From the collections of the Seeley G. Mudd Manuscript Library, Princeton, NJ

These documents can only be used for educational and research purposes ("Fair use") as per U.S. Copyright law (text below). By accessing this file, all users agree that their use falls within fair use as defined by the copyright law. They further agree to request permission of the Princeton University Library (and pay any fees, if applicable) if they plan to publish, broadcast, or otherwise disseminate this material. This includes all forms of electronic distribution.

Inquiries about this material can be directed to:

Seeley G. Mudd Manuscript Library
65 Olden Street
Princeton, NJ 08540
609-258-6345
609-258-3385 (fax)
mudd@princeton.edu

U.S. Copyright law test

The copyright law of the United States (Title 17, United States Code) governs the making of photocopies or other reproductions of copyrighted material. Under certain conditions specified in the law, libraries and archives are authorized to furnish a photocopy or other reproduction. One of these specified conditions is that the photocopy or other reproduction is not to be "used for any purpose other than private study, scholarship or research." If a user makes a request for, or later uses, a photocopy or other reproduction for purposes in excess of "fair use," that user may be liable for copyright infringement.

The Princeton Mathematics Community in the 1930s
Transcript Number 22 (PMC22)
© The Trustees of Princeton University, 1985

JOHN KEMENY

(with ALBERT TUCKER)

This is an interview of John Kemeny at Bradley Hall, Dartmouth College, on 7 June 1984. The interviewer is Albert Tucker.

Tucker: Do you remember your first encounters with Fine Hall?

Kemeny: I remember many things about it. I entered Princeton as an undergraduate in February 1943, when conditions for universities were poor. My class entered in three instalments; I was in the tail end of it. People were being drafted almost as fast as they entered. I was sixteen and a half when I entered, so I would be there for almost two years before I got drafted. I had a rather unusual undergraduate education, as a result of the wartime conditions. One statistic is interesting: everyone I had as an undergraduate teacher was either a full professor or became a full professor before I graduated from Princeton, which is not typical of anyone's undergraduate education anywhere. That was because the younger people had been drafted.

Tucker: But it is still true of Princeton, as it is true of Dartmouth, that all professors engage in the full range of teaching.

Kemeny: Yes. I was unusual, however, in having only senior members of departments.

Tucker: Who were your undergraduate teachers?

Kemeny: In my first year Princeton had just made a decision that analytic geometry was not a prerequisite for calculus, so I signed up for calculus. I had someone named Al Tucker as my first college mathematics teacher. I got nervous as to what analytic geometry I had missed, so I took it as an extra course. I had Claude Chevalley for analytic geometry. It was one of the weirdest courses anyone could have had. He was superb for me and terrible for the rest of the class. He considered me the only one in that class who had an interest in mathematics.

I had Alonzo Church for both integral calculus and differential equations. I later wrote a junior paper and senior thesis and a Ph.D. thesis—none concerning calculus—under him. So I had a strange beginning relationship to a world-famous logician.

Tucker: Was he as thorough in teaching calculus as he was in a lot of other ways?

Kemeny: In some ways it was an unbearable course. I owe him a great deal, but I must say that the teaching style which is ideal for mathematical logic—particularly on an introductory lével where every detail is covered with a thoroughness only Church is capable of—can be a bit boring in a calculus class. We learned an enormous amount about foundations and the basic concepts, but we covered probably only half of the material. He's a conscientious teacher, but ...

Tucker: Did you, at that time, make use of the Fine Hall library?

Kemeny: Yes, but not a great deal as an undergraduate. I occasionally went to look things up. If my memory serves me right, there were shelves at one end of the library that had books selected for undergraduates. I found that extremely helpful, and in a way the Mirkil Room here at Dartmouth College is patterned after that—a mathematics library made accessible to the undergraduate. As far as I know my high school didn't even have a library. I had no experience searching in a library, and in a large reference library you have to have some sophistication to find what you're looking for. So the biggest use I made of the Fine Hall library was my use of those shelves, where I could get books that were both good and at a level an undergraduate could understand.

Tucker: Did you have any contact with the people at the Institute?

Kemeny: Not at that time; I did when I came back from the war. My experience with upper-division courses was also unusual: so many undergraduates had been drafted that I had three courses with extremely small enrollments. I had Chevalley again, this time for complex variables; there were three of us in the course. I had a course where there were two of us. But the most remarkable was my modern-algebra course. It was announced that there would be offered either Eisenhart's differential-geometry course or Wedderburn's modern-algebra. I had had neither one, so I gleefully signed up. It turned out to be Wedderburn's course, and five of us were signed up. But two of the five had signed up hoping it would be differential geometry, and they dropped out. For reasons I don't know, the other two dropped out before the week was out. So I went to Professor Wedderburn and said, "Clearly you don't want to teach a course for only one student." I offered to resign from the course. He wanted to know if I meant that I did not want to take the course. Of course I was dying to take it, so I was his only student in that course. As far as I know, I was his last algebra student.

Tucker: I think that's quite possible.

Kemeny: It was a remarkable experience. Wedderburn was a gentleman of the old school. It was funny—a course with one student. And it was a straight lecture course. At the beginning of class he would say, "Are there any questions?" More often than not, I had questions. It was an excellent way to teach a course; I learned a great deal.

The only time I had a personal conversation with him was once, for some reason, we were moved to another room and had to walk across campus. He asked me what I might be interested in. I was just beginning to be interested in logic, so he said he would take up "the algebra of logic", now called Boolean algebra. Otherwise he knew absolutely nothing about me, including whether or not I had any mathematical talent. I found this out from the big work for the course, which was a set of problems that had to be done in ten days. You had to do four out of five. I went home and did four in two hours. I could have handed the exam in then, but of course I wanted to do the fifth one. But I absolutely couldn't do the fifth one, so I went back two days later horribly upset. It turned out the problem had a typographical error. So he gave what was for any good student an absolutely trivial exam. In that semester he did not find out that I really had some mathematical talent. I don't mean to criticize him, because I thoroughly enjoyed the course and learned a great deal from it.

I had Lefschetz for mathematical physics, except that Lefschetz decided that I shouldn't learn that. He didn't let me come to class. Instead, he made me work on some independent project. Actually as a result of that independent project I learned an enormous amount from him, but I missed out on mathematical physics. I did have Eisenhart's differential-geometry course later, and I had a real-variable course from Bohnenblust. And of course, logic courses from Church. It was a remarkable array of teachers. No one could have had a better undergraduate education.

Tucker: Let's talk about when you became a graduate student.

Kemeny: I went off for military service for a year and a half, and came back to finish my undergraduate work. Artin had come to Princeton, so I did my graduate algebra with him. I had Chevalley for point-set topology. The Chevalley course is the one I most enjoyed; in fact, I made point-set topology my specialty in the general exam. Of course I specialized in logic, so I took all the logic courses I could. I got to be close friends with Leon Henkin, who was slightly ahead of me in graduate school. We talked a great deal of logic to each other; you know how graduate students were with each other.

Tucker: That was expected.

Kemeny: There was an atmosphere conducive to students talking with one another. As a matter of fact, much later I was thanked publicly by someone I had as a student, and I could have sworn I made no contribution to his education. This was when he introduced me publicly somewhere. He said, "I don't think I'll ever forget this. We were both standing by the bulletin board in Fine Hall, and I asked Professor Kemeny what a recursive function is." I had no reason to remember this two decades later. But the graduate student was Hartley Rogers, who later wrote a definitive book on recursive functions, and he remembered the incident vividly. That was what Fine Hall was like.

Tucker: You've mentioned Hartley Rogers and Leon Henkin. What other students do you remember?

Kemeny: I overlapped with Dick Bellman somewhere. I have trouble remembering exactly how we overlapped at Princeton, because I got to know him much better later on. Do you remember when Dick was at Princeton?

Tucker: It was right after the war.

Kemeny: That's what I suspected. We overlapped there.

Tucker: Did you know he died recently?

Kemeny: Yes, I heard.

Tucker: Another person you knew who died recently is Ulam.

Kemeny: I did not know Ulam well. I knew him from Los Alamos. He gave Peter Lax and me some private lessons at Los Alamos.

Tucker: He wasn't at Los Alamos during the war?

Kemeny: No, not during my time there.

Tucker: He was already working with von Neumann on things such as the Monte Carlo method.

Kemeny: Did you know that he had a serious brain problem? I got to Los Alamos in March of 1945. I remember him coming there about a year later; he had just recovered from serious brain surgery. I remember it vividly because of the story he told. He was about to undergo this operation; it was a very serious one. Paul Erdos came in to see him, and to reassure him Paul said, "Don't worry. If you don't survive the operation I'll finish your work." For some reason this did not reassure Ulam.

I'm glad I mentioned Paul Erdos. You do remember that Paul was continually in and out of Fine Hall; he was one of my unofficial teachers. Besides the fact that he liked young mathematicians—he would all his life—I was a fellow Hungarian, which was irresistable. So I learned a great deal. As a matter of fact, I have a lifelong interest

© The Trustees of Princeton University, 1985

STEPHEN C. KLEENE and J. BARKLEY ROSSER

This is an interview with J. Barkley Rosser and Stephen C. Kleene in Madison, Wisconsin on 26 April 1984. The interviewer is William Aspray of the Charles Babbage Institute.

Aspray: I'd like to begin by asking which years you were in Princeton and in what capacity, student, faculty, whatever. If you would start, Professor Kleene.

Kleene: I went to Princeton in the fall of 1930 as a half-time instructor. During that year I took course work in modern mathematics. The third year I had a fellowship. The second year I didn't have any university support; I supported myself by being a proctor at the Hun School. Those three years ended with June 1933. At that time I left Princeton, having submitted my Ph.D. thesis. I returned for the final public oral exam in the fall of '33, and the Ph.D. was granted on the basis of the thesis. In the fall term of 1933-34 I was on my family farm in Maine. I returned to Princeton with a research assistantship on February 7, 1934, and remained there through the academic year 1934-35.

In the fall of 1935, it was planned that I would be a visitor at the Institute for Advanced Study, but a telegraphic invitation to accept an instructorship at Wisconsin came in, and I left for Wisconsin. In my deciding whether to accept Wisconsin's offer, one consideration was that I would be giving up this year of research support at the Institute for Advanced Study. I was, however, told that if I accepted Wisconsin's offer (which, incidentally, they advised me to do and I'm very glad I did) I could have my year at the Institute a little later. I cashed in on it in the year 1939-40.

Aspray: What about you, Professor Rosser?

Rosser: I came to Princeton in the fall of 1931. I had a teaching assistantship from '31 to '33; from '33 to '35 I had a fellowship.

Aspray: Could you tell me something about what your background was prior to going to Princeton and why you chose to go there.

Kleene: I had a liberal arts education at Amherst College where I had two majors, mathematics and philosophy. As I came near the end of my undergraduate years, I was undecided as to whether I would go into philosophy or mathematics. I had some hesitations about philosophy because, if you worked out a philosophical theory, it was hard to know whether you were going to be able to prove it or whether other theories had just as good a claim on belief. In other words, there was a certain vagueness and a little uncertainty as to whether the foundations there were really solid, whereas mathematics, at least as an undergraduate sees it, was presented as having a solid foundation. In this situation it was suggested to me that I go to Princeton to work in mathematical logic with Professor [Alonzo] Church. In effect I would be splitting the difference between mathematics and philosophy. In fact the subject was often called mathematical philosophy. (I don't know where the name originates from.)

Two of the professors at Amherst had studied at Princeton, so they had contact. So later they sent out formal recommendations for me, and on their arrangement I went down for a couple-days' visit to Princeton. I met Professor Church, and then they offered me this half-time instructorship, which now I think is called teaching assistantship. On the strength of this, I decided to split the difference between math and physics and go to Princeton. It wasn't until my second year that I got to actually work with Church. My prior reading on mathematical logic and foundations was confined to Bertrand Russell's *Introduction to Mathematical Philosophy* and Alfred Whitehead's *An Introduction to Mathematics.* Those were the only things. I read one or two other books which gave me a background in mathematics other than logic.

Aspray: Professor Rosser.

Rosser: I was born and raised in Florida, and I went to the University of Florida. I took a bachelor's degree in mathematics in 1929. At that time financial assistance was the order of the day. They couldn't get me an assistantship to go on in mathematics, but the physics department, of all things, offered me a teaching assistantship in physics. I worked two more years there and got a master's degree in physics in 1931. Well, there I was with no clear prospects. It was still true that I was more interested in mathematics than physics, but I was enjoying the physics very much. The people in physics worked hard and got me an offer of an assistantship in physics at Wisconsin. The people in mathematics worked hard and got me an offer of an assistantship in mathematics at Princeton. The assistantship at Princeton was $200 more than the assistantship in physics.

Aspray: Now you both worked in logic at Princeton, and you both seemed to get involved in it fairly soon after you got there.

Rosser: When I got there, Church was offering this course, so I took it. Steve had been there a year before that, so he hadn't had any course contact with Church. Church offered this course in the fall of '31-'32. I fell head-over-heels in love with logic.

Aspray: Did you have any interest or experience in logic before that?

Rosser: Yes. In fact, I had read all of *Principia Mathematica,* which is an amazing feat.

Aspray: You're probably one of the few people who have done so. I'd like to turn to logic as a topic. One of the things I'd like to do, if we can, is to identify the people who were involved, both directly and peripherally, including students and faculty.

Kleene: Well, I think Barkley and I were the only two who were working with Church for a Ph.D. There was a student there named Alfred Leon Foster, who I think had finished up just a bit before we got there. I had no contact with Leon Foster. I suppose Church's course had six or eight people in it, but I can't remember the name of even another one of them now. None of them went into logic. My previous contact with logic was, as I told you, Whitehead's book and Russell's introduction. I never read *Principia*. Not even later. So all my further training in logic then was what I got in a little bit of background reading in Fraenkel's *Introduction to Set Theory*, a 1928 book, I think. And what I learned in Church's course. He trained us intensively in his new system, which he was just developing. Two papers were presented. I think the second paper wasn't published until well after the course was finished. Church had me write the logic notes for the course, so of course I learned it well. Many standard parts of logic I never learned there. I never learned the predicate calculus. I learned it later on my own somewhere else. I received notes on some beautiful lectures on it delivered by Bernays at Princeton in the academic year 1935-36 (after I was in Princeton). I read them and Hilbert and Ackermann's 1928 book when I got to Wisconsin and had to put together a course in logic. During Church's course itself we studied carefully Goedel's 1931 paper, which we got a preview of in a lecture by John von Neumann. A synopsis of Goedel's paper is in the last section of the notes.

Aspray: While you were at Princeton, were there other courses offered by Church that you took. Or any seminars that he offered?

Kleene: No. Of course when I came back to Princeton in February, 1934, Goedel gave his lectures which are now mimeographed notes, which Barkley Rosser and I took; but there was no other course in logic. Of course I took the standard courses—real variables, complex variables, differential and Riemannian geometry, things that you needed to make up for your Ph.D.

Aspray: Were there other faculty members interested in logic?

Kleene: I think Veblen had an interest in logic. Church had written his Ph.D. thesis under Veblen. Veblen, of course, had done some work on postulate theory, which is a branch of logic, and I think Veblen sort of kept it under his wing. But I didn't actually talk logic with him.

Aspray: Was he more or less the senior person on the faculty who supported the study of logic in the department?

Kleene: Yes, I would say so.

Aspray: Was he responsible for the hiring of Church, do you know?

Kleene: Well, since Church was his own Ph.D. student, I would infer, without having any direct information, that he was pretty well involved. Church certainly wasn't hired against Veblen's wishes. I think that after Church got his Ph.D. he studied in Europe, maybe in the Netherlands, for a year or two. Do you know about this Barkley?

Rosser: I think he was actually at Goettingen for a while.

Kleene: I thought Church paid attention to Brouwer's work, but he didn't need to have been in the Netherlands to do that. I'm sure Church got some of his ideas from this trip to Europe.

Aspray: What sorts of problems were you and Barkley and Church interested in at this time?

Kleene: My first job was to take the notes in Church's course. Sometimes I had a little input myself in how I worked out details in those notes, like proving some theorems, recognizing the structure of formulas, and so on. Then I wanted a Ph.D. thesis topic. Church, in the last paragraph or the last page of his second paper on the foundation of logic, proposed the problem of developing the theory of positive integers on the basis of his system. There was a ready-made Ph.D. thesis problem. With my very limited knowledge of the area at that time, I don't think I could have dreamed up a problem for myself. It proved to be a challenging problem, and I did it.

Aspray: Were there problems in set theory that people were discussing at the time?

Kleene: I have no recollection. I don't want to say there were none, but they didn't touch me.

Rosser: I didn't hear of them either. John von Neumann was interested in ...

Kleene: Yes, we should mention that von Neumann was interested in logic, but I don't think he was there when we came. I think he came a year or two later.

Rosser: Von Neumann actually wrote his Ph.D. thesis on definition of integers in logic. He would come out once in a while and have a chat with us about things.

Kleene: This was probably about '33-'34.

Aspray: He arrived in '31-'32. He was there for two years as a visitor before taking the appointment at the Institute.

Kleene: Okay. He arrived the same year, but I don't think I had any contact with him until '33-'34.

Aspray: He was only at Princeton half-years at that time. He was splitting his time between Princeton and Berlin, I think it was.

Kleene: It was von Neumann's math colloquium lecture on Goedel that put Church, Rosser, me and the rest of us onto reading Goedel.

Aspray: Could you spend a few minutes comparing places where logic was being done at the time, both in the United States and outside? Where were the other centers of research in logic?

Rosser: Harvard had a long tradition in logic, and Quine was sort of the centerpiece of it. He had other people that he was picking it up from, but I don't remember their names.

Kleene: Well, there was [E.V.] Huntington.

Rosser: There was Huntington, but I think there were one or two others.

Kleene: Church was really the first, except that some of Veblen's work is pretty close to logic.

Rosser: Tarski was at Berkeley.

Kleene: I'll tell you when they picked him up. When World War II broke out in September of 1939, I was attending the Fifth International Congress for the Unity of Science at Harvard. Tarski had come to speak at the congress, and while it was going on, the very week of it, Poland ceased to exist, after Hitler's invasion. So Tarski was stuck there. He was very upset; he was cut off from his family; he didn't get to see them until after the war. Hirschfelder and I took him up to climb Mt. Monadnock in New Hampshire. Then [Joseph] Hirschfelder drove Tarski and me to my farm in Maine. We stayed there for a week or so and climbed some mountains and did other things. Then he got a job at some place in New York City on short notice. I guess the Berkeley job was a sequel to that.

Rosser: E.H. Moore was sort of like Veblen, except I think that E.H. Moore took quite a bit more active interest in logic than Veblen ever did.

Kleene: When did Curry arrive at Penn State? Maybe not till a year or two later. He certainly was there during our stay at Princeton.

Rosser: I vaguely remember Haskell Curry being on an NSF fellowship in 1932 or thereabouts, at Princeton for a year and after that at Harvard.

Kleene: Did he actually reside in Princeton? I don't think so. I think it was only visits.

Aspray: I don't remember seeing his name on any of the lists of people at Princeton.

Kleene: I'm almost sure it was only visits, but very frequent visits. Princeton and Penn State aren't very far apart. Was it you, Barkley, who made the joke one time that four-fifths of American logic were at Princeton.

Rosser: Something like that, yes.

Kleene: It was Rosser, Kleene, Church, and Curry, the other fifth being Quine, who was at Harvard.

Aspray: What about European centers? Were there active places in the early to mid '30s?

Kleene: Goettingen, Amsterdam.

Rosser: Goettingen was very strong.

Kleene: Goettingen was probably the strongest, and after that, Amsterdam, because of Brouwer with his own school.

Aspray: Who was in Goettingen at that time then?

Kleene: Hilbert, Bernays, Ackermann. When I got to the Netherlands in 1950, Scholz at Muenster was active, but back in the '30s, I don't know. There were Schuette and Schmidt at Marburg, but this again was somewhat later. I think they had been there for sometime. In the '30s, of course, Zurich became important.

Rosser: Then, of course, Russell and Whitehead. They had been at Oxford.

Kleene: Was it Oxford or Cambridge?

Rosser: It was probably Cambridge.

Kleene: I've heard Whitehead and Russell each lecture once. Whitehead when he was at Harvard. I think he was a visitor for a year, and I got into one of his lectures once. Russell, I heard lecture at the International Congress of Philosophy at Amsterdam in '48.

Aspray: Were either of you, or was Alonzo Church, in regular contact with any of these other centers at the time?

Kleene: Not that I know of. As I say, I'm sure Church drew inspiration from some studies in Europe before he wrote those two fundamental papers, but I'm not sure if he was in contact then.

Aspray: Is it fair to say that the people at Princeton who were doing logic, with the possible exception of von Neumann, were working on Church's system and outgrowths from that, with inspiration coming in from Goedel?

Kleene: That certainly applies to me. Later, before the '30s were over, I definitely familiarized myself with—my interest may have been partly aroused by Church—the intuitionistic logic, drawing mainly on the literature. I used recursive functions to interpret intuitionistic logic. While the work was published in 1945, it was started in the late '30s, but not at Princeton.

Rosser: Church also picked out a topic for me. It was to figure out the connection between the lambda calculus and combinatorial logic. One of the things that I did in the course of that thesis was very important to Kleene in some of the things he did later on.

Kleene: Well, you did it fast enough that I got it very soon. I feel pretty sure you got that result in the spring of '31-'32. I couldn't have gotten anywhere without that. I suppose it was actually submitted and published a year later. Maybe at the time I did my work I was depending on your still working out some details. I got the picture of what you were going to do, and used it.

Rosser: There weren't really too many details to work out. I had these two functions I and J and showed how you could get all the rest.

Kleene: Yes, I guess you probably had it already by that time. I can remember I got my idea one night in McCosh Theatre.

Aspray: Were there other people working on Church's system?

Kleene: I don't think any of the other people worked on it.

Aspray: And no outsiders, as far as you know?

Kleene: No outsiders, very clearly, no outsiders. Frederick Fitch began to work on combinatorial logic. Was that still in the '30s?

Rosser: Yes.

Kleene: He was at Yale, and he came down once to talk with us at Princeton. They sicked you and me, and maybe Church, on him. All three of us. We thought he must have been somewhat flabbergasted: how did all that knowledge come to be there in Princeton?

Aspray: Since you both had close associations with Church, I was wondering if you could tell me something about him. What was his wider mathematical training and interests? What were his research habits? I understood he kept rather unusual working hours. How was he as a lecturer? As a thesis director?

Rosser: In his lectures he was painstakingly careful. There was a story that went the rounds. If Church said it's obvious, then everybody saw it a half hour ago. If Weyl says it's obvious, von Neumann can prove it. If Lefschetz says it's obvious, it's false.

Aspray: So he was very careful.

Rosser: He would start in the left hand corner of the board.

Kleene: With very big legible handwriting.

Rosser: If you were a bit taller, you could see what he was going to say.

Aspray: Was he, then, lecturing to the bottom of his class?

Kleene: He wanted to be very careful that it came out the way he intended it to come out. He didn't want ambiguities.

Aspray: What about in one-to-one conversations? Was he the same?

Rosser: He was not a very good communicator. In fact, I had really very little communication with him. He found me this problem. I worked on it. After a while I came back with a manuscript. He kept the manuscript for two or three weeks. He gave it back and said, "I think that will be a very good Ph.D. thesis." That's all it was. He and I later proved what is now called the Church-Rosser Theorem, and on that we did work together some, but I still didn't have very much contact with him. I'd work on it a while, get some ideas, and explain them to him. He would catch me working on it at home and say, "Yes, I have some more ideas." Quite sporadic contact.

Kleene: You and Church let me read the manuscript and set the footnotes. I corrected something in it.

Rosser: You did indeed.

Aspray: Professor Kleene, what was your experience with Church?

Kleene: Well, he gave me the problem. I worked on it with no close day-by-day contact with him. The first thing I faced was getting a predecessor function. I proposed to do it by changing the set of the positive integers so that I would easily have a predecessor function. I took it to him, and he said, "Well, the trouble is I set up my set of integers in such a way that the definition by induction works." And that's perhaps the only point in which he influenced my project in doing the thesis. I don't know that there were any others.

Of course I was trying to find all sorts of functions which I could prove are lambda-definable, and occasionally he would come up with suggestions for such functions. Church dug up a function using a double recursion on two variables, a method used by some Italian. He dug up a few other odds and ends of functions; they weren't quite ones that I had done already. He'd give me some hints of things to try. But mostly I worked on my own.

My January 1981 article in *Annals of the History of Computing,* Vol. 3 No. 1, gives details on the lambda calculus and this research (for six clarifications and corrections, see Footnotes 10 and 12 of Martin Davis' article in *Information and Control,* vol. 54 No. 1/2, July-August 1982).

Of course he suggested the problem, of representing ordinal numbers in the lambda calculus. I don't think he contributed a solution. He made some suggestions, but I figured out something that I thought would do a good job. He agreed. Of course we wrote it up as a joint paper.

Aspray: Can you tell me something about his work habits? I've heard that they were rather unusual at the time—starting in the late afternoons and working all night.

Kleene: I know how I worked in those years. I started my serious work after supper and went to the early hours of the morning. I then slept till noon, maybe played chess or Kriegspiel in the afternoon, and didn't get down to serious work again until the following evening.

Rosser: The first year I was there, I was a teaching assistant, for three days a week. Of course I was staying at the Graduate College. I don't think they served breakfast then after 9:30. I hated to miss breakfast as I had paid for it, so I managed to get out of bed just in time to get breakfast.

Kleene: I don't know if I really missed breakfast. There were times I worked like that, but probably that was not what I did all the time. Of course the first year I wasn't doing any research yet and was teaching.

Rosser: My contacts with Church were pretty sporadic.

Aspray: Did the two of you talk much about the problems you were working on?

Rosser: Yes. We had a lot of discussions. As Kleene says, he wrote my thesis and I wrote his.

Aspray: Can you tell me something about the circumstances which brought Kurt Goedel to the Institute?

Kleene: Well, I'm sure von Neumann was back of it. Wasn't von Neumann at that Koenigsberg Colloquium in September 1930?

Aspray: I believe he was.

Kleene: He was, I know. It's come back to me. He heard Goedel present the preliminary version of his first incompleteness result and was very much excited by it. They exchanged thoughts with each other there and by writing afterwards. Von Neumann was a pretty sharp fellow; he recognized that this was the most brilliant man in logic that had showed up. So I'm sure he told the people at the Institute, "You'd better get him over here." Goedel was having trouble getting a good job in Vienna, so he was only too willing to come. Of course he came at first to visit. It wasn't until about 1939 that he became permanent. They dragged their feet on offering him a regular position in Vienna. In '39 he came to Princeton via the Trans-Siberian railroad, and I believe he never left the United States again.

Aspray: He was made a permanent member at the Institute, but he was never made a faculty member.

Kleene: I think he was made a faculty member in the late '40s. He became a permanent member in, I think, 1939. I've got a biographical memoir which I've written for the National Academy; it should be published very soon.

Aspray: What sort of presence did he have at Princeton? How did he affect research that was going on there?

Kleene: He affected research very much in that he gave lectures on his undecidability stuff. In the course of that, he produced a notion of general recursive function. This became the subject of my research, which was a sequel to the things I'd done with Church. I guess I've never quit working in that area.

Aspray: Professor Rosser, did you have regular contact with Goedel?

Rosser: Very little. Goedel was not the kind of man who spoke to people on his own. It was hard to get to talk to him at all. But he was very polite about things. If you called and said you would like to talk to him, he would hem and haw, and finally he would say, "Would 10 o'clock next Tuesday be all right?" And so you would present yourself, and he would talk to you, answering whatever questions you had to ask him. Very seldom would he volunteer any himself. You'd ask him questions, and unless you saw him again, that was the last you heard from him.

Kleene: I think Barkley wrote the first part of Goedel's lecture notes, but I wrote the second part. I sort of remember it that way. But his lectures ran just February through May, 1934. He was about to leave for Europe. I gave him a set of the notes, as we had them ready just before he left. He was going to take it to his hotel room in New York, prior to flying. Or rather sailing; it was probably a ship in those years. I told him, "We have a copy at Princeton too. If you don't send us word before you embark, we will bring them out from the copy we have." When he gave his Gibbs lecture at Yale somewhat later, somebody should have played the same kind of trick on him: recorded it and said, "If you don't give us the manuscript, we'll bring it out from our copy," because they never did get a copy of that.

Aspray: How was he as a lecturer?

Kleene: He didn't have that extremely careful, slow, deliberate style of Church, but I think he was pretty clear. We took notes, sometimes we would have to check them with him a little bit. But when he sent our notes back to us, you would see there were a page or two of notes that were missing. He visited my farm in Maine with his wife in the summer of '41. It could have been '42. No, it was '41, because I wasn't in the Navy yet. We exchanged some mathematical thoughts. I told him about my realizability interpretation of intuitionistic number theory, and he said he had an interpretation also using partial recursive functions. I guess he told it to me, but I'm not good at getting something by ear. I remember we were walking through some of the fields on my farm, and he would sit down and write it out. That came out, I guess, in an article or two later.

Aspray: Did he attract post docs or graduate students to Princeton after he came? I know you weren't there most of the time.

Kleene: Of course we were already in Princeton when he first arrived in '34, and I went back to the Institute in '39-'40, and in '65-'66, because I wanted a congenial place to work, where I would be supported and have my time to myself. I didn't go there because I was attracted by him, but I had some contacts with him.

Aspray: How much did you two and Church participate in other areas of mathematics at the time?

Kleene: As far as I'm concerned, not at all. As I told you, I didn't get taught much of the classical logic; that I learned on my own. I came here to Wisconsin in the fall of '35, and I gave a math club talk on Goedel's theorem, which I made to depend on the notion of general recursiveness using Church's thesis. It went over big. For example, they asked me to teach a graduate course that fall. And in the summer of '36 they gave me a summer research appointment, during which I prepared my course and studied a lot of the things, like Hilbert and Ackermann's book, which I had not previously learned.

Aspray: What were your experiences along these lines, Professor Rosser, the rounding out of your logic education?

Rosser: Well, as far as conversing with Church, not at all. We found that the lambda calculus was of considerable interest, and we began working on that. Of course its importance came out in Church's thesis. Church and I, and probably Church and Kleene, had quite a few conversations about Church's thesis. Church, as far as I know, never worked in any area except logic.

Aspray: Where did you pick up the rest of your logic background?

Kleene: I should mention also that I got a lot out of Hilbert and Bernays' 1934 book, which I read pretty soon after it came out, and Heyting's *Ergebnisse* of 1934. Those were among my main sources for rounding out my logic education.

Rosser: I filled in my education essentially by reading Heyting's book. It would have been nice if *Principia Mathematica* were done this way.

Aspray: Had either of you seen much of Brouwer's work at that time?

Kleene: I don't suppose I started reading Brouwer's own papers till the late '30s, when I undertook the project of seeing whether I could tie up his ideas of constructiveness with Church's thesis—and with things that could be represented by general recursive functions. Then, of course, I studied it and the first thing to do was number theory which was fairly straightforward; but then I wanted to extend it to intuitionistic analysis, and at that point I had to study all that Brouwer stuff very carefully. But my extension to analysis was late in the '40s. In fact, I went to the Netherlands in the spring term of 1949-50 on a Guggenheim Fellowship and was there with Brouwer, and Heyting, and Beth, and van Dantzig. Of course I knew some of their work before I got there.

Aspray: While we're on this topic, may I ask how the Turing computable-number paper affected the way you talked about effective functions.

Kleene: We recognized immediately that this gave further support to Church's thesis. It was immediate support, because as I put it in my paper in *Annals of the History of Computing*, Turing's definition of computability was intrinsically plausible, whereas with the other two, a person became convinced only after he investigated and found, much by surprise, how much could be done with the definition.

Aspray: On the other hand, you continued, when you did your own work, to work with general recursive functions.

Kleene: I think it is actually easier to work with general recursive functions than with complicated Turing-machine tables. In fact, E.L. Post criticized the details of Turing's paper. I read the paper myself. The content is beautiful, but the way he worked out his details is somewhat of a mess. But by the proofs of equivalence, we know anything you can do in one system can be done in the other, and that is a significant thing. Of course Turing did it independently, and he discovered what we'd done just as he was ready to go to press. We discussed that problem. [My recent information is that Turing's work was later than ours (*Biographical Memoirs of Fellows of the Royal Society*, vol. 1, Nov 1955, p. 254). S.C.K.]

Rosser: Of course Turing did it before us. Turing, of course, was awfully concerned with the correct way to do it. "You do it on a machine, see."

Aspray: Where does Post enter into this? Post had been coming down to Princeton from time to time, hadn't he?

Kleene: Yes. Post came up with an idea which is essentially the same as Turing's, independently of Turing, although at the time he knew about our stuff. There was another approach of his in work he did in the '20s but never quite pushed through then. A formulation using his "canonical systems" from that was published by him in 1943.

Rosser: That first paper of Post's was very similar to Turing's.

Kleene: Very close to Turing's, yet independent.

Aspray: Did Post come down to see Church regularly?

Kleene: I don't think so. Post and I did a joint paper on degrees of recursive unsolvability. It resulted from his having published an abstract claiming certain results in this area. I wrote to him, "Please publish the paper. Because as long as you've got that abstract published, with sort of a promise of a paper to follow, nobody else wants to invest any time on it." He said, "Well, why don't you people do it?"

So I gave it to a graduate student that I happened to have at that time. He had the biggest string of A's in courses that you ever saw, but he never did any research. You'd just give him problem after problem, and these were often problems which were solved by somebody else a year or two later, so they could have been solved. This was one of the things I said to Mr. X, to try to work up something from Post's ideas on the degrees of unsolvability. X didn't do it, so I did it myself. Working from something which I undoubtedly had in my files, probably a long letter from Post, I worked up a manuscript. I sent it to him and said, "Emil Post, I am very pleased to say that in this manuscript I have answered what you considered the principal unsolved question in that letter you wrote me."

He died suddenly. He was given shock treatment, and in the shock treatment he burst an artery and died. I don't think Post often came to Princeton during the '30s. I can't remember ever seeing him in Princeton. In his famous paper "Recursively Enumerable Sets of Positive Integers and their Decision Problems", he depended on intuition to recognize that things were effective and therefore, by Church's thesis, recursive. We had some correspondence about whether you had to depend on intuition or whether there were uniform methods by which you could do it almost automatically. I wrote to him that I had found a uniform method, which I call "the recursion theorem". I proposed to him that I come up and show it to him. He never found the time.

I've been together with him several times. I was in the Navy when he presented his paper "Recursively Enumerable Sets..." at a meeting of the American Mathematical Society [on February 26, 1941]. I was teaching midshipmen in the U.S.S. John Jay. That's not a ship, it's a Columbia dormitory. But when midshipmen entered, they said "Request your permission to come aboard, sir." I only had to walk 200 yards to go to the lecture. After the lecture I had Post over to my apartment. I had an apartment just off the Columbia campus. There I presented to him—I think it was already in press, if it wasn't, it was in manuscript—my paper, "Recursive Predicates and Quantifiers", which had a very close relation to what he was doing. That's the first he knew of that.

So we had some collaboration, but it was rare that I met him in person. He didn't come down to Princeton to talk with us regularly. Maybe he came down to talk with other people in the late '30s, but I doubt it. I don't think it was his habit. As I say, he hardly showed up at meetings. There was a meeting of the Math Society in New York City, and my wife and I had an excursion to West Point on a riverboat. He talked with me on that riverboat. It was the longest visit I ever had with him, going up to West Point and turning around there and coming back.

Aspray: Some mathematicians today view logic as a somewhat peripheral area of mathematics. How was logic viewed at the time by the rest of the Princeton mathematical community?

Rosser: I would say it was accepted. Part of that was probably due to Veblen. When I got my degree and was going around trying to get a job, I'd hear, "What are you writing to us for? Why don't you write to the philosophy department?"

Kleene: Well, logic was considered peripheral at that time, and that I'm here is due to the fact that Mark Ingraham was very much like Veblen. Although he worked in algebra and geometry, he definitely was much interested in logic. I guess he read or heard something about Brouwer's work, so he had an interest there. I'm sure that he accounts for Wisconsin's picking me. He invited me to give a graduate course in logic my second year at Wisconsin. There weren't many places that thought of logic as something that the department must have. Now it seems to me that in between, when we got results on decision problems that have arisen in mathematics outside of logic, that is, in topology, algebra, and so on, showing some of them to be unsolvable, people who had been trying to solve those decision problems had to admit the relevance of logic to other branches of mathematics. So there was a period in which I think logic was widely accepted. Now you say it's regarded as peripheral. Well, I didn't realize that is back.

Aspray: Some mathematicians view it that way.

Kleene: We never expected to have everybody in on it. As long as we got quite a few people, widely distributed, sold on it. Of course the Association for Symbolic Logic brought the various practitioners together. For example, the philosophers who were interested in logic were probably rather logical for mathematicians. But the ASL got us together, so we could talk to each other and publish in the same journal. Of course I don't know how the split in membership between the mathematicians and philosophers is now.

Aspray: Was there any contact at Princeton with people in philosophy?

Kleene: Scarcely. I went to Princeton from Amherst, where I split my interests between mathematics and philosophy. When I got to Princeton

I made a point of attending the Philosophy Club and listening to the lectures, but I didn't get involved in any discussions in those clubs. I guess after the first year, I dropped that. I can't even remember who was in philosophy at Princeton. I can at Amherst, but not at Princeton.

Aspray: Was there an undergraduate course taught in logic at that time?

Rosser: Not that I know of, though that does not mean there were nt any.

Kleene: I don't know. Here at Wisconsin we didn't get an undergraduate course in mathematical logic until the '60s. I'm sure the philosophy department had some course in logic, but it wasn't heavily mathematical. Philosophers can't help it, they're made that way. It goes way back to Aristotle.

Aspray: Can you tell me something about the founding of the *Journal of Symbolic Logic?*

Kleene: As I say, there was this movement to try to bring philosophers and mathematicians together into an organization where they would talk to each other. An organization wasn't effective unless you had a journal. That's about all I know.

Rosser: As a matter of fact, I was one of the ones involved in the founding of the journal. Quine was another.

Kleene: You were all in the East, and I was pretty far out west then. I might have had a little correspondence about the *Journal*, but I didn't actually meet the people involved.

Aspray: The fact that Church was Editor, is that indicative of his major role in the founding of it?

Kleene: I don't know what organization it had. There was Ducasse, a philosopher, and some other people at Brown. I don't know whether Church was one of the people who helped push for it, or whether they took advantage of him. Now it must be noted that, while Church was very much concerned with his own system and that's essentially the connection in which we saw him, he had a great historical interest and a great bibliographical interest. He was very much interested in the broad field of the literature.

Aspray: Wasn't it in the first issue that this bibliography of logic appeared?

Kleene: Bibliographical material appeared from the first onward, in that the reviews were intended to provide a complete bibliography of the current literature. Church's *A Bibliography of Symbolic Logic*, for the literature up to 1935, appeared in Vol. 1 No. 4, with *Additions and Corrections* and a subject index in Vol. 3 No. 4.

Aspray: We've already mentioned your getting your first teaching positions, but I'd like to come back to that for a few minutes, and I have a few related questions. I've been told that Veblen felt strongly that certain positions at certain schools should go to people in certain specialties. For example, there was once an applied mathematician whom he wouldn't recommend for half of the jobs, because he felt they should be reserved for pure mathematicians. Do you think that some faculty were reluctant to recommend you to certain places because of your field?

Kleene: I haven't the slightest idea. After I got my Ph.D. I was for a year and a half a resident at Princeton and an obvious candidate for a job. The only thing that opened up was at some small college in southern Vermont, but there I would have to coach an athletic team, in a sport in which I hadn't participated. I did participate as an undergraduate in swimming and track, but it was something else. Then I was offered a job in the New York City school system, teaching at the high school level. Several new Ph.D.'s who were on the market did that, but I chickened out. The job in Wisconsin was the first genuine offer of an academic job in a university which I received.

Rosser: Because of the Depression there weren't many jobs.

Kleene: Of course Wisconsin offered me a teaching position, 15 hours a week, at a salary which, I think, came to $1325. The official salary was $1800, but there was a voluntary waiver because of the Depression. It could well be that Veblen was consulted in deciding what names to suggest to Wisconsin and thought maybe "A logician might not be bad for them." And as I say, Mark Ingraham may have played a role here. He had a strong interest in logic, although he didn't work in it, and he was the Chairman at the time, which was, of course, an influential position.

Aspray: How did Cornell come to hire a logician? Can you recall the circumstances?

Rosser: For a number of years they had not been hiring anybody, but finally money was becoming available, and also they had a Dean who was clamping down. They had a rule that you couldn't be on the faculty more than six years without promotion to tenure. And they had three or four people who were approaching that six-year limit. So all of a sudden, Cornell had to fill about four places. Saunders MacLane and I came at the same time, which gives you an idea of what was going on.

Aspray: I understand it was a fairly common practice at Princeton in the '30s, that if a junior faculty was the person most closely associated with some graduate student's work, a senior faculty member would nominally be responsible for the thesis. Was that true in either of your cases, since presumably Church was the closest one?

Kleene: That is a story I haven't heard before.

Rosser: I hadn't heard that before. I think at that time Church was only an instructor.

Kleene: I think he was assistant professor.

Aspray: Who were your readers? Who was on your oral-finals committee?

Kleene: I can remember H.B. Robertson was a thesis reader, because he fell in the bathtub that summer, which delayed my thesis being read. Church was a reader. If there was a third, I can't remember who it was. It might have been Lefschetz, but I'm not sure.

It was a toss-up whether they would let me back at Princeton for my second year. The first year, you see, I didn't do research. I just took courses, and I didn't do a good job as an instructor. I overestimated the students' abilities. I made it a little too hard for the students. That was my first regular teaching job.

So there was a bit of a question, and I went around to talk to some people. One I remember talking to was Solomon Lefschetz, and they decided to give me the second year. As I said, I'd done no research, so they had no way of knowing whether I had research ability or not. Well, I guess they changed their mind about that after I did my first few weeks of research on Church's system. I think I was up in two weeks to my lambda-definition of the predecessor function, of which Church had just about convinced himself there weren't any.

So the second year I was there I didn't have any support from Princeton, but I got a proctoring job at the Hun School. I got room and board for answering the telephone and sleeping there to help watch the students.

Then I did some research. Before I was done with my second year, I'd probably made the basic discoveries about the potentialities of lambda-definability. Maybe I didn't have the recursion theorem, but at least I had the predecessor and the P-function, which is the least number operator. So they changed their mind, and the third year I had a fellowship.

Aspray: Princeton is certainly different from Wisconsin today in respect to what constitutes a Ph.D. program in mathematics, both in course work and in the length of time it takes to get a degree. I understand Princeton is also somewhat different from even its main rivals, Harvard and Columbia and Chicago, in respect to these things, how long it takes to get a degree, how important the course work is. I wondered if each of you could discuss for a few minutes what constituted your degree program. What kind of courses did you take? What were your qualifying exams like? Perhaps you could give me some idea of the philosophy behind the graduate program at Princeton.

Rosser: I don't think I can tell you a thing about the philosophy, because coming into the program with a master's degree, I was fairly advanced. I was there for four years. I had more than enough of everything, so there was never any question that I'd done what I should for this or that requirement.

I do remember, the fall I came, talking to James Alexander, who was in charge of helping the freshman graduate students decide on their programs. He was a charming gentleman. He told me to come to his office. We had an interesting conversation. He was a very fluent conversationalist. We talked for a whole half hour, and finally he said, "Well, I assume you've figured out what you want to take. Nice to meet you. Goodbye."

Aspray: As I understand it, at the time students took three standard areas in the qualifiers and then chose two additional areas to be examined in. Is that correct?

Rosser: I don't know. I was minoring in physics, so I was taking physics courses too.

Kleene: I can't answer your question directly. I don't remember. I can pretty well remember what courses I took. I took differential and Riemannian geometry from [Luther] Eisenhart. I took algebra from [J.H.M.] Wedderburn. I took real variables from Einar Hille. That probably was the best course I had; at least I got the most out of it, except for the logic course. I think I took complex variables from [H.F.] Bohnenblust. I probably took a topology course. I don't remember any designation of majors or minors; I was examined on what I'd taken when the prelim exams came up.

Rosser: When Eisenhart was there and taught these courses in Riemannian geometry, differential geometry, and such, I took them. I did well in his courses. He was on my committee.

Aspray: Do you remember people whose courses you thought were particularly good, or particularly badly taught?

Rosser: I don't think so.

Aspray: A number of people have mentioned to me Bohnenblust, for example, was a very good lecturer.

Kleene: Bohnenblust was very good, no doubt about it.

Aspray: Could you tell me something about your teaching assignments while you were graduate students?

Kleene: I taught the first year half-time. I probably met two sections, I imagine two different classes each two times a week. I think that was the load, and I think it was beginning calculus. It turned out that by the time the year was over, I found I had taught on too high a level. It was a little hard for me to get down to their level.

Rosser: I had had some experience teaching at the University of Florida when I came to Princeton. There were two different calculus sequences. One was for the engineering student, and the other was for anybody else. They decided since I had experience in physics, I could communicate better with a physicist student, so they had me teach the engineering students. They used a different calculus textbook.

Aspray: Do you think the department was particularly upset about your performance?

Kleene: They were upset enough; it was a question whether I would come back for my second year. I can remember going down and being in [Solomon] Lefschetz's home discussing this. They decided to let me have another try, another year. They had no way to measure my research ability, and I don't think they did very much about measuring how you did in the classes.

Rosser: They usually didn't give any final exams.

Kleene: If some student had volunteered some contribution to the course, the professor would have noticed. But if you just took the course and studied the material and learned it, there was no way for their ever knowing you had done it. I took Church's course as the notetaker; he undoubtedly was sometimes surprised when I put something of my own into those notes beyond what he had said himself. But in the other courses, like real variables—now I liked the material and became master of it—your performance wasn't apparent. I think that Hille was very friendly with me. I wasn't so enthusiastic about Eisenhart's stuff. As I say, they didn't really find out what I could do or not, until I had a chance to start doing research in the middle of the second year, except for that notetaking.

Aspray: I've heard that Eisenhart and Veblen had disagreements over how important the teaching responsibilities were of the graduate students and post-doc students. Veblen was so concerned about having a research community there that his attitude was, "Don't put extra time into it," and because of that Eisenhart had to go to bat for the administration on a number of occasions concerning teaching.

Rosser: This is an area where I have no information whatsoever.

Kleene: I don't have any information. I don't blame Eisenhart for being concerned. When the year was over, they realized, and by that time I guess I realized, that I had tended to talk over my students' heads. Well, you know, I found mathematics so easy. I had graduated from Amherst College summa cum laude and so on, so I probably wasn't much in contact with the average and the below-average student.

Rosser: It reminds me of a story I heard about a graduate student. He was supposed to teach trigonometry. So after three weeks, he came to the head of the department and said, "Well, I taught them the trigonometry, what do I do the rest of the semester?"

Kleene: There are some stories about Stan Ulam when he taught at Wisconsin, concerning what subjects he worked into his courses. I can't remember what the subjects were, but in one he worked in things that were beautiful mathematics, but you'd never think of putting them in the course.

Aspray: Were you aware of any problems between the math department and the rest of the university? The reason I ask this is that the University has always thought of itself as an undergraduate institution, with graduate education a small appendage. But in mathematics the role was really reversed. It was clear that the main interest was in graduate education and mathematical research. Did you see any battle that went on along those lines?

Kleene: I can't give you any information.

Rosser: The fact is that I didn't see any conflict there.

Aspray: You were, of course, graduate students, intent on your own work and part of the research community. Why don't we then turn for a short while to questions about this research community that was fostered. It seems that Veblen especially had a large role in doing this: designing the new building, creating an atmosphere for people to talk with one another, the tea, the common room, and such. Can you talk about how Princeton was as a community for people to get together, and maybe compare it with other places you have been?

Kleene: After they got the new building, of course, it was very good in mathematics.

Rosser: I couldn't do that very well, having been someplace else since. Of course I did have a master's degree in physics. At Florida, it was a very small department, and it was easy to get together.

Aspray: Did you both regularly attend the teas?

Kleene: Yes.

Rosser: Yes. One year I was in charge of tea. I think that was one of the years I had a fellowship. They said, "You've got something to do: you have to run the tea."

Kleene: I think that the fellowship, which I had my third year, included some work on the tea. I guess I was part of the group of Fellows that had to run the tea.

I was going to say when we were thinking about a new mathematics building here at Wisconsin, Mark H. Ingraham told me the National Science Foundation had money for research facilities and wondered if we could get some benefit out of that for math. So I wrote up a proposal. I said that it's just as important for mathematicians to have a room in which the faculty and students could get together and stimulate one another's thinking as it is for the physicists to have an accelerator, the chemists to have a centrifuge, and so on. I said that more mathematical ideas had originated per square foot of floor space in the common room of Fine Hall at Princeton and Eckhart Hall at Chicago than in all the rest of the buildings. We stripped the NSF of $100,000, which helped us get the top floor of Van Vleck Hall [= the common room].

Aspray: I was going to ask about that.

Kleene: Of course the floor cost more than $100,000, but having the $100,000 to start with, the state put up the money to finish the thing off.

Aspray: With regard to the social environment, I understand that some of the faculty members at Princeton, in particular the Alexanders, the von Neumanns, and the Robertsons, were very social people, who had grand parties, often inviting the graduate students to come. Were you made to feel part of the math community in that way? Did you attend?

Kleene: Oh yes. And Church was very hospitable.

Rosser: Mary Church would have me around three or four times a year for dinner. Churchill Eisenhart (son of Dean Eisenhart) was taking this course under Bohnenblust. It was a calculus course, and Bohnenblust was hard. Churchill would come around and get me to do integrals with him. Every time Bohnenblust decided that Churchill was getting the integrals he'd say, "By golly, I'm going to fix up an integral so hard that old Dean Eisenhart can't do it. Harder and harder and harder, I've been busting my brains."

Kleene: The Dean never saw them.

Aspray: Who won out? Were you able to solve all the integrals?

Rosser: I don't remember who won out.

Kleene: At Princeton I profited very much from two circumstances. The first was that Church had just come out with his new system, in which was imbedded the lambda calculus as we later dubbed it, that nobody else (except, in the preliminary stages, Church himself) had had a chance to work on, and the second was that Goedel's incompleteness theorems and his general recursiveness became available, which I eventually melded together in generalizing the first incompleteness theorem. The big inputs were from Church and Goedel. And then I profited from a very important thing that Rosser did—making available the relation of Church's lambda calculus to the combinatory logic.

Rosser: Certainly these things were overlooked when they started having the Institute there, because for the first two or three years, they had the complex system of Fine Hall, Fine Hall was really crammed with mathematicians.

Kleene: Was Fine Hall there your first year?

Rosser: Yes.

Kleene: My first year was the basement rooms of the Palmer Physical Lab.

Rosser: In fact, I think they had the dedication ceremony when I was first there.

Kleene: My point was that Goedel was brought for the Institute, not the University. Of course it's very nice to have a beautiful environment, as at the Institute, for doing research, which I took advantage of two different years later in my life. I was all set to referee Gentzen's 1936 paper on the consistency of number theory. Hermann Weyl got the manuscript and he gave it to me to be the referee. Within a day or two I got the offer from Wisconsin, so he took it back and gave it to somebody else.

Aspray: I understand that almost all of the refereeing for *Annals of Mathematics* went on within Fine Hall. Can either of you tell me something about Veblen? He seems to be such an important figure in getting research started at the University, and then at the Institute. What dealings did you have with him?

Rosser: He was one of the very active ones from Chicago at the turn of the century, a bunch of whiz kids. There were Veblen, Eisenhart, and a couple or three more of them. Veblen went to Princeton. Oh, G.D. Birkhoff was one of them. So, when Veblen got to Princeton, he set himself the task of transforming Princeton into being what he remembered Chicago as having been.

Kleene: I can add to that. Veblen wrote these beautiful books on projective geometry and a number of interesting papers. My contacts with Veblen were always very friendly, and I felt I had a strong supporter in him.

Rosser: At the University of Florida I had a course in projective geometry, and we used Veblen's textbook.