The Princeton Mathematics Community in the 1930s
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OVERVIEW OF MATHEMATICS AT PRINCETON IN THE 1930s

This is an interview on 8 October 1984 of Albert Tucker in Princeton, New Jersey. The interviewer is William Aspray.

Aspray: In this session we will try to give an overview of the important developments in Princeton in the 1930s. In an earlier discussion off tape, we talked about four major kinds of contributions of the Princeton mathematical community: (1) the Ph.D.s produced; (2) the overall environment for visitors to get inspiration and, for a time, to do research; (3) the research of the faculty members; and (4) the opening up of new areas of research. I am hoping to get some perspective on which of these you see as the most important contributions. And perhaps you will talk a bit about what the particular contributions were.

Tucker: The thing that strikes me first is the research program at the graduate student level, in other words, the training of graduate students in mathematical research. At Princeton this was a broadly exercised effort. The students were carefully selected for admission. No students were admitted just to get a master's degree; all admissions were for the doctor's degree or for mathematical research beyond the doctor's degree. These students seemed to live and breathe their mathematical work. The library was right there in the building and was open all hours. There were recreational facilities in the common room, and indeed there were external recreational facilities, such as on the tennis courts, and showers in the building. So Fine Hall was not only a study place, and it was not only the place where lectures and seminars occurred. It was also a place where people met for tea in the afternoon. It was a mathematical club.

Aspray: To get a good understanding of this we might differentiate Fine Hall from some other place. You say that one thing unique about Princeton as a training ground for mathematical research is that it's a place where people do their research in a supportive social and intellectual environment. There were a number of other centers in the United States at this time that were producing large numbers of math Ph.D.s: Harvard, Johns Hopkins, Chicago, Cornell, Columbia, Yale, ...

Tucker: Illinois, Michigan, Pennsylvania.

Aspray: How does Princeton differ from any of these other places?

Tucker: The main difference I was able to observe was the fact that mathematics was concentrated in one building. Whenever you wished to be involved in mathematics, all you had to do was to go into that building and participate. The students learned a great deal from one another, and students often wrote their theses using a variety of advisers. A student could, if he wished, stick to one advisor, but he could use any of the faculty members. And he could use his fellow students. There was always a room where you could go and use a blackboard and argue things out with another student or faculty member.

Aspray: Let's take an example. You spent some time in the early 1930s at Harvard.

Tucker: I was at Harvard in the spring term of 1933.

Aspray: Would you compare the situation at Harvard with the situation at Princeton?

Tucker: At Harvard it seemed as though the mathematical activity was decentralized and disorganized, because the professors had offices various places. There was no common place where there were mathematics offices. The graduate students had nothing like the Fine Hall common room. A student's social activities were not organized around mathematics, but around the people in his dormitory or boarding house. While I was there I had no mathematical activities except when I went to see my supervisor Marston Morse or when I attended the weekly mathematics colloquium or when I sat in on a course that Marston Morse was giving. I would see other people in the classroom, but the moment the class was over we all went our separate ways. Most of my time was actually spent either by myself or with people who were not mathematicians.

Aspray: Some outsiders might think that mathematical research is a solitary activity. What kind of impact did the Princeton environment have? What was the outcome of having people there breathing and talking mathematics all the time? Can you make any differentiation between the Ph.D.s from Princeton and those from Hopkins or Harvard or Chicago?

Tucker: My belief—and I think this can be backed up by statistics—is that the Princeton Ph.D.s in mathematics were, by and large, more productive in the writing of papers and monographs and so on. I think they often became mathematical leaders in the universities where they got positions. I think that this was so because of the close contact that the graduate students and post-docs had with members of the faculty, with Veblen, von Neumann, Eisenhart, Lefschetz. Through their seniors they began to acquire what you might describe as mathematical statesmanship. And when they took positions elsewhere they tried to carry some of this Princeton excitement with them.

Aspray: You mentioned statistics a moment ago, and when we were talking before this interview you cited a particular study.

Tucker: You mean the study of the productivity of mathematicians that was made somewhere around 1960?

Aspray: That's correct.

Tucker: In this study—we should look up the reference—the mathematics Ph.D.s who had been out so many years from their Ph.D. were put in classes according to the number of papers that they had published. These classes were then related to the institutions that had given the Ph.D.s. Princeton was spectacularly highest in the most productive of the Ph.D.s, even though it was not at all at the top in the total number of Ph.D.s.

Aspray: In addition to productivity in the sense measured by the number of papers written, let's talk about productivity in the sense of finding new ways of looking at things, or of forming new fields, or of crossing disciplines. Do you think the way that education went on at Princeton, where one wasn't closely tied to an advisor but had a whole smorgasbord of mathematics to experience, somehow translated itself into mathematicians who were more willing to go outside the narrow confines of traditional discipline-boundaries?

Tucker: Oh, yes. Among the Princeton Ph.D.s there were unusually many who went into things that were on the edge of traditional mathematics—or altogether outside of traditional mathematics.

Aspray: Can you give some examples?

Tucker: Well, Henry Wallman is an example. He took his Ph.D. at Princeton in the late '30s, and then became involved in war-work at M.I.T. During the war this work was classified, but I think it had to do with radar. It was at any rate work in electronics. After the war he was at M.I.T. for a while, but then was appointed to a professorship in Sweden. He was professor of "electrotechnics" at the Chalmers Institute of Technology in Gothenburg, Sweden. There was another Princeton math-Ph.D. who went into engineering, Paco Lagerstrom. He became professor of aeronautical engineering at Cal Tech, but was trained in mathematical analysis.

Aspray: He continued to use his mathematical training in his later work?

Tucker: Oh yes.

Aspray: Is John Tukey another example?

Tucker: John Tukey is a good example. His Ph.D. was in topology with Lefschetz, but in his wartime work he became very much interested in statistics as well as in data structures. So after the war it was appropriate that he should take a position that was half-time at Princeton University and half-time at Bell Labs. At a later time when the Statistics Department at Princeton separated off from Mathematics Department, he was the first chairman of the Statistics Department. And I think that he became some sort of deputy director of research in mathematics, statistics, and information processing at Bell Labs.

Aspray: One more example might be Marvin Minsky.

Tucker: Marvin Minsky came later, in the early '50s.

Aspray: I see.

Tucker: He took his Ph.D. at Princeton in mathematics, and then after being a member of the Society of Fellows at Harvard he took a position in, I guess it was, electrical engineering. He has since come to be the head of the artificial intelligence lab at M.I.T.

Aspray: Wasn't it the case that his doctoral dissertation had something to do with this?

Tucker: Oh yes.

Aspray: In a field in which mathematicians had not really worked before.

Tucker: That's right. He had these ideas, and I for one felt that it was more useful to the world to have him develop these ideas, which were completely original, than to do something say in topology, which he could very easily have done.

Aspray: Was that attitude, though not necessarily about Minsky, shared by the other faculty members in mathematics?

Tucker: Yes. This was true of some members of the department and not of others. Different people had different ideas of what is good mathematics, but Lefschetz for example was very sympathetic to students who wanted to do their own thing. He certainly agreed with me that a Ph.D. in mathematics can be given for good work, whether or not the mathematical community agrees that it's mathematics. After all, it's the University that's giving the degree, not the Department. So that the degree should be given for creative work, and the more original the work the more likely it is not to fit into any of the established channels.

Another person who was at Princeton in the early '30s and who became a professor of electrical engineering was John Landes Barnes. His thesis was one of the first mathematizations of the Heaviside calculus, which back in the '30s was regarded as something that was quite outside mathematics. You couldn't fit it into any of the ideas that mathematicians had of analysis. Later on, of course, this was gradually absorbed into extensions of analysis, but at that time it was a daring thing to do that as a mathematics thesis. On the strength of that he first was an instructor in electrical engineering at M.I.T., and then he went to U.C.L.A. as a professor in electrical engineering.

Aspray: So to this point in the interview we've discussed how the mathematics community was a unique place for the the production of creative and productive mathematicians.

Tucker: One other person we should have mentioned is Alan Turing, who took his Ph.D. in 1938 with Alonzo Church.

Aspray: Yes. What were other distinguishing features of Princeton in the Thirties?

Tucker: Another important factor was the production of mathematical publications. I'm not referring just to the usual journal publications, although the *Annals of Mathematics* came to be, in the 1930s, one of the leading, if not **the** leading, mathematics journal in the world. This was largely the doing of Solomon Lefschetz. But over and beyond the *Annals of Mathematics*, in the intense activity of Fine Hall in the Thirties, course notes were generated by the various professors. There would be perhaps 20 sets of course notes produced in a year. These were lectures by von Neumann, Morse, Lefschetz, Church, Robertson, and others. Then there would be some seminar notes that were put out, and sometimes notes from courses given by visiting mathematicians, such the topologist from Czeckoslovakia, Eduard Cech, and Carl Ludwig Siegel, the great German number theorist.

A Princeton custom, which had started earlier on, was to save the individual members of a course the trouble of taking lecure notes. The note-taking was done each time by one individual, perhaps in some sort of rotation, and then these notes were mimeographed and everybody in the course got a copy. This practice proliferated in the mid-Thirties. It was helped along by the fact that the various professors at the Institute had assistants, and the assistants really had little to do, but one of their duties was to write up the lecture notes. One year Weyl had as his assistant, Richard Brauer, a very capable mathematician. Brauer would write up and even embellish the mathematics that Weyl had presented in his lectures. The mimeographing of the notes was actually done by undergraduates, who got paid for it out of the W.P.A. funds that were made available for student employment. This was the Depression.

These notes became known throughout the mathematical world. People would write in and ask how they could get copies, and we would sometimes re-do the stencils. The notes were usually sold for an amount which was enough to pay for the paper and the ink. So you might be able to get a set of notes for a dollar, or a dollar and a quarter, or something like that. When we filled the outside orders we sold the notes at the same price plus postage. But before we knew it we were in the publishing business. One of the secretaries—there were 2 secretaries, the Math Department secretary and the Institute's School of Mathematics secretary—Miss Gwen Blake, the one for the Institute, was spending about half of her time taking care of the correspondence that had to do with the notes. There was a bank account for the Princeton Mathematical Notes where the money we got was deposited.

Well, finally something had to be done, so we tried having them planographed by a company in Ann Arbor called Edwards Brothers. Then there was the distribution problem. Princeton University Press was persuaded to serve as the distributor for the notes. Once or twice a year we would send, to those on a mailing list we had, a list of notes that were available and the prices, and then we would fill these orders. But that was still not very satisfactory. The final step was to set up the *Annals of Mathematics Studies.* Thus the mathematics that was being generated at Princeton at that time got wide dissemination by the notes and then later by the *Annals of Mathematics Studies.*

Aspray: What role did these notes play across the country? How common were such graduate texts, monographs, and lecture notes before Princeton starting producing them?

Tucker: Sets of notes were occasionally, I think, put out, but they were usually known of only locally. For example, during the term that I was at Harvard I think that Marston Morse was having notes produced from the course I sat in on, but if there were other notes available at that particular time I didn't know about them.

Aspray: Did the *Annals of Mathematics Studies* get used in graduate education or research in many other places?

Tucker: Oh yes. They were very inexpensive. Towards the beginning—that would be around 1940—we would price them at a cent a page plus a dollar. And we were able to do better than break even on this. I think the importance of the Princeton notes and the *Annals of Mathematics Studies* was in establishing an existence theorem, that this sort of thing could be done successfully. Successfully in the sense that people all over the world found the material useful and also that it could be made to pay for itself. Of course what was not paid for was the work the people did in preparing the notes—that was a labor of love—not to mention the work of the lecturer who gave the course.

The lectures that were given at that time were, for the most part, research. They were not preparatory courses. Now today I couldn't make a particularly strong case for the *Annals of Mathematics Studies* being continued by the Princeton University Press. If the Princeton University Press were to say, "I think we've done our share for mathematics and we should stop the *Studies*", I would say that's fine. Nowadays there are several publishers, mainly commercial publishers, putting out series of lecture notes. The thing that showed that this could be profitable as well as worthwhile was, I think, the *Annals of Mathematics Studies*.

We weren't publishing only things from Princeton. Manuscripts would be sent in to the *Studies*, just as they would be sent in to the *Annals of Mathematics*. Indeed a reason for giving it the name *Annals of Mathematics Studies*, was to broaden it to include the whole mathematical community, both in accepting manuscripts and disseminating the material. And at that time there was no other publisher doing this.

The American Mathematical Society started a series of publications, in paperback. They're called *Memoirs* later. I don't think they've ever been as successful as the *Annals of Mathematics Studies*, largely because by the time they got started there were other things going. *Studies* was first and set high standards and became known all over the world. In a mathematician's office anywhere in the world you can spot *Studies* on the bookshelves.

Aspray: I understand that at that time for a commercial publisher even to consider publishing a set of notes, a series of lectures, or even a book, they'd almost have to have some sort of subsidy.

Tucker: That's right.

Aspray: Where was that subsidy money coming from at the time?

Tucker: Mainly from the National Research Council.

Aspray: Was it adequate? If someone had a major book to get out, could he find the money to subsidize the publication?

Tucker: If that person was prominent, and if the article or monograph was promising, I'm sure it got published. But it might take two or three years to arrange. In the second or third of the *Annals of Mathematics Studies* was Goedel's proof of the consistency of the continuum hypothesis, and at that time I don't think that that could have been published inexpensively anywhere else. The alternative would have been to have it published as a journal article, but then that journal would have used up much of its quota of pages for the year.

Aspray: Let's turn to some other important contributions of Princeton of the Thirties. We've talked about the training of Ph.D.s and about publications. What other things come to mind?

Tucker: The next step up is the post-doctoral training then taking place at Princeton. In the 1930s there were many National Research Fellows in mathematics. I've got a list here somewhere, but I would say that there were 50 or 60 National Research Fellowships in mathematics held at Princeton from 1920 to 1940.

Aspray: How does that compare with other major research institutions in mathematics?

Tucker: I think it was a much larger number than at other places, and this I think was caused by the congenial math community at Princeton. People had heard that Princeton was a good place to be because of the working conditions: the excellent library, the availabilty of carrels in the library, and the fact that it was not too difficult to get a room or an apartment quite close to the Princeton campus.

It is of course no longer true, but back in the Thirties during the Depression, there were many families that were glad to take in a roomer or set aside an apartment. Then single men could stay at the Graduate College, which was a handsome place to live. It was much more expensive than to live in town and take your meals on Nassau Street, and because of the amenities of Fine Hall it wasn't necessary to have much in the way of quarters other than a bed to sleep in. Indeed, some people tried to sleep in Fine Hall, but Dean Eisenhart put a firm stop to that.

Aspray: In what way did this special community at Princeton have a beneficial effect on these post-docs? Can you see some result of their spending time at Princeton that might not happended at Harvard or at Hopkins?

Tucker: I think it's just what we were saying earlier with regard to the graduate students. They got a much broader spectrum of stimuli at Princeton; if they went somewhere else they would probably be working with one particular mathematician. At Princeton you really had to be terribly single-minded and have blinders on to concentrate on just one thing that was going on there. It was, as you put it, a smorgasbord.

Aspray: Are there other things that you want to mention about Princeton's overall contribution to American mathematics and to the American mathematical community? There are two things that come to mind that you haven't mentioned. One is the specific research of the permanent faculty members, and the other is areas of research that were opened up by the Princeton community. When I think of the contributions of Princeton in the 1930s, I think of recursive function theory and topology, rather than of the community of scholars and the training programs and such.

Tucker: Well, the part of that I knew best is what was going on in geometry and topology. In geometry, research was mainly in differential geometry and tensor applications and such things; the leader for this was Veblen. He was aided in this by Tracy Thomas, who had been his student. This mathematical research was of interest at that time mainly because of general relativity and mathematical physics. It seemed to be exciting and promising at that time. But looking back now it doesn't seem to have led to further developments. Perhaps developments have occurred on the physical side, with field theories and that sort of thing, but I can't speak to that.

Topological research, on the other hand, led to developments in all sorts of directions. It's ironic that in 1915 when James Alexander was getting his Ph.D., he was advised by Veblen to do his thesis in something other than topology because Veblen thought it might very well be a passing fad. Of course it wasn't called topology, it was called analysis situs. So J.W. Alexander did his Ph.D. in complex variables with T.H. Gronwall. The Princeton topology was, of course, not a fad at all. It was started by Veblen and Alexander and then carried on especially by Lefschetz and his students.

Lefschetz, who came to Princeton in 1924, became very active in the Princeton mathematical community. He had the knack of working with students, and he attracted good students (I except myself). He had students such as Paul A. Smith, Norman Steenrod, and Ralph Fox, and he worked intensely with the visitors who came. So the Princeton school of topology I think of as headed by Lefschetz and later by Steenrod. It has had a world-wide influence. I think that in other areas there hasn't been a contribution as spectacular as that in topology.

You referred to recursive functions. This takes us to Alonzo Church and his students. And of course to Goedel, who came to the Institute for Advanced Study three times as a visitor and finally in 1939 became a permanent member. But it was Church who worked with the young logicians. There was great admiration for Goedel, but he was regarded as rather inaccessible. It was Goedel's work, his publications, rather than his teaching that was important. I don't mean that he wasn't willing to talk to a student. In some sense he didn't know how to do this. The student quickly appreciated this and tried Church, who had infinite patience and who seemed, in a very impersonal way, to be able to give students the help that they needed. Here again we should mention that Church was Veblen's protege, and Veblen supervised the thesis, so that we can give some of the credit for mathematical logic to Veblen. This stemmed from the fact that Veblen's own Ph.D. thesis, done with with E.H. Moore in 1905 at the University of Chicago, was on a set of axioms for Euclidean geometry—an improved version of the Hilbert axioms. This was the place from which Veblen started his progression through projective geometry, analysis situs, and differential geometry until he had—except for algebraic geometry—sometime in his life worked in every area that has the name geometry attached to it. Indeed when I knew him as a graduate student, Veblen was trying hard to find a definition of geometry that would encompass all of his interests and would separate these interests from the rest of mathematics. He concluded that it was impossible to define geometry so as to include what he felt it ought to include and not include all of mathematics.

Aspray: Are there other areas of contribution you want to mention?

Tucker: There was always work going on in the area of—mathematics referred to as analysis. There was a turnover in the personnel in the Thirties. When I first came to Princeton in 1929 the Princeton analyst was Einar Hille, who had been around for several years then. Later on when I became a member of the faculty I had as fellow assistant-professors Bohnenblust and Bochner, both very fine analysts. Hille by this time had gone to Yale. But somehow or other the research that was going on in analysis—and there certainly was research going on—didn't catch the attention of others. The people who worked in that area were usually ones who had gotten their start in that area before they came to Princeton. So they didn't have an open mind when they were deciding on a topic for a thesis. I think we heard about that from Greenwood; he tried various other things, but went back to what he felt comfortable with.

Then there was in Wedderburn a development in algebra, but somehow or other Wedderburn was much more highly thought of outside Princeton than he was in Princeton. He is credited with being one of the founders of modern algebra, but I think he had only four or five Ph.D.s all together. And the students who took his courses in algebra did so because those courses were the only way to prepare for the algebra part of the general examinations. Every student on the general examination was examined in real variable, complex variable, algebra, and two optional topics.

To some extent I would say the same thing is true of analysis, that people took the analysis courses in large numbers, but relatively few of them continued in analysis for their research. So that compared with other universities, analysis didn't seem so important at Princeton. Mathematical physics was a very lively thing, but it was more in the Physics Department than in the Mathematics Department. Howard Percy Robertson was the principle one in mathematics, although his appointment was a joint one in the two departments. Eugene Wigner much more in physics. We did have two or three outstanding Ph.D.s in mathematical physics who happened, by the luck of the draw, to be enrolled in the Mathematics Department. John Bardeen is the outstanding example of that, but also the Englishman, Maurice Pryce.