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

The Princeton Mathematics Community in the 1930s
Transcript Number 35 (PMC35)

ALBERT TUCKER

THE PEOPLE AT PRINCETON IN THE 1930s

This is an interview on Friday 13 July 1984 with Albert Tucker in his office at Princeton University. In this session we will talk about the backgrounds, the personalities, and the roles of the people in mathematics in Princeton in the 1930s.

Tucker: I am going to start with the list of professors of various ranks in the department of mathematics in 1930-31. J.W. Alexander. We've mentioned him very often. He was a Princetonian by birth, as well as in terms of his education, both undergraduate and graduate. Then he became a member of the faculty at Princeton. He was a member of an old Princeton family. He was quite well to do, so that it was really never necessary for him to work for his living. He had in his earlier days, say around 1920, been quite interested in the socialist movement. At that time Norman Thomas was the perennial socialist candidate for the presidency.

Aspray: Yes.

Tucker: Norman Thomas was a Princeton alumnus, very highly thought of by Princetonians whether they agreed with his politics or not.

Aspray: I see.

Tucker: But Alexander apparently did agree with the politics of Norman Thomas. I think that at some point Alexander was active as an organizer for the socialist party in and around New York City. I'm trying to present the flavor of the man. He was very handsome, always nicely dressed, but not in a formal way, in sports clothes. He was very kind to students, but in a rather fatherly way.

When I came as a graduate student he was the so-called Graduate School Representative for the department, and it was necessary for me to arrange certain formalities with him, such as the courses that I was going to sign up for. Later on, when I was ready to take my general examination, I had to arrange this with him, and he had to approve the list of topics that I was submitting. And in all of this he was very kind and meticulous in his looking after the details. But he was not one to engage, at least to my knowledge, in chit-chat. He had been the student of Veblen, and Veblen clearly had the very highest opinions of Alexander and was usually trying to nudge Alexander into doing things in the way of departmental responsibilities.

Aspray: I see.

Tucker: But clearly Alexander did not take to these things, and after doing it for a year or two he would ask to be relieved from the responsibility.

Aspray: What sorts of things do you have in mind?

Tucker: I'm thinking of such things as the Graduate School Representative, or to be in charge of planning the courses and who would be teaching them. In other words, he would be asked to share in the administrative responsibilities of the department. He would take a turn at them, but clearly did not relish this. Also, he did not like, I think, the responsibilties of teaching courses.

Aspray: Yes.

Tucker: It was only something that he was interested in for his own mathematics. He was a good teacher, an excellent teacher, but I don't think that he enjoyed teaching.

Aspray: You talked in an earlier interview about his performance in seminars where he was presenting new ideas, how his perfectionist tendencies usually led to discontinuation of a seminar.

Tucker: Yes.

Aspray: Did he teach undergraduate courses? How did he feel about those?

Tucker: He did not teach undergraduate courses when I knew him, but I think that up until sometime in the '20s, mid '20s, that he taught a regular teaching load. But at some point he asked to be given a half teaching-load, and, correspondingly, a half salary.

Aspray: Yes.

Tucker: He didn't need the salary, and he found it irksome—to be obliged to teach courses that he wasn't particularly interested in.

Aspray: Yes.

Tucker: Now he was a person who, if he agreed to teach a course, would be very conscientious about doing it. But he would probably find, if it was an undergraduate course, that there was no textbook to his taste from which the course would be taught.

Aspray: What would he do if he had to teach a course like that?

Tucker: I think that he would try to work up a set of notes with the help of the students, and do the course without a textbook. This, of course, is a lot of work.

Aspray: Yes, very much.

Tucker: And he didn't like that. But if he had to teach the course, then he did it whether he liked it or not. He was, with his wife, very active socially, I mean in terms of parties. And he liked to play tennis, but if he played tennis then he wore white flannel trousers of the approved variety for social tennis.

Aspray: I see. Yet he was quite a competitor, wasn't he, at tennis also?

Tucker: Yes. Of course he was in perfect physical condition, because in the summer he climbed mountains and in the winter he went on skiing trips. He was very expert in both of these respects.

Aspray: Can we come back and talk a little bit more about the relationship between Alexander and Veblen, on an intellectual level. How much was Alexander able to break the student-mentor tie that he had with Veblen, having been trained by him?

Tucker: Oh, I never felt that there was this tie noticeably by, say, 1929. And by that time Alexander was going completely his own way in topology, or analysis situs as it was still called. Veblen had left analysis situs around 1920 and gone into various forms of differential geometry.

Aspray: What about in other professional matters, whether at the university or outside in the larger mathematical community. Would Veblen be likely to ask Alexander's advice on professional questions?

Tucker: He might, but I would say his main relationship with Alexander was when there was something that Veblen wanted to get departmental approval for. He would go around like a political boss and go to the people that he had ties with and make use of those ties to get them to join in the enterprise. Veblen was able to do this in a very slick way. I became well aware of it myself, because he would try to get around Lefschetz' opposition to things after he was no longer a member of the department by coming and talking to me and trying to get me interested in the idea and even suggesting that I propose it to Lefschetz as my idea. I don't mean to imply that he was underhanded or anything like that. He wasn't at all, but he was adept at getting the people with whom he had ties to do things for him.

Aspray: Veblen was clearly a political animal in this regard.

Tucker: Yes, yes. He at no time was anxious to have a title such as chairman of the department. The fact that when the Institute was set up there was nothing in the way of a head of the school of mathematics was, I'm sure, Veblen's idea. Then of course the second director of the Institute for Advanced Study was Frank Aydelotte, who was following the Quaker tradition. The Quaker tradition is that there is no chairman of a meeting except in the sense of someone who preserves order.

Aspray: Coming back to Alexander for just a minute. It seems almost as though he didn't play a very great role in the development of Princeton as a major mathematical center other than in his own personal intellectual contributions.

Tucker: That's right. It's perhaps a harsh thing to say, but he was a very beautiful ornament as far as Princeton mathematics was concerned in that he was creative, he was an excellent representative at all levels, so to speak the ambassador figure.

Aspray: Yes.

Tucker: It was impossible to dislike him in any way, because he was so kind and courteous and thoughtful. But there was no drive, there was no competitiveness, except possibly in mountain climbing, none in mathematics or academic ways. Alexander took his Ph.D. in 1915, somewhat late. He had already been writing papers for three or four years. Getting his Ph.D. was really just a sort of an afterthought, again an indication of this non-competitiveness.

Church, another protege of Veblen, came along in the 1920s. He, like Alexander, was a Princeton undergraduate. Veblen discovered him as an undergraduate. Church just kept on here at Princeton as a graduate student. But Church was an opposite sort from Alexander. Church's father, I happen to know, had been a judge, but had lost his eyesight. From the times I knew him, he was living in the Church home and did not get about very much because of his blindness. Church after getting his Princeton degree in the mid '20s, Ph.D., had a post-doctoral fellowship in Europe, and then was appointed to the Princeton faculty.

Aspray: How common was this? Was there any concern for doing this, sort of inbreeding questions?

Tucker: I don't think there has ever been concern about that in the Princeton mathematics department. Perhaps because there has always seemed to be a pretty strong infusion of those from the outside. But at any time there have been up to half of the people appointed who have had previous connections with the department. I think it's only when there is such a group and it becomes dominant, and controls everything, that there is danger from that inbreeding.

Church also made very little contribution to the department outside of his own research, which of course was of the highest quality. There again he went his own way. He had been the protege of Veblen, but once he had his doctorate he followed his own line of interest. Veblen probably at times tried to mix in. We've already heard that from Foster, who said that he had started on this Ph.D. thesis with Church, but that he was adopted—that was the word that Foster used—by Veblen. I would say 'taken over'. I'm sure that Veblen felt that he had a wider experience on things than Church did, which was certainly true, and that Foster would benefit from this greater perspective by having the thesis done with Veblen. And of course it didn't make any difference to Church. He was glad to help Foster, but if Veblen wanted to take this on, that was fine. No bitterness.

Aspray: Would it be fair to say that Church was shy, or a recluse of some sort?

Tucker: Most people would regard him as very shy or just as not being interested in ordinary day-to-day activities. But I have been told by people that I was wrong in this. I remember on one occasion commiserating with some faculty wife who had been seated next to Church at a departmental dinner party and saying, it must rather tough to have had Church sitting next to her. But she said, "Oh no, we had a very nice conversation." I was so surprised that I asked, "Well, what did you talk about?" "Oh, we just talked about ordinary things." It was almost as though Church felt that nobody was really interested in him or cared about talking to him, but if they did, then he met them halfway.

Aspray: I see.

Tucker: He was very conscientious in his teaching, but due to his logical bent he was never able to teach in a course where there was a a common examination, where it was necessary for the various sections of the course to keep in step. He had to dot every 'i' and cross every 't', and the result was that he would cover in a term only about half as much as the other people teaching in the course would cover. So it was necessary to have him teach in a course where this didn't interfere too much. This was always a great difficulty in planning Church's teaching. Now, of course, he would usually teach a graduate course, usually in logic or, say, recursive functions. There, there was no problem, but to have him teach in the undergraduate program was difficult.

Church, unlike Alexander, had to earn his living. He had to provide for a wife and a dependent father and then for children that came along, so Church was very dependent on his university salary. His great contribution to the department, I feel, was in his work with graduate students and, coupled with that, his work as editor of the *Journal of Symbolic Logic*. He and Bochner, whom we'll be talking about later, supervised more Ph.D. theses than any other members of the department. This is impressive, especially in view of the quality of his students. It would be interesting to make a count of the total that

Church supervised. My guess is that it would be somewhere between 30 and 40 over the period that he was at Princeton. And from quite early on, he had the main editorial responsibilities for the *Journal of Symbolic Logic*. He continued on that all the way until he retired from Princeton. Indeed he retired from Princeton early to go to UCLA because of the *Journal of Symbolic Logic* being transferred to UCLA. And as you know, he is still actively teaching and working on the journal at UCLA.

Aspray: Yes.

Tucker: But outside this really tremendous contribution there was very little else. He was never given any departmental responsibilities. He simply taught whatever he was scheduled to teach and worked with his students on his own. The students would very often have to make their own formal arrangements. The ordinary thing when a student's thesis was ready for submission was that the member of the faculty who had been supervising the thesis would arrange for the formal reading of the thesis and the departmental approval of the thesis and notification to the graduate school and the setting of a time and place for the final oral examination. With Church the student would have to do this; Church just didn't give this a thought. He wouldn't come to department meetings unless he was especially asked to do so. Just a notice, a written notice, coming around to the members of the department he would pay no attention to. This was not any deliberate oversight on his part. It was just that he had other things on his mind that such notifications made no impression.

If you went to his office and went in, he'd speak quite pleasantly, and you'd say that there was a department meeting and such and such important matters had to be discussed. And he would say, "Well, would you like me to be there?" The answer would be "yes, we would very much like you to be there." He would say, "Well, remind me or get Agnes to remind me just before the meeting," because he might be in his office and engaged in other things. I don't know quite how it was he did succeed in meeting his classes, but he did do that as far as I know. He did not fail to show up in his class. He just seemed to be the epitome of the absent-minded professor.

Aspray: Did this at all get in the way of his advancement?

Tucker: Oh yes. He started out as assistant professor, which was unusual. The usual start was as instructor in those days. He started out as assistant professor, but he was an assistant professor for, I think, well over ten years before he was promoted to tenure. Then his promotion to full professor was the last in the group; it came somewhere around 1947. To take myself as an example, I started as a graduate student when he was already an assistant professor. But I became a full professor before he did.

Aspray: I see.

Tucker: Of course I was fortunate in having good outside offers that led to me being promoted perhaps a little earlier than I normally would have in terms of my service to the department. My own service to the department was very much in an all-around way. I did more than my share of departmental chores. I worked well with the undergraduate courses and the variety of courses. I taught graduate courses, although not as often as, say Church, would teach them. So it was very hard, I think, for the Princeton administration to perceive the value of Church. In fact I heard the Dean of the Faculty say at about the time that Church was being promoted to a full professor that he regarded Church as a very great disappointment. Here was the one member of the department who was a Princeton undergraduate alumnus as well as graduate, yet he seemed to have so little part in things. It required a great deal of argument to make the case for Church as a valuable member of the department. But no one in the department felt that Church was not a worthy colleague.

Aspray: I see.

Tucker: This was true of everyone without exception. So it was this unanimous feeling within the department that protected Church and got him his proper status, but not as soon as it might have. Any further questions about Church?

Aspray: I don't think so.

Tucker: Well, the next name on the list is Eisenhart. Eisenhart was really a remarkable man in terms of the number of things that he contributed to the department. He came to the department in 1900 and retired in 1945. A 45-year continuous span. He taught every term of the 45 years.

Aspray: No sabbaticals.

Tucker: No sabbaticals. He usually went off with his family for the summer. And he was active in national organizations. He served as president of the American Mathematical Society at one point. He was a member of the National Academy of Sciences, and served on committees of the National Academy. Particularly in later years he was active in the American Philosophical Society in Philadelphia. He served from about the time of World War II on, until near his death, as the executive officer of the American Philosophical Society, commuting by train to Philadelphia once or twice a week. And he served the university as Dean of the Faculty starting around 1923 and was regarded as the main father of what was called the "four-course plan." This plan was introduced in the mid '20s and called for juniors and seniors to concentrate in some particular field, in other words to be departmental majors. Each term in junior and senior year they were to have four courses, two courses in their department and two elective courses outside the department.

Aspray: Yes.

Tucker: Then counting as having the weight of a fifth course was independent study done under the supervision of the department. In the mathematics department, the junior independent-work was usually a sort of reading course of the student's choosing and then culminating in a so-called junior paper. The student would usually spend the fall term immersing himself in the particular subject and then in the spring term writing this up while continuing the reading. Quite a few students in mathematics used this independent work as a means of preparing for the actuarial examinations. Because at that time about the only profession that mathematics prepared for specifically, other than the teaching of mathematics, was being an actuary. So some of the subjects that the student chose were to study for the actuarial examination. Indeed, I think the first study of statistics that was done in the mathematics department at Princeton was in this independent work, preparing for actuarial examinations.

Aspray: I see.

Tucker: But there would be other things. I had taken the actuarial examinations myself when I was an undergraduate at the University of Toronto. So I was often called on at Princeton to do this sort of thing. I have had students study the subject of life contingencies, which is the theory by which the life tables are made up. Then the senior independent work was dignified by calling it a thesis. That often led to quite respectable, almost research, done by undergraduates. All majors had to do a senior thesis, except those in the engineering school, in the engineering school the rules were somewhat different. But in a candidacy for the bachelor of arts degree, which was the degree given in the department of mathematics—in many places it was the bachelor of science degree—and all of this was subject to the four-course plan that Eisenhart was the leader in getting set-up. Then he was made Dean of the Faculty in order to implement the plan.

Aspray: Yes.

Tucker: So he was Dean of the Faculty, working out of Nassau Hall from 1924 until 1933. When Dean Fine died he became the chairman of the mathematics department. So he was a very key figure in the adminstration of the mathematics department, because he had been the main one that Fine counted on to help when Fine was chairman of the department. Also he was very heavily involved in university-wide administration.

During the interregnum between President Hibben and President Dodds, he served essentially as acting president, though he wasn't called that. The one who was called the acting president was the chairman of the Board of Trustees, a man who was at that time head of the Prudential Insurance Company. But Eisenhart was the acting president on campus as far as the day to day operations of the university were concerned.

Aspray: How important was his holding administrative positions in the department's getting as great support as they did from the university over the years?

Tucker: Well, I'm sure that through him the needs of the department were very clearly presented. I imagine that he leaned over backwards as far as using his Deanship to favor the department. So that the value to the department was that it had a completely informed spokesman at the center of things. After he retired and Lefschetz became the chairman of the department, the department was inclined to suffer because of lack of communication between the central administration and the department. This, of course, was supposed to go through the chairman, but Lefschetz would become so excited and argumentative when he was talking to the Dean of the Faculty that the Dean of the Faculty came to feel that it was an emotional appeal that was being made rather than something that had solid substance.

Aspray: Rather than a rational one.

Tucker: Yes. Indeed, Lefschetz learned from bitter experience to take someone else along with him when he went to call on the Dean of the Faculty. And that someone else was usually me.

Aspray: I see.

Tucker: I found that it was my job to present the facts and make the hard case for an appointment or a promotion or something of that sort, because quite early on Lefschetz wanted to appoint a couple of assistant professors. He had three candidates, and he was even hopeful that he would get all three, but he felt certain that he would get two. He ended up by getting one, and the one that he least wanted. This I'm sure was nothing but a lack of communication, a failure of the Dean and Lefschetz to talk the same language.

Aspray: I see.

Tucker: Well, nothing of that sort could possibly have occurred during the period that Einsenhart was actively involved in administrative matters.

Aspray: As able as Eisenhart was, all of the attention that he gave to activities outside the department must have caused some loss of time and attention that he could give to departmental matters. Or is that not right?

Tucker: That is right, but Eisenhart was a master of delegating duties. As an example, during the time from 1933 on until 1945 he depended on Bohnenblust and me, as far as the matters of undergraduate instruction were concerned. We would meet with him in his office in Nassau Hall, the Dean's Office, about once each term to plan the next term. For the most part the procedure we followed, as far as the undergraduate program, was: What did we do last year? We'll do the same thing this year. There was very little in the way of

changes in the courses, or in the textbooks we used. So that it was a fairly simple thing to decide on who would teach what, and how many sections we needed of such and such a large course. But it was essentially Bohnenblust and myself who did this. Eisenhart took the responsibility as chairman of the department, but he counted on our doing the detailed part.

Lefschetz as the holder of the H.B. Fine Professorship was the representative on the University Research Committee, which controlled the research funds for the department. Lefschetz handled that completely. Also Lefschetz was the departmental representative for the Graduate School, so he was on the Graduate School Committee. And he handled the program of graduate studies and was the father figure for all of the graduate students. It was because of that that Tompkins referred to Lefschetz as the "great white father". Lefschetz, as research professor, had no teaching duties. He didn't even teach a graduate course. He would run a seminar.

He became the research professor following Veblen. Veblen had been the research professor from 1926, when the professorship was set up, until 1932, when he moved to a professorship at the Institute for Advanced Study. Lefschetz was then appointed to the Fine Professorship. Neither one of them taught courses, even graduate courses, but each one of them would have a seminar. So Lefschetz was the head of the graduate work and also of financial support of the research program of the department. Also Lefschetz was the main editor of the *Annals of Mathematics*.

Aspray: I see.

Tucker: Well, we got to talking about Lefschetz rather than Eisenhart, but I was pointing out how Eisenhart delegated these matters. I think Bohnenblust was usually the undergraduate departmental representative. Then early on I got appointed to the faculty committee on scheduling. Now this was a fairly important thing for the mathematics department, because we had so many freshman-sophomore courses, and these courses had to be scheduled to satisfy the engineering school as well as various departments. So it was always important for the schedule committee, which had only a few members, to have a member from the mathematics department on it. So the administration of the department was decentralized. Also we had a very able departmental secretary in Agnes, Agnes Fleming as she was at that time. She wasn't good at keeping files, but she was very good in dealing with people and handling the arrangements that were necessary on a day-to-day basis. She had an excellent memory; her memory was sort of the file for the department.

So because Eisenhart was in Nassau Hall and seldom came to Fine Hall, only for his teaching, there was no feeling in Fine Hall of there being any administration. Things were decentralized so that administration within the building was scarcely noticeable.

Aspray: Other than in representing the department's needs to the university administration, what sorts of administrative responsibilties would Eisenhart take within the department? What sorts of decisions would he make himself rather than delegate out?

Tucker: Well, I'm sure there were some pretty hard decisions that had to be made at the time that Veblen and company moved to the Institute for Advanced Study payroll. There were big gaps in the personnel of the department. But I wasn't in Princeton during that year 1932-33. I visited Princeton a couple of times during the year, and of course I heard things, but I don't know whether even if I had been here I would have been aware of what must have been going on to preserve the department.

Aspray: On questions of appointment and promotion how large a say did Eisenhart have? Was the decision made by the senior faculty as a group? If he objected to the decision of the other senior faculty members, would things go through?

Tucker: I don't know because I wasn't a senior faculty member. The general rule—and I suppose it is the same other places—is that if it's a question of promoting someone to full professor, then only the full professors meet. If it is promotion to associate professor, then only people already on tenure meet. And if it's an appointment of instructor, then it's assistant professors and up who deal with that. I think that Fine had a quite democratic attitude when he was the chairman of the department. I think he must have regarded Eisenhart as his second in command. But, of course, he had a good relation with Veblen.

Veblen was off in England—he had changed places with G.H. Hardy—the year that Fine died, so that he was not here when it was necessary to name a new chairman of the department. Of course the choice of a chairman of the department at Princeton is entirely in the hands of the president of the University. He chooses, and I don't think Veblen would have wanted the job. Anyway, Eisenhart was immediately named the chairman of the department.

Lefschetz and Eisenhart had a very good relationship as far as administrative matters were concerned. To my mind Eisenhart was the great stabilizing influence in the department. This, it seems to me, was the great role that he played. Veblen had been, over the period say from 1910 to 1930, the leader in the department as far as research interests were concerned and in the developing of students such as Alexander and Church. That role then passed from Veblen to Lefschetz. But with both Veblen and Lefschetz, Eisenhart was the one who was relating the department to the rest of the University and providing a stable base from which Veblen and later Lefschetz could operate. I'm sure that if either of them came to Eisenhart with a proposal that they wanted to have presented to the administration and Eisenhart felt it was an unwise proposal, he would have no hesitation in saying so. And their respect for him was such that that would end the matter.

Aspray: I see. On a social level, what was the relationship between Eisenhart and Veblen?

Tucker: Well, when I arrived at Princeton, Eisenhart lived on campus in the house that was reserved for the Dean of the Faculty, and Mrs. Eisenhart was a very skilled hostess. So the social atmosphere with the Eisenharts was a university atmosphere. There was never just a mathematics get-together at the Eisenharts'. The Veblens, on the other hand, went in for teas and dinner parties, dinner parties practically in the British tradition, because Mrs. Veblen was British. It would be a dinner party say of twelve, very very carefully chosen people. At a certain stage at the end of the dinner, the ladies withdrew and the men then had brandy and cigars. Later they joined the ladies. But the party would be entirely from mathematics, perhaps some from physics, but it would be a mathematical group. So that although both families entertained in rather an old-fashioned way, with the Eisenharts it was directed very much at the University, and with the Veblens it was directed towards their friends.

Aspray: I see.

Tucker: Now with the Alexanders there were parties, but these were really quite apart from the University. There might very well be stockbrokers from, so to speak, the other Princeton, people who liked to live in Princeton because it's a nice place and have business interests in New York or Philadelphia.

Aspray: What role did Eisenhart play in the training of graduate students?

Tucker: Each term he taught a graduate course and an undergraduate course. The undergraduate course was usually a section of one of the main calculus courses, or I shouldn't say that because the first freshman course was not a calculus course. It was a course in analytic geometry for which, at some point along about 1930, Eisenhart wrote a book. That book was then used as the textbook. He called his book *Coordinate Geometry*, so the course was called Coordinate Geometry rather than Analytic Geometry. But the ordinary set-up was one term of analytic geometry followed by a term of differential calculus. Sophomore year a term of integral calculus followed by a term of differential equations. The textbook for the calculus was the textbook that had been written by Fine, Fine's calculus; a variety of books got used for the differential equations course.

It was through having taught in that first term a freshman course in analytic geometry and being dissatisfied with the textbook that had been used, which was a book by Fine and one of Fine's students, H.B. Thompson, through dissatisfaction with teaching from Fine and Thompson that Eisenhart wrote his own book. It was, however, not a good book for freshmen to use because things were said once and only once. That book caused a great deal of trouble, and something would have been done about it if it had not been for the tremendous respect in which Dean Eisenhart was held. I remember that when he retired

(the end of June 1945); it was not known until just a few days before he retired who was to be the next chairman. And the choice was Lefschetz. I had been up in Canada visiting my parents and had also been at the first meeting of the Canadian Mathematical Society at McGill in June 1945. I got back just at the end of the month to find that Lefschetz had been named chairman, but with the understanding that Lefschetz should designate someone who was actively involved in teaching—because Lefschetz was the research professor—to be an assistant chairman as far as the undergraduate responsibilites of the department were concerned. Lefschetz had designated me for that. So I had a call on the third of July from the Dean of the Faculty asking if I was willing to undertake the responsibility of being assistant to Lefschetz. I said that I was. And he said, "Well, could you come to see me tomorrow, and we'll work things out." So I spent most of the fourth of July, 1945 in the office of the Dean of the Faculty working on the plans for undergraduate mathematics. And one of the first questions he had asked me was 'Is it necessary for the department of mathematics to use Luther's textbook in the first course in freshmen year?' My immediate answer was, "No, it is not." And by the time we had finished our conference I had been authorized to plan a complete revision of the freshman-sophomore mathematics and to visit certain places such as M.I.T. to find out how they were doing things.

Aspray: I notice that Eisenhart had four graduate students in the '30s. You have them listed. Do you want to say something about his training of Ph.D. students, in the writing of dissertations?

Tucker: We have heard someone talk about this, haven't we?

Aspray: Now that you remind me, that's right. Okay, let's turn to something else.

Tucker: Eisenhart was a wonderfully able administrator. If there was a fire, he would put it out without wasting any time. And he did this with kindness but with firmness. He would not put up with any nonsense, so that people all over the University had the greatest respect for him as a person who was fair, would get the information on a question, and be judicious in reaching a decision. He did not hesitate when it was clear that he must act. But on the whole his role seemed to be that of a balance wheel. Except in the matter of the four-course plan, which I didn't know of directly, in which he made a name for himself, I didn't feel that he had qualities of leadership in the same sense that I would say this about Veblen or about Lefschetz. They had a certain dynamic aggressiveness about them that was not present in Eisenhart. This meant of course that Eisenhart in working with Fine and with Veblen and with Lefschetz was able to work with those people very smoothly. You really could not become angry at any time with Eisenhart. With Lefschetz it was so easy to become angry, less so with Veblen. But still Veblen sometimes could become very exasperating.

Aspray: Yes.

Tucker: The next person on our alphabetic list is William Gillespie. He was a Canadian from London, Ontario who came to Princeton by way of a Ph.D. at the University of Chicago. He must have come to Princeton before 1900. He was a beloved teacher of undergraduates who, as far as I know, did not participate in the other activities in the department. He was not involved in research, I believe after his thesis, and really played no role in the general life of the department. He customarily taught in the lower-level courses for the students who were simply taking courses to meet the mathematical requirement.

In those days each student was required to have a year of mathematics, and for the benefit of some students there were some less demanding courses than the regular courses which were intended for the would-be engineers and physicists and chemists and such. It was always a moot point as to whether the pre-medical students should take the Gillespie courses or take the more substantial ones. He participated in the life of the University. He was one of the faculty members who looked after the affairs of the University Chapel. Also at some point in the 1920s he became the Master in Residence of the Graduate College. He was a life-long bachelor, amd a very fine-looking well-spoken man even in his later years. When I knew him he stood erect and well over 6 feet. He was liked and respected by everyone, I think, in the University. But as I said really played no important role in the department.

Aspray: Did he have any research that he did himself?

Tucker: Not that I know of.

Aspray: Did he take part in department meetings and decisions and such?

Tucker: Oh, he would come to department meetings, but he did not as I recall play any role at all in decisions. I suppose if there was a vote, he voted, probably taking his cue from Eisenhart.

Aspray: I see.

Tucker: Than we come to Hille, Einar Hille, a very fine analyst. He came to Princeton in the early '20s. He was Swedish, although he had been born in New York City. This was somewhat accidental when his parents were on a business trip to the United States. He was educated at the University of Stockholm and, as I think I have said before, was very much in the Mittag-Leffler Swedish tradition. He left the department in 1933 at the time that the School of Mathematics of the Institute was getting started. Exactly what bearing that had I don't know, but he was offered a position at Yale, and he went there. I think he probably felt somewhat uncomfortable at Princeton, because there was so much emphasis on geometry and other things rather than analysis. In most mathematics departments, analysis is rated very-high on the scale of values, but that was not the case at Princeton. It was not at all looked down upon, but I think he felt that the spotlight was taken at Princeton by things in geometry and topology and algebra and mathematical physics.

Aspray: How well did he hold up the tradition of analysis while he was here?

Tucker: He was a very fine lecturer and had two or three Ph.D.s. Bohnenblust was one of his Ph.D.s. And he served as the principal editor of the *Annals of Mathematics*, taking over that role from Wedderburn who completely gave it up in the late '20s. Then Lefschetz was working with Hille as an editor while Hille was here, but it certainly seemed that Hille was the one who was in charge of the *Annals*.

I remember being asked by Hille to look over some papers, not as a referee—these were papers that had been accepted but were papers written by foreign authors in English—he wanted me to look over the papers for English. He seemed to feel that because of my Canadian training I had a somewhat more reliable knowledge of English grammar. In fact, I think he felt that he was under a bit of a handicap himself in this respect. This was at a time when it was a standard policy in the mathematics department at Princeton that any senior member of the department could ask a graduate student or a younger member to do something and it was done. There was no question of saying, "Well, I don't know whether my job calls for this." I rather like that spirit, which doesn't seem to prevail any longer. "Why pick on me?" is the attitude now. Somehow at the time it seemed rather a privilege to be called on to take on some helpful undertaking.

Until Bochner developed, Hille was certainly the outstanding analyst at Princeton. Earlier on there had been another Swedish analyst at Princeton by the name of T.H. Gronwall. Indeed it had been with Gronwall that Alexander had written his Ph.D. thesis in complex variable in 1915. But Gronwall was an alcoholic and finally had to be eased out of his position at Princeton. I think a job was found for him for a few years working in industry, but he died sometime in the 1920s. Indeed it was Hille who wrote the very fine obituary notice of Gronwall that appeared in bulletin of the American Mathematical Society. Except for the scuttlebutt, my knowledge of Gronwall is from that obituary notice written by Hille.

I found Hille a very likable and approachable person. At the University of Toronto I had been quite well trained in analysis. Certainly there it was the center of everything, and already by the time I came to Princeton I had had three different courses in complex analysis and one fairly substantial course in real analysis. I sat in on a course by Hille during my first year here, and he realized from the questions and comments that I made in class that I was very well prepared in that area. He was a member of my general examination committee. I remember that quite vividly. The only mistake he made was that he was practically too gentle by starting with very elementary things. I felt that somehow there was some hidden depth in these questions and hesitated in answering them, when all that was intended was to warm me up to the later questions, which I was expecting to start right out with.

Aspray: Did he have other duties in the department, other than his position with the *Annals*?

Tucker: No, I don't think so. He taught his share of undergraduate courses. Indeed, on the occasion that I was moved out of the teaching in the freshman course with T.Y. Thomas I was put into teaching the sophomore course which was under the direction of Hille. I remember that that course went very smoothly and very effectively, that Hille handled the responsibility of leading that course in a very fine fashion. So I was very sad when Hille left and went to Yale.

Now the next person on the alphabetic list is Morris Knebelman. He taught the special mathematics courses for engineering students, courses in engineering mechanics and the special sort of differential equations that the engineers wanted to have. He had been for many years before that on the faculty of Lehigh University. He came to Princeton in the late '20s to get a doctor's degree, and served as instructor while working on his Ph.D., which was in differential geometry with Veblen and Eisenhart.

Aspray: He had experience with teaching engineering type courses at Lehigh, since it is an engineering school.

Tucker: Yes, that's right. He took his Ph.D. actually in 1928 and stayed on for a further year to continue work in the differential-geometry area. Quite possibly he had a National Research Council Fellowship for that purpose. Yes, in 1928-29 Knebelman had a National Research Council Fellowship to work with Eisenhart and Veblen. He probably could have continued that for a second year, though probably not at Princeton since there was a prejudice against people spending all of their National Research Council Fellowship at the institution where they had got their doctorate.

But in the summer of 1929, Professor MacInnes died. He had been teaching the mathematics course especially for the engineering school. I happen to remember exactly about his death. I never saw him, but one of the first things that I did as a member of the mathematics department when I arrived in September 1929 was to attend a memorial service for Professor MacInnes. MacInnes, besides teaching these engineering mathematics courses, had been, I think, an assistant dean of some sort, not I believe in the engineering school, but for the arts students.

This, of course, suddenly left a pressing need for someone to take over the courses that MacInnes had taught. His death was a rather sudden thing. He was middle aged and it was completely unexpected. So Knebelman was appointed to the faculty as an assistant professor to take over this responsibility. He continued in this capacity at Princeton until, I think, 1938 when he was appointed to a professorship and became chairman of the department at Washington State University in Pullman, Washington. He went on to have a fine career there, becoming Dean of the Graduate School, but at Princeton he did not rise above the rank of assistant professor. He moved elsehwere when the

opportunity presented itself, because he understood that there was no future for him at Princeton. He did participate in the research in differential geometry, and he worked with Eisenhart and T.Y. Thomas in that area. I remember in particular one paper that I wrote which was published in the *Annals of Mathematics*. He served as referee for that paper. So in addition to his being the department's liaison with the engineering school, he helped, as did other members of the department of course, in the work of the *Annals of Mathematics* and in the research program of the department.

Aspray: Is it likely that any graduate students went to him to work with him on projects?

Tucker: No. He did not engage in any thesis supervision that I am aware of.

Now Solomon Lefschetz. We've already by now said so much about Solomon Lefschetz that all I will do is recapitulate that he was the most active member of the department in working with graduate students and also with the editorial responsibilities of the *Annals of Mathematics*. From 1933 until he retired, he was the Henry Burchard Fine Professor with no teaching responsibilities, simply responsibilities towards the graduate program which he directed and the administration of the research funds of the department. And from 1945 until he retired in 1953 he was the chairman of the department.

H.P. Robertson, Howard Percy Robertson, we have not talked about very much. Until the advent of the Institute for Advanced Study he was the main person in mathematical physics. He had been an undergraduate student at the University of Washington, Seattle and then had been a graduate student at Cal Tech. He was very highly regarded at Cal Tech. He came to Princeton first as a National Research Council Fellow, and he was a National Research Council Fellow at Princeton from 1925 to 1928, for three years. It was unusual for someone to hold such a fellowship for three years. Then in 1928 he was appointed as an assistant professor in the mathematics department. His interests were in the mathematical physics of relativity and quantum mechanics, and from this he became very much interested in what was described at that time as cosmology. He kept in very close touch with Cal Tech, and I think spent a leave of absence back at Cal Tech. He was a very quick, energetic person. Very hail-fellow-well-met, he was interested in everything that was going on in the department and also in the physics department.

Aspray: How close were his ties with physics?

Tucker: Oh, very close, and he certainly was cross listed as a member of the physics department. His wife was a very lively person of Hungarian parentage. Because of that the Robertsons became very close friends of the von Neumanns and of Wigner—I say Wigner because he wasn't married until later. Robertson fitted in beautifully with everybody, but it was almost a natural with von Neumann and Wigner. And his mind worked, well, not nearly as fast as von Neumann, but sufficiently fast that he could almost keep up with von Neumann.

Aspray: I see.

Tucker: He would have, I think, become a world name in physics if it hadn't have been that for various reasons his interests were too widely spread. He also teamed very well with E.U. Condon, who was here in the mid '30s. We don't have him on that list; he was as an associate professor in mathematical physics housed in Fine Hall.

Aspray: Yes.

Tucker: He just hit it off with Veblen and Eisenhart in a quite remarkable way.

Aspray: What about Einstein?

Tucker: Well, no. I don't think that anyone at Princeton hit it off with Einstein, unless they actually worked with him in the same way that Banesh Hoffmann did. It was in a master-student relationship that people had to approach Einstein, and Robertson was beyond that. But Robertson hit it off extremely well with Weyl. I think Weyl had been at Princeton for a year, back around '28-'29. He'd been brought over to be the first holder of the Jones Professorship of Mathematical Physics. He was just a visiting professor for a year.

I remember being told the story about Weyl giving a course. The course started out with many people in attendance, but it gradually dwindled down until there were just three in the course. One was Bohnenblust, who was Weyl's assistant, another was Robertson, and I've forgotten who the third was. And Weyl had said that if the course dwindled any further that he was going to stop lecturing. So one day the third attendee in the course didn't show up, was ill. So Bohnenblust and Robertson went out and got one of the janitorial staff to come and sit in the room, so there would be three people in the room and Weyl would give his lecture.

Then, of course, Weyl came back in 1933 as a professor at the Institute. By that time Robertson was well established. At the time that he was attending the Weyl course that the story was told about, he was still a National Research Council Fellow, he hadn't at that time been appointed to the faculty of Princeton.

Aspray: Yes.

Tucker: He was a very outspoken person, and graduate students very much feared that they would have him on their generals or finals. Indeed one of the people that we have interviewed was failed on his final oral examination because of Robertson.

Robertson seemed quite equal to any challenge that came along. So the coming of the School of Mathematics in the Institute for Advanced Study with Einstein and von Neumann and Weyl did not bother him in the least. It, as far as he was concerned, just widened the horizons. When Knebelman left in 1938, he took over some of the responsibilities

that Knebelman had had towards the engineering school. Not very happily, but it showed his willingness to fit in and take care of what was needed. Working with the engineers was not particularly to his liking. He much preferred to work with the physicists, whom he regarded as superior. He regarded the engineers as rather low-brow.

Quite early in war-time preparations, Robertson was brought in to activities in Washington. You see, the scientific mobilization was begun even before Pearl Harbor. MIT and Cal Tech were drawn on and Vannevar Bush from MIT and people such as [Theodor] von Karman from Cal Tech were sought out by the Department of Defense for scientific advice in preparing for hostilities. And because of Robertson's tie with Cal Tech, he was very quickly involved.

Aspray: I see.

Tucker: Indeed the fact that Washington was more accessible from Princeton than it was from Pasadena or Boston played, I think, a role in this. So we saw very little of Robertson from about 1940 until something like 1946. He was in Washington much of the time. He also was in Europe.

I think 'SHAEF' stands for 'Supreme Headquarters of the Allied Expeditionary Forces'. When the invasion of Europe began, from D-Day on, there was a unified command of all the Allied forces, and the name for that was SHAEF. Robertson was the chief scientific advisor to SHAEF. And immediately after armistice he had to do with the efforts that were made to examine what the Germans and others had been up to in their scientific research for military purposes. All of this of course was quite hush-hush, so that I don't know all the things that Robertson was involved in. But I do know that when he came back to Princeton about 1946 that he seemed to have aged about twenty years since I had last seen him. He was no longer the ebullient person that he had been in the '30s.

He did take an active role in the 1946 conference on mathematics that was a part of the Princeton bicentennial celebration. Robertson played an important role in the planning and the actual holding of that conference. He served as the toastmaster for the final dinner at which Herman Weyl was the principal speaker. Other speakers were Saunders MacLane and ... I've forgotten who the other person was. He was a very witty toastmaster on that occasion, just perfect for the occasion. No other person in the department could possibly have filled the role the way he did.

It was about this time that Cal Tech was ready to open its Mount Palomar Observatory, and it was expected that cosmology was going to make a quantum leap as a result of observations that would be done at Mount Palomar. Mount Palomar, as you know, is under the supervision of the California Institute of Technology. So Robertson was offered a professorship in cosmology at Cal Tech to be the mathematical physicist who would work on material that would come from the Palomar observatory. But his health continued to deteriorate and he died just a

few years later. Actually his death was directly the result of an automobile accident, but the automobile accident was caused by drinking. His war time experiences and so on had taken so much out of him. He was always a rather heavy drinker, but he had during the war, I guess, gone much farther. There was a verse that was composed by a number of people, of whom I was one, [C.B.] Tompkins was another. This was in the late '30s. There were verses about many of the principal people in the mathematics community. The one about Robertson was, I felt, particularly apt. "Here's to Robertson, Howard Percy,/ On whose soul there'll be no mercy./ Round of belly, yet deft of toe,/ His forehead's high but his mind is low." He was quite stout, "round of belly", but he was very nimble on his feet. He and his wife were very good dancers. Indeed, they once won the rumba contest at the, what's it called, the Rainbow Room at Rockefeller Center. So that's the "deft of toe". And "his forehead is high", that was really true. Anyone who would draw a caricature of him would draw a very high forehead, a very large head and a very high forehead. Certainly he had great intellectual powers. But "his mind was low", he loved dirty stories.

Aspray: Like von Neumann in that regard.

Tucker: Yes. Indeed they were constantly trying to outdo one another in this respect. I remember on one occasion I was sitting beside Robertson at a dinner meeting of the American Mathematical Society. The after-dinner speaker was George David Birkhoff, who was holding forth, in very high-sounding fashion, about the greatness of mathematics and American mathematics and so on. And H.P. Robertson leaned over to me. He said, "Al, do you know what a spoonerism is?" I wasn't sure, but I said "Yes." He said, "What a shaft of wit."

I feel that Robertson played a very incomplete role in the department. It was great while it lasted, but it's a great pity that he wasn't able to bear out the very great promise that he showed when he first joined the faculty.

Now, T.Y. Thomas, Tracy Yerkes Thomas. I think he did his undergraduate work at the University of Arkansas. He came to Princeton as a graduate student and took his Ph.D. in 1923 with Veblen, with Eisenhart aiding and abetting. The thesis would have been in tensor differential geometry, the area that Eisenhart and Veblen were working in that time, with interest in that stemming from the theory of general relativity. He too was a National Research Council Fellow for three years at Princeton University. Then he was appointed assistant professor. He was very ambitious.

Aspray: To a fault?

Tucker: To a fault, I would say. Very aggressive. He gave good graduate courses. He did his share of undergraduate teaching, but without very much sensitivity to the needs of the undergraduates. Certainly his attitude in undergraduate teaching was take it or leave it. I've already reported on the fact that during my second year as a

graduate student I was teaching under his supervision and got into trouble because I objected to the high-handed way in which he was going much too fast in the course.

He was not liked, and there was bad blood between him and Lefschetz. Because in some sense he still had the protege relationship with Eisenhart and Veblen, he seemed to get along quite well with them. But he seemed to be rather jealous of other people in the department. He was the only person who I ever heard make any allusion to Lefschetz' handicap. No one spoke of this, but he did on one occasion and not in a pleasant way. He didn't like Lefschetz, and Lefschetz didn't like him. So when an opportunity arose for him to leave Princeton, he did. He went to the University of California at Los Angeles in 1938, and I think everybody was relieved. I imagine he was also.

Aspray: How was he as a mathematician?

Tucker: He was a very good mathematician, a very hard worker. One of my best friends, E.W. Titt, who was a contemporary of mine, did his Ph.D. with Thomas. He found Thomas very helpful in working on the Ph.D. Also Thomas was concerned about helping him get a position, and, when there wasn't a position forthcoming, insisting that Princeton provide him with an instructorship until a position could be found.

Aspray: I see.

Tucker: So he was loyal to the people who undertook to work with him. He was the only person, though, that I knew of who actually would approach a graduate student and suggest that that graduate student work with him. Indeed, that was the beginning of my trouble. He asked me to work with him, and I simply said at that time, which was actually the fact, that I hadn't decided in what area of geometry—I knew it was going to be there—I would try to do a thesis. I was somewhat embarrassed by his question. Anyway, he regarded what I said as that I was giving him the cold shoulder. I certainly hadn't intended it in that way, but I can understand how from his point of view that seemed to be the case. So I think he had it in for me already when the opportunity arose to charge me with insubordination in the teaching of the course.

I hope I'm presenting a fair view of Tracy Thomas. He certainly was the one person that I had to deal with on the faculty that I have very bad memories of. He died just recently. From UCLA he went to Indiana University, where he got some special university chair; actually an autonomous institute separate from the mathematics department was set up for him there. I think he mellowed a great deal in later years, especially when he felt he had made it. He was a member of the National Academy of Sciences and then had this special institute for him at Indiana University. He returned to Los Angeles to live after he retired at Bloomington, and I think he died in March of this year in Los Angeles.

Aspray: I see.

Tucker: This man [Robert] Carmichael, who sent in a paper to the *Annals*. He didn't have a bachelor's degree, yet he came to Princeton and was launched on a mathematical career. As far as I know that was Veblen's doing.

Aspray: Okay.

Tucker: And Veblen's spotting Alexander and bringing him along. And Church and bringing him along. We've heard the same sort of thing of a later period from Wallace Givens. And didn't Bob Walker tell us that when he arrived at Princeton one of the first people he met was Veblen. Veblen took him around. And Foster used the term 'adopted', saying that Veblen adopted him. It's very clear that all along Veblen had a knack of spotting young people and getting them started.