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The Princeton Mathematics Community in the 1930s
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HERMAN GOLDSTINE

(with ALBERT TUCKER)

This is an interview of Herman Goldstine at his home in Princeton, New Jersey, on 22 March 1985. The interviewers are Albert Tucker and Frederik Nebeker.

Nebeker: Perhaps we could begin, Professor Goldstine, by asking you about Oswald Veblen.

Tucker: I think that you got to know both Veblen and von Neumann before you came to Princeton.

Goldstine: Yes, that's true. I really got to know Veblen through G.A. Bliss in Chicago How exactly, or on what visit Veblen made to Chicago, I don't remember, but I met him. I know that I got notification to go into the Army in July something of 1942, and I was sent to the Air Force in Stockton, California. Bliss got in touch with Veblen who was then the chief scientist at Aberdeen Proving Ground. Veblen had been in the first war—he was a major in the first war and a civilian in the second war—and he started the wheels moving. It was touch-and-go whether I would go overseas or Veblen would get there first, but he got there first, and I received orders to leave Stockton.

Well, in fact I got orders to leave Stockton from the Adjutant General, and simultaneously I got orders from the local post to proceed to I've-forgotten-where on the way to Japan or some eastern place. I called the commanding general, and he said, "Which do you want to do?" I said, "I want to take the Aberdeen post." And he said, "Well, the orders from the Adjutant General in Washington obviously take precedence over the orders from a post adjutant in some fort in Stockton, California." "So," he said, "if I were you, I would get out

of the camp. If you've got an auto," he said, "I'd get in the auto, and start driving. Let the paper work catch up later on, because otherwise you'll just have an impossible time." So I got in the car and drove east, and Veblen assigned me to work for Albert Bennett. Do you remember Albert Bennett?

Tucker: Oh, yes.

Goldstine: So that was my first real day-to-day encounter with Veblen.

Nebeker: And Veblen knew about you through Bliss?

Goldstine: Yes. At that point I was the research assistant—I think that was the title—to Bliss, who was the chairman of the math department at Chicago.

Tucker: But you'd done your Ph.D. with Lawrencé Graves, hadn't you?

Goldstine: Yes, but Bliss liked me and asked me if I would be his assistant. So that's how that went. My thesis subject was the calculus of variations in abstract spaces, so it fitted Graves's abstract spaces and Bliss's calculus of variations.

Nebeker: So you worked with Bliss even on the Ph.D.?

Goldstine: No, I didn't work with anybody. Graves was my thesis advisor, but he was away the year I wrote my thesis. And Bliss—I didn't even know that Bliss had any interest in it, but he was following it for some reason.

At any rate, that's how I got to know Veblen. From time to time I was very impatient of Albert Bennett, who was a nice old gentleman—in those days I guess he was a major at Aberdeen—but he was a very precise, methodical, plodding person who drove me up the wall. I'm sure he must have driven generations of other people up the wall, too, at Brown [laughter]. So I kept doing whatever I felt had to be done, and once in a while Veblen would call me in, and in a very nice way he would say to me, "Try to be nicer to Bennett, if you can." But Veblen became a very close friend, and it was just the other night—we're moving, and we were cleaning out stuff upstairs—I was throwing away letters from Elizabeth and Oswald from trips when they would go overseas and write us notes. He was a very nice old gentleman.

Tucker: Someone else who is a very good friend of mine that you knew at that time was Sergeant Douglas.

Goldstine: Yes, when I was at Aberdeen, I had the good fortune to have associated with me Ed Douglas. He was ...

Tucker: Mother Superior of the enlisted crew.

Goldstine: Right. I guess he taught mathematics and eventually became headmaster at the ...

Tucker: No, he became headmaster, but that was only in an interim period. He was acting headmaster at Taft.

Goldstine: I see. He was just a marvelous guy. In the Army you can't really get anything done unless you know a sergeant. And this sergeant was sort of like a Father Confessor to all the enlisted men at the Ballistic Research Laboratory. If you needed sheets or pillowcases or blankets or another field jacket or heaven-knows-what, Sergeant Douglas could always produce it for you. In fact, that was a very superior group. Another man who was there from Princeton was Martin Schwarzschild. He was in the laboratory, and he is a superior person.

Tucker: And then you had some very promising young mathematicians there as enlisted men, who didn't observe Army protocol [laughter].

Goldstine: It was a wild and woolly group. It was a very nice group of people. I guess that during the first war there was a fun group, too. There were a few officers like Everett Pitcher. I got a note from Everett the other day. But, anyway, it was my great good luck that Bennett's boss was a man named Paul Gillon. He was a regular Army officer. And Gillon took me to Philadelphia very soon after I got to Aberdeen to look over a substation which the Ballistic Research Laboratory was running at the Moore School at the University of Pennsylvania. We went there and it turned out that the thing was terribly run. It was just a shambles. They were trying to train people to be what in those days were called computers. That was the word for a human being and not a machine. A computer in those days was a human who sat at a desk calculator and pounded out numbers. Also, the BRL rented from the University something that was called a differential analyzer, which is a big analog computer. It was a sister machine to the analog computer at Aberdeen. The whole operation was being done badly, and Gillon said to me, "Herman, why don't you come up here and be in charge?" And I said, "That's great." So for the balance of the war I was nominally at Aberdeen, but in fact was mostly at the University of Pennsylvania.

Let's go back for a minute. I guess one of Veblen's greatest mathematical accomplishments was finding Johnny von Neumann and bringing him to Princeton University. At least I suppose that was his greatest achievement among many achievements.

Nebeker: In your book on the history of computers you pay tribute to Veblen in administering the Institute and attracting people there.

Tucker: But it was earlier on that he brought von Neumann, and at the same time Wigner. They filled one position between them.

Goldstine: That's true. I think all of Veblen's life he was a natural administrator and leader.

Tucker: But he always did this by indirection.

Goldstine: Well, right, he did [laughter], but, by God, he did it. He did it. He was the kind of guy who would keep dripping water on the stone until finally it eroded. If it didn't happen otherwise, he just kept at it, and at it, and at it.

Tucker: I've always thought of him as a mathematical analog of a political boss, the old-fashioned hemming and hawing type of thing.

Goldstine: I think that's probably true. I don't really know much about how you'd rate these people. At Chicago, there was a man who was the founding chairman of the math department named E. H. Moore. Moore had a lot of students, but three that I can think of at this moment are Veblen, G.D. Birkhoff, and G.A. Bliss. Of the three, Birkhoff we can dismiss, since he really doesn't come into the picture, but Bliss and Veblen were two of that group.

I've never quite understood how well Veblen did as a mathematician. He obviously was a prominent mathematician, and I suppose he was the most prominent in the sense of moving American mathematics to the forefront. That's my personal appraisal of it. Of course Al knows all about these things more than I do, I have them sort of partly from Veblen's view, and that may not be unbiased. For example, his description of how he got Fine Hall built. Then I've had Jim Alexander's story about what a pest Veblen was in building Fuld Hall at the Institute. Because every little door knob, every little gargoyle, every little piece of stained glass that has a word on it, was something that Veblen personally supervised. Alexander cared very little about any of this stuff, and Veblen was constantly involving Alexander, or trying to involve him in these things.

Tucker: Well, Alexander's mother actually had some interest in interior decoration, and she is credited with having selected most of the furnishings for Fine Hall.

Goldstine: I see, so that's how come poor Jim got cursed with all of this business.

Nebeker: I was wondering if you could analyze this leadership role that Veblen played. One thing you mentioned was his attracting the best people he could to Princeton. What other things would you mention?

Goldstine: Well, when I was at Aberdeen, he was competing with the Los Alamos people for mathematicians, physicists, chemists, engineers, you name it. There was an old boy network out there. He knew the chairman of every mathematics department, and probably the physics chairman, and maybe the chemistry chairman, etc., at all the major schools. I think that was very important. To Aberdeen he brought Jimmy McShane, he brought John Kelley, he brought Chuck Morrey, he brought Tony Morse. And he brought L.H. Thomas, the physicist; Ted Sterne, an astrophysicist; Leland Cunningham, an astronomer; Dorrit Hoffleit, an astronomer—well, we could probably sit here and go through a list. I guess he brought Everett Pitcher. He brought a

whole scad of people, and he did this by his going to friends and saying, "Who do you think's available, and who would be good?"

Nebeker: Was he also stimulating to the mathematicians there working under him?

Goldstine: Yes. He was a warm, lovely human being.

Tucker: But he didn't try to annex people, in the way, for example, Marston Morse did. He was interested in the man doing his stuff.

Goldstine: No, he was not like Marston at all. Veblen was married to an English woman, and he tried awfully hard to be an Englishman. I think it would have been a great ambition of his, if it were possible, to become an Englishman. But he had that kind of business of being very nice, but standing off. Now Marston Morse and Richard Courant were examples of guys who pushed in different ways on people. I think that's fair, and they wouldn't let you alone. But Veblen believed in people and not in what they were working on. I think Morse, for example, is more interested in the problem than the person, and I think Veblen was more interested in people than in the particular mathematical thing.

Tucker: That's right. A moment ago you said you didn't know quite where Veblen stood in mathematics. Now just recently I saw an interview that was done by one of the mathematics magazines, the one that's called the *College Mathematics Journal,* an interview of H.S.M. Coxeter. At one point Coxeter is asked, "Who are the great mathematicians?" He starts with Archimedes and Apollonios—not Euclid because Euclid just organized what already existed. Then he went on and made mention of more modern days: Gauss, Lobachevski, and Veblen. That's pretty high praise.

Goldstine: I think that's putting him much higher than I would have put him. I liked him and I admired him, but I certainly would never mention him in the same paragraph with Gauss, but there's hardly anybody that I would.

Tucker: Of course Gauss was much more than a geometer. Geometry was just incidental, so to speak, to his research, but it was the whole thing as far as Veblen was concerned. Veblen was actually trying, at the time that I was a graduate student here, to come up with a definition of what a geometry is, somewhat in the way Felix Klein had done in the Erlangen Programme. He finally came to the conclusion that anything that he could not draw any distinction between the things that he wanted to call geometry and the rest of mathematics.

Goldstine: Yes, that's very interesting.

Tucker: He said that one would have to say that something was geometry if there were experts with acknowledged taste who said it was geometry.

So he started off with E.H. Moore; his doctoral thesis was on postulates for plane Euclidean geoemtry. He went on from that to projective geometry, because he felt that Euclidean geometry and the non-Euclidean geometries could all be regarded as specializations of projective geometry. So if you wanted to really get to the foundational roots of geometry it was Hilbert. Then around 1920, when Einstein's general relativity came out, he immediately started in on differential geometry, but not like Eisenhart. Veblen was trying to get to foundations of differential geometry. In between, of course, he had gotten interested, working with his student James Alexander, in analysis situs. He sort of ran the gamut of geometry, so if anybody could come up with a definition about 1930 it was Veblen. Well, Coxeter was a post-doctoral fellow here at about that time, and I sure think he's a pretty good geometer, but he takes off his hat to Oswald.

Goldstine: Well, I think the nicest part about Veblen in this respect is that however great a mathematician he was—and he certainly was a great mathematician—he recognized greatness in mathematicians and in scientists, and as far as I know he had no envy for people who were greater than he. And that's not trivial. He delighted in Johnny von Neumann, for example. I think in a sense he viewed Johnny as his intellectual child, almost. He and Johnny always talked—I mean Veblen would be in and out a lot—but never about mathematics. Always about some political thing at the Institute, or administrative problem, or world problem.

There was a time he was talking with me about the idea of a national institute for advanced study and things like that. He was always concerned about the good of science in America, and furthering it. But as I say, I think the fact that he could take somebody like Johnny or people at Aberdeen or wherever and view them with perfect equanimity, even though they were as good or better than he, is a mark of a considerable person. I think you'd find it hard to say that Marston would view a superior person with ease. In fact, that put Marston's hackles right up, the way when two male dogs approach each other.

Tucker: Pretty much the same was true of Birkhoff.

Goldstine: Of Birkhoff, too, of course.

Tucker: But not of Bliss.

Goldstine: Not of Bliss, no. Both he and Veblen had this very gentlemanly quality of just treating people the way they were. That was a remarkable quality both of them had. At any rate, I liked and admired both of them very much. I don't know two men that I really liked, respected, and admired so much, and who were so nice to me as those two men.

Tucker: When did you first encounter Johnny?

Goldstine: I first met Johnny in the pre-war days. There was a conference on modern integration theory at Ann Arbor, Michigan. For some reason the American Mathematical Society asked me to be rapporteur for the conference. The speakers included Wiener, von Neumann, and a variety of other people. But of all the people the one who really impressed me was von Neumann. The one that least impressed me was Wiener. Wiener gave one of those papers that he could give where everything was totally disorganized. He was muddled. He was a great person. For the purposes of this record let me say that I wrote a book review of his collected works, and I thought they were fantastic. I just think he was one of the really great people. But boy, when he got up and gave an unprepared lecture, it really stank, and that was a stinky lecture.

Whether von Neumann's was prepared or not I have no idea, but it was just the way von Neumann did all his lectures. It was just like being out on glass, it was so smooth. You never heard him lecture. Well, his lectures were totally different from his printed articles. The printed articles tend to be very Germanic, long, hard German sentences; not very insightful papers. But when he would get up and speak about the topic, he somehow knew exactly how to get you through the forest. Whenever he gave a lecture, it was so lucid, that you wouldn't sit there and take notes because it was like magic, it all seemed so simple you didn't need to take notes. Then afterwards when you got home and thought about the subject, you realized that you didn't know at all what the trail was through the trees.

I could tell you a story about this and it's true. I guess I had better not mention the man's name. One time when von Neumann was available for people to come and discuss their problems—this was his wont—this chap came to see him. He was a temporary member at the Institute. He came to see Johnny because he couldn't prove a certain theorem. Von Neumann said, "Well, this is how you prove it." And he went up to the blackboard and away it went. This guy sat there, watched the whole performance, thanked von Neumann profusely, and left. That was, say, on Wednesday, and on Saturday night the von Neumanns had a big bash, which was fairly customary. This guy came over to von Neumann and said, "Excuse me, but you know that proof you made the other day? I neglected to write it down, and I just don't understand how it goes. Would you mind telling me again?" So von Neumann rattled the thing off for him again, with a highball glass in his hand. As the fellow walked away, von Neumann said to me, "That son of a bitch, that he had the nerve to make me twice give the proof, and he's not going to give me even a footnote reference in the paper. The proof is going to be put down as his proof. That's the way people are." That was sort of typical.

At any rate, von Neumann was extraordinarily lucid. He was in no way like Veblen. Veblen and Bliss were both, I suppose, the best American types that you could find. And von Neumann represented a very sophisticated Central European culture. People came from far and wide to tell Johnny stories, mostly dirty jokes. I can't conceive of anybody going up and telling either Bliss or Veblen a dirty joke. In

fact, I don't think they had much capacity for jokes. I mean, that wasn't their thing. But von Neumann was amused. He had a kind of a chameleon-like quality, to adapt to the people he was with. If they were very dignified and if it was going to be a full-dress affair, with everybody on their dignity, he would be on his dignity, and there would be no nonsense about it. He could be the Herr Geheimrat just the same as anybody else. But left to his own devices, he was more like Abe Lincoln, who would suddenly say, "Well, that reminds me of a story," and then would tell you the story. You'd proceed along, and then the discussion would be broken up by these stories.

Whenever you'd go into his office, having spent the last week working on something, and say, "Johnny, I've got an idea," and start to write, you'd get maybe the first half-a-line down before he'd say, "Yes, let me have the chalk." Then he'd get up there, and for the rest of the hour he would be putting it down in the way it ought to be done.

I was trying to compare him to Fermi. Fermi had an intensity. They were both extraordinary virtuosi in their fields. They were probably the fastest guys on their feet that I've ever met, each in his own field. But Fermi was incredibly intense. When Fermi worked at something, he worked at it. Fermi became friendly and affable, and he was a charming, lovely person. But when he was focusing on the thing he was interested in, that was everything. Whereas Johnny had no problem with putting up with any number of shennanigans on the side while the main problem was going forward.

I remember when, at some point, Fermi suddenly got interested in computers. He called me to Chicago and sat me down at the end of a table. He sat at the other end, and a man named Sam Allison, who was a physicist at Chicago, sat in the middle. He sat for a couple of hours and did nothing but grill me about the computer, in every possible aspect. No break whatsoever, no levity, nothing. It was just the kind of an examination that you could imagine might take place in the basement of a police station somewhere, you know, when you'd been caught doing something ghastly. And at the end of that, he understood everything and was all conversational and chatty and pleasant. But I never really saw Johnny, hardly, when it wasn't possible for him to smile and relax, and that didn't seem to stop his flow of thought.

Nebeker: Could I ask about von Neumann's work habits? Was he in his office all day long, for example?

Goldstine: His work habits were very methodical. He would get up in the morning, and go to the Nassau Club to have breakfast. And then from the Nassau Club he'd come to the Institute around nine, nine-thirty, work until lunch, have lunch, and then work until, say, five, and then go on home. Many evenings he would entertain. Usually a few of us, maybe my wife and me. We would just sit around, and he might not even sit in the same room. He had a little study that opened off of the living room, and he would just sit in there sometimes.

He would listen, and if something interested him, he would interrupt. Otherwise, he would work away.

At night he would go to bed at a reasonable hour, and he would waken, I think, almost every night, judging from the things he told me and the few times that he and I shared hotel rooms. He would waken in the night, two, three in the morning, and would have thought through what he had been working on. He would then write. He would write down the things he had worked on. I think I recounted in that book the nice story that he told me about the Goedel theorem.

He, under Hilbert's tutelage, was trying to prove the opposite of the Goedel theorem. He worked and worked and worked at this, and one night he dreamed the proof. He got up and wrote it down, and he got very close to the end. He went and worked all day on that part, and the next night he dreamed again. He dreamed how to close the gap, and he got up and wrote, and he got within epsilon of the end, but he couldn't make the final step. So he went to bed. The next day he worked and worked and worked at it, and he said to me, "You know, it was very lucky, Herman, that I didn't dream the third night, or think what a state mathematics would be in today." [Laughter.]

So those were his work habits. He was a very methodical worker. Everytime he thought about something, he wrote it down in great detail. There was nothing rough or unpolished. Everything got written down either in the form of a letter or a memorandum.

Tucker: I've looked at his files at the time that A. Taub was editing the collected works and just saw the raw material. It was essentially a diary, except that it was kept in a filing cabinet. Everything was filed by date, not by subject matter.

Goldstine: Yes, that's right. He was very methodical, but he was not only methodical about these things, he also had all kinds of instincts. I remember one time he gave me a paper that he pulled out of that filing cabinet. He said, "I wonder, Herman, if you'd take this and read it for me. I've kept this here for a long time and I've never quite known whether to publish it or not." So I took it home and read it, and I found something I thought was wrong. So I waited a few days and read it again and again. I really felt it was wrong, so I brought it in and showed him the mistake and he said, "Damn it, of course. There is some instinct that kept me from publishing that paper and it must have been a realization that I had a mistake somewhere in it, but I just never knew where it was."

He could give lectures on material that he hadn't seen in 20 years. He originally wrote this material in German, and he was up there at the blackboard at the Institute giving this lecture. I was sitting in the back with the German text just for fun. He was translating, because word for word, symbol for symbol. He used exactly the stuff that was in those papers. So he had a remarkable filing system in his own noodle.

He had another quality which I always thought was unbelievable. He and I worked at trying to prove something about bounds on eigen values one time without any success. One day I saw in *Math Reviews* a statement that Kolmogorov or somebody had proved a theorem, and I said, "This is what so and so proved." He said, "Sure, this is how it goes." And he went to the blackboard and he proved it. Somehow, just knowing that it was true, and not just a conjecture of ours, made it possible for him to see the proof. I don't know how or why or what.

I watched him reading a math paper. He would read it like this, just the way you might read a mystery story, page by page. At just about the time you could run your eye down the page, he would be turning it. And at the end of it he had it. I always remember one time, Bochner, von Neumann, and I were in a room, I guess Johnny's room in the Institute. Bochner was presenting material to us, and he got stuck. He hemmed and hawed for a while, and he said "If you'll wait a minute, I know where the book is that has the proof of this. I'll run upstairs and get it." Johnny said, "Don't do that, I don't know what book it's in, but I'll prove it for you." And he did.

So he had a remarkable mind, a really remarkable mind. He was very different guy from Wigner, who was another extraordinarily talented man. But Johnny and Wigner were really different. I guess they both must have been absolute holy terrors. There were three of them. There was von Neumann, Wigner, and Teller. The three of them went to school together essentially from kindergarten right on in Budapest. Once Johnny told me that Wigner and he used to team up against Teller. And I suspect—this is my own theory—that Teller's attitude toward the world, his general hatred of everything, is due to the fact that he was repressed constantly by two guys who were brighter than he was [laughter].

He is a very bright man, Teller, but to be opposed by two men of such formidable brilliance as those two guys must have been impossible. That goes back to this thing that we were talking about, about Veblen being happy to deal with people who were brighter than he. I don't think Johnny suffered brilliant people easily. I think he got contentious, and I think he didn't handle that one as well as Veblen would.

Tucker: He could get rather irritated when he felt someone was challenging him.

Goldstine: Yes, that's right. Absolutely.

Tucker: I remember two of my students, who were working in game theory, went to him to tell him about things that they were working on. In particular Harold Kuhn thought that he had found a counterexample to one of the results that's in the paper on the expanding economy that von Neumann did in 1937. Johnny actually got angry, and I really think that he was thinking very fast to get himself out of it. The example that Harold Kuhn had come up with was one in which the coefficient of expansion—the theorem was that every expanding economy

had a coefficient of expansion which was a real number—was infinity. But Johnny immediately said that, well, at the time he wrote this he was under the influence of Hahn who regarded plus infinity and minus infinity as real numbers to make the whole system compact. So that anyone knew that plus infinity was a real number [laughter]. It seems to me that if that was the view you were taking you would at least put in a footnote to say so.

Goldstine: Well, Johnny was not like Veblen in this sense. He didn't have that same attitude. He had hatreds or dislikes for people. He had a lot of dislike for Wiener. Wiener was a pain to him, and of course Wiener was a pain to many people, including George Birkhoff. Maybe even to Mrs. Wiener, for all I know. Especially, I suppose to Mrs. Wiener [laughter].

Tucker: There was a tangle among the three of them having to do with ergodic theory.

Goldstine: Yes, there was. All three of them made great discoveries in the ergodic theory, and I think not one of them wished the other two did at that point [laughter].

Nebeker: How did von Neumann get interested in computing?

Goldstine: I don't know how he originally got interested in computing. You see, he started life as a chemical engineer. Whether it was due to his engineering training, I just don't know. For all I know when he was a kid of three he was already squaring numbers on box cars. While he was at Goettingen, Courant, Kurt Friedrichs, and Hans Lewy produced a paper. I think the date is about 1928, but I'm no longer sure. I've also totally forgotten the title of the paper. But it's the paper in which they show that partial difference equations have stability problems connected with them unless you observe certain inequalities. The so-called "Courant condition" came into being out of that. I think they did that as rather a formal exercise, but von Neumann was around and he understood it and tucked it away in the back of his head. He not only made use of that, but he put it into a Fourrier context which made it more usable.

When he was at Los Alamos he had to do a lot of computing, and that's the reason I'm hesitant. I suspect that right away from early times that he understood and had no problem with writing down differential equations which describe physical phenomena and was quite prepared to integrate those equations. I think that's why he was so interested in this Courant-Friedrichs-Lewy paper. When he went to Los Alamos he made several great contributions, one of which was that he taught people there that it was possible to write down differential equations which described the actions of the phenomena that they were dealing with, instead of just trying to do physical experiments which were kind of a masked form of analog experiment. The second thing is, specifically, he took a very active part in something called the implosion problem.

There were two bombs in the early days. The first consisted of two halves of a sphere, and the sphere itself would be critical if the two halves were together. The two halves were kept apart, and there was a gun fired, essentially, that drove one half-sphere against the other half. When the two came together the total sphere became critical and the nuclear process took place. That was the crudest bomb. The second bomb was more sophisticated. The idea there was you started with a sphere, and you packed conventional explosive around it carefully, and you had lenses which would focus the energy in such a way that the sphere would be compressed tighter and tighter. Its density kept going up and up, and as its density goes up, the size that it needs to be to be critical gets smaller and smaller. So eventually you reach a point where the sphere becomes critical.

That's the thing that von Neumann worked on. He got Los Alamos to rent from IBM a tremendous collection of punch-card machines, and he had them doing partial differential equation calculations all over the lot on this problem. The only thing, therefore, that I can tell you with certainty is that at Los Alamos already he was very sophisticated in big partial-differential-equation calculations.

Tucker: That is my impression. As I heard it put one time, he wanted to be able to try out certain ideas about partial differential equations. This was beyond his own computer, I mean his own mental computer. So what he wanted was something that would extend his powers of experimenting with partial differential equations. Of course this had something to do with his interest in the weather problem. There he was into an area of partial differential equations that hadn't really been dealt with.

Nebeker: But given the fact that these systems of partial differential equations had resisted the best efforts of mathematicians a long time, it's perhaps remarkable that he would think that the right approach is the computational one, that it was after all feasible to solve these equations numerically.

Goldstine: Well, I think that during the war period a lot of things were moving forward. He tried one calculation on the Harvard Mark I, which was an electromechanical machine and was simply too slow. But people had never really tried solving partial differential equations numerically.

Tucker: They tried analog solutions, but not numerical solutions.

Goldstine: Right. And Johnny had a lot of confidence somehow in his own fate—that he could do it. Then he and I got together. We ran into each other, and he knew then that the apparatus was available that would do it.

Nebeker: This was the ENIAC?

Goldstine: Yes, and von Neumann was a consultant at Aberdeen Proving Grounds where I was. He was more than a consultant. One of

Veblen's remarkable contributions to Aberdeen was forming a scientific advisory committee. And on that scientific advisory committee he had a number of great people. He had von Neumann, he had Henry Norris Russell from Princeton. He had Philip Alger, I think was his name, from General Electric, an extraordinarily nice, brilliant engineer. He had a man named [Hugh] Dryden, whose first name I've forgotten now, who was the head of what is now NASA. (It was Langley Field, and it had a different name in those days. It had to do with aerodynamics.) There was a man named Bernard Lewis who was the head of some scientific office in the Bureau of Mines. There was Harold Urey, the chemist. There was George Kistiakowsky. It was a whale of a good group of people, and von Neumann, as I say, was one of that group.

That's how von Neumann and I got together again, and whether in fact he remembered that I had met him in Ann Arbor I sort of doubt. But it certainly is true that as soon as he heard what we were building at Philadelphia, he really knew that that was the device that was going to solve hyperbolic partial differential equations. I think that that moment changed his life for the rest of his days.

Tucker: A footnote: Alger was a student of Lefschetz' at the University of Kansas.

Goldstine: Was he? I had no idea. He was a very charming gentleman. And that explains, I guess, how Veblen got a hold of him. I never knew where he came from.

Nebeker: Could I ask you about James Alexander? You mention in your book that he was helpful in getting the computer project accepted at Institute. Did he have an interest himself in computing?

Goldstine: I think not in computing. I think that Alexander's interest when I knew him in those days was in electronics. He loved electronic gadgets. He was building amplifiers. I think he was running away from mathematics at that period as fast as he could. So I don't think he wanted to compute anything. He was just a great gentleman, and he wanted to do nice things for Johnny. And I guess he realized that this was very important, and he was glad to help. I was very fond of him.

Nebeker: We wanted also to ask about Goedel. Did you know him?

Goldstine: That would be an overstatement. Deane Montgomery knew him better than anybody that I know. I always remember one time Gwen Blake being very upset because I was seen talking to Goedel.

Tucker: She was the secretary.

Goldstine: The secretary to the department. In those days a department had one secretary, and everybody queued up with their math papers to have her type them. So there would be Veblen and von Neumann and Einstein and Alexander and Weyl, all these people would be in a queue waiting with these monumental papers to get them typed. Whereas nowadays there are probably forty-seven secretaries in the

department. But Miss Blake was very upset because she saw me talking to Goedel, and she came to the conclusion that I was talking Goedel out of his office. And how the heck that was, I don't know. Why she should suspect me of wanting Goedel's office. At any rate, I think she went to Veblen, and Veblen came to see me to find out if I was really going to dispossess poor Goedel. It was the last thing I wanted to do [laughter]. No, I really didn't know him. We would chat a little bit, but he was so far out, I couldn't talk to him. Did you ever have any luck with him?

Tucker: No.

Goldstine: He wasn't the kind of guy that you would just go off and chat about something with, you know. He was crazy. You had to have something that you really were vitally concerned about, mathematical, that he could help you with or you could talk to him about. In the cafeteria at the Institute there were tables. You would bring your tray and sit down. You could sit next to Hermann Weyl, and he was perfectly prepared to chat with you. And I'll always remember the time on that score when he said to me, after I kept saying Professor Weyl to him, "You must call me Hermann." It took a tremendous gulp for me to do that because this was one of the great figures to me.

Anyway, Goedel was never the guy who came in and plunked his tray down, and you would just chat about what the Brooklyn Dodgers did yesterday, you know. I don't know what he and Einstein talked about every day when they went back and forth, but Goedel had all kinds of obsessions and problems, and if anybody in the world besides Einstein talked with him it was Deane Montgomery.

Tucker: Well, Goedel got quite interested in unified field theory.

Goldstine: Did he?

Tucker: Yes. I learned this from Oskar Morgenstern. They both came from Vienna, and Oskar regarded Goedel with great awe. But at the same time he felt that Goedel was someone who needed protection, that he needed looking after.

Goldstine: That certainly is true.

Tucker: Oskar Morgenstern was quite willing to take on that responsibility.

Goldstine: Well, he brought him over, too. I think he took a lot of responsibility. Oskar was a great gentleman.

Tucker: Yes, and because of game theory, I was quite closely involved with Oskar.

Goldstine: Of course, you would have been. There's a nice story. I forget who told it originally. It's supposed to be true. The story was that Goedel decided to become an American citizen. Somebody

convinced him he should be an American citizen. So he read the Constitution and found what he thought was a contradiction in it. And von Neumann carefully argued Goedel out of this by some sophistry. He showed him that it really wasn't a contradiction, that you could read it in such a way that it was all consistent. And Goedel bought this [laughter].

Goedel picked Morgenstern and Einstein to be his sponsors. They went off to Trenton and appeared before some Jewish judge there who was really wowed. (The reason I mention Jewish is because Albert Einstein to him was the great figure.) So he spent most of his time chatting to Einstein . He could care less about this little Austrian who wanted to become an American citizen. After he finally talked as much as he reasonably could to Einstein, he realized he had to say something to this guy before he made him a citizen. And he said, "And of course, none of this that we have been talking about could happen in a country like the United States, could it, Professor Goedel." And Goedel says, "Well, you know I think maybe ... " And Morgenstern gave him a jab in the ribs and got him to say no, it couldn't happen in America. Then they made the proper signs over Goedel head, and he became an American citizen [laughter]. But, no, I didn't know him, really.

Nebeker: We also wanted to ask about Hermann Weyl. Are there any anecdotes, anything you remember in particular you remember about him?

Goldstine: I always was struck by the difference between him and Johnny von Neumann. There are jokes, one of which Johnny always swore was false. That's the story that, I don't know, Hermann Weyl was going to prove some theorem, a very deep and profound theorem, let's say it was the Riemann-Roch theorem. I don't know if it was the Reimann-Roch theorem, but that was one I always have trouble with, so let's say that was the theorem. And Weyl gave a lecture on why this is a very deep, profound result, and he gave a very complicated proof. And the apocryphal story goes that at the end of the lecture there's this kid who is supposed to have raised his hand at the back of the class and said, "Professor Weyl, may I show you a proof?" And goes up to the board and goes zip, zip, zip, zip, and in about 15 lines has a brilliant proof of this thing.

I asked Johnny about it, and he said no, that wasn't true. But it is true, if you talk to Natasha Brunswick, who was in those days Natasha Artin. Natasha says that there was always Johnny with these tight pants on. All of Johhny's life, whatever size suit he bought, he always ate too much, and the suit was always one size smaller than Johnny. Even as a student in Goettingen, his behind was always ready to bulge out of his pants. I guess Natasha and everybody in the class were always charmed.

But Joachim, who was one of Hermann's children, told me that when Hermann used to work in his house on Mercer Street, in the study in there, you would hear groans coming out of the study. That Weyl

worked at things in sort of anguish, that it was hard for him, that he delivered his theorems practically like a woman giving birth to a child. That's so different from Johnny, because when he and I would be working at something, when we'd get stuck, he'd say, "Okay, that's it," and pack it up. It might be that he'd phone at two in the morning to say, "This is how the proof goes." But it might be three weeks, a month or so later, or it might even be I who would come in a month or so later and say, "This is, maybe, how to go." But he never struggled with something. When he got stuck, he filed it somehow, and it just came out easily. I suspect that Weyl was probably the deeper of the two mathematicians.

Tucker: And also the broader.

Goldstine: And the broader, yes.

Tucker: Weyl is the only complete mathematician that I've ever had the privilege to know.

Goldstine: Yes, I think that's probably true.

Tucker: Johnny was essentially an analyst. I've seen him give one of his quick proofs on the spot, of a topological result, and it was clumsy.

Goldstine: Yes. He told me at one time that he had no facility at all in topology. He said he never felt comfortable with that.

Tucker: Whereas Weyl was an excellent topologist.

Goldstine: Everything he did was beautiful. Everything he did. I guess the difference is that von Neumann could run rings around anybody speedwise. In that way he was probably the most brilliant mathematician that I've ever known. I suppose maybe he's one of the quickest there ever was. Weyl was probably one of the deepest and broadest that there ever was. And that's a real difference. I mean, if a guy combined both of those they would call him Isaac Newton, and probably they did. But I think that's the difference, really.

Tucker: This is the centennial year of Hermann Weyl. And it's being observed by the University of Kiel. They're having a conference there right at the end of June. One of the speakers is the younger Weyl.

Goldstine: Michael.

Tucker: Yes. Joachim is, of course, gone. But Michael is presenting a talk about his personal recollections of his father.

Goldstine: How nice. Well, his father was a remarkable gentleman. Weyl was a charming person, too. It's so funny. When he was married to Hella, he was the real German Geheimrat. Rather pompous, very dignified. Nobody was allowed to smoke near him, because Hella said he had allergies. One had to treat him very much the way you would

treat the great German professor. After she died, he used to come to our house because we had one of the few television sets around in his neighborhood. He'd come to our house, and the thing that he always wanted to watch—we would have to turn it on for him—was wrestling [laughter]. He loved these phony wrestling matches. And he was a real German grandfather with our daughter. He was actually a kind of Santa Claus type. He wasn't pompous at all, he wasn't stuffy, he wasn't the Herr Geheimrat. He was just a very sweet, relaxed old German gentleman. And it was just very nice.

Nebeker: How long did you know Hermann Weyl?

Goldstine: Well, from '46, when I came to the Institute, until whenever it was he died. We knew him best in that little interim period. But when his first wife was alive he was sort of difficult to approach, and then when his second wife was alive—and she was a very approachable person—they lived at least half the year abroad.

Tucker: For legal reasons, I think she had to. She was in some sort of business or property in Switzerland that required her to be there.

Goldstine: In fact her husband, and there were a couple of brothers, they owned what's called the Baer Bank. The Baers owned a private bank in Zurich, and they're wealthy as can be. Hermann, at the end, if he hadn't died I guess would not have been able to get back into the United States. I only half remember the story, but as a non-native born American, in those days, his rights as an American citizen were not as good as our rights as American citizens. The immigration people finally told him that if he left the country that they might not be willing to readmit him unless he came back within a certain number of months.

Tucker: Well, this is for income tax purposes.

Goldstine: Well, this wasn't just taxes. This actually had to do with his citizenship, and it was very mean. This was now the McCarthy period we're discussing. There was a lot more behind it than taxes. This had to do with foreigners. There was a xenophobia in America, really. A number of us tried to get a private bill put through by the senator, Alexander Smith—was that his name? And he wouldn't do it. He was also afraid. It was rather pitiful to think that this great figure, who was such a monument to America, would be excluded from America, whereas some of these rats who had been communists and had defected from Russia to the United States were being brought in, given citizenship, and given all kinds of money. It was really a terrible way for a man of such great stature to be treated by our country. I guess it wasn't really until John Kennedy came along that some of that whole McCarthy spirit got washed away. But I don't remember the story in any detail anymore. Except that it was not a good story, I mean not good in the sense of not reflecting on the United States very well.

Nebeker: I'm fascinated by what you said about the Weyl visit in your house. Are there any other things you can think of, things he liked to do or things you did with him?

Goldstine: No, I don't remember. These things are all so long ago, anyway, and a lot of one's memories get perverted so completely, and some of the things I remember, it's pure happenstance. Probably some I've remembered wrongly anyway. But that's all I can really remember about Weyl.

Tucker: With regard to his Geheimrat behavior, there were verses that were composed by young mathematicians at Fine Hall in the 1930s along the lines of some things that the seniors did. They had step singing, and they had something called the Faculty Song. A verse, for example, about Eisenhart: "Here's to good old Luther Pfahler, in four dimensions he's a whaler. He's built a country club for math, where you can even take a bath."

Goldstine: That's marvelous.

Tucker: Then verses were made up just for Fine Hall purposes. There was never one made up for Johnny von Neumann, that I know of. But there was one for Herman Weyl.

Goldstine: Oh, was there?

Tucker: Yes. "Here we have a punning Aryan, who likes to make groups unitarian. He is that most saintly German, the one, the great, the holy Hermann."

Goldstine: That's marvelous.

Tucker: 'Holy Hermann' was a name that Lefschetz invented.

Goldstine: Right, 'Heiliger Hermann'.

Tucker: And the one about Veblen: "Here's to Uncle Oswald V., lover of England and her tea. He is that mathematician of note, who uses four buttons to fasten his coat." You know he always had a fourth button on his coat because he was so tall and slim.

Goldstine: We always had a theory with Veblen that after he bought a new jacket and pants he would hire somebody to wear them for a few years so that they wouldn't look new when he put them on. I don't ever remember seeing him in anything that looked new, do you?

Tucker: No.

Goldstine: He did something to make them old, I don't know what it was.