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The Princeton Mathematics Community in the 1930s
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RECOLLECTIONS OF PRINCETON, 1939 - 1941

This is a written contribution, dated 30 December 1984, by Robert Hooke.

School had scarcely started when [Solomon] Lefschetz called the new graduate students into his office to give us an orientation lecture. He left out a lot, but it began our understanding of how a man can be lovable and terrifying simultaneously.

On the first day of classes I showed up at [Salomon] Bochner's course on complex variables and was surprised to see Lefschetz sitting in the front row. A few minutes into the class it developed why he was there. He stood up and announced that there was no textbook for the course and no official class notes had been done before, so he was calling for volunteers to take official notes. No volunteers were forthcoming, so he put on his most affable look and said, "Mr. Dolph and Mr. Hooke, why don't you volunteer?" He then appointed Brock McMillan, who already had his Ph.D. but was auditing the course for some reason, to oversee our work, which was a good thing. Later in the term, as Bochner's lectures grew more and more incoherent, we were heard a few times about the impossibility of making sense of them. These complaints were echoed by others in the class who were taking their own notes. To put us in our place, [Claude] Chevalley volunteered to interrupt his unending game of Go long enough to attend a class. At the end of the class he presented us with a complete set of notes for that lecture, written clearly in a precise hand, in ink, and in French. Years later I returned to Princeton and found that Bochner had become one of the most respected lecturers. It was said that his trouble in the fall of 1939 was that he still had family in Poland.

One vaguely irritating thing about that class was that if anyone was the best student in the class, it was a physicist. I felt better about this years later when Dick Feynman won the 1965 Nobel Prize in Physics.

By Christmas I was totally discouraged. I felt that everybody else was smarter than I was and had had a better collection of previous courses. Lefschetz had told us that the prelims were the only important exam we would have, and the courses that were offered, except for Bochner's, were not helping me make any progress toward learning what was needed for prelims. (A dozen or so years later Al Tucker told me that almost all students felt this way, but that didn't help me in 1939.) Fortunately for me, the majority of January was devoted to a "reading period", during which time I learned what I needed to know about real variables, point-set and combinatorial topology, and modern algebra. This also provided invaluable aid in learning how to learn mathematics without the aid of a professor.

At this time we could see the war coming into our lives, and I wanted very much to finish my Ph.D. before that happened. I had thought I would specialize in analysis, but I seemed to be far behind everyone else in that area. I had had a good course in continuous groups from Nathan Jacobson at North Carolina, however, and that gave me the feeling that I could get my degree faster in algebra than in anything else. So in the second semester I took [J.H.M.] Wedderburn's course in matrices and Chevalley's in algebraic geometry, in addition to Bochner's in measure theory.

Wedderburn's lecturing style was unique, to say the least. He was apparently a very shy man and much preferred looking at the blackboard to looking at the students. He had the galley proofs from his book *Lectures on Matrices* pasted to cardboard for durability, and his "lecturing" consisted of reading this out loud while simultaneously copying it onto the blackboard. Ernst Snapper, who claimed to be only the fourth person ever with the courage to write a dissertation under Wedderburn (and one of the other three had lost his mind) told me this story explaining why Wedderburn was a bachelor. It seems that an old Scottish tradition required that a man, before marrying, accumulate savings equal to a certain percentage of his annual income. In Wedderburn's case his income had gone up so rapidly that he had never been able to accomplish this.

Chevalley's lectures were very well prepared and very precise, so that the following event stands out in my memory. One day soon after his lecture began, he became stuck on a point in the proof he was giving. He stepped back a few paces from the board and stared at it. No one in the class knew how to help. After some 40 minutes of this completely silent cogitation, the bell rang and he walked out of the door without a word. I remember this well, not only because it made for a very long and memorable class, but also because the entire process was repeated a couple of weeks later.

In the spring Lefschetz stopped me in the hall one day and asked me why I was not taking prelims. I had considered that it would be at least a year before I was ready, and at first the thought of taking this one big exam with three weeks of preparation seemed ridiculous. But he was persuasive and in one of his affable moods, so I decided to do it. I secretly thought he must have believed I'd been there a year longer than I had, but I decided that since he thought I was ready he would do what he could to avoid being proved wrong. As it turned out, the prelims went very well, his confidence in me having had a very helpful effect.

One reason that I had not thought of taking prelims at this time was the infamous "Part Zero" exam. This was one of the things that Lefschetz had left out of his orientation talk at the beginning of the year. We heard rumors that in midwinter the new students would be notified to come around for this exam, with no prior notice given of the date. This seemed like such an unlikely way of doing things that we didn't believe it, treating the story as if it were a normal kind of upperclass teasing of freshmen. So when we found messages in our boxes to show up for this the next day, I was horrified. The purpose of the exam was to help determine what fellowships to give us, if any, the following year. I was very nervous for this exam and felt afterwards that I had disgraced myself and would no doubt be invited to look next year for an easier school to go to. However, it turned out that some others felt the same way, and I did get a fellowship for the following year, but it was not until my later conversation with Lefschetz that any of my confidence began to return. Incidentally, Lefschetz was not always so good at inspiring confidence. Just before we took our prelims that year he got us all together for a little pep talk. One of the things he told us was about the famous mathematicians who had flunked their first attempts to pass Princeton prelims. Actually, I think he thought this would alleviate our worries about flunking, but all it did was to make us think how hard the exam must be.

After passing my prelims I wanted to get started on a thesis so that I could work on it over the summer. If I were to write in algebra there were two people under whom I could do it, Wedderburn and Chevalley. Chevalley was young and inclined toward acid commentary, but Snapper had warned me about the perils of working under Wedderburn, so I collected myself and went to ask Chevalley if I could work under him. So far as I knew, Gerhard Hochschild was the only one who had done this. Chevalley was very helpful and gave me a bunch of topics to try out. I selected a conjecture of Zassenhaus to work on.

One small event that sticks with me from the following fall has to do with a party, which was a slightly-more-than-usually-festive tea that I believe was given in the department to introduce new students and the faculty to one another. I was talking with H.F. Bohnenblust and telling him how sorry I was that he had been on a leave of absence the previous year. (I had heard that he was an excellent lecturer.) In the midst of the conversation I looked up and saw a very nice looking young woman coming in the door. This was such an unusual event that it called for comment, so I said to Bohnenblust, "Who is the good-looking woman?" "You asked the wrong man," he answered. "That's my wife."

Having passed my prelims, there was no need, officially, to take any further courses, so I concentrated on my thesis. The loose Princeton system, however, allowed us to sit in on whatever courses we wanted to try, just for general education. One of those that I attended for a while was Chevalley's course in differential equations. On the first day the classroom was packed with people wondering what he would say on the subject. People soon started dropping out, however, and eventually I joined them. At the end of the semester I knew Hochschild was still going, so I asked him how many others were also doing so. His answer was that the only others were Hermann Weyl and John von Neumann.

I soon found that Zassenhaus's conjecture was false, and I wrote a paper proving it was. Chevalley thought it could be considered a thesis, but the rule was that no negative theses were accepted, so I began to work on another of his topics. By spring (1941) I had made enough progress to start looking for a job for the following year, so I talked to Dean Eisenhart and asked for advice on how to go about it. The Depression was still on, but the war had caused it to abate a little, so I hoped he had heard of an opening or two. He knew of no openings, but he suggested that I attend some upcoming meetings in Chapel Hill, and since I had come from there he thought I might be able to unearth something. (By contrast to today's methods, this minimal advice was the only counseling I ever received from any school, except for being told at Chapel Hill that Princeton was the place to go for graduate study.) The dean must have given the same help to others, since most of those I knew returned to their home states to teach. I did go to the meeting, hoping to find something at Duke or Chapel Hill, but they had no openings. Instead I heard of a job at North Carolina State, which I eventually took.

One day Lefschetz stopped me and asked what I was planning to do the following year. I told him I was looking for a job. He strongly advised staying in Princeton and offered me a teaching assistantship for whatever the going rate was then. (I think it was $750 a year. It was certainly no more.) I told him I wanted to get married and that was not enough to support a wife. "Nonsense," he said, "she could get a job, and you could do very well." This might have been true, but I doubt it; besides, she had another year of college to go, and I was ready to leave school. Later when I got the job at N.C. State I told Lefschetz, and he asked what they were going to pay me. When I answered $1800 he said, "You can't support a wife on that!" Annis and I were married in June, and we spent the summer in Princeton while I finished my thesis. In September we went to Raleigh, I returned at Thanksgiving to take my final exam, and she finished college in Raleigh.

As I've said, I felt hurried during those two years, and except for the mentioned snatches of conversation with Lefschetz my contacts with the faculty were largely brief and formal, and I didn't come away with any particularly warm feelings toward them. Ten years later, though, I became interested in statistics by teaching a course to students who had asked for something they could use without becoming teachers; at the same time, postwar inflation was making it hard to live on a professor's salary, so I decided to move into statistics myself. I wrote back to Princeton for advice and received valuable help from John Tukey, Sam Wilks, and Al Tucker, who made it possible for me to spend three years in Princeton with two research groups. During those three years we came to know Princeton much better, and I am very grateful to Princeton for being so helpful to me in making this change that so affected my life.

in number theory. I never had a course in number theory, just contacts with Paul.

Tucker: Do you remember much contact, when you were a graduate student, between Fine Hall and Fuld Hall?

Kemeny: Yes. I had some personal contacts. In my last year as a graduate student, I became Einstein's research assistant, and for a year I was a member of the Institute for Advanced Study. I must say that during that entire year I did not get to know a single person I had not previously known. Somehow, unlike Fine Hall, the Institute was not conducive to getting to know new people. It was a wonderful year for me, because of Einstein.

I got to know von Neumann at Los Alamos, because I was assigned to the so-called computing division. Von Neumann had really set it up. He had figured out how to use bookkeeping machines to solve partial-differential equations. He would stop in periodically to see how things were going. Peter Lax and I became good friends there. We would occasionally corner von Neumann to chat, so that is where I got to know him. I saw him occasionally at the Institute after that, and also at least one summer at Rand—we overlapped for a while at Rand.

Strangely enough, though I got to know von Neumann well, I got to know him best at Los Alamos and at Rand. At Princeton von Neumann gave the Vanuxem Lectures, which for complicated reasons he could not write up. I was picked to write up those lectures, and this led to a *Scientific American* article

There were three people at the Institute I got to know well. One was von Neumann, one was Einstein, and one was Goedel. I met Goedel through a mutual friend, Paul Oppenheim. That's how I met Einstein also, rather than through some official machinery.

Tucker: Could you tell us a bit about Goedel?

Kemeny: I'd be happy to. Is Goedel still alive?

Tucker: He's been dead several years. I remember Steve Kleene came to Princeton for the memorial service. He had been appointed by the National Academy of Sciences to represent the Academy at Goedel's memorial service.

Kemeny: As everyone knows, Goedel was somewhat strange in personal habits. Paranoid, I think, is the right word. I was one of the few people he seemed to trust completely. Not because of who I was, but because he trusted Paul Oppenheim. Oppenheim was an old friend of Goedel's from way back, from Europe, and someone introduced by Oppenheim was okay. So I had many visits with Goedel and many conversations with him.

A strange thing happened years later. One of the young logicians at Princeton was dying to see Goedel. I was at Princeton myself giving the Vanuxem Lectures. He asked if I would write a letter to Goedel vouching for him. I said, "I'd be delighted, but this is crazy. Why don't you ask Church to do it? You're a student of Church's." It turned out that Church had written three times and Goedel hadn't replied. So I wrote a letter for him. A couple of weeks later he sent me a note saying that he had gotten a prompt answer from Goedel thanks to my letter. I mention this as an example of the strangeness of Goedel: I was okay, but Church wasn't. But to me, Goedel was always extremely pleasant. We had interesting conversations, mathematical and otherwise.

There was an incident my wife would want to recount. We were at someone's house. It was a pleasant social affair, and Goedel and his wife were present. In the middle of the evening, well before people were ready to leave, a little wrist-alarmclock went off and Goedel said, "I'm sorry, I have to leave." There was some incredibly trashy television show, and Goedel publicly announced that he had to leave to see this television show.

I should mention one more thing. When I really got to know Goedel was during the year that I was Einstein's assistant. They had gotten to be good friends and often walked home together from the Institute. I was with them on a number of these occasions. Incidentally, there was another sort of strangeness at the Institute you might be interested in. Do you know how Goedel and Einstein got to know each other?

Tucker: No

Kemeny: It's a Paul Oppenheim story. Oppenheim was a great story-teller. It's the story of what he described as his only contribution to science—a typical Oppenheim statement.

When Goedel started working on the mathematics of general-relativity theory, Paul Oppenheim asked him, "What does Einstein think of your work?" Goedel said, "Unfortunately, I don't know Einstein." Paul was amazed at this: first of all because two such famous people at the Institute should know each other, and secondly because they were surely the only two people at the Institute working on relativity theory. Goedel said, "Yes, it strikes me as strange too, but I just have never met him." Paul decided to do something about it, and went down to Fuld Hall the next day. It turned out that Goedel had been moved quite recently; he actually had the office across the hall from Einstein's. So Paul said his one contribution to science was to lift his two hands and knock simultaneously on two doors. The doors opened, and he said, "Einstein this is Goedel, Goedel this is Einstein." By the time I worked with Einstein they were close friends. But it took somebody not connected with the Institute to introduce the two of them to each other.

Tucker: Oskar Morgenstern was another person who was a close friend of Goedel's.

Kemeny: Yes, as a matter of fact, I think—I'm never certain of these things—it was a party at Oskar's house when the wristwatch incident occurred. I got to know Oskar very well. He would visit Dartmouth periodically. As a matter of fact, he made a major contribution to *Finite Mathematics*.

Tucker: Oskar used to talk to me about Goedel. Indeed, he made no bones about saying that Goedel was greater than Einstein. This was the time when Goedel was working on the unified theory. Oskar thought that Goedel was going to pull a coup and surpass Einstein.

Kemeny: I don't know if he ever completed that.

Tucker: I knew Goedel only as someone I saw and said good-day to, because I was never a logician.

Kemeny: One more Goedel incident. The only public lecture I heard by Goedel, I think, was during the Princeton Bicentennial. A horrible thing happened. The lecture notes were good, but Goedel walked in and faced the blackboard and delivered the half-hour lecture facing the blackboard without ever writing anything. It was the most uncomfortable thing I ever sat through in my life. I wished that he would pick up a piece of chalk and write *one word* on the blackboard just as an excuse. It was clear that he just could not face his audience.

I heard a couple of lectures by von Neumann, which were, of course, brilliant. I heard one by Einstein, in Fine Hall, which was excellent. The content of Goedel's lecture was excellent, but the lecture itself was a disaster because of this peculiarity he had of not facing the audience.

Tucker: When did you get interested in computing?

Kemeny: I was forced into that at Los Alamos.

Tucker: That was the start?

Kemeny: Yes. During the war that was such a crazy operation there, and of course everybody was trying to think of ways to cut down the time it took.

Tucker: Did you have any contact with computing while you were a graduate student?

Kemeny: Absolutely none.

Tucker: You had no connection with von Neumann's work?

Kemeny: No. I might, though, mention one other connection I did have with von Neumann. He gave a lecture at Los Alamos, which for some reason hasn't been reported. To the best of my knowledge, the only place it appeared in print is in my little book *Man and the*

Computer. I was there together with quite a few other people while von Neumann tried describing what he felt computers should be like. This was either late 1945 or early 1946. In effect he outlined in that one lecture what I consider to be all the fundamental principles of a modern computer. I had no idea at that time that he was actually planning to build such a thing himself. I wasn't aware he was doing it until I became a member of the Institute in '48-'49. But I didn't have any connection with it, nor did I get to see the product, till '53 at Rand.

The rekindling of my interest in computing came through Rand. As you know, at Rand von Neumann worked on some very interesting mathematical problems, many of which required a large amount of computing. So I had some connection with computing in '53, and in '56 I played a strange role. It resulted from one of those periodic federal-budget cuts. Rand had asked me to come in the summer of '55. I couldn't, so they asked if I could come the next summer. I said I could, but before the summer arrived, the budget had been cut way back. Consultants, of course, are one of the first things to be cut out. They found they could meet the commitment to me, but they had promised to have consulting in both mathematics and computing and they had money for only one consultant. So they asked me if I would divide my time between mathematics and computing. I told them, "Look, I haven't had all that much experience with computing, but I'm fascinated by it. I'd love to." So I spent half the summer working with the computer people. I would have to point to the summer of '56 as the beginning of my serious interest in computers.

Tucker: You once invited von Neumann to Dartmouth, didn't you?

Kemeny: Yes. We got the dean's permission for an annual man, what we called a "big-shot visitor". We asked von Neumann, and he accepted. He had to back off, though, because of illness, which turned out to be terminal. So we never did have him up.

Tucker: I was very briefly a member of the von Neumann project, just at the end of the war. The work that I had been doing had terminated and the University here was not ready to start, so I was temporarily unemployed. Von Neumann very kindly said he would be glad to have me work on his project.

Kemeny: When did you start the building of the computer?

Tucker: I had nothing at all to do with that. I was supposed to be working at a sort of topological problem, namely, 'what would be a good way to generalize finite-difference methods to higher dimensions?'. Of course, in higher dimensions you have many more ways of cutting things up, and that was as far as I got. I looked into other ways of doing it than rectangularly, doing it in terms of hexagons in the plane, or equilateral triangles for example. But before anything came to a head in this, I suddenly had to go back and teach fulltime. Some people who continued, at that time, to work with von Neumann were Valentine Bargmann and Deane Montgomery. Herman Goldstine was already there as the chief helper of von Neumann. That was, I would guess, in 1946. The answer is to be found in the book of Goldstine's book *Computers from Pascal to von Neumann*. Incidentally, have you seen the new book about Turing?

Kemeny: No. A couple of people have recommended it, but I haven't yet had the time. Is it as good as people say?

Tucker: Well, I plowed through it. I read it for the people. There is a thorough index; people are indexed every time they get mentioned. I was particularly interested in the topologists who were involved in the code-breaking during the war. The one who was in charge on the outfit Turing worked for was a well-known topologist, M.H.A. Newman. My first Ph.D. student, Shaun Wiley, was another member of that group. Peter Hilton was too, so there were a lot of familiar names. But a great deal of the book is devoted to the technical details of the those code-breaking machines and of the hardware of the computer that was started under Max Newman at Manchester. Turing was brought there and was working on that machine when he took his life. There's a lot in the book on his problems as a homosexual. I actually was a member of the generals committee for Turing, in about 1937 I think.

Kemeny: Did he take his degree at Princeton?

Tucker: Yes, his Ph.D. was under Alonzo Church.

Kemeny: I did not know that. Some of Turing's great papers appeared at about the same time as some of Church's.

Tucker: Yes, Turing's great paper on computable numbers was written just before he left England to come to America.

Kemeny: So he had substantial publications before he took his degree at Princeton?

Tucker: Not exactly substantial, but one great paper. He actually learned mathematical logic from Newman, who was a combinatorial topologist and who occasionally taught something he didn't know well in order to learn it. He taught a course at Cambridge in mathematical logic, and that was what got Turing interested.

Kemeny: I understand these things, that graduate students sort of learn by lore. Kleene and Rosser are always mentioned as Church's students; I don't remember hearing Turing mentioned as Church's student.

Tucker: As in so many cases, his Ph.D. thesis was his own work. Church was supervisor only formally. Turing at that time had a fellowship at King's College, Cambridge. He came to study with Church at the suggestion of Max Newman. He was allowed to use his fellowship from King's for the first year. Then the Cambridge Procter Fellowship became available, so he was able to stay a second year. He decided to get a Princeton Ph.D. while he was around. Following that he could have stayed for a third year and worked with von Neumann on the computer project at the Institute, but King's College said he couldn't have a third year of leave from his fellowship. So he returned to England and resumed his fellowship.

He was a strange person. This is reflected in the title of the recent biography, *Alan Turing: the Enigma,* which refers both to Turing himself and to the code-breaking machine he was associated with. No, very few people realize that Turing is one of Church's Ph.D.s.

Kemeny: As I said, the summer of '56 was the beginning of my serious involvement in computing. I might tell you an anecdote. At the end of that summer I was asked to write some recommendations for computing at Rand. Of course the memory is hazy after so many years, so I can't swear to the details, but I remember the point I stressed most was that it horrified me to see the well-known mathematicians at Rand, being paid large salaries, waiting around for hours to get a few seconds of computer time. I remember big shots fuming for hours waiting for one 5-second program. I suggested that that was quite unkind to the big shots and also economically unwise. I was trying to describe some sort of system by which mathematicians could have easy access for a few seconds of computing. I said there must be some way to "interrupt" the system. That was the beginning of the idea of time sharing, changing from batch processing.

Tucker: Anything else?

Kemeny: My most interesting experiences, except for getting to know many of the great figures at Princeton, were outside that period. Maybe I should say something: Lefschetz got absolutely furious at me when I decided to go into logic. You remember he was not very fond of logic.

Tucker: No.

Kemeny: Lefschetz liked me and took an interest in me as an undergraduate, and he got furious at me for picking mathematical logic as my specialty.

Tucker: Well, you'll agree that his personality wasn't suited to mathematical logic and vice versa.

Kemeny: No, he was at the opposite extreme. I remember a guest lecture in a course of mine. The freshman class was totally lost. Halfway through the lecture he still hadn't said anything that they understood. He mentioned invariance under translation, and there he noticed that the class was really lost. He said, "Oh, you know what translation is. You take a point, say three-quarters, and you move it three-quarters." Then he wrote that this equals seven-eighths. There was a stirring in the class. I was sitting at the back of the class. One of the better students, who was sitting next to me, asked me if I thought that was right. I said, "What do you think?" He said, "I think it's wrong." "Well, why don't you ask Professor Lefschetz?", I said. So he said, "Sir, I think that answer is wrong." Without hesitation Lefschetz erased his answer. He took it for granted that it was wrong. Then he stood there and thought about it for a minute, and then wrote down a second incorrect answer. Then half the class raised their hands. He went on, totally undisturbed by this, and finished what would have been an excellent lecture.

The whole next class we devoted to discussing how someone can get to be one of the world's great mathematicians and not know how to add two fractions. It was a fascinating classroom discussion. I took the occasion to explain that there are several different kinds of mathematics; numerical work underlies some parts of mathematics, but is essentially irrelevant to other parts. They got an enormous amount out of Lefschetz's lecture, but mostly because he made it clear that he couldn't add two fractions.