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The Princeton Mathematics Community in the 1930s
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ALBERT TUCKER

THE EDUCATIONAL PROGRAM AT PRINCETON IN THE 1930s

This is the third in a series of interviews with Albert Tucker in his office at Princeton University. The interviewer is William Aspray. The date is the 12 April 1984.

Aspray: The subject of this interview is the educational program at Princeton in the 1930s. I know there were a number of expectations of students, but let's begin by having you describe the formal requirements that graduate students in mathematics had to meet at this time.

Tucker: In the first place it should be said that admission was very selective. No one was admitted who did not have recommendations indicating that, barring some unexpected circumstances, that person would go through to the doctorate. No students were taken in just for master's degrees. They were all doctoral students from the very beginning.

Aspray: What were you looking for in an applicant?

Tucker: Of course, at that time I didn't have very much to do with the selection. But in terms of when I later on did have quite a bit to do with the selection, we looked, of course, at the student's transcript. We expected a student to have had a good year of mathematics beyond advanced calculus; they all had at least advanced calculus, but some of them didn't have much beyond advanced calculus. Also, we were interested in students highly recommended by people that we knew. Later on, we were especially interested in students of our previous students, because we felt that that they knew what the student was coming to get. Also they knew that if they sent us a lemon, it would

be held against them. So if we got a high recommendation from someone that we knew and trusted, and felt that the reputation of this person was behind the recommendation, then we would almost automatically admit the person. That was the top category. The Graduate School would tell us that we could admit so many. We always admitted two or three more, because we were aware that students would apply to us and elsewhere. We had a pretty competitive batting average, but it wasn't 1.000.

Aspray: Sure. Of the people who didn't come here, where did they often go? Did you follow up on that?

Tucker: Well, no I didn't. I've heard by accident about some. George Dantzig has told me that he applied to come here and was not taken, and he went instead to the University of California at Berkeley where he worked with a statistician, Neyman. He said that he was quite upset at the time that he wasn't accepted here, but the way things went for him at Berkeley that now he regards it as almost fortunate that he wasn't. I think we certainly lost some students that we would have been glad to have to Chicago. But in the Thirties I don't think we lost very many, because we had much more to offer in the way of financial support.

Aspray: Why was that?

Tucker: This was because of the Scientific Research Fund and the fact that we would have eight or ten of the graduate students supported on these research assistantships.

Aspray: I see.

Tucker: And then there were teaching assistantships, as well as a limited number of straight fellowships. But Chicago did not have these research assistantships, and because Chicago was mainly a graduate school there wasn't much in the way of undergraduate teaching assistantships. Also at that time Harvard didn't seem to have very much interest in graduate students from the outside. Their good graduate students were mainly their own undergraduates.

Aspray: I see.

Tucker: And I might say that we were suspicious of any Harvard undergraduates that applied here. We thought that they must be second rate if they didn't stay on at Harvard. So there are all sorts of little quirks of this sort. Now, and then, there were other strange things that happened such as with me. I didn't know about deadlines for application and so on. My application was made in an informal way by Dean DeLury at the University of Toronto writing to Dean Fine and the letter arriving after Dean Fine was dead, and nothing being done about it until it was too late for me to apply as a graduate student. But it turned out that they needed a half-time instructor and wanted somebody who already had teaching experience. Normally you don't get a first year graduate student who has already had teaching experience.

Aspray: Right.

Tucker: But I had been teaching up to nine hours a week in the fifth year that I stayed on at Toronto, so there was no problem with my teaching experience. So I was actually hired as a part-time instructor, and this entitled me to being enrolled as a graduate student without tuition. I came in through the back door, you see. But there weren't more than, say, six or eight new graduate students each year at that time. As we went along in the Thirties, the number became greater.

Aspray: When choosing students was there any consideration given to the particular field of mathematics they said that they were interested in doing research in?

Tucker: I don't think so, no. And at that time I don't think students said very much about that. I think if I had applied in a regular way, that it would have been stressed that I was interested in geometry. But then that would have covered Eisenhart, Veblen, Alexander, Lefschetz, Thomas.

Aspray: If someone had expressed a strong interest, however, in number theory, say, would they still have been admitted if they met the general requirements of the program?

Tucker: Oh yes. They might have got informal advice to the fact that there wasn't much number theory going on here.

Aspray: Now, what was formally required of a graduate student?

Tucker: Sometime during the first year, the graduate student had to pass at least one foreign language examination. These were usually French and German. Later there came to be some use for Russian, but I don't think in the Thirties anybody took a language examination in Russian.

Aspray: Okay.

Tucker: The language examinations were usually an oral examination given by some member of the department. When I took my language examination Lefschetz was the one who conducted the examination. I went to his office and said, "Professor Lefschetz, I would like to arrange to take my examination in French." And I thought an appointment would be made for next week or something. Instead, he just said, "Well, go over to the shelves there and pick any one of the French books off the shelf and I'll open it up at random," or rather "you open it up at random," because he couldn't, "and translate orally." Well, I felt very comfortable about French and I went over to the shelves and I happened to see there the *Traite d'Analyse* of de la Vallee Poussin, which I had used as a sort of a textbook at one stage at the University of Toronto. It was an old friend, and I took that down. I opened it up at random and started translating. I couldn't speak French, but I could read French very easily. And I just read off the translation, and after I had translated about three sentences,

he said, "That's fine. You have passed the French examination." He said, "Now what about German?" I said, "Well, I wasn't expecting to take a German examination yet." "Oh," he said, "let's see," and he asked me to get a German book off the shelves, which I did. I stumbled quite a bit on the translation, and he helped me out with an occasional word I wasn't sure of. On the other hand he could see that I understood German construction, that with the use of a dictionary I could certainly read German without very much difficulty, at least mathematical German. So he certified me in German. I went in to make an appointment, and I left having passed both of the oral examinations. I tell that largely to show the complete informality that there was about this thing.

Aspray: Now that does speak to the informality. What about the seriousness of that requirement, presumeably a requirement that was imposed by the Graduate School, not by the department.

Tucker: That's right.

Aspray: And I know that today in mathematics education sometimes those language examinations are not taken very seriously. Were you expected to be able to read the literature in French and German for your work?

Tucker: We were, yes. If necessary, making use of a dictionary. At that particular time in higher mathematics there were many cases where there was no satisfactory textbook in English. This was before the days of the Dover and Chelsea translations and that sort of thing. Since, at that time, 1930, Germany was the principal center for mathematical research, it would be hard for someone to work seriously in mathematics without a reading knowledge of German. Now I don't think it was as necessary in French, because it was only with the advent of Bourbaki towards the end of the Thirties that there was a resurgence in French mathematics. Still, the classical treatises of Goursat and so on, but even then Goursat had been translated into English by Hedrick.

Aspray: There were people that wanted to read Lebesgue.

Tucker: Yes, and they would need a reading knowledge of French for that purpose. It was only the more popular things that were usually translated into English. So analysis situs, for example, Poincare on analysis situs I read as a graduate student and would have been handicapped if I couldn't read French. And there were people who failed their examinations, and failed them more than once.

Aspray: I see.

Tucker: Anyone who thought that it was just a formality to take the examination and that you automatically passed was mistaken.

Aspray: What about other requirements?

Tucker: The big requirement was the general examination, or as it was called in those days the "prelims", the preliminary examination for the doctorate.

Aspray: When was this taken?

Tucker: There were specified times. The graduate school said that this examination was to be given a certain week in May each year, or a certain week in October, or, I think, a certain week in January or February. The first time you could take it was in May of your first year, and the last time you could take it was in May of your second year. So you took it either in May of the first year, in October at the beginning of the second year, or in winter, or the following May. If you hadn't passed it by then, you were out. And, incidentally, admission was always just for one year.

Aspray: I see.

Tucker: So there had to be a formal recommendation for readmission of a student who was going to stay on a second or a third year, and the department would have a meeting on readmissions. The person who was in charge of graduate students would take the list and go through it, name by name. In some cases where there would immediately be a member of the faculty who would speak up and say, "I know that that student is doing well, is making good progress. He should be readmitted." And then if no one objected, they would go on to the next name. If they came to a name which was a question mark for everybody, also if somebody said, "Now he shouldn't be readmitted," unless there was an objection then that settled it. But then there were the borderline cases. These were usually first-year students who had not made an impression on anybody and in that case there would be a committee of two appointed to examine this student. It was called a "Part Zero" examination because the preliminary examination was called "Part One". That committee would report either to recommend readmission or not.

Aspray: How high was the attrition rate?

Tucker: Oh, I don't know, about one in three or four perhaps.

Aspray: Was it mostly at the time of the prelims or later than that?

Tucker: It was partly at the end of the first year with either a student who was given the blackball or a student who failed the "Part Zero" examination.

Aspray: I see.

Tucker: Then at the prelims, there might be a recommendation of the committee which passed the student that by applying for it the student got a master's degree, but at the same time the committee imposed a rider, no readmission.

Aspray: I see.

Tucker: If a student failed the prelims, even though it was the May of the second year, that student still had the right to retry the prelims in October.

Aspray: I see.

Tucker: Or if he failed at the mid-winter, the student would still have the opportunity to try in October. He wouldn't have the opportunity to try in May, but there was a second chance available in October, but without readmission. The student would simply come and take the examination in October, and if he passed it and the committee recommended readmission he would be readmitted as of then. The examination came fairly early in October, so that it meant that the student didn't miss more than a couple of weeks. But very few students failed three times. The ones that I would know of, I could count on the fingers of one hand.

Aspray: These were written examinations?

Tucker: No, they were oral examinations. There would be a committee of three, and each candidate was supposed to be examined in real variables, complex variables, algebra, and two other topics that were chosen by the student subject to the approval of the graduate school representative. That means the department member who is in charge of the graduate students.

Aspray: What would be a typical one of these other areas?

Tucker: Well, in my case the other two areas were differential geometry, or Riemannian geometry, and topology.

Aspray: I see.

Tucker: During my first year I had taken a two-semester course with Eisenhart in Riemannian geometry using his Princeton University Press book on that subject. I'd also taken a year course on analysis situs from Lefschetz, from which he would write his colloquium lectures. I think the examination committee that I had consisted of Lefschetz, Hille, and Thomas. Thomas was known to be a mean examiner, Lefschetz was unpredictable, and Hille was known to be very thorough but very kind. And I remember it was Hille who started out my examination in real variables. I remember the first question that he asked me because I muffed it.

Aspray: What was that?

Tucker: "What is the criterion that a set should be infinite?" He sort of prompted me a bit and got me to give the answer. Often students had trouble, and other students would coach them and give them a dress rehearsal examination. Nobody had done that with me. I was expecting more difficult questions. Hille's style was to start with a few questions that supposedly anybody could answer and gradually step the thing up, which was an excellent idea, but I was caught unawares by this technique.

Aspray: What was the level of expertise the students were expected to have in these fields?

Tucker: Well, in real variables you were expected to know about Riemann integration and the measure theory that goes with Lebesgue integration. That and other things too. Especially you were supposed to know fundamentals of series convergence and things of this sort. The most taxing part of the real variable was the measure theory.

Aspray: What about in algebra? What was the level? Do you recall?

Tucker: In algebra at that time, since the only algebra that was taught here was Wedderburn's matrix theory, the course was almost entirely on matrices.

Aspray: I see.

Tucker: Similarity of matrices, congruent matrices, and eigenvalues.

Aspray: So, some linear algebra.

Tucker: It was mainly linear algebra, I would say.

Aspray: Nothing in terms of groups and rings?

Tucker: Oh yes. You were supposed to know a bit about groups and rings and fields. But really nothing more than you would find in Dickson. Van der Waerden didn't exist at that time.

Aspray: We've done two basic fields. If you can reconstruct the complex analysis that will complete the picture everybody had to go through.

Tucker: Well, the complex analysis was a cinch as far as I was concerned, because by the time I arrived at Princeton I had already had about three courses in complex variables. I had a course from the Weierstrass point of view, everything defined in terms of power series, and I had a course from the Cauchy point of view with things determined based on the the integral theorem and that sort of thing. Then I had—the most advanced course that I had had—a course from the Riemann-mapping point of view. So I knew a function in terms of power series, I knew a function in terms of the integral theory, I knew a function in terms of mapping, conformal mapping. I think that by and large most students were prepared on the integral approach to complex analysis, and only knew about power series from the Taylor series point of view, and not very much if anything about the conformal mapping aspect.

Aspray: But that was enough to get them through.

Tucker: That was enough to get them through, if they did this crisply and clearly. I think probably that there were sort of grades given on the examination. These were confidential, the student usually wasn't

told these, but the report, which was an oral report, would usually say, "Oh, the student passed an excellent examination." Or, "The student passed a very good examination except in so-and-so."

Aspray: I see.

Tucker: And it was how the student handled the two fields of his own choice that carried the most weight. In other words, a student was really expected to be able to handle the compulsory topics, but the indication of his potential for doing a thesis was regarded as being shown with the special topics.

Aspray: Could a student choose a special topic that was not one of the areas of specialty of the faculty?

Tucker: Yes.

Aspray: Now, in terms of the educational philosophy of the department, what was the intent of these examinations?

Tucker: Oh, the intent of the examination was to determine whether the student seemed to have what it takes to go on to the doctorate.

Aspray: So it was both an investigation of their research potential, but also of their mastery of major portions of math? It was intended to do both?

Tucker: Yes. It was an examination that usually ran for two or three hours. As a bit of a joke, I will say that one technique that students were told, by other students who'd been through prelims, was to try to start an argument between the examinors.

Aspray: I was told that when I took my oral prelims.

Tucker: Yes. You see you knew your committee ahead of time. Perhaps you weren't officially advised of the committee, but in one way or another, through the grapevine, you would find out who it would be. In the first place, you knew who the chairman of the committee was because you were told to appear at a certain time at a certain office, and that office was the office of the chairman of the committee. Then you usually were able to find out at least who one other member of the committee was. And some faculty members were known to have standard questions that they asked. I had been coached to this extent. There were certain questions that T.Y. Thomas was certain to ask.

Aspray: What other formal requirements were there for the degree?

Tucker: Submitting a thesis, which then would be read by two readers. A report would be made to the department by the readers, and on the basis of that report the thesis would be accepted or rejected. Almost never rejected, because it would have been headed off before then. Then this was reported to the Dean of the Graduate

School, that so-and-so had submitted a thesis to so-and-so, which had been read by such-and-such, and that the department had voted to accept it. Then the Dean of the Graduate School would set an examination, would set a date for the examination, and usually on the recommendation of the department would name the examiners. The examinors were in theory named by the Dean of the Graduate School.

Aspray: I see.

Tucker: That again was an oral examination. I know of at least one instance where the examination was not successful, the candidate had to retake the examination a year later, when he passed. I would say the main purpose of the final oral examination was to give anyone on the committee who hadn't read the thesis a chance to ask questions and find out what the thesis was. Then any further questions that were asked were questions that were, so to speak, to determine whether or not the candidate understood what he had done and how it impinged on other things. So you had to be in a position to say what you thought the thesis would lead to and where it fitted in.

Aspray: Was this a conscious attempt to avoid the situation where a faculty member might lead a student through, step-by-step, to a dissertation and a student wouldn't really be able to have done more than follow instructions?

Tucker: That's right. It's to determine that the student had really done the thesis, and that he understood what the thesis achieved and, so to speak, what it was good for.

Aspray: Now were there any restrictions on the kinds of topics that people could choose? How did one usually go about finding a topic? Was it done in close consultation with a faculty member?

Tucker: All sorts of ways. In some cases, a student would just go to a faculty member and say, "Would you please suggest a topic for me, a thesis topic for me?" But there were many cases where a student worked quite independently, and until the thesis was actually submitted in its appropriately bound forms, it wouldn't be known. It would be known that the student was working on a thesis and that it was, say, topology, but it wouldn't be known precisely what the thesis was about. This was what John Tukey said about his case. He just developed this thing and turned it in. While he's very careful to point out that Lefschetz was the chairman of his final examination committee, he doesn't feel that that meant that Lefschetz was his thesis advisor. He would say that there wasn't any thesis advisor.

In my case my thesis was a revision and an extension of certain parts of Lefschetz' book and lectures on topology. And which at the beginning he fought tooth and nail. It was only my stubbornness and persistence that finally made him say, "Well, you better make this your thesis and get it out of your system." Then it was only after the thesis was already accepted and published that he started to become enthusiastic about it. For years after that, somebody would come to

him with an idea and he said, "Have you checked to find out whether that's in Tucker's thesis?" He didn't really know what all was in my thesis, but he finally had come to feel that there was quite a lot.

Aspray: Were there restrictions on the types of topics that one could choose?

Tucker: No, though I think any person choosing a topic remembered that the thesis had to be read.

Aspray: But I think of your story about Minsky.

Tucker: That was a very unusual case.

Aspray: Maybe you should tell that on tape.

Tucker: All right. The most unusual thesis supervision that I had was with Marvin Minsky. This was in the mid-Fifties. He was assigned as a research assistant to the project that I was directing for the Office of Naval Research. Through contacts with him I had very quickly got to realize that he was unusually gifted and had many very original ideas. Sometime I guess at the beginning of his second year here, I asked him what he was planning to do in the way of a thesis. He said, "Oh, I think I'll do something in topology." He already at Harvard had written some sort of paper with Andrew Gleason, and he felt he could quite easily do a thesis in topology. But I felt that his heart wasn't in that. So I said to him, "What would you really like to do?" Then he began to describe to me the ideas that he had been thinking about concerning automata. And I said, "I really think that, since that's what you're interested in, you should take that as an area for your thesis." Then he said, "Well, who would read it?" "Well," I said, "I'll try to read it and I think Tukey will." I said, "If you want to go ahead and do that, I will offer to serve as your thesis supervisor and answer for you." And so he did.

The only help I think that he got on his thesis was from fellow graduate students, Lloyd Shapley, John McCarthy. John McCarthy was just as much interested in artificial intelligence (now called) as Minsky, but McCarthy decided to do his thesis with Lefschetz on nonlinear differential equations, with which Lefschetz was then engaged in doing research. So Minsky took a chance. I realize that, and in a sense I took a chance in encouraging him to do it, because ultimately the thesis had to be read and had to be approved by the department. To help with this when the thesis was finally turned in—it was called "Neural Networks and the Brain Problem"—because he made in beginning some physiological assumptions about how the nervous system of the body works, we got a professor in biology, as a matter of fact the chairman of the biology department, to check these assumptions and say that they were reasonable from a physiological point of view. He took no responsibility for what came out of those assumptions, but he said that these were reasonable assumptions to start with and was glad to put this in writing. So with that and the report from I think it was Tukey and me, the thesis was accepted. So that was that. It was really

far-out at that time. But I told Minsky that the only requirement I know of for a research thesis is that it should contain an original contribution to human knowledge. Nothing about that it had to be in mathematics, or physics, or so on.

Aspray: I see.

Tucker: I thought that quite sincerely, and I think that while some other members of the department might have felt that it was a little bit out of line to do that, I think they also respected the point of view that originality should be encouraged no matter where it leads.

Aspray: Was there ever an attempt during this period to institute something like this Doctor of Arts degree—survey arts, surveying rather than burrowing in a field?

Tucker: Well that came up later on, in the late Fifties I think. John Kemeny was a leader in that. At that particular time, Dartmouth didn't give the doctor's degree. John Kemeny was rather eager to have the mathematics department give the doctorate, so he worked up the idea of a Doctor of Arts degree for a scholarly or expository thesis. Then this got bandied about nationally. I remember a meeting that was held, I think somewhere around 1960 or 1961, that was supposed to be a meeting of representatives of the leading graduate schools to say that they were willing to have a Doctor of Arts degree introduced and wouldn't raise objections to this. But my recollection is that that pretty well ended in a deadlock.

Nevertheless there was sufficient favor given to it that John Kemeny pushed ahead with his idea at Dartmouth. When it really came to a head, the dean at Dartmouth got in touch with me and got me to serve as a sort of informal mediator. The upshot of it was that the mathematics department was given the right to have a doctorate, but it should be called a Doctor of Philosophy degree, and that if they wanted to make it expository that that was all right. Now my colleague Norman Steenrod, I remember, said at that time that he would like to see the same thing at Princeton and that he would volunteer that any student who wanted to do an expository thesis could come to him and Norman would supervise that. We all knew that Norman's standards were terribly high, so there wasn't any concern. And as far as I know no student ever took up that challenge.

My son Tom took his Ph.D. at Dartmouth with a research thesis, and in talking with him he wasn't able to tell me of any thesis that he specifically knew of that was done at Dartmouth as an expository or scholarly thesis. But at the time we were looking at this nationally it turned out that there were certain schools that seemed to have been giving such degrees for years. Of course in other fields the scholarly thesis was the norm.

Aspray: Sure.

Tucker: I remember when the department of music at Princeton applied for permission, and the question came up in a faculty meeting whether or not the Ph.D. in music could be given for composition. People in various departments of the humanities got up and said this would never do. "Imagine getting a Ph.D. in English for writing a novel." Finally I got up and said that many people thought there was a certain affinity between mathematics and music, certainly there were many mathematicians who were very gifted in music, such as—I didn't say this—my colleague Artin and my colleague Fox. Fox could have been a concert pianist if he wanted to be. And I said that in mathematics the only thesis that was always acceptable was a thesis that was analogous to composition, and that it seemed to me that in different departments, different fields, there could be quite different standards. No more argument on that.

Aspray: One thing that you haven't mentioned in terms of formal requirements is coursework. Were there any requirements for coursework?

Tucker: None.

Aspray: What was the attitude of the department about coursework?

Tucker: Well, a student was expected to follow about three courses. But since there were no course examinations, how seriously a student followed a course was undetermined.

Aspray: Now, students were encouraged to start their own courses, weren't they? Or get people to run courses? Is that correct?

Tucker: Yes. These were usually not courses, but they were called seminars. The only things that were called courses were the official courses that had a number and were listed in the graduate school catalog.

Aspray: I see.

Tucker: But seminars sprang up and disappeared fast, so that there had to be a seminar bulletin that came out at the end of the week to announce the seminars for the following week. There might be as many as twenty of these seminars, often very small, with just three or four people, sometimes quite large. Then there were things that were referred to as baby seminars. In a given year perhaps there was not a course in complex variables. So students who had to prepare complex variables for the general examination were encouraged to band together and have a little seminar, perhaps get some analyst to map out a program for them and then to just take turns presenting the material.

Aspray: I see.

Tucker: This I think was a very good idea, and it seemed to work very well. The department did not feel any obligation to cover all of the required topics in courses in a given year. If a student complained

there wasn't a course, he was just told, "Well, use the library upstairs" or "Get together with some other students in the same boat and have a baby seminar."

Aspray: This is very much in a British model, in some ways.

Tucker: Oh yes.

Aspray: Could you tell me about how long it took students to complete their degree program?

Tucker: The normal time was three years. There was occasionally a student who would do it in two, such as Tukey. To use four years in residence, it was necessary to get special permission, and this special permission was really given only when there were extenuating circumstances, illness or some family difficulties. Three years was really the time limit. Now you could go off without submitting a thesis, and later submit a thesis and come back and take your final examination. You could do that any time up to five years after the prelims were passed. After five years, you didn't have the right to submit a thesis, but if you submitted a thesis and the department was willing to consider it, that was all right. I had one student who received his Ph.D. with a thesis that was submitted twenty-five years after he had passed the prelims. The five-year limit was just a way to keep a student from making a nuisance of himself and submitting an unacceptable thesis year after year.

Aspray: Is this fairly typical of the length of mathematics graduate education at American universities?

Tucker: Oh, no. Particularly at state universities where they used many teaching assistants such as Berkeley, people were often around for five or six years before they got their degree.

Aspray: I see.

Tucker: I think, though, that at, say Harvard or MIT, three, or exceptionally four, years was the normal thing. But at the state universities—this would include Illinois, Wisconsin and so on—because of the need for teaching assistants, there was an incentive to keep them on. An experienced teaching assistant is more valuable than a beginning graduate student.

Aspray: We haven't spoken yet at all about undergraduate education. Would you like to say something about Princeton's role in undergraduate education, both the mathematics department's and the university's philosophy?

Tucker: When I came to Princeton in 1929 the Princeton student body was, as is the case now, very carefully selected from well qualified applicants. At that time, something like 60 percent of the Princeton undergraduates had come from private preparatory schools. Schools like Lawrenceville, near here, and Exeter and Andover and other

schools in the East and the West. Indeed, about 50 students a year would enter from Lawrenceville. That was the largest contingent. This gave the undergraduates much greater homogeneity than now. And, of course, Princeton at that time was not coeducational. There were no black students. Indeed, Princeton, of all the Northern schools, had the largest contingent of Southern students.

When a student entered, there were just two options open. To enroll for a Bachelor of Arts degree or for a Bachelor of Science and Engineering. There was a quota on the engineering students. I think there were not to be more than a certain percentage, say 15 percent, of the entering class. So the engineers were segregated from the beginning. Of course they took their courses—not those that were specialized in engineering, but their courses in mathematics, physics, chemistry, English, so on—with the rest of the students.

Aspray: I see.

Tucker: A student could occasionally make a transfer from one to the other, from arts to engineering, or vice-versa, at the end of the first year. Perhaps in a few cases later than that, but the only regular transfers were in the first year. But these were not very many. The arts students took five courses each term through their first two years, their underclass years. They took four courses in the remaining terms, with two of the four courses in each term in their major field.

Aspray: I see.

Tucker: It would be very exceptional that a student would take less than two or more than two. It would have to be for very special reasons, that would be decided on as individual cases.

Aspray: Okay.

Tucker: The framework in mathematics. We started with a semester of analytic geometry, followed by a semester of differential calculus, then a semester of integral calculus and a semester of differential equations. That program was required of all the engineering students, and, of course, of physics students and students that were going to major in mathematics. I think the chemists mostly took that program, and other students who wished might do so too. The choice of courses was rather free within the first two years.

A student was expected to have a certain balance of courses in underclass years. He was not supposed to take more than one course in any one department at the time. At the same time the student had to make sure to meet the entrance requirements, the prerequisites, for any major field that he wanted to choose. That choice was ordinarily made in the second term of sophomore year, to take effect in the junior year.

Aspray: Was that entirely up to the student, or did the department have some power to veto choices?

Tucker: The student could only enroll in the department with the signed permission of that department.

Aspray: I see. How large were the enrollments of these courses in the first two years, this sequence of four courses?

Tucker: We would start out with somewhere around 400 students. By the time this got to the final course in the sequence, the differential equations, it would probably be down to somewhere between 150 and 200.

Aspray: I see. Were these taught in single-section lecture classes?

Tucker: There were three 50-minute classes a week with about 20 students in a class.

Asray: So there were multiple sections for a given course.

Tucker: Sometimes for reasons of room size and scheduling, there might be sections that had as many as 30 students. Of course, for those sections, careful attention was always given that there should be a very capable instructor, who would have no problem in handling such a large section. A regular-size section was supposedly 20, but was usually 17 or 18 because there would be some attrition after the courses started. Students could change from their course selection. They had to pay a small fine for this privilege. They could choose to change their selection during roughly the first month of the term. In the mathematics courses, the change was almost always out rather than in.

Aspray: What faculty members would teach these courses? Was it a mixture of senior faculty, junior faculty, graduate students, instructors? How were they assigned?

Tucker: Well, there would be one regular member of the faculty who would be in charge of the course. Eisenhart, the chairman, very often had charge of a freshman course himself. Others among the senior faculty did not. I don't remember Wedderburn ever having charge of a course. In the fall term of my second year, I was started out teaching two sections of the main freshman course, which had T.Y. Thomas in charge. He would have then been an assistant or associate professor. I had the misfortune to make a bad impression on him. He mapped out the week-by-week program of the course, which sections of the textbook were to be covered and such. His section had been picked by the registrar to have some of the very best students who were in the course, so he was able quite comfortably to maintain the fast pace that he had set.

Now I had two run-of-the-mill sections, and I felt that I could not go at the pace that he had set and at the same time give the students a thorough understanding of each thing we did. So I protested, and this protest was taken very badly by him. He called a meeting of the instructors, and at this meeting the first order of business was to exact a promise from me that from that point on I would follow the

program that was laid down exactly as it was laid down. I declined to make this promise. I said that I would be glad to promise to do the best job of teaching that I was capable of, but I couldn't promise more than that. He immediately went to Dean Eisenhart and charged me with insubordination.

The instructors in the course all sympathized with me because they were having the same difficulty I was, but they weren't as audacious as I was in speaking up to Thomas. Another instructor in the course was W.W. Flexner, who already had his doctoral degree at that point and was a post-doctoral instructor. He immediately went to Lefschetz and said, "Tucker's in trouble," and explained it to him. Then the two of them went to see Eisenhart very shortly after Thomas had been there.

Eisenhart then called me in and was very gentle and said that he understood that there was some problem between me and Professor Thomas. He said he would like to hear from me the way I viewed the matter. I explained this to him, and I guess I had not told Thomas that I would do the best job I could, because when he asked me why I had declined to give the promise that Thomas had asked for, I said, "Well, I didn't feel that that was a proper thing." And I said that if he had asked me to do the best possible job of teaching in the course that I possibly could, I would have gladly given the promise. That was my aim. But I felt the pace that he was setting just made it impossible to do a thorough job on the material as we were going along, and I felt it was better for the students to thoroughly understand things than to cover a lot of ground.

Eisenhart simply thanked me for telling him this and dismissed me. The next thing I knew I was taken out of teaching in that course and was kicked upstairs to teach the main sophomore course which was under the supervision of Einar Hille. Somebody else, I've forgotten who, had to be shifted in to take my place. With Hille things were much more relaxed and there was no problem.

Thomas was unusually autocratic, but many of the students who worked with him ... one of my best friends Ed Titt did a thesis with Thomas and got along beautifully with Thomas. I just had the misfortune to be a little obnoxious. But from what happened it was clear that Eisenhart was on my side. Indeed, years afterwards I heard Marie Fleming, who was Dean Eisenhart's secretary at that time, say that he had called Thomas in and given him a tongue lashing.

Aspray: This practice of stacking certain sections with abler students?

Tucker: This was something that had been started long before. I don't know how it got started, but the registar, Wilbur Kerr, in those days, just automatically did this and thought it was wanted. Much later on this was stopped. As a matter of fact the sectioning of the students came to be done by the mathematics department itself rather than by the registrar. But in those days, the registar handled all such matters.

Aspray: Were there people on faculty, regular faculty, who were not at all eager to teach these lower division courses?

Tucker: Because of Eisenhart being the chairman of the department and setting an example by doing it himself and making it clear that this was part of the job for everybody, I think any discomfort was kept quiet.

Aspray: All right.

Tucker: But I was going to say Wedderburn did not ever do any of this. I think he had done earlier on, but he was regarded even in 1929-30 as being in not good health. He had some sort of a nervous breakdown a year or so before that and had stopped being the chief editor of the *Annals of Mathematics*. He became rather a recluse. So he, like Eisenhart, would teach one graduate course each term and one undergraduate course, but the undergraduate course would be at the upperclass level.

Aspray: Were there any people that weren't assigned to these courses because there was some feeling that they couldn't handle students at that level? People who were usually given more advanced courses?

Tucker: No. Of course, Velben as research professor had no teaching duties at all. All he had was a seminar. Gillespie taught nothing but freshmen, and Knebelman was doing the engineering mechanics course and that occupied him completely. Except for these special cases we shared the spectrum of teaching, except the junior people didn't get to teach a graduate course every year. They would perhaps get to teach one term out of two. Alexander was another special case. He was on a half-time teaching program. It was my understanding that he received half salary.

Aspray: He was independently wealthy.

Tucker: Yes. The same sort of thing happened at the Institute, where after being professor with the responsibilities of professor for a number of years he asked to resign his professorship and become a permanent member. He kept the same office, he did more or less the same things, but he didn't have the responsibilities of attending department meetings and helping choose the people who would come to the Institute.

Aspray: Why don't we turn to the upperclass curriculum?

Tucker: There were some engineers who took advanced calculus. Electrical engineers got a special version of advanced calculus. There were courses in so-called higher geometry, and there would be courses in analysis. These were one-term courses. Also a course, which I think I began, in combinatorial topology. The freshman courses were given 100-numbers, the sophomore courses 200, and junior courses 300, but we rarely had 400-courses. The 300-courses would very often be offered only in alternate years, and so could be taken either by a

junior or senior according to the luck of the draw. There was a 400-course, Math 405-406. This was the course that Bargmann spoke about when we were talking to him. The first course that he had taught was a course that he took over from H.P. Robertson when H.P. Robertson started to become involved in pre-war work.

Aspray: This was in mathematical methods?

Tucker: 'Mathematical Physics' was the title of this course. It was a two-term course, and it really covered the waterfront of the mathematical methods for physics. And there would occasionally be a special course given to the undergraduates. In theory a course had to be approved by the course-of-study committee and go before the faculty. This had to happen before it could go in the catalog, and it had to go in the catalog before it could be taught. So you had to start about two years in advance to introduce a new course.

Aspray: I see.

Tucker: There were some courses from former times that were really not taught anymore, but they still were in the catalog for trading purposes. When you wanted to put in a new course you had usually to trade in an old one. And even before a course was traded in, we would often give a course that wasn't the course in catalog, but we would give it under the number and title of that other course.

Aspray: I see.

Tucker: And I think the only reason that we were able to get away with it was that Dean Eisenhart was so highly regarded in the University that he could do no wrong.

Aspray: I see.

Tucker: I forgot to say that the juniors and seniors took only four courses a term. The weight of a fifth course was given to independent study. So in junior year each student studied something on his own and usually wrote a report on what he had studied.

Aspray: Was this really on his own, not under the direction of a faculty member?

Tucker: Well, it could be on his own if he wanted to make it that way, and there were some students, very good students, who really went out on their own. Other students depended on some member of the faculty and would come to see that faculty member perhaps once a week. I had students of that sort, because while I was at the University of Toronto I had passed the actuarial examinations and was actually an associate with the Actuarial Society. I didn't go on to get my fellowship, but this meant that I had gone through the basic course of study for an actuary. So I was called on to serve as the helper for any students who were looking towards an actuarial career. They would use their independent work to study for the actuarial examinations.

Aspray: I see.

Tucker: Once in a while there was a junior paper done that got published. Then each senior had to do a so-called senior thesis, and this could be done in conjunction with reading. The actuarial students would do a thesis that was pretty much a summary of a certain parts of actuarial science, or they might try to embark on something that was verging on research.

We found, by the way, that better work came out of the junior papers than out of the senior theses. Somehow or other the students doing the senior theses became very self-conscious about it and took it so seriously that they weren't thinking freely in the way that happened with the junior papers. We would often tell a student, "Well, the junior paper is just to give you a certain amount of experience to provide a background to do your senior thesis. Don't take it too seriously, just do whatever you can with it and that will be fine." But the senior thesis, besides being called a thesis, had to be handed in in bound form; there was almost as much rigmarole with it as with a Ph.D. thesis. I think in a way that it was unfortunate that it was weighted so heavily.

Then a student at the end of junior year had an examination, which would be partly written and partly oral in his major field. Of course the oral part would have to do with his independent work.

Aspray: But the written part would be a general examination of his preparation in classes?

Tucker: During his junior year it would be advanced calculus, this sort of thing.

Aspray: I see.

Tucker: In senior year, it would be a paper that would have, oh, a vast number of questions covering all the courses that any of the seniors had taken. They would be told that they must answer some number of them, say twenty questions out of the hundred that were on the paper.

Aspray: I see.

Tucker: And then again, there was an oral examination for the seniors.

Aspray: And that was on their thesis?

Tucker: No, it was on both.

Aspray: I see.

Tucker: Indeed, there was probably an oral examination on the thesis earlier on, because by the time that the examination occurred at the

end of May, it was really too late to do anything about the thesis. So there was an informal examination conducted by two members of the department on each thesis that would be half an hour to an hour. Again, this was largely to make sure that the thesis was the student's own work and that he understood what it was about. Now, honors were given entirely on the basis of the departmental work, other courses and achievements didn't count, solely on the basis of a student's work within the department in his junior and senior years. Highest honors, or high honors, or honors could be given. It was quite unusual to get highest honors. Every year there were at least one getting high honors, one or two with honors, and about half of them, no honors.

Aspray: What size was an undergraduate class of mathematicians?

Tucker: Well, there would be somewhere between five and ten each year who would graduate with a major in mathematics. This meant, taking the juniors and seniors together, that you would have somewhere between ten and twenty. Dean Eisenhart had the feeling that there weren't enough students who majored in mathematics, and he was constantly trying to find ways to attract more students into the mathematics major. We tried various experiments of this sort, such as setting up a mathematics club to encourage the sophomores who were interested in mathematics, and urging those in the sophomore courses to come to that mathematics club.

On another occasion, this would have been about 1938, Dr. [C.B.] Tompkins, an instructor, and I set up some monthly lectures on various aspects of mathematics beyond the undergraduate level. For example, I'm sure I gave a talk on topology, the sort of research that was going on in topology. And Tompkins, who had worked in calculus of variations for his Ph.D., gave some talk about calculus of variations. I remember in particular we had one speaker, Professor J.A. Wheeler from the physics department, who talked in a somewhat general way about nuclear fission, and this was shortly before nuclear fission became a very carefully classified matter.

Another thing that happened, the Eisenharts would invite undergraduates, particularly at the sophomore level, to tea. They were living at that time at Wyman House, because Dean Eisenhart was the Dean of the Graduate School then. They would invite picked undergraduates to tea along with members of the mathematics department. This again was a show of hospitality. The hope was to make these students more likely to major in mathematics.

Aspray: What was the philosophy of the undergraduate program? Were you trying to encourage people to go on to continued education in mathematics? Were you emphasizing applications and careers that one could get after an undergraduate degree?

Tucker: The latter. First of all an actuarial career. Another was law, because we were always making the argument that the rigorous nature of mathematics was excellent training for law. Indeed, we were

able to quote statements by professors at law schools that they always liked to see a student entering with a strong mathematical background. And we were beginning—this was a favorite of Dean Eisenhart's—to emphasize statistical possibilities. Quality control was just beginning to come in at that time. Nowadays, quality control is done everywhere, and there are handbooks so that even a good shop mechanic can do quality control. But at that particular time it was thought you had to be a statistician to do or supervise quality control.

I felt when I was an undergraduate majoring in mathematics that I had only two options to use my degree for my living. One was actuarial science, and the other was teaching. I tried to keep both those possibilities open up until the time I got my bachelor's degree. Then I opted for the teaching. I thought this would probably be high school teaching, but I was encouraged by the chairman of the mathematics department at the University of Toronto to think of university teaching.

Aspray: Did you train many secondary school teachers here at Princeton?

Tucker: Yes. We didn't have any formal program in that. But later on, after World War II, Princeton University did set up a council to help students who would like to become teachers to choose courses and aim in that direction. It was called the Committee on Teacher Preparation, or something like that. There was even a short while when we had students in this teacher-preparation program go to certain schools, watch teachers who were regarded as excellent teachers, watch how they handled the classroom, and then, under the guidance of this teacher, give a class.

Aspray: I see.

Tucker: I've forgotten exactly when that was, but I was one of the people involved in this Committee on Teacher Preparation. I'd forgotten all about it until I just happened to be leafing throught this Princeton Companion and suddenly found my name there, and I looked to see what it concerned, and it concerned the Committee on Teacher Preparation. You can't look me up, I'm not in the index, but I'm in the book.

Aspray: In the upper level courses, how much flexibility was there in terms of the content of the courses?

Tucker: Oh, it was completely at the discretion of the professor teaching it.

Aspray: So the department hadn't decided that these are the things that need to be covered on a course on, say, complex variables.

Tucker: It was only the courses that were used as prerequisites for other courses where certain things were expected. That is, if such-and-such a course was a prerequisite for a course on complex variables, then there were certain things that should be done. But this didn't put very much restriction on courses.

Aspray: How were faculty members assigned to the upperlevel undergraduate courses? Did they get their choice? Was there lots of competition? Was the assignment by the senior staff members or the department chairman?

Tucker: I think, certainly in the 1930s, that the department chairman, Eisenhart, did this. But I remember that every time when he did this ... I shouldn't say every time, but I remember several times when he did this, and he called in Bohnenblust and me to help him with that. We would sit around a table and do it. And it would be done in a couple of hours.

Aspray: I see.

Tucker: That sort of thing happened before the teaching assignments were made for a given term. In many cases, it was fairly obvious. One system was that if a certain professor took the main course in the first term of freshman year, then the next term he took the next course, and so on and would follow through the whole sequence.

Aspray: I see.

Tucker: This of course made sure that the courses fit together nicely.

Aspray: So it was quite common then for an undergraduate physics major to have, say, Professor Hille, for four terms?

Tucker: Yes, if he was in Hille's section. When I first began teaching, we used books by Fine as our textbooks. Fine, along with somebody by the name of [H.B.] Thompson, had written an analytic geometry, which we used in the first term of the sequence. From that we went to Fine's calculus, which was one volume that covered differential calculus, integral calculus, differential equations, and even an introduction to complex analysis. I've got one up here. It was not a large book. I would say it was somewhere between 400 and 500 pages. So to cover all that ground it didn't ever say anything twice.

There were complaints every year from the secondary schools—these were of course mainly private secondary schools—that they had sent very well prepared students in mathematics who then had difficulty and floundered in the courses at Princeton. This was partly blamed on the fact that the teachers were insufficiently skilled in teaching and that the textbooks were not appropriate textbooks. So to try to counteract this, two or three of us—I know Bohnenblust was involved in it, I was involved in it, and I think Sam Wilks was involved in it—prepared some alternative chapters to Fine's calculus. These were mimeographed and sold to the students for what they cost to make, about fifty cents or something like that. They were sold over at the University Store, and the students were supposed to use these to supplement or replace certain parts of Fine's calculus. I don't think that these were really an improvement. I felt they were at the time, but I think that the improvement was in the eyes of enthusiastic young mathematicians and that Fine probably knew more about teaching undergraduates than we did.

Because the criticism was directed particularly at the first-term freshman year, the analytic geometry, Eisenhart wrote a textbook for that first course and he called his textbook *Coordinate Geometry* rather than analytic geometry. Again I think that this was no help. Indeed, it went rather in the direction of abstraction rather than in the direction of simplification. The style, if you read that book, except for the more elementary content, is exactly the same style as the serious textbooks that Eisenhart wrote on differential geometry, Riemannian geometry, continuous groups, and so on. It reads in just that same style. Eisenhart always numbered each equation, and then when he would refer to it, even though it was pages later, by that number.

Aspray: Just the number?

Tucker: By number. In other words, the reader had to have a certain sophistication in how you read serious mathematics in order to be able to read the Eisenhart book. Really the only thing that kept the administration of the University from making a thorough investigation of the teaching of mathematics at Princeton was Dean Eisenhart and the fact was that he was somehow superior in seniority to everybody else in the administration—how could they attack him?

Now Eisenhart retired in 1945, on the 30th of June, 1945. And on the 4th of July—I remember because it was the 4th of July—I was in the office of Dean Root, the Dean of the Faculty at that time. I was there to speak for the mathematics department. One of the first questions that he asked me was 'Is it necessary to continue using Eisenhart's *Coordinate Geometry* as a textbook?' I said, "No, it is not." And we went on from there. He authorized me personally to undertake a revision of the freshman and sophomore courses. He had me go to M.I.T. and someplace else. I'd told him the course was very good at M.I.T. I think that the teaching at Princeton really was excellent, but that the text material that was being used tended to be over the heads of the average student.

Aspray: I see. Since we're on the topic, Do you have any comments along those lines to make about textbooks for other courses offered, whether they be upperclass or graduate courses?

Tucker: Well, I myself am a great believer in teaching by example. When I was chairman in the late '50s of the College Board's Commission on Mathematics, that commission was trying to revise high school mathematics, and we wrote some sample text material. One of the members of the commission invented the name the 'Tucker Principle,' that each lesson, section, should begin with an example and end with an example. One should use an example to open up the section, to show what it was about, and then an example at the end for the student to check whether he had understood what came in between.

Until Bob Rourke formalized this process by pointing out that this seemed to be my plan, I had not thought about it this way, but it's an exact description of my attitude. Yet the tendency, I think, of mathematical text at that time was to launch right into something. Then

if there was an example, to end with that as an illustration, rather than using an example to get into the topic, to take this example and abstract from it the things that you want to structure.

Aspray: I see.

Tucker: Able undergraduates, the ones that seemed to be likely to go on in mathematics to graduate school, were often allowed to take graduate courses. The one problem about this business of taking graduate courses, of course, was that there were no exams in the graduate course. This sometimes led to problems. There were some students who caught on to this and tried to take graduate courses instead of undergraduate courses. They could count the graduate course, you see, in place of an undergraduate course, and they wouldn't have to undergo a rigorous examination.

I can think of one example of this. This is a very highly thought of mathematician at the present time, at M.I.T., by the name of Gian-Carlo Rota. He was an undergraduate here at Princeton. He, I think, ended up, certainly in his senior year, taking nothing but graduate courses in mathematics, because he seemed to talk a very good line, he was just so voluble. But it turned out that he didn't graduate with the highest honors, which I think he really deserved, because in his examinations in the senior year which determine the sort of honors he would get, he didn't do very well. He hadn't had the conditioning for them. So there was a certain danger in that for the undergraduates who were allowed. If it was just one course, as was ordinarily the case, there were no particular problems.