The Princeton Mathematics Community in the 1930s
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ISRAEL HALPERIN

(with ALBERT TUCKER)

This is an interview of Israel Halperin at Princeton University on 25 May 1984. The interviewer is Albert Tucker.

Tucker: Would you tell me how it was that you came to Princeton as a graduate student?

Halperin: Well, I came in 1933. Prior to that I had done a year's graduate work at the University of Toronto. Before the end of that year I applied to a number of institutions in the United States with graduate schools of high repute for an opportunity to go there to do Ph.D. work. I was accepted at several places including Princeton, so it was just a case of choosing which I preferred. Since one of Toronto's graduates, Albert Tucker, had gone to Princeton, that was a place that I knew something about.

Tucker: There was a second one.

Halperin: That's true, but Albert Tucker had not only been there but he, by this time, had taken his Ph.D. and gone on to higher things. The reputation of the Princeton staff had become rather solid at Toronto. Yes, there was a second one, Malcolm Robertson, who was in the course of doing his Ph.D. That was enough for me. It was also so that another student with whom I was in close touch at Toronto—we were close friends—had an offer to go to Princeton to do graduate work in physics.

Tucker: That was John Blewett?

Halperin: Yes. Together we decided we would go to Princeton.

Tucker: Do you remember your first days at Princeton?

Halperin: Oh yes. Of course we were very poor. We were all very poor in those days, and we came by bus. We arrived at the bus station at the edge of Princeton, as it was then. So I phoned Al Tucker and said, "We're here, what should we do?" He said, "Stay right there, I'll come." So he came and took us to a place where we could have a room. He had been there, a place called Brown Hall, a residence of the Theological Seminary.

Tucker: Of course by then, Fine Hall was in use. Fine Hall opened for business in the fall of 1931 as I recall.

Halperin: It is very interesting that to those who had been there, this was a rather new building, but to someone like myself who had just arrived, it might as well have been there 100 years.

Tucker: From whom did you take courses in your first year? Do you remember?

Halperin: Oh, I remember very well. I got the impression I wasn't taking courses. But those of us who were in our first year of graduate work were apparently expected to take [Luther P.] Eisenhart's tensor-calculus course. When I started that course, it was all very dull because I had been through all of this with [John L.] Synge in Toronto.

Tucker: Yes.

Halperin: I got rather fed up, in spite of the fact that Eisenhart was the dean and a stern-looking gentleman, and I just didn't go to lectures after a few. The other courses were so casual that I sort of dropped out of them too. I went to Bohnenblust's lectures for a while, but I had had most of the stuff from Professor William J. Webber in Toronto. But the one person whose lectures I had not had before was von Neumann, and I wouldn't have missed any of that.

Tucker: What was he lecturing on that year?

Halperin: Well, I recall the story I heard later. Someone is said to have asked Eugene Wigner, "What is von Neumann lecturing on this year?", and Wigner replied, "I don't know what he'll call it, but it will be Hilbert spaces." Yes, he was lecturing on Hilbert spaces, on the spectral theorem. At first I couldn't understand how you could have Hilbert spaces with complex numbers. I had never heard of such a thing. I asked Malcolm Robertson, "How do you define the inner product if you have complex numbers?", and he told me.

Tucker: Did you participate in the activities that went on, such as those in the common room?

Halperin: Oh yes. The common room was a wonderful place. As I said, we were all poor—all the students were poor—but I didn't see any trace of competition or friction. It seemed to me we were all monks in a monastery, all working with the purest motives to discover mathematics and to share it with others. The common room was a very lively place. Those were the days when refugees were coming out of Europe, and those in mathematics seemed to head first for Princeton, because the Institute and the University's math department were both there. It was a tremendous concentration of talent. There was hardly a day that in the common room we wouldn't see a new face and ask who that was, and the answer would be some mathematician we'd heard of, who was a great researcher.

Tucker: Was [Stan] Ulam around at that time?

Halperin: He wasn't around when I came; he arrived later. In fact, when he arrived I was deputized to show him around. I remember taking him, among other places, to the gym where you could go swimming. I enjoyed swimming enormously, so with great satisfaction I showed Ulam the possibilities of going swimming. He turned his nose up at that; he wasn't interested.

Tucker: I think Veblen had me meet Ulam when he arrived by boat. Might you have come along with me on that trip?

Halperin: No, the first time I saw Ulam was when I was called by Lefschetz and told to take Ulam around and show him the University.

Tucker: Before that I was told to meet him.

Halperin: So you went to New York to find him?

Tucker: He was in Hoboken, as I recall. I think a Polish-American liner had arrived in Hoboken.

Halperin: I see.

Tucker: Have you heard that Ulam died just recently?

Halperin: I heard that just yesterday. I was quite surprised because I had been in correspondence with Ulam. I was out of touch with him for many years, but then I sent him a copy of the 1981 American Math Society Memoir by von Neumann. After that we were in correspondence a bit. I was going to meet him in Washington, but it turned out that I was there the week before he got there. I wrote him after that, but I did not get a reply before he died.

Tucker: When did you start working on your thesis?

Halperin: In those days, as is perhaps still the case, the graduate student was expected to qualify in what were called prelims.

Tucker: Yes.

Halperin: There wasn't much expectancy of getting involved with the research topic until then. Bochner wasn't very happy about that; he once said to me rather vigorously, "You should be working on a problem." I was just accumulating information the first year. I passed my prelims in the fall at the beginning of my second year.

Tucker: Whom did you have on your prelims committee?

Halperin: That was a remarkable situation. The committee consisted of Solomon Lefschetz, H.F. Bohnenblust, and T.Y. Thomas. I had been told that the examination was to start at 3:30. At 3:00 Boni was going by a lecture room and saw me in the room, and he said, "Why aren't you at your prelims?" I said, "It doesn't start until 3:30." "No," he said, "it starts at 2:30." So he took me down to the room, and there Lefschetz and Thomas were, waiting and talking. So my prelims got started. It had gone on for about 15 minutes when Thomas got up and said, "I've got to go to tea," and out he walked.

Tucker: Lefschetz and Boni went on?

Halperin: That's right. It was a very casual affair. It wasn't what I had anticipated. I had thought it would be a pretty rigorous examination. My friend Al Tucker had taken me for a walk and given me a little coaching on how to handle these difficult situations. He said, "If somebody asks you a question, and you don't know the answer, just say, "I don't understand the question put that way." And just wait quietly, and they'll put it in such a way that you'll know the answer." But this never happened. I not only didn't have any trouble, I didn't have any questions that were really probing.

Tucker: Well, I had a hard time with my prelims, because Einar Hille, who started the examination, started at a very low level, and I was unprepared for something so elementary. His first question, I remember, was, "What is the characteristic of a set being infinite?" I didn't know what to say to this. Hille was a very gentle person, so he essentially told me that. After the exam got going into things like Lebesgue measure, I was fine, but I wasn't prepared for so fundamental a question as I was asked.

Halperin: I must correct an answer I gave you to a previous question. My prelim committee did not include Lefschetz; he was on my final-orals committee. The one on my prelim committee was H.P. Robertson. He was really a fine person. He asked me a question connected with differential operators. I didn't know the answer, and he asked me another question. Before I knew it he had led me into talking about the possibilities of differential operators. I found that fascinating; it had never occurred to me to think about differential operators before. He was very stimulating.

Tucker: Did you ever take lectures from him?

Halperin: I don't think so. No, I didn't take lectures from him, now that you remind me. One of my duties as an assistant was to mark papers for somebody, and I was assigned to Dean Eisenhart. After some weeks Dean Eisenhart asked me for a record of the marks, and I said I hadn't kept any record of marks. He was furious. The next thing I knew, I was no longer marking papers for Dean Eisenhart; I was marking them for H.P. Robertson. Well, I marked them for H.P. Robertson, and one student made some gross error like A/B + C/D = (A+C)/(B+D), something preposterous like that. I wrote across the paper, "This is terrible!" Well, H.P. Robertson cornered me the next day and said, "It was terrible, but perhaps this is not quite the thing to tell the student." So after that, I didn't mark papers for anybody.

Tucker: I detoured you from talking about your getting started on your thesis.

Halperin: Well, I wanted a problem, so I first spoke to Boni [= Bohnenblust], and he said he would think about it, but nothing happened. I was going to von Neumann's lectures. I was absolutely fascinated with von Neumann; I still am. I screwed up my courage and one day walked up to him in the hall and said with my heart beating furiously, "I'd like to work on a problem for my Ph.D. Would you give me a problem?" He said, "Very well, come to my office at 10 o'clock tomorrow," and I danced away.

I went to his office, and he gave me a problem. It turned out to be solvable by a simple trick. I solved it, and, I think the next day or the day after that, I stopped him in the hall and said, "You do it this way." He said, "Oh, so easy? Well, perhaps you should do the same sort of thing for differential operators in more than one variable." I did a little, but I didn't find that very interesting. He suggested I read Hadamard's book. Von Neumann obviously had great respect for Hadamard. I looked at Hadamard's book, but couldn't make heads or tails of it, and besides, von Neumann was doing work with operator theory with which I was intrigued. And shortly after that, he invented continuous geometries. That started with such basic notions that I could get right in on the ground floor, which I did. So that's how my mathematical career started.

Tucker: I think that it's really a shame he didn't stick with the first name he had for continuous geometries, 'pointless geometries'.

Halperin: Well, actually he used the word 'continuous'. But he used that word in a way many of us wouldn't agree with. People who worked with ordered sets had invented the notions of discrete, where you have jumps, and continuous, where you don't. But what von Neumann called continuous geometries ...

Tucker: It was the dimension that was continuous.

Halperin: Not only the dimension, but the operations which were continuous. That's really where the word 'continuous' should have come from.

Tucker: Yes.

Halperin: It reminds me of my sister's remark about the decisions of judges: there're usually good decisions made for the wrong reasons. I think the same of the name 'continuous geometry'. I prefer it to 'pointless geometry', but not for the reasons for which it was chosen.

Tucker: Was there anyone else doing a thesis with von Neumann that you know of?

Halperin: Not that I knew of. In fact, it was pointed out to me not long afterwards that it was not proper for me to have done my thesis with von Neumann since he wasn't in the University. He had no obligations to graduate students, and technically I had been out of order.

Tucker: Well, you were not out of order as far as Princeton University is concerned. The only sense in which you were out of order is that you were asking something from von Neumann that you could have only as a favor, not as a right.

Halperin: Of course in those days there was no thought, in my mind or in the minds of people around me, of rights. It was one community.

Tucker: Of course, but I don't know of anyone other than you who is a Ph.D. of von Neumann.

Halperin: I was told years later that F.I. Mautner had written his thesis based on von Neumann's decomposition of rings of operators in Hilbert space. Mautner had done work of representation theory, and he needed von Neumann's work. So von Neumann agreed to publish a paper which he had had in his desk for ten years. I had the impression that Mautner was more von Neumann's Ph.D. student that I was, but I don't really know.

Tucker: Yes, Mautner was an unusual case. He was a member of the Institute for Advanced Study, and then it was arranged for him to get a degree at the University so he could use it for getting a job somewhere in the United States. At that time—and as far as I know it hasn't changed—the residence requirement was one year, so Mountner was a graduate student for one year. I don't know of any other case of this sort at Princeton. He was enrolled only one year. He took the prelims in May, which was the earliest he could take them, and he had his thesis all ready to put on the table as soon as he passed his prelims. He got his Ph.D., but I never regarded him as a bonafide graduate student.

Halperin: Yes.

Tucker: It was all done properly and legally, but he had several published papers to his credit before he became a graduate student.

Halperin: Well, I certainly was not in the situation of being that mature and sophisticated, so I was the beginner directed by von Neumann for the thesis.

Tucker: I'm sure that von Neumann threw off lots of ideas, as he went about, that led to Ph.D. theses. But in your case, I feel that you approached him, and he obliged, and the whole thing was in regular order. At the same time as that, Veblen was supervising theses. Given's thesis was supervised by Veblen.

Halperin: John Vanderslice's was another.

Tucker: Veblen set the style for things, and one of the great things, I feel, about this period is that it was all one group.

Halperin: Indeed it was.

Tucker: There was no attention paid to formalities, just as you didn't pay attention to the question of whether it was appropriate for you to go to von Neumann, who was not a professor at the University, for a thesis topic.

Halperin: I remember another occasion after I had been working with or under von Neumann for quite a while. He and F.J. Murray had developed their theory of rings of operators, which is now called von Neumann algebras, to an exciting state. I was talking to von Neumann about what I might be doing, and I said, "I would really like to work on what you and Murray are doing, if I knew where you were at." He said, "That's fine, I'll arrange with Murray that we get together tommorrow at 9:00." Well, it happened that the next morning I was to get a ride back to Canada. I was going back home for Christmas, and one of the boys who had a car was taking several passengers. So I explained to my older friends that I was sorry, but that I had an opportunity to listen to von Neumann and that I wasn't going to give that up even for a ride back to Canada. They appreciated this remarkable opportunity and said, "You go ahead, we'll wait for you." And they did.

Tucker: By the way, where is Blewett now?

Halperin: I saw him just yesterday. We had supper together. He remarried a little while ago. He was at the Brookhaven Physical Lab on Long Island for many years. He finally became vice-director of the lab and was very heavily involved in their high-energy work. He retired from that some time ago and has been acting as a consultant to different groups. He has some role at the University of Chicago and at Argonne Labs, and he has been invited to go to Taiwan within the next six months to give them some advice. So he's still very active.

Tucker: And living on Long Island?

Halperin: Well, he has his own home, the home he's had for years on Long Island, and his wife is historian at the Institute for Physics and has an appartment in New York. So they're usually in New York during the week and on Long Island on the weekends.

Tucker: I remember him very well, but in particular because he made a Klein bottle for me, and I've still got it. It's apparently quite a difficult thing to do because the shape causes awkward strains—it is not symmetrical. So it breaks very easily when it's being made.

Can you tell me about some of your fellow students? What do you remember of them?

Halperin: They're almost like members of the family. Norman Levinson was a little younger than I was, but he already had quite a brilliant career. He had worked with Hardy, and he was very sophisticated in his outlook.

Tucker: Because he was post-doctoral?

Halperin: Yes. He came, I think, in my last year. I was more or less aware that I already had a Ph.D.; it was just a matter of going through some formalities. I wondered what I would do the following year. Somebody suggested that I apply to Yale for their Sterling Fellowship. I told Norman that I didn't know what in the world I would say. He said, "Come with me, and I'll tell you what to say." He told me what to put in my letter, and I got the fellowship. I got to know Norman well after that.

Tucker: Well, Norman wasn't here at the time that the picture you spoke of, the picture of graduate and post-graduate students at Princeton, was taken and in which you thought you identified Norman Levinson.

Halperin: That's very odd.

Tucker: That picture was taken in the spring of 1932, and Norman wasn't here at that time.

Halperin: Yes. This is the second instance when I simply can't believe what I hear. I go by what I see, and I look at that picture and that's what I see.

You were asking me about other people. There's R.J. Walker. I once asked Walker to tell me what he was doing in algebraic geometry. So we went into a lecture room, and he picked up a piece of chalk, and his voice rose about two octaves. I couldn't believe what I was hearing. It was a voice I had never associated with R.J. Walker. We went to New York once together. We stayed at the Y. It was Easter Sunday, and he wanted to go to Saint Patrick's Cathedral. I went with him, and I enjoyed that very much.

Then there was Nathan Jacobson, who always wore white shoes. You could tell Jake a mile away. He never ran. Other people might hurry, but not Jacobson. Oh, I remember them all. There was Ball. (That was his surname.) He got rather overloaded at a party that von Neumann and Veblen gave.

Tucker: I don't think he would get overloaded at a Veblen party, but he would at a von Neumann party.

Halperin: No. It was a von Neumann and Alexander party, I remember now. Ball wasn't perfectly coherent; it was just that he had consumed a large amount of beer. When von Neumann came through, Ball stopped him and said, "Is there some place where I can go to the toilet?" Von Neumann, with an enormous smile, said, "We have a room especially for that purpose."

Tucker: That was when von Neumann was living at the corner of Stockton Street and Library Place in a great big red-brick house.

Halperin: I guess so; I can't remember that. The only place von Neumann lived that I can remember was some place where the living room was at a lower level, two or three steps down from the entrance. That's all I remember about that place. There was the later residence at 26 Westcott.

Tucker: Westcott Road.

Halperin: I remember that well, because later on von Neumann was working out the theory of games and I would go to his house on Westcott Road in the morning. He would go over ideas or create them, and fill my head full of this stuff for an hour and a half. Then he would tell me to come back the next morning. So I had the most remarkable opportunity to get working on the theory of games before anyone else.

Tucker: When was this?

Halperin: This must have been when we came back from the meeting at Duke University. Before the meeting (in 1937), we had been talking about continuous geometries, and I made a remark that it was a pity that we didn't have something that corresponded to an orthonormal basis, a sort of continuous orthonormal basis. That seemed to set something off in his mind, and he started talking a mile a minute. This was before Durham. I would meet with him, and he would talk furiously and I would take notes.

Then at Durham we were at a dance. I was dancing, but he was not dancing. He was standing on the side of the room, very excited. As soon as he could, he called me off the dance floor and said, "I have solved the difficulty and brought the paper to a head." He said we should get together to finish it off. I said, "You know I'm not collaborating with you on this, I'm just listening. You shouldn't feel that I'm a partner to this. You just go ahead." He was relieved, and he went ahead and wrote the paper which was eventually published years later as the memoir, "Continuous geometries with a transition probability", in the American Mathematical Society series of Memoirs.

Tucker: I hadn't realized that at that time he was working on the theory of games.

Halperin: Well, you're right, I have mixed up the dates. When he talked about the theory of games was not on that occasion, but after he came back from Seattle. He had given some popular lectures at Seattle in the evening on the theory of games.

Tucker: What year was that, approximately?

Halperin: I know exactly when that was: the summer of 1940. I was up in Canada; I had gone there in '39 to teach at Queen's. I came back in the summer to Princeton to visit. Von Neumann had told me earlier that he was going up to Seattle for the summer as a visiting professor, and I had decided—I had a car by then—that I would drive there and spend the summer with him. He gave some popular lectures on games, and I saw quite a bit of him in Seattle at the University of Washington. It was after we both got back from Seattle that he talked to me about games. In the fall I saw him at the meeting, was it at Dartmouth?

Tucker: Yes, Dartmouth.

Halperin: And it was clear to both of us that I wasn't doing the theory of games.

Tucker: Was he in contact with Oskar Morgenstern at that time?

Halperin: Not that I knew of. I didn't know anything about Morgenstern until I actually saw the printed book. What he was doing in these morning talks he was giving to me was working out the theory of many-person games.

Tucker: Yes, that was what interested him.

Halperin: Well, at Seattle he had talked only about two-person games. The other types were not even mentioned.

Tucker: You see, I had no connection at that time with the theory of games. I didn't become involved with the theory of games until 1948. Of course I had had a lot of dealing with von Neumann one way and another. Indeed, just at the end of the war period I was for a few months employed on the von Neumann computer project. I was working with Deane Montgomery and Bargmann. They stayed on with that project and actually published papers—papers of von Neumann's, but they were listed as co-authers. It was a period when there was no teaching for me to do at the University because the students hadn't returned and the Army-Navy programs had been discontinued. There was nothing for me to do for a few months. I don't know who arranged it—it might have been Eisenhart—but von Neumann gave me the job because he thought of me as a combinatorial topologist. He gave me the job of looking at schemes for generalizing finite differences from one dimension to higher dimensions, where, of course, there are many more ways in which you can set up mosaics to partition things. I was just barely getting started on this when suddenly, bang, my teaching began again, and I didn't do more for the project.

It was to some extent because of the things I had done during the war that I became interested in computation. I had never been involved directly in it. So that was the thing that I knew von Neumann was doing in the mid '40s. It was a sudden thing that happened to involve me in the theory of games, something that had no bearing at all with my previous contacts with von Neumann, so it interests me to hear you tell that he had been giving lectures at Seattle in 1940 and that he had talked to you. Was it before or after Seattle that he talked to you?

Halperin: It was after Seattle. It was in the fall of that year. It was my impression that he wasn't just talking about it, he was doing the work, and that the reason he sent me home after each morning was that he wanted to think alone for a while.

Tucker: I somehow had the impression that it was the probing of Morgenstern that reawakened his interest in game theory. He had written a fundamental paper in 1927 or '28 that appeared in *Mathematischen Annalen.* In that he proved the minimax theorem, and he actually sketched out his ideas of n-person games.

Halperin: I didn't realize he had mentioned n-person games before then.

Tucker: Yes, and the point of the minimax theorem is that this is the tool by which you examine the n-person games. You simply think of one group of players as forming a coalition, and then you think of what the game is with those players as one player and all the rest working against them. Then this artificial two-person game is zero sum, because you make it so that it is competitive. This is what attaches a so-called value to the coalition. Each coalition has a value computed in this way, and this is superadditive.

Halperin: I have sheets of paper on which is mostly my handwriting, but also some of his, with triangles, in which he was developing coalitions and their values. That was the first time I heard the word 'coalition' used in this work. I realized I was right at the beginning of something very hot, but it wasn't the sort of thing I felt comfortable with. I was fascinated by whatever von Neumann did, but it turned out I wasn't going to be able to contribute or even to have reasonably interesting thoughts about the subject.

Tucker: Were you involved with him in computational mathematics?

Halperin: Not at all. In fact, the first person I knew of to be involved with computational mathematics was Murray himself. Murray wrote that little pamphlet, published at Columbia University, on the use of gears and the like. I didn't know that von Neumann had the slightest interest in that field until he came to lecture at McGill University in 1945, when the Canadian Mathematical Congress was launched.

Tucker: I remember that particular meeting. I gave a talk on the topological properties of disk and sphere.

Halperin: I remember that talk, a very elegant talk it was too. I told you so at that time.

Tucker: But I made the mistake of telling what I thought everybody would understand was a joke, but Richard Brauer didn't.

Halperin: I don't remember that.

Tucker: I told that on one occasion I was going on the train with Lefschetz and Oscar Zariski to New York to attend a one-day meeting of the American Mathematical Society. They got talking about a paper they had both read with interest on algebraic topology. Lefschetz made some remark that he wasn't sure whether that paper should be regarded as algebra or topology. Zariski, to sort of tease Lefschetz, said, "How do you distinguish between algebra and topology?" Lefschetz came right back, "Well, if it's turning the crank, it's algebra, but if it's got an idea in it, it's topology." I told that story just to liven things up a bit, but Richard Brauer came up to me afterwards and said, "Albert, you shouldn't say things like that."

Halperin: I once asked Brauer why algebraists weren't paying more attention to the work of John von Neumann, who I thought was opening up a tremendous chapter in the theory of algebras. He said, "Well, he uses an adjoint, some sort of star operation, that doesn't appeal to algebraists."

Tucker: We've covered a lot of territory. Do you think of some other reminiscences or anecdotes?

Halperin: Well, I won't say that I was a coward in those days, but I was certainly overwhelmed by the tremendous ability, competence, genius if you like, of the men around me. So I rarely ventured to ask questions that weren't strictly technical questions. But sometimes when I was with von Neumann, I went a little beyond that. Once we were talking about geometers, and I said, "Your work certainly means that you should be among the great geometers." He said he didn't regard himself as a geometer. Of course I thought continuous geometries were really geometry. I said, "Well, what would you classify yourself as?" He said, "An algebraist." On another occasion Hilbert's name was mentioned, and I said tentatively, "Was Hilbert a great mathematician?" He looked at me and said, "A very great mathematician."

Tucker: To me, von Neumann was an analyst.

Halperin: That's an interesting question, how you think of yourself or other people. I would say that von Neumann was a magician, a magician in the sense that he took what was given and simply forced the conclusions logically out of it, whether it was algebra, geometry, or whatever. He had some way of forcing out the results that made him different from the rest of the people.

Tucker: Did you have much in the way of dealings with Hermann Weyl?

Halperin: Not much, but occasionally we would be walking down the street together. He was very pleasant. He asked me where I came from. I said, "Canada." He said, "Oh, I've been to Winnepeg, a very interesting place." He talked about Winnepeg. He ran a seminar, after he came to Princeton, on different topics in mathematics.

Tucker: Yes, it was sort of a German seminar.

Halperin: That's right. He called for volunteers, and foolishly I volunteered to take one of the topics. It was on complex variables, the sort of stuff that L.V. Ahlfors and Alfing were doing. So I brightly wrote off to these people and got a big stack of papers that I was going to read. I just wasn't competent to do it. When the time came to give my topic, it was very badly done and Weyl was not impressed at all. But he was not very severe with me, though he could have been.

Tucker: I feel that Weyl and von Neumann were the greatest mathematicians that I have known. Weyl had tremendous scope. He seemed to have a good grasp of everything: algebra, geometry, mathematical physics, topology, analysis. In fact, I don't think that since Weyl there's been anyone that you could say was a master of everything.

Halperin: Yes.

Tucker: I wouldn't say that von Neumann was a master of everything. But in the areas he worked he was intensely creative, though except perhaps for his Hilbert-space work his work somehow seemed to me to be bits and pieces.

Halperin: That was not my impression. As you spoke, I began to think of the people I have had the opportunity of being near of whom I would have said they were broadly educated in a tremendous way. Certainly the two you named, Weyl and von Neumann, are there. Another person I would have named is Hans Freudenthal, who always amazed me by his knowledge of all sorts of things. The distinction I would make between Weyl and von Neumann—I've made it for a long time—is simply this: if you had worked in an area and knew it pretty well, then Weyl would really take you around, but if you hadn't been there, von Neumann would introduce you to it in a way that would make you really appreciate it.

Tucker: Yes.

Halperin: Von Neumann seemed always to start at the beginning of a subject. He rarely quoted deep theorems of other people's, whether it was continuous geometry or operator theory or logic. Somehow his papers began with elementary concepts and works—even his theory of games. Whereas Weyl was profound in what he said and wrote.

Tucker: Yes, and a bit pompous in presenting things, at any rate, very dramatic.

Halperin: Yes, he had a very hard voice. You could hear it all over the building. There was no hesitancy in his thinking. There was no hesitancy in von Neumann's either. I was at a meeting of physicists, and von Neumann was sitting at the back of the room apparently reading the newspaper. At the end of the lecture there was a discussion of about 20 minutes concerning some difficulty. Von Neumann put down his paper and pointed out exactly the difficulty and how to get around it. This was physics, so I'm not prepared to say that von Neumann wasn't a master of everything.

I wouldn't want to give the impression that I am an expert on von Neumann. I'm certainly pretty well dedicated to what he did. I don't think it's appreciated how kind he really was. I can tell you a story that exemplifies this.

He came down the hall laughing. I met him, and he explained why he'd been laughing. He'd been in Europe, and a student there had pleaded with him for some guidance in writing a thesis. So von Neumann had given him some ideas. This student later came back and asked von Neumann to do the calculations. He thought that was a good joke. I don't know whether he did the calculations or not. Most people would have been incensed and have complained about being abused. Of the people whom you'd feel should never be abused, von Neumann was certainly one. Yet he didn't take it that way.

Tucker: You mentioned in a letter you once wrote to me von Neumann's large car.

Halperin: Yes. Well, I was on a trip with von Neumann where he drove half the time and his wife drove the other half of the time. Their driving was perfectly normal, so all these stories about his saying that trees stepped out onto the highway and collided with his car, to me are just stories. What I wrote to you in the letter was that as a student, I would come to Fine Hall in the morning and look for von Neumann's huge car, some sort of a convertible, I believe. And when it was there, in front of Palmer Lab, Fine Hall seemed to be lit up. There was something in there that you might run into that was worth the whole day. But if the car wasn't there, then he wasn't there and the building was dull and dead.

Tucker: Do you know any other mathematics school you could compare the Fine Hall group with? Now, you were subsequently at Cambridge, Massachusetts. Did you find a similar atmosphere there?

Halperin: Yes, I taught for two years at Harvard as an instructor. They had no meeting place like the common room at Fine Hall, and most of the people I ran into at Cambridge were not mathematicians. The friends I made were mostly in other fields. I would occasionally see Marshall Stone. Once the great G.D. Birkhoff had us to his house for tea. I saw W.F. Osgood from time to time, but there were mostly occasional meetings.

Tucker: Yes.

Halperin: In the building where I gave my lectures there was a small room. Those of us who were instructors would run into each other there just before lectures. So I got to know Everett Pitcher, and Marshall Hall with his very Harvard accent.

Tucker: I was at Harvard for a term in 1933. I felt the loneliness of the place for mathematicians, as compared to Fine Hall. I was offered a Peirce instructorship at the instigation of Marston Morse, who very much wanted to have me around because I was able to provide him with some information about singular homology theory which he needed for the calculus of variations in the large which he was working on. Indeed, the time I spent there I spent as Morse's assistant, rather than working on my own ideas in topology. I was quite impressed with this offer, at the instigation of Morse, but made by G.D. Birkhoff. I said I would like to think about it, and the next weekend I came to Princeton to talk to Lefschetz and Eisenhart about it. They cooked up a research instructorship which they offered me at Princeton, and I gladly took that, although the salary wasn't as large as that offered by Harvard. I just longed for the life of Fine Hall and the common room and this intense atmosphere that was friendly as well as being highly charged mathematically.

I also spent the summer of 1933 at Eckhart Hall at the University of Chicago. Even though that was a building for mathematicians, the common room was really used there only about once a week.