands on a piece of property in Princeton. The land must be worth \$100,000 today. And it still stands there, idle.

STERN: Isn't it strange von Neumann kept him on the project for so long?

GOLDSTINE: Well, I wouldn't have, anyway. I tried and tried to get Johnny to get rid of him. But--Johnny wouldn't hurt his feelings really. And finally we got Pomerene as chief engineer and Pomerene did finish it.

STERN: Essentially the reason that he wouldn't relieve him of the job was because he didn't want to upset him?

GOLDSTINE: The devil knows why he didn't. How can you ever know why somebody doesn't want to do

something, Nancy?

STERN: I just assumed you were so close with him that you would have some insight.

GOLDSTINE: Well, I have insights, but when you get fundamentally down to it, who knows really what his belief was? You know? One gives reasons why he doesn't want to do something, but they may not be in fact the real reasons. This is like Psycho-History. I just don't believe you ever can know for sure.

STERN: You can hypothesize a heck of a lot.

GOLDSTINE: Oh, you can hypothesize all kinds of things. I think von Neumann was worried that he didn't have a replacement. I can tell you various things. I don't think he felt that Pomerene was a mature an engineer as Bigelow was, which may well be true. But Pomerene had a much greater sense of responsibility, and it was Pomerene who carried off the Williams tube project and pushed that through to completion, and that made all the difference. But really, it's very hard to know what was worrying von Neumann about this thing.

STERN: Now, I'm trying to sort out the responsibilities of the various engineers on the project--various people. You were Associate Director, is that the correct title?

GOLDSTINE: Right.

STERN: So that when von Neumann was away you were generally in charge of coordinating.

GOLDSTINE: That's right.

STERN: Now, Willis Ware initially worked on the pulse source, is that correct?

GOLDSTINE: I guess so. If you say so.

STERN: And Burks worked on the multiplier.

GOLDSTINE: Right.

STERN: Now there seemed to be some difficulty with Burks' work on the multiplier; that Bigelow was not happy with it.

GOLDSTINE: Well, Bigelow had his own ideas as to what he wanted to do. I don't know that there were any difficulties with Burks' thing, so much as it was just that I think Bigelow had other ideas. And then Art left and so that evaporated.

STERN: And who worked on the multiplier after Burks?

GOLDSTINE: I don't know. I don't remember.

STERN: Okay. There was also a good deal of correspondence between you and von Neumann regarding Snyder's accumulator. Do you recall--

GOLDSTINE: Just vaguely. Snyder wasn't very happy at the Institute and I wasn't very happy with him, and I don't think he was very happy with us. And he left then and went off to some company that made magnetic cores. And that's all I can remember. But I think the worries about his accumulator were that the thing just wasn't built to the kind of design specs that Bigelow felt were important.

STERN: Because my sense there again--it seemed that you wanted the accumulator completed and Snyder was near completion, but Bigelow wanted an entirely new design that wasn't feasible at that point.

GOLDSTINE: That's right. That's right.

STERN: So it might not have been the best design, but at least it would have been something that was completed at that point.

GOLDSTINE: That's right. But whether it would have been reliable--you know, this is something you can't answer. It just was counter to what Bigelow wanted, and he was the Chief Engineer, so there it was.

STERN: Okay--now, a few other administrative people that I need to clarify. Lewis Strauss was Chairman of the Board of Trustees at the Institute?

GOLDSTINE: Correct.

STERN: Was he, at the same time, working at the Atomic Energy Commission?

GOLDSTINE: He was the Chairman of the Atomic Energy Commission.

STERN: At the same time he was Chairman of the Board of Trustees.

GOLDSTINE: That's correct.

STERN: Now, at some point, the AEC decided to fund the IAS project.

GOLDSTINE: Right. In the first place, it had originally been all Ordnance. And then--why, I don't remember--we went from all Ordnance to a tri-service contract, splitting it between Ordnance, Navy and Air Force. Okay? And then we decided to bring in the Atomic Energy Commission, in large measure because it was important to the AEC to get

into this act. Now...

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GOLDSTINE: The worst thing that happened to us was that we had Strauss as Chairman at the Atomic Energy Commission; we had Oppenheimer as Chairman of the General Advisory Committee of the Atomic Energy Commission; and we had von Neumann as a member of the General Advisory Committee. Therefore we had such a conflict of interest with the Atomic Energy Commission that when the Atomic Energy Commission up decided one day that it was wrong for the Atomic Energy Commission to engage in research on electronic computers, we had nobody we could go to without all this fear of conflict of interest. So, it worked very much to our detriment to have all this influence, because we couldn't exercise it.

STERN: Well, when did this happen that the AEC decided that it would not continue funding?

GOLDSTINE: There was a new man named Tom Johnson who was the chief--more or less, and this isn't a true title, but he was more or less the Chief Engineer at Aberdeen Proving Grounds during the war period. T. H. Johnson. He never really cared anything about the ENIAC project, and when he went to the Atomic Energy Commission as Director of Research, he still didn't care about electronic computers, and so he decided not to keep the project going. And, as I said, there was no way, without a real conflict, for us to go over his head--there was nothing we could do. We just had to take the financial beating and get other funds.

STERN: When did this happen?

GOLDSTINE: I don't remember.

STERN: In the '50s?

GOLDSTINE: Oh sure, in the '50s, but when in the '50s I don't know.

STERN: Ah. Was this before or after von Neumann began as Commissioner for AEC?

GOLDSTINE: Oh, before. Much before.

STERN: And by that point, the AEC had become a major funding agency?

GOLDSTINE: Yes.

STERN: It took over from Army Ordnance--Army Ordnance had been the major funding agency until then?

GOLDSTINE: No. First it was Army Ordnance. Then it became a tri-service project. So then we had roughly, I suppose, equal amounts from Army, Navy, Air Force. And then the AEC became a fourth in it. I don't think they were major. Well--I think--my feeling is, and it's only a feeling, but my feeling is that we got the money for the building from the AEC. So they were major in that sense.

STERN: Now, what was Strauss' relationship to von Neumann during the early period of the computing project?

GOLDSTINE: They were good friends. All the time they were good friends. In fact, it was Strauss who appointed von Neumann to the Atomic Energy Commission. Strauss had tremendous admiration for von Neumann.

STERN: He didn't have a scientific background, did he?

GOLDSTINE: Strauss? He didn't have any technical background. He was a poor boy who started life with a pack on his back, kind of thing, and he became a major figure and married a woman whose family owned one of the big department stores in Baltimore. I think her name was Hutzler. He wound up a very wealthy man. But he had great love for

physics. He was the one who appointed Oppenheimer to the Institute. Oppenheimer was his candidate to be the Director. He was so impressed by Oppenheimer's direction of the Atomic Energy project at Los Alamos.

STERN: There was also a man at the Institute named Herbert Maass.

GOLDSTINE: Yes, Herbert Maass was the treasurer of the Institute.

STERN: And he was also an attorney, correct?

GOLDSTINE: Did I say "treasurer"? Excuse me. He was the lawyer for the Institute. He was on the Board of Trustees, and he did the legal work for the Institute.

STERN: He wrote contracts.

GOLDSTINE: Yes, yes. And he had a sidekick named Sam Leidesdorf; Leidesdorf was the treasurer of the Institute, and also a Trustee. These were all friends--original friends of Mr. Bamberger. Maass was his lawyer; Leidesdorf was probably his pinochle-playing crony and accountant and that's how they were on the board. You see, these were all--let me just stay with it for a second. People like Strauss--and there were a number of people, all of the kind of Jewish merchant princes, if you will-- Strauss was the department store, or his wife's department store; there was a man whose name I can't remember now from Bloomingdale's--Shap--something like that; and there were a number of these types. It was a New York or United States--Eastern United States merchant group.

STERN:refugees

GOLDSTINE: None was, as far as I know. But I don't know--let me--they certainly were not refugees from Hitler. Strauss had been here way before that, obviously. Leidesdorf, Maass--I'm sure were born in America. Or if they came, they came as small children. In any case, it had nothing to do with the Hitler business.

STERN: I'm trying to determine in the end what the Institute's complete project cost. Would you say a million dollars would be an approximate figure?

GOLDSTINE: Well, it's not preposterous, but Nancy, I honestly can't tell you. I think the budget was in the order of \$150,000--\$175,000 a year, and so I don't think a million is an unreasonable number. But that--you've got to include in that the engineering, the mathematics, and the meteorology groups.

STERN: Now, that would include the salaries of the engineering staff.

GOLDSTINE: Yes, and the salaries of the mathematical people, and the meteorological people.

STERN: And von Neumann's salary would also be included in that?

GOLDSTINE: No, von Neumann insisted his salary would not come out of the budget.

STERN: But everyone else's--

GOLDSTINE: Everyone else's.

STERN: When Jules Charney came, his salary--

GOLDSTINE: Charney and all the meteorology people were carried on an ONR Contract.

STERN: And the approximate date for that?

GOLDSTINE: I don't know.

STERN: Okay--it was late '40s or something.

GOLDSTINE: Yes. But these are things that you've honestly got to look up.

STERN: Quite frankly, the records at the Library of Congress are not adequate.

GOLDSTINE: I know they're not. And where you're going to get more accurate ones, I'm not sure, unless by going to ONR or somebody like that. Because, this is the weakness of all these oral things. It's a very long time ago, Nancy, as you know; it's the order of 30-some years, and in 30 years it's nearly impossible. I sort of remember about 100--I would say, if I had to guess, I'd say \$175,000 a year was roughly the ball park that we're talking about, covering everything.

STERN: You were involved in budgeting and things of that nature, as well as coordinating the whole project?

GOLDSTINE: Oh, sure. I mean, nobody else took care of anything like that.

STERN: How much time did von Neumann spend at the Institute during those years?

GOLDSTINE: Roughly half to two-thirds of his time. Very roughly.

STERN: And of that time, how much was devoted to the computing project?

GOLDSTINE: Well, I would say a quarter to half time. We'd spend a couple of hours together each morning. I'd go over and meet him about nine; we'd spend from 9 to 10, or 11, either working on something mathematical or just going over where we stood. And my clearing with him things I was going to do, and being sure that we were in accord. And then during the day he'd come down to see how things were going with his own eyes. If we were

working on something mathematical, it would maybe more or less take up his full time; and if we weren't, it might be only 10 to 15 minutes, half an hour, you know.

STERN: Considering the fact that he was away a significant amount of the time, it must have been difficult to coordinate on various aspects of the projects.

GOLDSTINE: We corresponded a lot on this sort of thing. And we talked by phone, by letter. It was easy to do that. That wasn't hard. Copies of some of those letters exist. And you can sort of see how they went. Usually what would happen would be I would write a draft for good and bad, and then we would either accept it or make changes, and then it would get iterated this way; the paper would go back and forth. He had a habit of telephoning at any hour of the day or night. I mean, even at 2 in the morning he might telephone and say "I see how to do this." And then tell you. So--that wasn't complicated at all, to work at long distance. The main problem with working long distance with von Neumann was that telephone connections weren't that good in those days, and von Neumann spent most of his time saying "Hello!" So that whenever the line was clear, all we were doing was being busy saying "Hello." But at any rate, in spite of these things, we got a lot done by these means.

STERN: In a sense, that's good for the historian. At least you have some written documents that you can go back to.

GOLDSTINE: Yes, there are a few documents, sure.

STERN: So that's a good thing. In terms of the project itself, you would relate to him the progress.

GOLDSTINE: Correct.

STERN: And at that point, would he give you direction, would you discuss it? How did that proceed?

GOLDSTINE: Well, normally that was a very easy thing, because usually I knew what I wanted to do, and I would just discuss with him what I planned, and if he didn't like it, we would modify it to suit what he wanted. But it was very relaxed, and he was really not very interested in how detailed things got done. He wanted it done, and he didn't give a damn in too many ways how it got done. He wanted it done, and he didn't want to have it on his back. If you can understand. So, his control was very very loose, and presumably he was satisfied with the way I was doing it, because we had very little problem to get agreement on things. And most of the time I would keep him informed; I tried very hard always to keep him fully informed or Oppenheimer, if he wasn't around, --get Oppenheimer to sign things, or I'd sign things, or whatever. And it was very easy. That's all I can remember.

STERN: It seemed to me--and it may be just because there were more documents available---that von Neumann was much more interested in the engineering end of the ENIAC and EDVAC projects than he was in the Institute's project.

GOLDSTINE: I think that's only partly true. He of course was always interested in engineering, physics, etc. But I think he saw much less of the ENIAC than he did of the Institute project. I think he got bored with being too close to it. Now he used to love to go over to RCA with Bigelow and me to talk to Rajchman; he would enjoy those conversations. I think he had an intellectual interest in things like this. It wasn't a sustained interest, you know. I mean, if there were a new idea, that thrilled him to understand it, that would be one thing. But the experimental business wasn't really for von Neumann. He understood that things took time to perfect and that there was a lot of horsing around, but that really didn't turn him on. When we were actually running, he would come down for the evening--I might be trying to do something, and he'd spend any number of hours with me as long as I was willing to stay--he was willing to stay--to try to get something mathematical fixed. That was a kind of a deeper interest for him. The other was a kind of nervous interest. He wanted to know all about a new thing. But once he understood the principle of it, the ghastly details like the fact that you'd have to put by-pass condensers on things, and all sorts of dirty engineering things--that didn't really interest him. He recognized that these were essential, but it wasn't his thing. He would not have had the patience to sit there and do it; he would have made a lousy engineer.

Well, you know--he was great for coming into a complex, theoretical situation where he could bring to it a kind of

clarity of mind that almost nobody else possessed and solve the problem. That was what he was great at. But if he couldn't bring that to it, then he was only annoyed with it, you know. So he would walk away. If that helps you to understand him--

STERN: It does. It struck me as odd that, based on reading, for example, the papers that you and he wrote--which are truly classic works in the computing field; you read this now and you can marvel at the insight that you and he had on some of the concepts. It strikes me odd that he would want a computing project.

GOLDSTINE: Well, he loved that, you see. If you ever look at Gauss' collected works, you'll see Gauss also love to calculate. You just find in Gauss' works huge calculations that he undertook. It was a form of recreation. Von Neumann loved to do these things. It was a kind of being in touch with reality in a peculiar way. He would live to play mathematical games, such as the question of whether the numbers on a box car were prime or composite. He did calculations in his head that nobody else could do. He loved to do things like that. It was just part of his make-up. So calculation was not something abhorrent. Again, if you look in Gauss' collected works, you'll find all kinds of tabular things that he did. In fact, it was probably relaxing for each one of them to turn to calculation just for the fun of it.

STERN: That's a very interesting comparison.

GOLDSTINE: Yes. I think they were very alike. I think different people's minds are differently constituted. I never particularly noticed any geometrical interests on von Neumann's part. He once told me he knew nothing about topology. Of course these have got to be taken as relative things. When he said he didn't know anything about topology, that probably meant he knew more than most people. But I think he love to calculate. If you look at his book on quantum mechanics you'll find a number of things that you might conceivably do by other methods, he did do them by not numerical calculation, but by algebraical calculation. He was masterful at it. He could take the most elaborate formulas and manipulate them down until they were a couple of terms. This he loved. This was part of his virtuosity. You know, there was just nothing he liked so much as to do that. Numerical calculations were a first

cousin to that.

So at any rate, I think that's one reason why he got into computing. Well you know, of course, that during the war he was involved with the implosion bomb, and it was the calculations for the implosion device that were what originally got him deeply into numerical calculations. You have to find out how to shape the lenses, because if you don't get them shaped just right--a hole may appear out of which the contents would just squirt. Were he alive today, the plasma physics lab here in Princeton, you know, would have interested him; the work on the Tokamak here at Princeton--would have been just what he would have loved; because there one tries to contain a mass of material at enormous temperatures, like the temperature of the sun for a reasonable length of time; one tries to contain the material in a magnetic bottle. This is an enormous design problem, and that he would have just eaten up. But at any rate, that was what he liked.

So--for him to go into numerical things was absolutely as natural as breathing.

STERN: Did he take part in, for example, the hiring of staff at the Institute?

GOLDSTINE: No. That was all mine.

STERN: He was also very much interested in weather forecasting.

GOLDSTINE: Very much. He wanted, you see, an example to show the world the importance of numerical calculation; to show that it was something which was fundamental to our whole society and not just something for a few scientists in a little laboratory. And he knew somehow about an Englishman, Richardson's work. Do you know about him?

STERN: No.

GOLDSTINE: Oh. Well, there was a man whose initials I don't remember--he was a Quaker who worked as a conscientious objector, if I understand correctly, in the First War as an ambulance driver, who was deeply interested in meteorology. He laid out numerical calculation for a weather forecast. But he didn't know anything about the Courant condition, so what he did was numerically unstable in the first place, and he lost the manuscript in the second place--everything went wrong. But I think he lived to see this work of von Neumann's. And you'll find a discussion of Richardson'ssssss work in a book by Phil Thompson. Let me see if I can get the reference for you--Philip Duncan Thompson, yes--and Thompson wrote a book: *Numerical Weather Analysis and Prediction*--Macmillan, New York, 1961, and Richardson's name is Lewis --Lewis F. Richardson, *Prediction by Numerical Process*, Cambridge University Press, Cambridge, 1922. At any rate, that's what got von Neumann into this thing. This was a very interesting project; there were some really very interesting people here in this project starting with Rossby--Karl Rossby. I think he was the most famous of the people who came but he didn't stay at the Institute; he didn't want to stay. But finally, after a certain amount of exploration, von Neumann got Jules Charney here, and Charney made a great success of the project.

STERN: What was Charney's primary responsibility?

GOLDSTINE: He ran that group. They formulated the differential equations, and then they solved them numerically, and kept modifying them until they succeeded; that was one of the things the computer was for.

STERN: And the computer was actually used for these calculations?

GOLDSTINE: Right.

STERN: Charney came, I think, in the late '40s, and the computer was not yet completed.

GOLDSTINE: That's right, but it took some years to get things formulated, too. I think also that they did do a calculation on the ENIAC down in Aberdeen in the interim period. Yes, in fact, I'm sure they did.

STERN: Are there any publications that indicate that this work was using computer for weather forecasting? Would you know? By Charney?

GOLDSTINE: Oh, Charney. Yes, he wrote a number of papers on this work. Right. The reason I suggest the Thompson book is just so that you'll get some bibliography.

STERN: Okay, fine. Very good. So Thompson and Charney and--there were numerous other people that worked on this project.

GOLDSTINE: Yes, well I think Thompson was not a very important person, in effect, between ourselves. The most important was Charney, and the second most important was a man named Norman Phillips, and you might want to get in touch with Norm. The reason I suggest Norman rather than Charney is --I understand Charney has lung cancer, so I don't know what shape he's in. But Norm Phillips is, I believe, still at MIT; also, Joe Smagorinsky. Smagorinsky is over at Forrester where he runs the Geophysical Research Laboratory. That's a branch of the U.S. Weather Bureau. If you call him there he'll give you any references or background information you want.

STERN: He'd be a good person to interview, too?

GOLDSTINE: Oh, sure, excellent. There was also a man named Bruce Gilchrist who was on the project. He's now the head of computing at Columbia.

STERN: And he worked in meteorology?

GOLDSTINE: That's right. He did his Ph.D. at London University in meteorology.

STERN: Anyone else that you can think of that worked on this project that made major contributions?

GOLDSTINE: There were quite a few. One of them in Chicago is George Cressman at the University of Chicago, professor of meteorology there. There was also a number of others including Arthur Bedient, Jack Blackburn, Fred Shuman, Al Stickles, Thompson and George Veronis, as well as a number of others who are mentioned in my I really suggest that you talk to Smagorinsky and get it straight. There were also any number of Scandinavians. There was a man named Bert Bolin who is now the head of the weather bureau in Sweden. And Ragnar Fjortoft from Oslo. He's the head of the weather work there. And there were others. There was a Frenchman named Paul Queney.

STERN: Yes, I was going to ask you about him. There were some--and again, the notes are very sparse--Aydelotte refers to a situation where--Queney--he said he acted badly.

GOLDSTINE: Queney was a sort of Charles DeGaulle type. He was very inflexible about his privileges and perquisites. This attitude offended Aydelotte. Aydelotte was a director built on the lines of the Duke of Devonshire. No, I'm serious. He was the kind of English county type, you know, and there were things that you did and things that you didn't do. Queney offended some one of these notions of his. He felt it wasn't "cricket" to make a stink about one's prerequisites. And that just didn't accord with Aydelotte's notions of how you did it.

STERN: These people had offices in the--

GOLDSTINE: Computer building where we had not only our engineering group but also the groups doing numerical mathematics as well as the group doing numerical meteorology.

STERN: Is that building still standing--the computer building?

GOLDSTINE: Yes.

STERN: It does not have a computer

GOLDSTINE: No, it doesn't, but it's still called the ECP [Electronic Computing Project] Building or something like that.

STERN: Now, the group that would be numerical mathematics consisted of--

GOLDSTINE: There was me, and then we always would have at least one other person--Frank Murray for a year; E.J. MacShane for a year. For several years we had Solomon Bochner. It was a rotating kind of thing but we always had some one or several people on it who--

TAPE 2/SIDE 1

STERN: ...Bochner was one of the people who worked in the numerical mathematics group? Was that the group that Tukey was involved with as well?

GOLDSTINE: Yes, these others--they all did a lot. Tukey really was involved only at the beginning for about a year or two. And he did little. It just didn't interest Tukey, or he had too much on his plate already.

STERN: Tukey seems to be involved in statistics.

GOLDSTINE: He is a famous statistician, yes.

STERN: But his involvement with the project didn't relate to statistics at all.

GOLDSTINE: Not at all. Not at all.

STERN: Okay now--I've mixed up my notes here a little bit. I'm interested in the I/O units that were used for the ECP

Project. Can you give me some information on that?

GOLDSTINE: Yes. When we first started, we had an arrangement with the National Bureau of Standards for them to make adaptations on teletype equipment for us to use as input and output. When we attached those onto the machine it took, my remembrance is, 8 minutes to read about 1000 words in, and 16 minutes to get a complete dump of the memory. This was so long that we in fact could never debug transient difficulties. So it seemed to me then the only thing to do was change, and we went over then to an IBM punched card machine for I/O.

STERN: And that's ultimately what you had--punched cards.

GOLDSTINE: That's correct.

STERN: These modifications to the teletype equipment was under the supervision of a man named Huntoon?

GOLDSTINE: Yes--I'd forgotten all about his name, but if that's the name, that sounds reasonable.

STERN: Okay, it's the first I ever heard of him. And I didn't know if he was related to any other projects--

GOLDSTINE: I just don't remember anything about him except the name sounds familiar.

STERN: Okay. Now, there's also some correspondences relating to the possibility of using photographic techniques for memory.

GOLDSTINE: There were lots of things that were discussed. That's one of them, yes. Nothing came of that.

STERN: Can you give me some information on the Selectron?

GOLDSTINE: Yes, the selectron tube? Right. The thing that RCA was supposed to contribute to the project was memory tube. Rajchman--Jan Rajchman--you've probably got his name--worked under Vladimir Zworykin at RCA Research in Princeton. Zworykin and he decided at the beginning that it would be best to store information in the phosphor of the cathode ray tube. (You store a charge.) They decided that it would not be wise to try to switch the beam to a given point by analog circuits which is the way a television set does. But that instead, you should put into the cathode ray tube a grid which had 4,096 windows, all of which were available. They ultimately cut it down to 512 or whatever the size was that they finally arrived at--windows. One and only one window could be opened at any one time. The way the beam worked was that it just sprayed electrons out, more or less hitting the whole wall, the whole wall being filled with windows, and trying to go through whatever window would let the current go through. The beam therefore could only go through whichever window was open. Now, that was the concept.

The windows were opened by suitable electrical impulses on each of two wires, and were closed by the same mechanism. It was a very neat idea, but it involved--let me just think now how many--well, it was 64--you had to have 64 by 64--if you had 4,096. And 64 is 2^8 --so you had to have 8 wires coming in for one direction and 8 wires for the other.

STERN: It's 2^6 , I think.

GOLDSTINE: I'm sorry-- 2^6 . You had to have 12 wires coming in to do the switching. As I understand it the problem of doing this with the technology available at the time to Rajchman was just too big a deal. In those days they just couldn't bring that many wires--24 wires out of a bottle, without having contamination and all sorts of problems. That wrecked them. Concurrently, Freddie Williams in England was busy doing much the same thing by analog switching, and that was the difference.

STERN: You would think that RCA would have opted for the analog switching.

GOLDSTINE: Well, they made the initial judgment that it was not safe, and that it should be switched digitally and

that's what killed it.

STERN: How many people worked on this project?

GOLDSTINE: With Rajchman? Rajchman and Snyder were the major figures. How many subordinates they had, I've no idea. But there must have been a crew of technicians.

STERN: There's, again, a letter in the file in which von Neumann said that he believed if George Brown--is that his name?--had stayed on the project it might have been a success.

GOLDSTINE: Well, I think von Neumann was wrong on that. George Brown is a mathematician, or a statistician, who left the project and went from Princeton to UCLA. It was Brown who worked out the switching arrangement for the tubes and he's very bright. But I don't think he knew a single thing about the engineering of the tubes, and I don't think that he would have succeeded in doing anything. In my opinion it was Rajchman who was the brains of the engineering.

STERN: Now, how long did it take before they realized that the concept was not viable?

GOLDSTINE: Well, it isn't totally fair to say that it didn't work, because the Rand Corporation built a computer and used those tubes in it.

STERN: At what point was the decision made to opt for the Williams tube?

GOLDSTINE: More or less as soon as we heard about the Williams tube.

STERN: Can you recall when that was--approximately?

GOLDSTINE: I would make a rough guess it was 1950-ish. Oh, incidentally, I suggest for things about this you might want to call Jim Pomerene. He's at IBM Research in Yorktown.

STERN: Which reminds me--Jule Charney--is he still at Princeton?

GOLDSTINE: No, Charney and Phillips both went to MIT. Phillips still works at MIT, but I think he's now a part of the Weather Bureau. Smagorinsky can tell you.

STERN: Okay. There's reference to two types of Selectrons. A sandwich type and a quadrant type.

GOLDSTINE: Well, I don't remember the details anymore about that, but those were different geometries to try to make the thing work. I don't remember more. The one I remember best, I think, is the sandwich. But this had to do with how the grid was put in there--that had the windows in it, and it's not very important. But Rajchman lives in Princeton--he's retired. You'll find him in the phone book, and you could interview him.

STERN: We have an interview scheduled for Wednesday.

GOLDSTINE: Okay, fine.

STERN: I'll speak with him about that. What was the relationship between the Computron--RCA's Computron--and the Selectron?

GOLDSTINE: The Computron was something that Rajchman and Snyder had worked on. Rajchman and his colleagues started very early working on electronic counting and comparable things. They had the idea of building special-purpose vacuum tubes, which was their big thing, to do fairly complicated things, such as adding. I think the Computron stepped up from 1 to 10, or 0 to 9, or however. So it was kind of an adding tube. Instead of doing this by a whole series of flip-flops, they chose to put it all in one envelope. That was the big thing that RCA could do. None

of this was a success. These were all attempts to carry electronic computing in the direction of special-purpose devices, which could be achieved by a master of the vacuum tube technology, and they all failed, because the thrust went in the other direction.

STERN: So Synder was in RCA working on a computer project for some time?

GOLDSTINE: Right.

STERN: What made him decide to go to the Institute? It seems like an unlikely choice.

GOLDSTINE: I don't know that he was very happy, I don't think he was a very happy person. He was obviously eclipsed by Rajchman at RCA. Rajchman was clearly better, senior to him, smarter than him, etc. And I think he was unhappy at RCA. Then I think he was unhappy at the Institute. I don't know what happened to him then. At any rate, that was his career.

STERN: Did the Institute have a policy about patents?

GOLDSTINE: Yes, we certainly did. You'll find correspondences to that effect. We had a patent lawyer named Townshend in Washington.

STERN: That was the same lawyer that was involved in the EDVAC patent?

GOLDSTINE: That's right. The first thing I did was to get a patent policy. Having gone through the ENIAC-EDVAC problem, I certainly wasn't going to have that one again. So we had a patent agreement drawn up by this man Townshend whose name was spelled--not Townsend--but "shend." But pronounced "Townsend." At any rate, we had a patent policy.

STERN: And what did that patent policy state?

GOLDSTINE: It gave everything to the Institute.

STERN: Did it preclude engineers from doing consulting work on the outside?

GOLDSTINE: It didn't address anything that they did on the outside. It only had to do with the work on the project.

STERN: I assume there were no objections to that on the part of the engineers.

GOLDSTINE: No. That may have been one of the reasons why Eckert didn't want to work at the Institute. But I made it a condition of employment, that's all.

STERN: Did you have much involvement with the computing projects that were offshoots of the Institute's like at the Rand Corporation, for example?

GOLDSTINE: Not really--no. We had Abe Taub at the Institute for a year or two, and Abe is the one who started the project at the University of Illinois, which led to the ORDVAC. And the ILLIAC.

STERN: What did Taub do at the Institute?

GOLDSTINE: Well, he was on the numerical project. He was one of the other people that I should have named for you.

STERN: The Rand Corporation--how did they learn of the IAS project?

GOLDSTINE: Willis Ware left the Institute and went to Rand and got them to make a copy. And I was a consultant

at Argonne, Los Alamos and Oak Ridge, I believe.

STERN: What about Los Alamos?

GOLDSTINE: Nick Metropolis was in and out of the Institute a great deal, and he made a copy of the Institute machine at Los Alamos. I was a consultant to them, but it was Metropolis's project.

STERN: But you were directly involved in the copy that was made at Argonne?

GOLDSTINE: And at Oak Ridge. Right, in some way or another.

STERN: And how about von Neumann--was he involved in these copies?

GOLDSTINE: As much as I was--I mean, they didn't need much help. They knew very well what to do.

STERN: There's reference to the fact that you had frequent visitors.

GOLDSTINE: Yes, we did. That was one of the great things about the project. Many people came; you see, the Institute is built that way. It's very easy for people to come for periods of time, ranging from days up to a year or two. And so these people would come and learn what we were doing and then they could go and do their own thing. In the case of Oak Ridge their first machine was made for them by Argonne, who hired Art Burks among others as a consultant, and so they had no problem anyway. It was all very simple.

STERN: It seems to me the Institute, more than any other project that I know about in the '40s, really did foster the transfer of technology by encouraging people to come.

GOLDSTINE: That's right. Right. We even had people come from Australia, and they made a copy of our machine

in Sydney called SILLIAC, I think.

STERN: There was a Czechoslovakian machine--.....BESK

GOLDSTINE: The BESK was a Swedish machine and BESM a Russian one. The Swedish government sent various bright young men here for a year or two to learn how to build a machine. How the Russians made their copy, I don't know. But they obviously did; it was a copy.

STERN: Was there any relationship between the Institute's computer and the IBM 701?

GOLDSTINE: Yes, it's the prototype for the 701.

STERN: It is the prototype for the 701? Were you involved in that?

GOLDSTINE: No. Von Neumann was a consultant to IBM. He went quite often to Poughkeepsie and to New York.

STERN: Did IBM provide any grant money to the Institute?

GOLDSTINE: Yes. In fact, they made a number of gifts over the years. Every year, they made a gift to the Institute, including a professorship in von Neumann's name.

STERN: Were there any stipulations as to how the money should be expended or was it just a general grant?

GOLDSTINE: It was a general grant.

STERN: So the grant money was not used just on computer projects.

GOLDSTINE: No, I don't think so. I don't think there was a need for it. But I wouldn't take any Bible oath on it.

STERN: From your recollection, was there any funding problem at all? Or was it just a question of organizing.....

GOLDSTINE: That's right. That was not a major problem, getting money.

STERN: That, too, was unique for that period, to have a project which was not subjected to monetary problems.

GOLDSTINE: We were getting very small amounts of money; by contrast to, say, Whirlwind, we had almost zero. I'm just reviewing for the *Annals* a book on Whirlwind by Redmond and Smith, and the sums involved there were very large. But we were very modest. If you take \$175,000 a year and split it between three or four agencies, it's not much.

STERN: There's also a good deal of difficulty on the Whirlwind project in terms of the dissatisfaction of the funding agencies.

GOLDSTINE: On the Whirlwind thing, sure. Well, one reason, I feel, that there was such a problem was because a) it was very expensive, and b) it was more or less a copy of the Institute machine.

STERN: Was it really?

GOLDSTINE: Yes. Forrester got his initial ideas from talking to us and he got copies of everything we wrote. And those were the ideas--of that period.

STERN: There was one document that I read--it said something about dissatisfaction on the part of the Army with the progress report that the Institute had provided. Do you recall anything relating to that?

GOLDSTINE: Well, we had a constant set of problems with Bigelow getting engineering reports written; at various

times Pomerene and I wrote the reports, just because we couldn't get Bigelow to write them on time, and things were delayed and delayed and delayed.

STERN: So that there was never any dissatisfaction with the progress?

GOLDSTINE: No, no, no.

STERN: Did John Todd spend any time at the Institute?

GOLDSTINE: Yes, he and his wife Olga, both. And that's another pair who participated in the numerical analysis project.

STERN: Okay--what about Harry Huskey?

GOLDSTINE: No. I don't remember Huskey coming. He was associated with me on the ENIAC.

STERN: There's been a lot written, but nothing very clear on the relationship between Turing and von Neumann. Could you shed some light on this; do you think Turing had an influence?

GOLDSTINE: Yes, in a certain way. You see, Turing was a graduate student of Church's at Princeton University in the period just before the war started. In fact, Turing wrote his thesis at Princeton. In those days, the Institute's School of Mathematics was housed in Princeton University's Fine Hall. So Turing's office was right near von Neumann's, and von Neumann was very interested in that kind of thing. You know, formal logics was one of von Neumann's deep and early interests. He knew all about Turing's work, and he offered Turing the job as his assistant for the academic year '38-'39; but instead Turing went back home to Bletchley to work for the Foreign Office. I'm sure that von Neumann understood the significance of Turing's work when the time came. The whole relation of the serial computer, tape and all that sort of thing, I think was very clear--that was Turing. And von Neumann, I'm sure knew

all about this.

STERN: How about the stored program concept?

GOLDSTINE: Well, there is a stored program in the Turing thing. So, you know, I think--that's how these things came out. In my mind von Neumann invented it, if you will--but how much of it he owed to Turing I can't tell because it had to be that the Turing idea had a great, great influence on him. And that, you see, is one of the reasons that I've tried to tell you, Nancy, in the past--why I never believed the arguments by Eckert and Mauchly about who invented the stored program, because the only person in the group who knew of Turing's work was von Neumann; and the only person besides von Neumann, as far as I know, who had ever read Turing's stuff was me and possibly Burks who is a logician. And so I always felt that, just based on probability it seemed to me that if you had to just guess where there was the most knowledge of those ideas, they had to lie on the Burks-von Neumann-Goldstine end, and not on the Eckert and Mauchly. And that's not to say anything to disparage them. But I just mean that it always struck me as improbable that they should argue that they had these ideas first.

Now, there is a computer scientist in New Castle-Upon-Tyne--Brian Randell, He's always trying to show that von Neumann had one or more meetings with Turing in London during the war. I don't know what the fact is about that. How can you know this? But I don't believe it. Turing came one time and spent a few weeks with von Neumann and me shortly after the war. Mostly we talked--we were curiously and independently both working on inverting matrices of high order. Turing's ideas were very complicated. He didn't have the right idea. When he left he still didn't see the right path--the trick. We had lots and lots of conversations; he was a very nice person, very intelligent, but very complicated. If you look at the design of his ACE computer, you'll see that it's an enormously involved machine. He loved complexity. He really did. He was just a highly involuted person. But a very very nice man, a very keen mind.

But I have no reason to suppose that he and von Neumann had conversations in London or Bletchley or Kansas City, for that matter. But, you know, there's *a priori* no reason why they might not. The only thing I can tell you is that von Neumann didn't go to England all that often during the war. It wasn't that easy just to go to and fro during

the war.

STERN: And he never discussed with you the fact that he had had any discussions of this sort?

GOLDSTINE: No, he never did. That, I think, is curious because he would not have had to reveal anything confidential to have said, you know, "I saw Turing and we discussed the following." Because we talked a lot during Turing's work at various times since the whole business of self-reproducing automata was very much on von Neumann's mind for a long time. At no time did he ever make any reference to that kind of thing, which was very unlike him. I feel sure that he would have made some remark, which would not have violated security. He was very good at saying things in such a way that he did not say anything that would violate security, but at the same time, if he felt it was relevant, he would let me know. So I think Randell is just chasing a--

STERN: The evidence seems to suggest that if it were an influence on him he certainly would have told you about it.

GOLDSTINE: Oh yes. Of course. I just think there's no question about that.

STERN: If you had to just summarize von Neumann's major contributions to the computing field, what would you say they were?

GOLDSTINE: Well, I think first of all--his logical design for the EDVAC, because I think that was the first logical design for anything, in the modern business--anything that I know about. So that's one.

Two: I think the stored program. Three: I think that fact that he was world famous and was able to persuade people that the computer was important. Four: This is such a complicated thing, but let me try to say something to you about it. Let me be a little long-winded, if I may.

There are always arguments about, who invented the calculus, or who invented relativity, or who invented the stored

program. Those are the wrong questions. All of these things evolved. You can go quite a way back. If you look at Whiteside you'll find quite a history of men who know how to differentiate and integrate going back to Archimedes. The thing Newton did was way more than that. The thing that Newton did was to create a whole system, the Principia Mathematica--it's a whole natural philosophical system; and that was his great contribution: to devise the tools and to use them to construct the Newtonian mechanics.

I think that was what, for example, contrasted him to Leibniz. I think that's what you can say. Leibniz probably knew the same formulas that Newton knew, more or less, but he didn't do a thing with them. So that was, I think, why, in my opinion, Newton's contribution was way greater.

And the same way with relativity. Various and sundry people obviously had pieces of the thing, but they didn't organize it and put it into a total system, which is what Einstein did.

And in the same way with von Neumann. Various people all contributed to this thing, and contributed heavily to this, that or the other. But the logical design thing for the EDVAC was putting the whole thing together--it really gave a unity to it which I think would have taken years to achieve by any other method. Watching Pres Eckert and Mauchly, I honestly just feel it would have taken years before the computer evolved to the point where it got immediately. That's one thing.

The second thing is that it simply is a fact that none of us was important enough to have persuaded people to accept this kind of thing. In the first place, von Neumann had a real built-in need at Los Alamos. So they were immediately persuaded to do computing. They had an enormous IBM punched card installation out there doing implosion calculations. People like Fermi were immediately impressed with the importance of computing. Fermi had me out to Chicago one time and grilled me for practically a full day without stop to understand what it was all about. He immediately saw the importance of it.

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GOLDSTINE: I just don't believe any of us could have gone and persuaded somebody like Fermi of the importance of numerical calculation the way von Neumann could because von Neumann actually was the only person that I know who could take a physical situation, write down the differential equations and put it into numerical form. He knew all about Courant's work back in the early--whenever it was--early '30s, with Friedrichs and Lewy--so he was the only person who really could do calculations involving partial differential equations; and he was also the only person who could formulate mathematically the incredibly complicated physical problems, which they were meeting. And so that's why people like Fermi were immediately impressed, and that's why the computer spread so quickly and so widely. I think those are the things which von Neumann contributed.

STERN: And I think he would be able to get a group to focus on numerical mathematics, as he did at the Institute, based on his reputation.

GOLDSTINE: Well, sure. It was based on his reputation, his stature. No other institution had a chance.

Whirlwind--if you read that Redmond and Smith book--had the half-time use of Philip Franklin, and two full-time assistants whose names they don't even mention. So it was a little mathematical project. Whereas the group at the Institute had all first class people--every one of them was anxious to come. It was the place where these things were going on Douglas Hartree used to visit the place periodically. You mentioned the Todds-- .. You can keep on naming the people--there was a very long list of famous people who were there. Very important things got done, as a result. All of this served to show the world, including the numerical meteorology people, that the computer was a very very important thing.

Again, I say without disparaging them, that Eckert and Mauchly's progress would have been a very slow affair in which ultimately they might have been successful but it would have taken a long time; I believe also that the organization of machines would have been successful, but it would have taken a long time; I believe also that the organization of machines would have been very unsatisfactory for a long time, compared to what they are now. Those are my prejudices on that topic.

STERN: What would you say were your most important contributions to the computer field?

GOLDSTINE: Well, in the case of the ENIAC-EDVAC as well as the IAS projects, I was in charge of the mathematical and logical sides. In the case of the Institute project, I invented or perfected flow diagrams. Von Neumann had a very vague geometrical way of making a little sketch, and I felt that that was an important thing, so he and I jointly did that. I believe also that I did the logical design paper, which means I feel more or less responsible for the logical design of the IAS computer in company with Burks and von Neumann. That's really based on the EDVAC, but it was the translation of that over to the parallel computer and involved many new ideas.

I had a big hand with him in the inverting of matrices and the diagonalizing of matrices. I don't know what else. I suppose that's it.

STERN: How about directing this computer project?

GOLDSTINE: Well, I directed it, in its entirety. No doubt about that. I think those were my contributions.

STERN: Did you attend many computing conferences between '46 and '52; did you provide papers to disseminate information about computers?

GOLDSTINE: Yes. When you say "many--I don't know--that's a word I can't answer. But certainly--I think more stuff was disseminated by people flowing through Princeton than anywhere else.

STERN: Information flowed outside or the other way around?

GOLDSTINE: That's right. People came through--you know--in fact, anybody who came anywhere in the East, would stop at the Institute and we would talk. One or the other of us, or both of us, would talk to the people.

STERN: Did you or anyone else at the Institute learn about other ideas?

GOLDSTINE: From the outside? No, I'd say that it all went out.

STERN: What about organizations--ACM was formed during this period.

GOLDSTINE: Neither of us ever belonged.

STERN: How come?

GOLDSTINE: Just antagonism due to Mauchly and Eckert.

STERN: I don't think Eckert was ever in it.

GOLDSTINE: Well, Mauchly was the founder or one of the founders.

STERN: Because Curtiss I knew was also a founder.

GOLDSTINE: John Curtiss? Could be.

STERN: He was involved in it. But was it seen then as a kind of commercial effort, as opposed to--

GOLDSTINE: No. I don't know why, but we just never did join. Just never did.

STERN: Did you hire programmers?

GOLDSTINE: Yes. We had a couple; we had wives who were programmers. A couple of women. For instance, my wife worked for von Neumann as a programmer, on a Los Alamos contract. And then we had two other, Hedi Selberg and Margaret Lambe who were wives of professors. Hedi's husband was a professor at the Institute, and Margaret's husband is now at Stony Brook.

STERN: Von Neumann's wife was also involved?

GOLDSTINE: Oh, von Neumann's wife was also involved, right.

STERN: Now, your wife and von Neumann's wife worked on the Los Alamos project?

GOLDSTINE: Correct.

STERN: Also the Institute's computer?

GOLDSTINE: Primarily the ENIAC in both cases, but not exclusively.

STERN: Okay. There's reference to a person named Sonya Bargmann.

GOLDSTINE: Sonya. I forgot about Sonya Bargmann. She was another one. She's the wife of a man named Valentine, Valya. He's a retired professor of Physics at Princeton University.

STERN: There was a problem with her security clearance.

GOLDSTINE: Is that so? I don't remember.

STERN: In one of the papers it is mentioned. I think you got it resolved.

GOLDSTINE: I don't remember.

STERN: Apparently it wasn't a problem to you recollection. It was simply resolved?

GOLDSTINE: I suppose so. Yes. She and her husband are emigres from Europe. Her family I think originally came from Russia, so I would imagine, probably that was the problem.

STERN: Especially at that time?

GOLDSTINE: Right.

STERN: Who is a person called Akrevoe Kondoprea?

GOLDSTINE: Akrevoe Kondoprea. Akrevoe was the secretary I had at the Moore School, and I brought her with me to the Institute.

STERN: Okay. There was also a person named Luella Trinterud.

GOLDSTINE: Miss Trinterud was an accountant for the Institute. She was the woman who kept the books on our projects, and she was one of these people you had to live with. You know, you had to satisfy her that things were in order. She was always a pain in the neck. But an accountant.

STERN: Were there any computing journals, or any journals at all that published material on computers?

GOLDSTINE: There were. There was one. [R.C.] Archibald had the Mathematical Tables and Other Aid to Computation (MTAC) I guess it was called. I remember, my wife and I wrote a paper in there on the ENIAC.

STERN: So that was the vehicle for publishing work. Did you publish anything on the Institute's machine? I think that series, MTAC, went out in the late '40s. And after that was there any other vehicle?

GOLDSTINE: Well, von Neumann and I put out a series of paper, you know. We just mailed them around to people on mailing lists that ONR, the Army and the Navy, and the Air Force all had. That was the prime vehicle for publication. And then, well, other things were published in journals like the American Mathematical Society's *Bulletin* and *Proceedings*. One paper we published in Courant's journal: "The Communication of Pure and Applied Mathematics."

STERN: Anything in SIAM?

GOLDSTINE: No. Let me just say one thing to you which may help you with this, Nancy. You may think it's peculiar, but we were interested in trying to get the computer into the mainstream of mathematics; whereas ACM, SIAM, these were all things on the periphery. And that wasn't our interest. I guess this is a better way to answer your previous question, I think. We wanted the computer to be accepted, and we wanted it accepted in the mathematical world so; for example, President [Gaylord] Harnell of the University of Pennsylvania had me down one time to talk with him and with Detlev Bronk, the Chairman of the Board of Trustees of the University, about where to put the computer at Penn, whether to put it under the Moore School or under the Math Department. I tried to urge them to put it into the Math Department. Our thrust in those days was to get the thing accepted, and not to start a poor relation sort of thing. Neither one of us at that time envisioned the notion of a computer science discipline growing up. In those days you either put it into the electrical engineering end of the business or into the mathematical end of it. And we wanted to push for a mathematical end. So that's why things went to the journals that they did. We felt that computers were very important instruments for applied mathematics, and that they should be accepted by mathematicians. We even wrote a paper--well, in fact, I did the work--but von Neumann's name and mine are on it together--a paper nominally with Emil Artin on the so-called Kummer numbers. He was Professor of Mathematics here at the university. He was interested in Algebra and in Number Theory. Kummer had made a

conjecture about how certain numbers behaved as functions of the primes. In a simpler case, Gauss showed a certain result was true. Kummer saw that in his case the situation was different and conjectured the behavior of those numbers. I did a calculation for Artin on this conjecture. It turned out to look very unlikely that the conjecture was true. So Artin didn't work on it. We published our results in one of the standard math journals. I don't remember which one. But the idea was to try to get the mathematicians to use these devices, and not to try to go off in another direction. Anyway, I think that's the reason why things like ACM, SIAM never really appealed to us.

STERN: Were those more commercial, do you think? Would you say--is that fair?

GOLDSTINE: No. I don't think they were--I don't mean it as commercial at all. That had nothing to do with it. The thing that we wanted, though, was to get people who did mathematics to think about using tools like this; or people who did physics to use these tools. And not to get engineers interested particularly because they were going to go along anyway.

STERN: I'm a little confused because SIAM is the Society for Industrial and Applied Mathematics.

GOLDSTINE: I know, but it wasn't the journal read much in those days. You would find them looking in the Mathematical Society journals. And those were the people we wanted to get to. It wasn't snobbishness--and it had nothing to do with industry. It had nothing to do with commercial things, or anything.

STERN: Who published in SIAM?

GOLDSTINE: Well, I've forgotten now. I think Don Thomasen was a leading light behind getting it started, and he worked for IBM. It was a great idea, and Sam Wilkes at Princeton University backed it and was one of the key people behind it. But we really tried to get our work into the standard journals. And there's the way we did it.

STERN: It seems to me you had a double problem because applied mathematics itself wasn't recognized by many

mathematicians.

GOLDSTINE: That's right.

STERN: So you had that problem to face--and then--

GOLDSTINE: That's right. And it didn't succeed. It succeeds now, but didn't then.

STERN: Why do you say it didn't succeed?

GOLDSTINE: Well, because most math departments cut computer science off. Separate department did get formed. Also at Chicago there is a big Math Department which contains both the Pure and Applied Math equally; this is also true of the Courant Institute and of other places such as Wisconsin. At any rate, it was a contentious period. There was a lot of fighting about the role of the computer.

STERN: Even Solomon Bochner who worked here at the Institute on numerical mathematics, in his obituary commented that he didn't believe that computing was a very important aspect of von Neumann's work.

GOLDSTINE: Right. That's Bochner's opinion. Up unto a year or two ago he was head of the Pure Math Department at Rice.

STERN: You would disagree with that?

GOLDSTINE: I would disagree with that. I think perhaps it was von Neumann's greatest accomplishment. That's what I would think.

STERN: Apparently Bochner's a person who came to the Institute and still believed that computing wasn't important.

GOLDSTINE: Sure. Admittedly, Bochner is a great mathematical analyst; he just never was persuaded of the importance of the computer. But that's his business, you know. Halmos in his obituary of von Neumann wasn't even doing mathematics at that period. And that's his appraisal.

STERN: Would von Neumann consider his work at the Institute to be pure mathematics?

GOLDSTINE: No. He was mainly interested in what you would call applied mathematics--what he would call mathematics.

STERN: When was the Institute's computer actually completed--mainly complete?

GOLDSTINE: The date's in my book. It was about '52.

STERN: What happened after that?

GOLDSTINE: We ran the computer there for a number of years. Von Neumann then got an appointment to the Atomic Energy Commission. His health was beginning to fail. I stayed on for a year or two, and went to IBM since I felt that the day of the academic computer was passing, and that the industrial organizations were going to be the places where the advances were made. And that was the way it worked, too.

STERN: Now, in '52 after the computer was completed, who worked on applications?

GOLDSTINE: The same people really. The same people.

STERN: There wasn't a question of funds drying up?

GOLDSTINE: That was no problem at all. We had all the money we needed. There was absolutely never a real problem on that score.

STERN: Was the Institute uncomfortable with the concept of an applied project after it was completed?

GOLDSTINE: That's right. Oppenheimer was not. But the pure mathematicians certainly were. But Oppenheimer was not.

STERN: And Oppenheimer wasn't concerned with the fact that it was originally proposed that it would take three years to complete the machine.

GOLDSTINE: No, he didn't care about any of that. No. None of that bothered him.

STERN: Are there any questions that I've left out? Anything else we should discuss.

GOLDSTINE: No, I don't think so, Nancy. I really don't. I think you've covered things pretty much.

STERN: In your papers at Hampshire College, do they have any records on this period?

GOLDSTINE: Not to much. The Institute period is less well documented than the ENIAC period, curiously.

STERN: It is curious.

GOLDSTINE: The Institute got rid of its files, you see. During the ENIAC-- EDVAC period, all the files were in my hands. And at the Institute, the files belonged to the Institute. The Army didn't want the files I had. Whereas, in the case of the Institute, the files went to the Institute which then proceeded to destroy them as near as I can figure out.

STERN: So that most of the papers in Hampshire would not be related to this period, but there are some?

GOLDSTINE: Probably some, yes.

STERN: The DAS internal reports were produced twice a year?

GOLDSTINE: If you say so. I don't remember.

STERN: Do you have copies of them? Do you know if anybody has copies of them?

GOLDSTINE: Yes, I think I have copies at the Institute. If need be. I have a certain amount of stuff in the basement here; and I have a certain amount at the Institute, none of which I've gone through in 100 years. So there are large masses of material. If you want to come down, you probably can find things here that you can use.

STERN: Would Bigelow have anything?

GOLDSTINE: I haven't the faintest idea what he's got.

STERN: It just strikes me as so odd that the Institute would destroy these papers.

GOLDSTINE: I know. Yes. It's crazy, but there it is. Well, Oppenheimer, at that point, certainly would never have destroyed the material. Why they destroyed them, I don't know. Whether it happened under the next director or what, I just have no idea.

STERN: Kayzen was the next Director of the Institute?

GOLDSTINE: Yes. Right. I have no way to judge; maybe they destroyed them immediately and nobody would have known it, you know. When they closed the project up--well, maybe some administrator just said, "Let's get rid of all that junk," and just emptied the filing cabinet. That's all I can answer you.

STERN: When you left in '57 or thereabouts, that was the end of the project; it was closed?

GOLDSTINE: No. It stayed on for about another year or part of a year, and then Oppenheimer gave the computer to the University. The University then, after using it for a couple of years, gave it to the Smithsonian.

STERN: Who was Director after you left?

GOLDSTINE: Hans Maehly. He was a Swiss who died. We had a number of Swiss mathematicians here, and Maehly was one of the last ones. Incidentally, talking of things like that, we also had a number of people here from Sweden; we had some people here from Germany. That's another way--we've talked about this transfer of technology. There was a man named Heinz Billing and Heinz who was the man in charge of building computers for the Max Plank Institute in Munich. One of the first digital machines was built there. And then, talking about this business of meetings, I remember--the dates are in my book--I went to a meeting in Darmstadt. We were responsible for getting the people in the University of Munich--not the Max Plank Institute, but the University. There was a professor named Piloti there, who with his son made a copy of our machine. I guess that was a result of talking to Piloti at a meeting. But at any rate, there was a lot of work going on--the world was ready for computers at that point, and there was enough money around so that academic institutions could afford to build machines; and did. It was a good period. It was a nice design, too.

STERN: Did Williams come to consult for the Institute?

GOLDSTINE: No, he never came. I'm not sure I've ever met Williams. I may be wrong, but I don't think he ever came. We sent Bigelow to England as soon as we heard about the project; we heard about it because Douglas Hartree

would periodically visit us, and he either wrote us or came and told me about it. We immediately sent Bigelow off to learn about the project in England. Bigelow put Pomerene on the project; and Pomerene really made it work. That was the most significant thing we did engineering-wise, I think, at the Institute.

STERN: Was there any problem with the patent?

GOLDSTINE: No, there were no problems at all. There were no patents that I know of, but we never used it commercially. At any rate there was no problem. You know, I think you're entitled to use somebody's ideas without any problems. Patents are to protect you, I think, against somebody making money off of your ideas. It's like Xeroxing something for yourself. I don't think there's any copyright problem there. At any rate, if there were problems, I never was aware of them.

STERN: When Bigelow went to England, he did communicate with Williams himself?

GOLDSTINE: Oh, sure, sure. He went to Manchester and stayed there and learned what Williams was doing, and came back, and then, as I say, Pomerene took up the project. It was he who made a parallel memory. Williams was using the tubes in a serial way. And that was a big difference. Pomerene made a very big contribution. Okay?

STERN: Thank you very much.

GOLDSTINE: Oh, you're welcome.

END OF INTERVIEW