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The Princeton Mathematics Community in the 1930s
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and EDWARD J. McSHANE

This is an interview of William L. Duren, Nathan Jacobson, and Edward J. McShane on 10 April 1984 at the University of Virginia. The interviewer is Karen Parshall.

Jacobson: I'd like to start with my introduction to the Princeton mathematics department. I came there in 1930, in late September I believe is when the term started. I had come from the University of Alabama, and of course it was very exciting to go to Princeton, which had a reputation of being one of the three greatest centers of mathematics in the country. It probably was the greatest at that time. The department had a meeting of the new graduate students with all the faculty, which included James W. Alexander, Oswald Veblen, Solomon Lefschetz, and J.H.M. Wedderburn. I think all came to this meeting. We were introduced to them, and then they all offered us advice as to how we should conduct ourselves to become mathematicians.

One of the things I remember most clearly was that Alexander's advice was to tell us how to get into the library in case the building was locked. We didn't have keys. The building was Palmer Laboratory, which was the building that was used before Fine Hall was completed. a year after that in 1931. Anyway, his advice was that we could climb up to the second-storey window. Alexander was a famous mountain climber; he even had a method of climbing Long's Peak (Colorado) named after him. He used to climb the Graduate School tower and do various other things of that sort. But that was his advice for us students. It started us out properly in our careers at Princeton.

The other thing that I remember in the early days was that we had tea. I think it was not regular; I think they had it on days when they had mathematics colloquia. The institution of tea that goes with academic life nowadays—the practice of having tea before a meeting of one sort or another—probably started at Princeton in Palmer Laboratory. We had a room upstairs, with a kettle and a bunsen burner to heat the water—Palmer Laboratory is a physics laboratory. The practice of having tea was introduced by Veblen, who was very much of an Anglophile. He dressed in English clothes; he had two four-buttoned suits as I remember, one green one and one dark one, that he wore on important occasions. He thought we ought to have tea like they had in England. After that they used to have it in Fine Hall every day at 4:30.

Fine Hall itself was a very impressive place. It was a wonderful building. It cost rather little compared to what buildings of that sort would cost now or compared to what the new Fine Hall cost, but it was a very luxurious building. I don't think they anticipated the growth of the University, and they certainly didn't anticipate the creation of the new Institute. It was just right for the population they had when they started it, but soon became overcrowded. It had very luxurious rooms for the professors, the oak-panelled offices. The oak was imported from England and cost quite a lot of money, a fifth of the cost of the building. It was furnished very well. I think Mrs. Alexander [wife of Professor J.W. Alexander] was responsible for selecting the furnishings in the offices and also most of them in the common room.

There were two common rooms. One was used normally. The other one was used on special occasions. It was called the Professor's Room, and it's the famous one that has the inscription by Einstein over the fireplace, "Raffiniert is der Herr Gott, aber boshaft ist er nicht." It also had some very interesting designs, in lead I guess it was, on the windows, like the Moebius strip and various other mathematical things. I think this room is now used by another department, Near Eastern studies, that has of course no interest in this, and they may have converted the common room to offices.

Princeton was a great place. Because it was a reasonably small department, it was very cohesive. Of course there were rivalries, as there are in any kind of a society, and I think there were also jealousies by the people who did not get selected for the Institute of those who were. Of the first faculty of the Institute, Einstein was sort of the key person.

Veblen, I think, had to do with the selection. He was the first head of the School of Mathematics. The Institute, as you probably know, was founded by Abraham Flexner. I don't want to go into why he did this. I think he was influenced by German academic institutions, and he got money from the Bambergers and Mrs. Felix Fuld, who was a sister-in-law, I guess. The Institute was housed at Fine Hall with the mathematics department for a number of years, but then it was clear they didn't have room enough. So they moved and built a building of their own, Fuld Hall. The original group was, I

think, Einstein, Alexander, Veblen, and von Neumann. Von Neumann had been at Princeton as Jones Professor; he shared the Jones Professorship of Mathematical Physics with Eugene Wigner. Wigner stayed at Princeton until he retired, so he was there for a long, long time. He was also distinguished; he won a Nobel Prize. Von Neumann moved to the Institute, and a year or two later Hassler Whitney, Hermann Weyl, and Marston Morse came. That was probably the faculty for a number of years.

As you know, they had visitors, younger people who came from all over the world. It was a really great place, and Princeton itself before the Institute was an extremely exciting place mathematically. It had contact with European mathematics, more closely perhaps than the other great places in this country. I think Chicago by then was somewhat on a decline, but Harvard I think was still quite strong, not I think in the class of Princeton. Princeton had this close European contact through von Neumann and Lefschetz. As a result a lot of important work was published in the *Annals of Mathematics*, which was Princeton's mathematics journal. So we heard about things at Princeton very early, soon after they were discovered. For example, something published in the *Annals* was Haar's introduction of a measure which started a new era in the study of topological groups and which led to von Neumann's solution of Hilbert's fifth problem for compact groups. Von Neumann also was responsible for giving perhaps the first exposition of the Goedel inconsistency theorem, which became known at Princeton immediately after Goedel discovered it. Another new thing which came was L.S. Pontryagin's duality theorem. This was something which was being worked on by a number of people in the United States, by [Norbert] Wiener, and by Alexander and Leo Zippin, who was his assistant at the Institute. They actually got part of the result—for the case of the duality between discrete groups and totally disconnected groups.

There were very many interesting things at Princeton, and there were also a lot of great visitors that came through at that time. I learned topology really from Paul Alexandroff, who was there the second year I was there, in 1931. Lefschetz was a dynamic person, who had very distinguished students, like Albert Tucker, Norman Steenrod, and a number of others, but you never really learned anything from Lefschetz. You got inspiration from Lefschetz, but you had to learn things on your own or from somebody else. Alexandroff was a great source of information who gave very fine courses. He was also a very interesting man. He was bald; he had a little hair, which he shaved off, so he was really completely bare of any hair. He loved to rub his head backwards. That apparently helped him think. Whenever he had to make, in class, the most trivial calculation, he had to do it beforehand and bring it into class. Sometimes to be on the safe side when he had to choose a delta for a certain epsilon, he would take delta to be epsilon over 1000. He also was quite athletic. He would go swimming in Lake Carnegie no matter what the weather was. He was there the second semester, and it was quite cold. On Sundays he would take his *New York Times* and walk over to Lake Carnegie, and he would sit there reading the *Times* and occasionally go in swimming.

Princeton was a very important institution, and so was the Institute. They had a great deal to do with the subsequent development of mathematics in this country, and they continue to have important influence because they have all these very fine visitors who came from all over the world. They have perhaps 100 visitors nowadays. There were not so many visitors then, but they were still very high-order people. I might mention that Polya taught a course there that I took. I actually distinguished myself by solving a problem on matrices with non-negative entries. [J.A.] Schouten was there. He was an interesting person who had a very complicated system of notation. The first day of the course he handed out about 70 mimeographed pages of notation. Wilhelm Blaschke was there. He was a notorious character who later wrote a nasty article about Princeton, referring to it as a "Negerdorf in Dollarland" where the professors had no students, but laid their golden eggs, or something like that. [I think I remember the remark quite accurately, "ein kleines Negerdorf, wo viele Professoren liegen ihre goldene Eier' vor fast keine Studenten"—McShane.]

Harald Bohr was there, and he had a wonderful course in almost-periodic functions. It was so good that you remembered nothing of it after he was through. It was so well organized and so well presented. You didn't have to do any thinking. You came there, sat, and listened to him. It was great while it lasted, but when you finished you didn't remember anything about it. At least that was my experience.

Von Neumann also lectured at the Institute, as did Hermann Weyl. Weyl had a tremendous influence because he introduced Lie theory in the United States. He gave a course on representations of Lie groups. Richard Brauer had been designated as his assistant. But Brauer hadn't been able to get there; I think maybe he hadn't been able to get out of Germany or wherever he was. He couldn't get there till the second semester, so they asked me to be Weyl's assistant. I took the notes for the first semester's course, which was on structure theory as it was known in those days of semisimple Lie algebras. The interesting thing is that at that time the term Lie algebra had not been used. It was used for the first time in connection with this course; and I think according to Bochner, it was I who suggested it. What is important about that is that Lie algebras became an independent discipline independent of the underlying Lie theory, the Lie theory of continous groups, that motivated it. It has developed in its own direction, and very recently some marvelous work has been done on the algebras of characteristic p by a couple of people, one of whom is [R.L.] Wilson at Rutgers and the other one was [Richard] Block at the University of California at Riverside. Anyhow that was one of the things that started then. This seminar that Weyl gave involved H.S.M. Coxeter, who did some beautiful geometric work concerned with groups generated by reflections.

Adrian Albert was there at that time, and he got interested in Lie algebras and nonassociative algebras; Brauer came the next semester and, as part of his seminar, solved an outstanding problem in

determining the Betti numbers of classical groups. At the same time this was done independently by Pontryagin using a geometric method—Brauer's was algebraic. There was also the introduction of almost-periodic functions into group theory, due to Bochner and von Neumann. Then Weyl did a course on invariant theory; he was hoping to revive the subject. I think he didn't succeed, but recently there has been a tremendous outburst of activity on invariant theory. A lot of people are going back and studying Weyl's work. They are finding that some of the things they need are in his book on the classical groups.

Alan Turing was there, Turing of the Turing Machine. He was one of the people who worked with von Neumann, and I think he developed the idea of the Turing Machine at Princeton, or he had it just before he came to Princeton. Today it is recognized as one of the greatest achievements of mathematics. Hodge came. Hodge is known for his tremendous work in algebraic geometry. It was amusing to listen to him lecture—he wasn't a very good lecturer—because he got incessant kibitzing by Lefschetz. Lefschetz would try to say something, but he had a very good way of putting Lefschetz down. He'd say, "Yes, yes I know, I know, I'll come to that."

The common room was noted as a place for games. Someone said—maybe it was you, Jimmy McShane—they could produce a champion in any game that was played sitting down. They had Kriegspiel, which was blindfold chess, with an umpire. Paul Erdos was one of those who used to play that. There were all kinds of silly games like pitching poker chips to see how close you could get to the edge of the big center table. They also had chesspiece stacking. Two competitors in that were G. Bol—who I used to walk around with—he was 6' 6-1/2" tall—and Hassler Whitney. Whitney won that competition. The idea was to get all the pieces stacked on one rook. The method was to place the rook upside down, place the two knights across it, and then build things up. They used to work on this for hours at a time. Immediately thereafter the game was generalized by someone claiming that he could put all the chesspieces and the *board* on one rook. That of course is easy.

Paul Adrian Maurice Dirac and Eugene Wigner introduced *go* to Fine Hall, and maybe even the U.S. It was a game that was very popular. Chess not so much so. Anyway, they were the first to play it. Dirac would generally win very quickly. He would annihilate Wigner, and Wigner would say, "Oh, I must take "zis" (this) back. I have to improve my technique."

There were also big solitaire games. And then they actually had money games. They played poker—all night poker games. One of the best poker players was Henry Whitehead; he was extremely good. Sometimes in the mornings Eisenhart would walk in, just after the poker game was finished. All these guys would be running to their classes. They would have to teach a class at 8 o'clock after being up all night playing poker. Whitehead kept his love of poker all of his life; I think he played at Cornell a few days before he died. He died walking

across the golf course in Princeton just a couple of days after he had been to Yale. He was a very interesting man and one of the most distinguished students that came out Princeton. As a student he worked with Veblen.

I must tell you also this: when I came to Princeton, the math department of the university was divided into two practically armed camps: the people who were in local differential geometry (this group was headed by Luther Eisenhart, Veblen, Tracy Thomas, and Morris Knebelman) and the people who were in topology (this group was headed by Alexander and Lefschetz.) At that time topology had just begun to be called that. It had previously been called analysis situs. Veblen had written a book in the '20s called *Analysis Situs*. When a student came there he was grabbed by one school or the other. I guess I was a maverick. I became an algebraist, studying with Wedderburn. That was a rare thing; he had only about four or five students.

Whitehead was part of the differential-geometry group. He wrote a thesis together with Veblen called "Foundations of Differential Geometry" or something like that. It became a Cambridge Tract, and the main question they considered was "What is geometry?" I think that they were reacting against what had previously been thought to be geometry, namely what Felix Klein in his Erlangen Programme had said geometry ought to be, the study of the invariants of a group of transformations. But it was clear this was not suitable for geometry as it had developed in the '20s and '30s; a broader definition needed to be given. Well, they ended up saying that geometry is what geometers think it is. No one could argue with that. What happened then was quite interesting. Whitehead went back to England. He was at Oxford, I think until he died, and he was one of the most important mathematicians in England. But he switched to working in topology. I don't remeber what he did first; I think it had something to do with triangulations of manifolds. He was notorious for being sloppy and making lots of mistakes. Nevertheless he had tremendous insights and did wonderful things, including important work in algebra, which turned out to be cohomology theory later on, cohomology of Lie algebras and associative algebras. These were later made abstract and further developed by Gerhard P. Hochschild, who was also at the Institute at one time a number of years ago. [Hochschild was a graduate student at Princeton University; he received his PhD. in 1941.]

There were many interesting anecdotes about Whitehead. He was a very colorful character. When he left Princeton to go home he had all his paraphernalia—skiis and everything else—on his back, and he was singing a song, "The cowboys lament" or something like that. He was completely uninhibited. He wore a jacket that was practically in shreds; the lining was hanging down. Mrs. Eisenhart—Eisenhart, who had been Dean of the Faculty and Acting President, was then Dean of the Graduate School— saw him and said, "Henry, aren't you ashamed of yourself, to wear a jacket like that with your position?" He had the title of Commonwealth Fellow or something like that. She said, "If you can't afford to get another one, you bring it to me and I'll fix it for you." He did just that.

Well, I think I've rambled enough, but Princeton is a place that one remembers. I should say something about Emmy Noether. Emmy Noether came to Princeton and gave a course around 1935. She died about a year and a half after going to Bryn Mawr. She was a marvelous person. Also unforgettable. She, like Veblen, had two outfits; I think they were sort of jumpers. One was a shiny blue one. It was so shiny, that you could almost see yourself in it. People used to call her Herr Noether, but that wasn't correct. She was very, very feminine. She had a tremendously strong maternal instinct. She even went back to Germany in Nazi days to see her former students; it was a very bad time to go. And at Bryn Mawr she immediately developed students.

She was really a wonderful person. I got to know her very well. She was very friendly. Whenever she visited Princeton, she would wear her best dress. She lectured to mostly men; I guess there were only men in those days at Princeton. Oh, I could tell you some funny stories about her. She was used to using a sponge to erase the blackboard, but she never waited for the water to dry. She would write on the wet part, and then it became impossible to erase it fully. One time she had written something below, and she erased above. The water was trickling down, and she didn't know quite what to do to stop that drop of water that was going to destroy her formula. So she blew at it.

Einstein, of course, was the key person when he came. An amusing thing happened on his arrival at the Institute. It was a little bit after the beginning of the term. Reporters and cameramen descended on Princeton, 20 or so people, also from the movie news services, and with all kinds of equipment. They wanted to get Einstein to walk, while pretending to be lecturing. They were going to film this and send it all over the country, I guess. But Einstein didn't feel like being interviewed or being photographed, so he locked himself in his office at Fine Hall. Well, it got to be noontime and he was getting hungry. The question was how was he going to escape? So Veblen pulled a fast one on the reporters. He got them all into the big lecture room downstairs and said, "Wait here and Einstein will come in and answer your questions." Instead of doing that though, Einstein escaped through a side door and went home. They were very angry. They were going to publish this—the fact that they had been fooled by Professor Veblen—but I guess they didn't really.

One of them, though, stayed around. I think he was science editor of the *New York Times*. He was persistent. Finally, he went to Einstein's house and did get an interview. At that time Einstein had already begun not to dress. In his younger days, when he was at Zurich, he was dapper and very well dressed, with wing collars and such. But when he got to Princeton, he was completely uninterested in his appearance. He would go around in tennis shoes and walk across the campus eating an ice cream cone. But he was a wonderful person; he always smiled and had a twinkle in his eye. We were introduced to him when he came. There was a reception in the faculty room at Fine Hall. Mrs. Einstein—I think maybe it was his second wife—made some

concoction that everyone tried to avoid. Well, I think that's enough of my rambling.

Parshall: Do you have anything to add, Mr. McShane?

McShane: A few trivia, partly inspired by what Jake said. Bill Duren gave me a puzzling question yesterday: "Jimmy, why did you go to Princeton?" After I got a degree from Chicago in calculus of variations, I got a National Research Council Fellowship. It was, of course, nice to have the honor, but it was even nicer to have a salary back in those days. Those were lean days, that's all I can remember. On the same fellowship I went to Harvard because Marston Morse was there, and to Ohio State because Tibor Rado was there. But there wasn't a single person at Princeton working on the calculus of variations.

As to why I went to Princeton, I remember G.A. Bliss, I believe it was, talking about the general excellence of Princeton as a university. Perhaps my clearest recollection is of a bunch of the students, including, for example, Nathan Jacobson. We were asked to sit around in what by modern standards were slum conditions in Palmer Laboratory. Let me give you an idea of the conditions. I liked to work at night. After everyone else was gone, I'd stay in the library and work until the small hours, roughly breakfast time, then get a meal and go to bed. One day I went to the main library and said to the woman behind the desk, "I have a message of great import that should be carried to the Head Librarian, if not to the President of the University." She said, "What is that?" I said, "In Palmer Library there are rats." She answered tranquilly, "Here there are rats." And the conversation ended. Those were the conditions in Palmer.

The students used to write every year some partly scurrilous quatrains about the faculty. There was one about Eisenhart: "Here's to Eisenhart, Luther Pfahler,/ At Mathematics, he's a whaler,/ He's built a country club for math/ Where you can even take a bath." This was a reference to the fact that there were showers in Fine Hall.

That was a very interesting bunch of students. And the faculty was everything Jake has just said. I can remember best going to some seminars, one by Lefschetz and another by Veblen. In the Veblen seminar was brought up the question that Jake mentioned, "What is Geometry?" We spent, I believe, three solid weeks trying to hit on a satisfactory definition of geometry, but we didn't come up with a nice definition, because almost every definition given by geometers could be twisted around to include all of mathematics. Anyhow, it was quite an interesting seminar; I learned a lot from it. Lefschetz was remarkably receptive to new ideas. A student would say, "But can't you do it better this way?", and make a proposal. Lefschetz would say, "Oh yes, that's a much better idea than mine." He would drop what he was doing, follow the other fellow's idea, and find it wasn't nearly as good as Lefschetz's idea—it just seemed better initially. He never resented an interruption and was always willing to work with a suggestion by a student. I remember walking with him once into his office. He looked

at the table, not the desk, but the table top, and in large letters wrote in the dust, "Dust this!" Conditions were never the best.

Many people there, in particular the students, were interested in the then-modern physics. Benedict Cassen and Banesh Hoffman were there; they always managed to bring the physics into it. That was before von Neumann had showed up. But they talked about the physics without quoting a single theorem I ever used again for the rest of my life. I may have; I don't remember. The attitudes of the faculty and students there influenced me very much.

After I finished my two years as a National Research Fellow, I had the good luck to be invited over to Germany to translate Courant's *Differential and Integral Calculus.* The financial situation of the time is well indicated by the following. Bliss called me in one day and said, "Mr. McShane, this year we have had one request for a suggestion for someone to fill a mathematics vacancy. It's a state teacher's college. I did them and you a favor by not recommending you." So I went over to Germany. This was in 1932, and in the middle of that academic year Hitler came into power. Goettingen then stopped being Goettingen. Jake's comments about Emmy Noether show something of the kind of person she was. Being Jewish, she was deprived of a position, which, since she was a woman, was a low position. She was not allowed to come to the university to teach classes. So she invited her students to come to her apartment and she lectured them there. She got no pay for it and even endangered herself by committing the crime of imposing Jewish mathematics on Aryan students. She got away with it, and, as Jake said, came over to Bryn Mawr. I guess it was either one or two years later; I've forgotten the exact dates. She became immediately a very much loved person. She was not only tremendously brilliant, but also totally likable.

At the end of my year in Germany, I received an appointment at Princeton in a rather unusual way. In fact, I was given two offers. Cohen at Hopkins wrote me a letter offering me an instructorship, which I would have accepted. But while that letter was crossing the Atlantic, Eisenhart sent me a cablegram offering me an instructorship at Princeton. By return electricity, I answered the cable, "Yes!" So I came to Princeton and stayed there. Besides the difference of having Fine Hall instead of Palmer Hall, there was, beginning about then, a very different feeling because Princeton was becoming flooded by people fleeing Hitler's regime.

The common room was, as Jake said, very much the social room. At all kinds of odd times you could see somebody playing *go,* or [Howard P. (Bob)] Robertson and somebody else playing this double solitaire game, or two men at one end of the tremendously long table in the middle of the room tossing poker chips at the go board. Steve Kleene generally won, because he was six inches taller than anybody else and could lean over further to make a toss. He did it very well, in fact. Princeton was never noted as the center of social life, but for mathematicians it centered on the common room in Fine Hall.

Many Europeans had come over, and by this time the Institute was functioning. It didn't have any buildings of its own. It moved into Fine Hall and was there several years before they got Fuld Hall built. I remember walking in once with Bob Robertson. We passed two people chatting with each other in French, and two more talking to each other in German, and a small group talking to each other in German. I turned to Bob and said "Sprechen Sie Englisch?" He said, "Ja, ein Bisschen."

Besides all the social activity, there was a terrific amount of mathematical activity going on. In fact, someone, I believe it was Bob Robertson, pointed out the danger for the younger people there. He said, "You can listen to so many lectures, that you don't have any time at all to do your own research." And it was entirely possible, *good* lectures that you really wanted to hear. I'm not going to say much about the mathematics at Princeton, because I was doing my own mathematics; in that respect, I think, I have always been a little out of line. There wasn't much activity in calculus of variations, but Al Tucker, the instructor I shared an office with, did give a talk on calculus of variations. That sounds out of character, doesn't it? But it was a perfectly sound lecture. Actually it was a discussion less of the calculus of variations in itself than of invariance properties of (generalized) differential equations. Nevertheless, it came under the calculus of variations.

There was always something interesting even when wildly different from the kind of analysis I have always favored. Von Neumann was there. I remember Lefschetz chiding a couple of us, "You young people annoy me with your exaggerated respect for John von Neumann. You speak as though he sprang full grown from the brow of Zeus. As a matter of fact, he is very good. But you have to think of the tremendous advantages he had, such as tutoring by the finest mathematicians in Hungary." I think we were right, though, in thinking he was several leaps ahead of the rest of us.

Einstein was not only a *super* scientist but also a kind person. I remember the first year of the Institute, Flexner gave a lawn party at Princeton Inn. By chance Virginia and I walked in only about 20 feet behind Einstein. As we walked across the lobby of the hotel, a Princetonian lady, of the Princetonian variety, strolled toward us. She was fairly tall and almost as wide, beautifully dressed, and she had an air of dignity. She strolled up to Einstein, reached out, put her hand up on Einstein's head, ruffled his hair all over the place, and said, "I have always wanted to do that."

At that time at Princeton you could find someone working very diligently in almost any field of mathematics. It had leaped from being a very good university to being, I would say, one of the two or three best in the whole world. Goettingen once made the claim of being one, but when Hitler succeeded in getting rid of all the Jews and relatives of Jews on the faculty, it sank badly, because even the survivors, who were in no danger of losing positions, were terribly disturbed about the injury to the university. In those days, if anyone was looking for a

super university in mathematics, he had to choose between Harvard and Princeton.

It was a marvelous place for anybody, not just to learn mathematics, but to learn to be a mathematician, because there you could meet mathematicians who were dedicated. Of course there are other things you learn from people. About Lefschetz I remember two lines: "When he's at last beneath the sod,/ He'll start right in to heckle God." About Bob Robertson: "Here's to Robertson, Howard Percy,/ On his soul there'll be no mercy./ Round of belly and deft of toe,/ His forehead's high, but his mind is low." I notice that he was the first to pick up names that now, 50 years later, are names that are still heard around the place, not just fossil names that are remembered by historians and by no one else.

Parshall: Mr. Duren?

Duren: When I went to the Institute it was from my little school, Tulane University in New Orleans. I had gone back there to teach after my Chicago graduate work. We had absolutely nothing, so it was coming from a mathematical desert into Princeton that I made the mistake Jimmy referred to: I found many courses so interesting that I wanted to take them all. I don't regret some of them, especially not Weyl's lectures on invariant theory, where Al Clifford was taking the notes, which I still have. I sat in on Alexander's course on topology. It was something very new. I'm told he introduced cohomology, but I didn't catch on; he never finished, or never produced notes, so I couldn't check up on it.

I sat in on Walther Mayer's differential geometry. Mayer was there as the assistant to Einstein. But my main assignment was in Marston Morse's course, where I was writing the notes, on calculus of variations in the large. For this I needed a knowledge of topology, which was a subject hardly taught at Chicago; we had some general topology—we didn't know it by that name—but no algebraic topology. When I went to work with Morse I began sitting in on Alexander's course, but I didn't get much out of it.

What I did get a great deal out of, besides the people in the faculty and Morse himself, were my colleagues, as members of the Institute, particularly from Reinhold Baer. I was tremendously impressed with Reinhold Baer. He was a very kind man. He put up with my ignorance very kindly, and we would walk in the evening together. He lived somewhere near me, and we had children about the same age. My boy Peter and his boy Klaus were together a great deal. Incidentally, Peter has since returned to the Institute as a mathematician in his own right. Klaus Baer went into Egyptology. But I was immensely impressed with the fact that Baer knew not only algebra, but also complex function theory or most any subject. He knew it far better than I did. He knew more calculus of variations, I think, than I did. I felt inferior as a mathematician there, these people knew so much. I said so to Morse; I was talking particularly about these chaps who knew a lot of topology. He said, "Don't let it worry you, ask them to work a problem." I felt a little better, but not much.

I think the thing that I did that pleased Morse the most was to make a mistake. He had a long and involved proof, a certain homotopy argument. I thought it was too involved, so I simplified it for him. Next thing I knew from him was he showed up at my apartment saying, "non sequitur, non sequitur." I had made a mistake. I had made it simpler, but only by making a mistake. The result was originally due to Lefschetz, and the mistake I made was the mistake that Lefschetz had made in the proof of it. That didn't make me feel any better either, because you're supposed to make original mistakes.

I remember one piece of advice Einstein gave. A few times during the year he would give a lecture. It was usually on "Are There Gravitational Waves?", and the answer would be after an hour or so, "There could be." In these Einstein lectures, there would be 100 people in the room, and yet he would speak as if he were talking coinfidentially to no more than two or three people. He would give advice. In particular, he didn't think too much of people who went to the library and looked up everything and read all books on the subject. He said, "Do not read the books, they will tell you wrong." Also, there was Leopold Infeld. He had just written a little book, *The Evolution of Physics,* with Einstein, a popular book on the history of ideas in physics. Infeld went back to Poland and founded a famous institute of mechanics.

Von Neumann, of course, was a famous man. There was a game going on between von Neumann and H.F. Bohnenblust. The rule was that if either one could catch the other working, the one who was caught working had to pay ten dollars. The rules were that you could burst into the other's office at any time, without knocking, in an effort to catch him working. Bohnenblust never caught von Neumann working. Apparently von Neumann did a lot of his work way into the middle of the night, if indeed he did it at all. He was just thinking about it, so that you couldn't catch him. Andre Weil was there. He was, of course, a tremendous mathematician. He was only a member at the Institute at that time; later he was on the faculty of the Institute, if he is not still.

Jacobson: He's retired now; he's fairly close to 80.

Duren: I guess I'll mention [Herbert] Busemann, who worked with us. Andre Weil was an austere person, but an extremely friendly person when you got to know him. I remember one instance of Salomon Bochner giving a talk. Bochner kept referring to the work of "Coussin", who was a compatriot of Weil's, and speculated about what Weil throught of this or that. At the end of Bochner's talk, nobody said anything. Then Bochner said to Weil, "Don't you want to say something, Professor Weil?" Weil rose and crossed his arms and said, "Yes, I do want to say one thing, it is pronounced *Cousin,* not Coussin." That was all the discussion.

I found working with Morse very, very helpful. As I said, I didn't know much; I didn't know enough topology. He was struggling with topology at the time. Essentially the only thing I could carry through

was the mod 2 topology, and that wasn't rich enough to do the job. Topologists later found a norm topology, coming out of the variational functionals themselves, which would do the job better. But at the time Morse was trying to apply one after another of the forms of singular homology. None of them would work; Vietoris cycle theory was one attempt at the time.

I'd like to finish by taking up something Jake mentioned. Even when I was there in '36-'37 there were still those receptions for which the ladies prepared dishes. The dish prepared by Mrs. Einstein was always very exotic and something to be avoided. So we all drew straws, and the one with the short straw had to go down first and find out which one was the Einstein salad and give the high sign.

Parshall: Interestingly enough, you three gentlemen have talked mainly about the Institute and not about the math department at the University. The most memorable people seem to have been the people associated with the Institute.

Jacobson: Well, that's sort of an accident. Princeton had a very distinguished mathematics department before the Institute. In fact, the core of the Institute wsa formed of people from Princeton, three out of four. Also Wedderburn, for example, was not mentioned particularly here. He obviously had a great deal of influence on me, because I was his student. Lefschetz was a very dynamic person, who had a great many excellent students.

A branch of mathematics that was developed at Princeton and not at the Institute was logic. The earliest work in logic was done at Princeton under Church. He had some wonderful students, like Stephen Kleene, who was one of the top logicians of the world, and John Barkley Rosser. Rosser, who went into working in differential equations at the Army Institute at Wisconsin, was a distinguished mathematician who did very important work in logic. So there was lots done at Princeton. Bochner, for example, who was not at the Institute did very important work.

I should like to mention one other thing about Princeton. It's true today and certainly was true in my day that the students learn from each other. They work together a lot. They took people from out-of-the-way places, who didn't have strong backgrounds in mathematics. I came from the University of Alabama. I had a couple of very good teachers I certainly owe a lot to, but I didn't know a lot of mathematics when I came there. They picked Steve Kleene, who came from some small school, I forget which one, Amherst, maybe. William Randels came from Alma College, Michigan. Of course the faculty gave us courses and so on, but we learned almost more from the fellow students. Once in a while there would be some student there who was very knowledgeable. Even those who weren't, were somehow able to talk about things and learn, and then study on their own.

The most knowledgeable person in our group was Bob Walker, so we learned a lot from him. We used to go together a lot. We ate

together. We practically all lived at the Graduate College. The University frowned on students being married. The graduate school was small, only 200 students in all fields. Mathematics had only 20 graduate students, and the number of entering students in my year was about four or five. So we lived together, we talked together, we sat together in the dining hall, and we annoyed everybody by talking mathematics a lot.

For relaxation we invented some games like rolling Oreo cookies down the table. They frustrated us by turning to square cookies. But that didn't stop us for long. We broke the corners off and made round cookies out of the square cookies. We weren't very popular with people in other fields, but we did learn a lot from each other, and that same system goes on today.

I was part of a visiting committee at Princeton a few years ago. I walked through the new Fine Hall and heard students talking to each other, clearly very sophisticated mathematics. That's the way they learn. Nowadays it's even more true than it was in my day, because now the department doesn't give the kind of courses that are given in most places, you know, the beginning graduate courses. They don't do that any more. Students are supposed to have the background or to fill it in themselves. The courses given now are specialized courses on a research level. It wasn't like that then, but still the spirit of the place was very conducive to learning mathematics, which was *the* most important thing in the lives of the people there. The students stimulated each other, and that's the way they became good mathematicians.

Parshall: Did you find the faculty unapproachable?

Jacobson: No, I think they were reasonably approachable. But this was learning mathematics every day, every hour, and you couldn't very well go and ask questions of the faculty whenever you thought of something. It was easy to talk to the students; they had kind of "walking seminars". It worked very well.

McShane: I'd like to add to that. I said before how much I talked with the other students. I didn't mention the other side of it. I didn't meet all of the faculty. When we talked about the common room earlier, the ones who were not gregarious were not mentioned. Bochner was not very gregarious. Church, certainly not. The least gregarious person I ever knew was Wedderburn. I used to meet him almost daily in the hallway. We had a conversation. The conversation consisted of the following: "Good morning." "Good morning," and then he went to his office. That's all we ever said to each other. I don't remember *ever* seeing Wedderburn in the common room. Did you?

Jacobson: No, he never was. But he was pleasant enough. He would never go out of his way to see anybody. He was too shy.

McShane: Perfectly friendly, and all that, but you had to seek him out yourself.

Jacobson: There's another thing which might be worth mentioning, something of historical interest. The topology that was done at Princeton was what became algebraic topology. At Texas was the school of R.L. Moore, the school of point-set topology. I don't know if they felt the same way about Lefschetz. Eventually what happened was that Raymond Wilder saw that the two things could be put together to become one subject. It was a great achievement of his, but before that there were always jokes being made about the Texas and Polish school. About, for example, simple aspicular cactoids, a bizarre configuration that R.L. Moore's school came up with. That was one of the things that added to the gaiety of Princeton.

McShane: The school you're talking about was referred to by Lefschetz as the "concerning school", since the papers were "concerning" this and "concerning" that. Lefschetz commented, "To write a book about topology and confine oneself to this subject matter, is like writing a book on zoology and confining oneself to the rhinoceros."

Jacobson: I'd like to mention something that had to do with Jimmy McShane. He actually solved a problem for me once. As a student I knew some theorems about ring theory. I wanted to find a counterexample to the semigroup analog of a standard result of ring theory. The problem was to construct a semigroup with a zero element, zero being something that absorbs everything (x times 0 equals 0, and 0 times x equals 0), is generated by a finite number of elements, with all elements nilpotent, and has the property that you could write down as long strings as you like that are not zero. I was talking to Jimmy, and he came up with the answer. The answer was something supposedly invented by Marston Morse: a sequence of 0's and 1's that had no block repeated three times.

It turns out it was not discovered by Morse, though it played a role in symbolic dynamics. It was developed by Morse and G.A. Hedlund, particularly by Hedlund, I believe. Morse got a lot of mileage out of it. He wrote a paper about an infinite chess-game he constructed using this sequence, and he was interviewed by the press about this chess game. Someone later discovered that this had been done many years before in 1916 or so, by [Axel] Thue, a Norwegian mathematician. Thue, who published in a Norwegian journal, had discovered a lot more than any of these people had; he had found many more properties of the sequence. Morse acknowledged this to Thue when he discovered it, but I think it's still a common misconception that this is Morse's sequence rather than Thue's. I think that the sequence may have been found even earlier than that, perhaps 1896 or so. Do you remember, Jimmy, that you told me the answer to this?

McShane: To be honest, I don't.

Duren: I remember some other graduate students from those days. One of them, this was later, was John Kemeny, who was a student of Church's. Many years later I went to attend some of Kemeny's lectures, when he was an assistant professor. He had already begun to develop the BASIC language for computers. It was based on the logic

that he worked on for his thesis, with Church, which had to do with finite logical systems. The BASIC language is a language that's used in practically all computer centers. It's the first language, so to speak, nowadays.

Carl Allendoerfer and Joe [Joachim] Weyl were there as young Princeton graduate students. Joel Weyl was Hermann Weyl's son, later distinguished as director of mathematical sciences, Office of Naval Research in Washington. I remember Carl and Joe as having a lot of energy and a lot of fun. they were not "grinds" in the sense that one often thinks of graduate students.

Jacobson: Another distinguished student they had was in mathematical physics when I was there: John Bardeen. He has been awarded two different Nobel Prizes in physics, one of the rare people to have this honor. And there are a number of others. The Panofskys were there. They were sons of one of the professors in art at the Institute, Erwin Panofsky, a very famous professor of Renaissance art. Both of the sons became famous, one of them is a top physicist I believe.

McShane: We met them in Princeton, and the father, Erwin Panofsky, referred to what Hitler had done to him as the expulsion into paradise. He liked it so much better in Princeton.

Jacobson: He was a marvelous lecturer at Princeton. I heard him talk.

Parshall: Can you gentlemen think of any other remarks about the Princeton mathematics community?

Duren: You can tell Al Tucker that if he asks us again, we'll think of at least as much more. It's fun talking about Princeton, particularly for us older people. We rarely get a chance to reminisce; my wife doesn't permit me.