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The Princeton Mathematics Community in the 1930s
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ALBERT TUCKER

AREAS OF MATHEMATICAL RESEARCH AT PRINCETON IN THE 1930s

This is an interview of Albert Tucker in his office at Princeton University on 11 July 1984. The interviewer is William Aspray.

Aspray: In this interview I thought we'd talk about the research areas at Princeton in the 1930s. I'll let you begin where you'd like.

Tucker: The principal area of activity was geometry, where this includes topology or analysis situs as it used to be called. Oswald Veblen and Luther Eisenhart were the principle senior members of the department at the beginning of the 1930s. Veblen had, of course, been around since 1905 and had already a distinguished career behind him. Eisenhart had been around even longer, since 1900. Eisenhart, though, had been involved very much in administration. I think that he became Dean of the Faculty early in the 1920s and continued in that office until 1933 when he became Dean of the Graduate School. So although Eisenhart was always interested and active in research—his field was differential geometry and various offshoots from differential geometry—he did most of his research during the summertime, when the Eisenharts went to their summer home in Greensboro, Vermont, right by the Canadian border.

I once visited the Eisenharts' summer place. I drove up there with Churchill [Eisenhart, son of Luther Eisenhart] one summer,and spent perhaps a week there. It was about 1937. The main house was one of these Vermont houses that went on and on in sheds. But set off a hundred yards from all this was a little house and that was off limits to everybody except the Dean. The sisters of Churchill told me that it was "Daddy's think house". That's I think where he wrote his books, such as *Riemannian Geometry* and *Continuous Groups*.

He always taught one graduate course and one undergraduate course each term, even during the year that he was acting president of the university, 1932-33. The graduate course would be taught from one of his books; one reason he wrote his books was to be able to use them for courses. This made the teaching of the course less exacting for himself—all he had to do was walk into the room. He usually began by asking if there were any questions; he had given a reading assignment and suggestions for the exercises. (His books always contained exercises.) From these courses that he taught he got converts who wanted to pursue the sort of thing he had written about.

Dean Eisenhart was very concerned about any student who started working on a thesis with him. I myself had an experience of this sort. During my first year at Princeton I ventured to suggest an improvement in his book *Riemannian Geometry*. He listened to what I told him and said, "Well, I think you should write that up for me." I wrote it up, and he then took that and looked over it and asked me to amplify certain parts and to make certain changes. This happened two or three times. As a matter of fact, he would have me meet him up in a room in the attic of Nassau Hall. He usually worked in his dean's office in Nassau Hall. But he also had a hideaway there where he could go if he wanted to get away from things, and he had me meet him there. I assume that was the sort of thing he did with thesis students.

Anyway, at the end of this process he said, "Mr. Tucker, I would like to submit this to the *Annals of mathematics* for publication." I was astounded, because all I thought I was doing was explaining something to him so that if he did a second edition of his *Riemannian Geometry* he could change the way in which he handled subspaces of intermediate dimension, something between the curve and the hypersurface. From this experience I think I know the way in which he worked with students: leading them on, in a mild way, until he felt that they had a reasonable body of material.

Now I don't know how Veblen worked with his students because I didn't have that experience. But considering the number of theses that he supervised, even after he was no longer paid by the University and was a professor at the Institute for Advanced Study, he certainly had an attraction for students. We have heard from A.L. Foster that although his thesis was in logic and with Church as advisor, nevertheless Veblen adopted him, and to some extent took over. But this is getting away from the research areas.

Veblen started out in axiomatic geometry. As a matter of fact, his Ph.D. thesis at Chicago with E.H. Moore was on a set of axioms for Euclidean geometry, a variation on the axioms of Hilbert. From that Veblen moved into projective geometry, which he worked on from about 1906 until about 1911.

Aspray: Building on any particular person's work?

Tucker: He was particularly impressed by the work of Felix Klein and by the *Erlanger Programm.* Indeed it wasn't just projective geometry. It was the various subgeometries that one could get from projective geometry by restricting the group of transformations. In this way you can get affine geometry, you can get Euclidean geometry, you can get non-Euclidean geometry, you can get conformal geometry, and you can even get the so-called finite geometries that have become popular in recent years but were actually studied a bit back then. Veblen wrote a 2-volume work on projective geometry. The first volume was done jointly with John Wesley Young, who was a young assistant professor here at Princeton somewhere around 1907. Later Young moved to Dartmouth where he was a leader in mathematics until his death many years later.

It was in the second volume of the projective geometry that Veblen came to grips with questions of orientation. Now in the Euclidean plane there is a very definite sense of orientation. You can talk about clockwise and counterclockwise. In 3-dimensional Euclidean space you can talk about a lefthanded system of coordinates and a righthanded system of coordinates. But in the projective plane there is not a sense of orientation, except locally. That's because you can just go off to infinity and come back with clockwise and counterclockwise interchanged. But in 3-dimensional projective-space you go off to infinity and a righthanded system of coordinates does not change into a lefthanded system of coordinates. I think it was this paradox of orientation that got him interested in topology. Topology was called analysis situs because Henri Poincare, the great French mathematician, had written a series of articles starting in 1895 on what he had described as analysis situs. It was this that turned Veblen's attention from projective geometry to analysis situs. With the aid of a student and protege, James W. Alexander, he gave his attention to analysis situs. This study culminated in colloquium lectures, which he gave in 1916 and which were published around 1920 as a volume in the Colloquium Lecture Series. But following publication of Einstein's general theory of relativity in 1916—though it wasn't widely known until after World War I—Veblen and Eisenhart became interested in differential geometry, especially Riemannian geometry and tensor analysis, because these were the mathematical tools that Einstein used in his general theory of relativity.

Aspray: Were these topics commonly studied in the United States and other places?

Tucker: No, not at all. Einstein got his information from the work of two Italians, Levi-Civita and Ricci, who had, back around the turn of the century, worked out something they referred to as the absolute calculus, or some term like that. It was something that they thought of as akin to the work of Grassmann, Hamilton, and others in the 19th century. In any event, this was a research area that was scarcely known anywhere. It was Einstein's general relativity that turned the spotlight on it. Veblen and Eisenhart took it up. With Veblen's experience with a variety of geometries and Eisenhart's previous work in differential geometry, they were ideally qualified to lead research in the geometry associated with general relativity.

Aspray: Do you think that this, at a later time, made Einstein more interested in coming to Princeton?

Tucker: Yes. Indeed, sometime around 1920 Einstein was invited to come to Princeton for a term or a year. Eisenhart, who was spending a summer in Europe, was given the duty of personally extending Princeton's invitation to Einstein. I happen to have seen in University files a letter from Eisenhart to Hibben, the President of the University, reporting on his visit to Einstein. I had the duty of writing up the faculty obituary for Eisenhart, and it was in gathering information for that that I ran across this letter. It indicated that the meeting with Einstein, who knew of the work of Eisenhart and Veblen, had been very cordial, but at that time, for I think domestic reasons, Einstein did not feel that he could accept the offer. But he did agree to make a visit and to give a series of lectures. During that same visit he stopped at Cal Tech.

Aspray: What year was that?

Tucker: About 1921. The lectures led to a volume published by the Princeton University Press called *The Meaning of Relativity*. I think that it was this kindred feeling that Einstein had for Veblen and Eisenhart that made him willing, in 1933, to come to Princeton to stay.

Aspray: I see.

Tucker: Veblen's influence accounts for several young mathematicians at Princeton going into this area of math. T.Y. Thomas, for example, who did his Ph.D. with Veblen in about 1923. He was appointed to the faculty of Princeton and stayed at Princeton until he left for UCLA in about 1938. There was also Joseph M. Thomas (no relation), who was here as a postdoctoral fellow about the same time. But Eisenhart also had an important influence in this respect, though less so after 1923 when he became Dean of the Faculty.

James Alexander had continued to work in analysis situs. Alexander became impressed in the early '20s with the work that was being done in an area between analysis situs and algebraic geometry by Solomon Lefschetz, who was at the University of Kansas. Alexander apparently urged Veblen, Eisenhart, and Fine to bring Lefschetz to Princeton as a visiting professor. I think it was the year 1923-1924 that he was a visiting professor. At the end of that year he was appointed associate professor, with tenure, and did not return to Kansas. From that point on it was Lefschetz who played the leading part in the development of analysis situs.

Aspray: I see.

Tucker: Veblen meanwhile was investigating various ramifications of the "relativistic geometry", together with people like T.Y. Thomas and Morris Knebelman. Knebelman got his Ph.D. in the late 1920s at Princeton in the "relativistic geometry". I have just invented that term at the moment to describe sort of geometry used in Einstein's general theory of relativity.

Aspray: What was it called at the time?

Tucker: Various terms were used. For example, there was a book, not very well known, written by Eisenhart called *Non-Riemannian Geometry*. This was a geometry in which you didn't have a metric, but you did have a sense of parallelism. Sometimes it was referred to as the geometry of paths, because you had the idea of a straight line not as the shortest distance—because you didn't have distance—but you had a path that could be characterized in such a way that through any two points that were close enough together there was a unique path joining them. There was something else called non-holonomic geometry. I have forgotten what that was exactly. Still later—this would be well into the '30s—Veblen worked with students such as J.W. Givens in something that was called spinors. Veblen also had a small monograph that was published in German on so-called projective relativity.

When I was a graduate student Veblen was working especially with Henry [J.H.C.] Whitehead, the British geometer and topologist who did his Ph.D. with Veblen. They did a Cambridge Tract on the foundations of geometry. This wasn't foundations of geometry in the Hilbert sense at all; it was an attempt to form a new synthesis of geometry which would define geometry in a 20th-century way corresponding to the *Erlanger Programm* of 1870 of Felix Klein. Veblen had by that time gained a great deal of experience in the forms of geometry that could not be studied by transformations. Analysis situs is sometimes defined as the geometry determined by the group of homomorphisms. They form a group, but this group doesn't have a useful structure. So that to approach topology in terms of groups is to be defeated before you start. Nor does differential geometry lend itself to characterization in terms of groups.

Aspray: Do you feel homeomorphisms don't form a useful group?

Tucker: No. Perhaps some subgroups of them do. But as soon as you go to that you're putting on additional structure. You are not sticking with the topological ideas.

So Veblen was trying to find a new synthesis which would include the projective geometries, topology, differential geometry, and anything else that should be called geometry. Well, he finally concluded that any definition that he could make that would include all these geometries would also include the rest of mathematics. So he decided that the best he could say was that something is a geometry if there are sufficiently many people who say it is. For many people this was a sort of a death knell for geometry. Nowadays geometry does not seem to be in the forefront of mathematical research the way it was in, say, 1930. Topology is still in the forefront, but it has split off to become a field in itself.

Aspray: Yes.

Tucker: Whereas when I was a graduate student, and in the years preceding that, geometry seemed to be in its heyday. That was probably somewhat peculiar to Princeton.

Aspray: Was there much geometry being done at Chicago and Harvard and Columbia?

Tucker: Yes, geometry was being done in those places, but the reason I came to Princeton was the large number of geometry graduate-courses listed in the catalog—five or six were listed. I found out after I got here that some of them weren't being given anymore—a graduate course in projective geometry for example. But it was the geometry courses which attracted me to Princeton. I was urged at the University of Toronto to go other places; they thought my idea of coming to Princeton was not a good one.

But I was stubborn. I did write off to get information about geometry courses at Cambridge (England), at Harvard, and at Chicago. I knew, at least from catalog descriptions, the sort of courses that were being taught at these other places, but those courses didn't attract me the way the courses at Princeton did. I know that I had nothing except naive intuition to go on. But looking back now I think I was completely right. There just was not the vitality in geometry anywhere else in the world at that particular time. At Cambridge, it was still 19th-century geometry. There was a man there by the name of Baker who wrote a multi-volume work on geometry, but it was pretty much from a 19th-century point of view—seeing the various sorts of geometry as lovely little edifices in themselves and not trying to get an overview. Even though Veblen failed to achieve what he set out to do, he certainly accomplished a tremendous gathering together of geometric ideas and encouraged young mathematicians to work in these areas. His enthusiasm on these matters was infectious. So there is no doubt that, in the period that we are interested in, the outstanding area of research was geometry.

Aspray: What about topology?

Tucker: Lefschetz was a dynamic person, but he wasn't easy to understand, he wasn't easy to do work with, and he wasn't consistent in the things that he did. There were all sorts of difficulties with him. But he was just full of ideas—many of them wrong—and they spilled over on other people, especially pre-doc and post-doc students. So that building on the foundations that had been laid by Veblen and Alexander, Lefschetz had really a school of topology from about 1928 on. And people came from various other places. Alexandroff came from Moscow in 1928, and Heinz Hopf came from Zurich. (Earlier Hopf had been at Goettingen.) And Max Newman came from England. From my correspondence with Angus Taylor following our interview I learned that the topologist Kerekjarto was here at Princeton about 1922 because of Veblen.

The topology of the combinatorial or algebraic sort, originally called combinatorial analysis situs, is without any doubt a Princeton development, that was begun by Veblen, aided by Alexander, and continued by Lefschetz. So I would say that topology was the leading research area in the 1930s at Princeton. I must immediately add that I was a member of that group and so can't be counted on for objective evaluation.

Aspray: I think that what you say, though, is true.

Tucker: Lefschetz had some very fine students, leaving myself out. First of all there was Paul A. Smith, who had been a student of Lefschetz's at Kansas and had followed him to Princeton. He did his Ph.D. wth Lefschetz, sometime around 1926, and then became a professor at Columbia University. In the 1930s seven or eight people completed their Ph.D.s with Lefschetz; the outstanding ones were Norman Steenrod, Ralph Fox, and John Tukey (who turned statistician). Others were Henry Wallman, Hugh Dowker, and Ed Begle. Actually one of Lefschetz' Ph.D.s in that period was in algebraic geometry. That was Robert J. Walker, who had a fine career at Cornell.

There were a number of post-doctoral people here in that period. We've heard from Leon Cohen about how he was sent here by [Raymond] Wilder from Michigan to check up on the Princeton topology. I might mention some of the other National Research Council fellows who were around at that time to work with Lefschetz and Alexander. There was Adrian Albert from the University of Chicago; of course he came to work with Lefschetz in algebraic geometry. S.F. Barber, Theodore Bennett, Leonard Blumenthal, Arthur Brown, Leon Cohen, Donald Flanders, who was later at New York University, G.A. Hedlund at Yale, Daniel C. Lewis, and Deane Montgomery. Sumner Myers came from Harvard and then went to Michigan. Selby Robinson, Hassler Whitney, and Jacob Yerushalmy, an Israeli, and Leo Zippin. So it was just as much at the post-doctoral level as the pre-doctoral level that there was a school of topology here.

Aspray: Can you trace the dissemination of this Princeton style of topology out to other universities in the U.S.?

Tucker: That's a tall order.

Aspray: Oh, I'm not asking you to document it; I'm just wondering if were aware of this kind of a spread.

Tucker: Oh yes. Shortly before he died Norman Steenrod was trying to do a citation index for topology. I don't think he got far enough that it went on after his death. But the combinatorial, algebraic type of topology spread out from Princeton, not only in the United States but to other parts of the world. This is quite distinct from the research in point-set topology, which was centered partly at the University of Texas with R.L. Moore and his students, and partly in Poland, with Sierpinski, Mazurkevich, and Kuratowski. The Poles were willing, though, to use combinatorial methods, so there are things such as work with dimension theory that are between the Princeton school and the Texas school. And the Russians have followed the Alexandroff tradition that was built on the Princeton tradition. The Russians wouldn't admit this of course, but it is a fact that Alexandroff spent ~~a~~ formative year here at Princeton. It was out of meeting at that time with Hopf, who was also here, that the famous topology textbook of Alexandroff-Hopf was developed.

Aspray: You haven't said much yet about Alexander.

Tucker: No. Alexander was a perfectionist in his mathematics. He published very little, and he had very few students. James Singer was one who did his Ph.D. with Alexander; it was on 3-dimensional manifolds. I think he got his Ph.D. in 1931.

Alexander always seemed to be almost what one would describe as a dilettante in his attitudes about mathematics. He had independent means, and he would never accept a full salary. He accepted some partial salary later on when he became professor at the Institute. But after a few years he resigned that professorship and simply took the title of permanent member. He didn't need the salary, and he didn't want to feel that he had the responsibilities. Particularly in the '30s he would start in the fall, with great enthusiasm, to give a seminar on some idea that he had. He would start out with a room full and give a very interesting talk, and then continue it a week later. After three or four of these, a notice would go up saying Professor Alexander would not meet his seminar that week. Then there would be two or three intermittant meetings, and then a notice would go up that Professor Alexander would not meet his seminar until further notice. There would never be further notice. In developing the idea he started out with, he would run into difficulties, and if he couldn't resolve a difficulty in a reasonably elegant way, he would give up for the time being.

There was always a special topology seminar that met every week where talks were given by visitors, by post-docs, or by graduate students. I remember that at meetings, after the talk, Alexander would always ask questions. Say the speaker was a young post-doc presenting his doctoral thesis. Alexander's typical question would be, "Well now, how do you apply what you have done to such and such?" The young man would say that he hadn't thought about that. Then Alexander would say, sort of half talking to himself, "Well, I think I got just about as far as you did one time, but I felt that unless I could settle this question that it wasn't worthwhile." A devastating comment, though Alexander certainly didn't mean it that way. Alexander was not being derogatory. He had become excited by the talk and recalled that he had had an interest in this sort of thing. He was trying to recall why it was that he had been interested in it, and why it was that he hadn't followed it through. But to the young topologist it often seemed to be damning criticism.

Because of this perfectionist attitude that Alexander had and also his feeling that he didn't want to undertake responsibilities—really, his only reason for being a professor of mathematics at Princeton or the Institute for Advanced Study was for his own edification—all of this meant that he didn't have very much influence on students. In this list, which runs to three pages, of National Research Council Fellows who were at Princeton in that period, we can see after many names "under the direction of S. Lefschetz and J.W. Alexander". They were often put together that way. That's the case, for example, with Leon Cohen, but you will remember that Leon Cohen had a rather special relationship with Alexander because he went one summer with Alexander to France and then they did some mountain climbing together. So he was more a social companion than as a mathematical companion. Another person who had very close contacts with Alexander was Henry Wallman, but their bond was radio: they were both addicts to radio and hi-fi and this sort of thing. This did extend into mathematics, but the real bond was not the mathematics, but the other things. With Lefschetz, on the on the other hand, a personal bond was based on the mathematics. In fact, Lefschetz had practically no social contacts except to be the center of a group of young mathematicians at a beer party or a tea party or the like.

Aspray: Was this attitude that Alexander took towards his mathematics and his mathematical career a factor in his appointment at the Institute?

Tucker: It is a puzzle how he came to be chosen as one of the Institute professors. Of course, he was a student of Veblen. And Alexander had a considerable amount of social prestige. He was someone who could fit in beautifully at a diplomatic reception, to take an extreme case. He was completely fluent in French, and his wife was a White Russian, so he was someone you could describe as a man of the world in the higher strata of society. And there was absolutely no doubt about the quality of his mathematics. The quantity was very small, but some of the fundamental papers in the early days of topology had been written by Alexander or by Veblen and Alexander jointly.

Aspray: Before we turn to another research area, I thought maybe we could get a statistical overview: the number of Ph.D.s that were produced in the late '20s and in the '30s in this area of geometry and topology?

Tucker: Well, there were relatively few Ph.D.s in mathematics at Princeton until starting about 1930. In the 1920s there were 13. These were Henry Brahana, who did his work with Alexander in topology; Edward Hammond, whose field I don't know; Philip Franklin, Norbert Wiener's son-in-law, who was later at MIT and who did topology with Alexander. Then in 1923 there were four: William Cleland, R.E. Gleason, George Raynor—I don't know about them—and Tracy Thomas, who was with Veblen. Then in 1924 there was Harold Hotelling, who did his Ph.D. with Alexander, and Harry Levy, who worked also, I think, with Alexander or Veblen. In 1926 Paul Smith completed his Ph.D. with Lefschetz; in 1927 Alonzo Church with Veblen in logic; in 1928 Carland Briggs with Alexander in knot theory, and Morris Knebelman with Veblen and Eisenhart in tensor analysis. In 1929, no one. And then you come to the 1930s, and in the 1930s 30 Ph.D.s were awarded in mathematics. Of that number 20 were in geometry, including topology. That is, two-thirds of the Ph.D.s in the 1930s were in geometry or topology.

Aspray: Do you want to break them down for me by the person who directed them?

Tucker: Sure. Veblen directed five of them, I think. This was in the type of geometry that I referred to earlier as relativistic. Seven were with Lefschetz; they were all in topology except one, Robert Walker's in algebraic geometry. Four were with Eisenhart in some form of differential or Riemannian geometry, and three with T.Y. Thomas were of a similar type. Then in other fields, it's just a scattering. In analysis there were three with [Einar] Hille, three with Bohnenblust, and I think there must have been one or two with Salomon Bochner. There was one with Alexander that I should have mentioned with the topological ones: the James Singer thesis I that spoke about earlier. There were three with Alonzo Church. A fourth one that was really with Alonzo Church got credited to Veblen; that was the Foster thesis. In mathematical physics there was one with Eugene Wigner, one with Howard P. Robertson, and one with [E.U.] Condon I think. Then there was the thesis of Israel Halperin with von Neumann, which was in continuous geometry , a rather special field that von Neumann developed. The first thesis done for S.S. Wilks was in this period, done by Joe Daly. I think that we have gone through the list.

Aspray: You said that there were a total of 30. I count 35 or so, so it's 40 not 30. And I think when we add all the topology ones, it comes out slightly higher than 20.

Tucker: Geometry and topology, yes. Now let's turn to the area of analysis. I am not able to speak about this area as directly as I might, because it hasn't ever been my particular field of interest. When I came to Princeton in 1929, the leader in analysis was Einar Hille, and he had already been here for four or five years. Although he was born in New York City, his parents were Swedish and he was educated in Stockholm in the Mittag-Leffler tradition. He played an important role, I feel, around 1930; he had several doctoral and post-doctoral students working with him. The one I knew best was H.F. Bohnenblust. Hille also served as the editor of the *Annals of Mathematics* along with Lefschetz, but Hille at first took the primary role. Before that, the editor for many years had been Wedderburn, but in the late '20s Wedderburn withdrew from many things, and Hille took over the active editorship of the *Annals of Mathematics*.

I had a course from Hille, and there was a very active group that seemed to work with him. Hille left in 1933 to go to Yale. I don't know, but I think that the starting up of the Institute for Advanced Study made him feel that perhaps he would be better in a mathematics department away from Princeton. I guess he had a good impression of me, because after he went to Yale he got Yale to make me an offer to go there as an assistant professor. And it was this offer from Yale that got me promoted to assistant professor at Princeton.

Bohnenblust became an instructor at Princeton in 1932. He got his Ph.D. a year or two before that with Hille. He was soon promoted to assistant professor. The void that was created when Hille left was filled by Bohnenblust and by Bochner, who was appointed in 1934. Graduate students generally regarded Bohnenblust as the best teacher in the department, and his courses in real and complex analysis were considered must courses for any graduate student. They were regarded as the best preparation for the prelims. Bochner and Bohnenblust teamed together to give these courses. I think Bohnenblust's preference was real analysis, but he also taught complex analysis.

Aspray: What were their areas of research within analysis?

Tucker: Well, various. I think that at that time Bochner was working more in Fourier analysis and Bohnenblust on various matters of series and convergence. My poor answer to your question reflects my lack of interest in analysis.

Aspray: Can you say something about Princeton in comparison with other centers of research in analysis?

Tucker: I think that Harvard and MIT were regarded as much stronger in analysis than Princeton. The Harvard/MIT combination was especially strong with Norbert Wiener, Walsh, Widder, and Marshall Stone. To a certain extent G.D. Birkhoff was an analyst. And there was W.F. Osgoode. Princeton was comparatively weak in analysis at that particular time.

Aspray: What about the Ph.D.s produced in analysis?

Tucker: I suppose that out of the 40 Ph.D.s in that period of the 1930s, there were only four or five that were in analysis.

Aspray: Are there any people in that group who stand out in your mind?

Tucker: Well, no, I guess not. I'll change that. There were John Barnes of UCLA and Robert Greenwood of the University of Texas.

Two other fields were algebra and logic. Wedderburn is regarded I think as one of the founders of modern algebra. He wrote rather in the 19th-century style, but he is credited with much of the American development in modern algebra. His outstanding student—he had only three or four Ph.D.s—was Nathan Jacobson. But there were people who came to work with Wedderburn and seemed to get a great deal of support from him. One of these was MacDuffee, Cyrus Colton MacDuffee. Another was Adrian Albert from the University of Chicago. I don't remember that there was ever a weekly seminar in algebra. I think that all of Wedderburn's influence was in the consultations that people had with him in his office. He was not at all gregarious, but was rather aloof, almost a recluse at the time I knew him. I think earlier on he had been somewhat more gregarious, but he had some sort of illness in the late '20s, and from that time until his death around 1946 he was really pretty much a recluse. He was a bachelor and a Scotsman, and had no living relatives in the United States. I remember that when he died there was quite a problem about handling his funeral and estate, because there were no relatives to take charge.

Aspray: Emmy Noether?

Tucker: Emmy Noether was around for the very brief time which she remained alive after she came to the United States from Goettingen. Though she was not at Princeton—she was at Bryn Mawr—she came over at least once a week by train to participate in seminars. And of course Hermann Weyl was probably more interested in algebra in the '30s than he was in other parts of mathematics. He was a mathematical universalist, but at that particular time he was working in algebra and in groups. He had very distinguished people working with him, such as Richard Brauer and his brother Alfred Brauer. There were always people here who were interested in algebra and who managed to get together, but it was rather unobtrusive.

Aspray: I see. How would you compare this with other centers in the U.S. and Britain and Germany?

Tucker: In the United States I think of Chicago, where the Dickson School was very strong in terms of graduate students and such. In the early '30s the best work in algebra was going on in Germany, particularly at Goettingen just before Hitler took over. That of course was broken up. There certainly were graduate students at Princeton in the '30s who in preparing for their general examinations were told to read van der Waerden rather than to read Wedderburn. The Wedderburn-MacDuffee-Jacobson stream has not been entirely separate from the Noether-Artin-van der Waerden stream, but it nevertheless has been distinct and much less known and studied.

The field of logic was quite active at Princeton in the 1930s. This was due initially to the fact that Veblen was interested in logic from the point of view of axiomatics. He encouraged Alonzo Church as an undergraduate at Princeton, and then as a graduate student, to follow his interests in logic. After Church got his Ph.D. at Princeton in the mid-Twenties, he had a year of post-doctoral study, partly at Amsterdam and partly at Goettingen. I don't know whether he was in contact with [L.E.J.] Brouwer at Amsterdam. Do you remember from your interview of him?

Aspray: I believe he said that he had some contact.

Tucker: I think that Church was very much his own man and developed things pretty much from scratch. Was it [Paul] Bernays who was later at Princeton, after the Institute got started? Anyway, Church by 1930 was a recognized logician and started having students, the first of whom was A.L. Foster. Of course in the mid-30s Goedel came to Princeton. There wasn't much interplay between Goedel and Church, but I think that many of the people who came to the University or the Institute thinking that they would work with Goedel ended up working with Church because he was so much easier to work with. Goedel was very shy. Church was no hail-fellow-well-met, but if you asked Church a question, you got an answer. If a student or someone doing post-doctoral work went to Church and asked, "Do you think such and such is a good thing to work on?", Church would immediately give an opinion and probably some references and suggestions. The result was that over the years Church and another man—Bochner—were the ones who supervised the most Ph.D. theses.

So the group in logic, although it was small and not especially flamboyant, was a very effective group. The two outstanding students that Church had in the mid-30s were J.B. Rosser and Stephen Kleene; Alan Turing came later. They carried on the Church tradition, Rosser at Cornell and Kleene at Wisconsin. Rosser got involved in other things such as ballistics and computing—computing in the sense of numerical analysis—but Kleene stuck very much to logic, especially the logic of recursive functions. Church, besides working with students, pre-doctoral and post-doctoral, served as the principal editor of the *Journal of Symbolic Logic*. I think that Church did a great deal of the refereeing himself, in addition to doing the editing. And for papers where people weren't quite sure whether the propositions had really been proved, Church was the final authority. So I would say that in the Princeton logic community, though Goedel was the star because of his tremendous research-accomplishments, it was Church who carried the freight.

Aspray: I assume that Veblen did not continue to do any work in logic?

Tucker: No, none at all. As far as I remember there was no one else doing logic at Princeton, other than the students, Goedel, and an occasional visitor such as Bernays. Actually Brouwer was around for a few weeks. I remember meeting him and being much impressed by him. Although he was about 80 years of age, he still looked his height of about 6'3". And his erect posture was remarkable. I remember a lecture that he gave in the large lecture room of Fine Hall. It was called "On the fixed-point theorem". Actually the title of the lecture might have been "Why the Brouwer Fixed-point Theorem is False". Brouwer had published the fixed-point theorem back around 1911, and this was before he had become interested in intuitionism. He told me that his interest in intuitionism developed during World War I when he was in hiding from the Germans who had overrun Holland. He lived in a attic room, or something like this, and never went out for many months. Food was simply passed into him. He was nonexistent. During this period he used his time to think about the foundations of mathematics.

Aspray: He seemed, though, to be inclined toward intuitionism much earlier than that. His doctoral dissertation in 1907 has many ideas, though perhaps not well formulated, going in that direction.

Tucker: Yes. But his famous fixed-point theorem certainly makes use of a limiting process that is non-countable. So the lecture he gave was essentially giving another theorem, which said given any large N, you could find, for any continuous mapping of a disc into itself, a point that moved less than 1/N. And so it was all the same except the last step of the theorem. I remember going in with him to New York. We were going different places, but I gave him very careful instructions. We got off at a certain subway stop, and I told him how he was to get to the train that he was to take. The last thing I saw he was getting on the wrong train. I never saw him again.

Getting back to Church, I have never seen another mathematician so completely devoted to his subject and his students. He was in correspondence with many, many people, mainly as a result of the *Journal of Symbolic Logic*. As far as I could tell, he was a nerve center of mathematical logic.

Aspray: I see. What role, if any, did von Neumann play when he appeared on the scene?

Tucker: I have heard that as soon as he heard the result of Goedel he said that he was not going to work anymore in logic. He had been trying to follow up the Hilbert program, and this was completely undermined by Goedel. He did, however, work on the theory of automata—a connection between logic and the theory of computing machines—but that was for actually building computers. Of course he is credited with having developed the first internal-programming for a computer. No, von Neumann seemed to be working in the '30s, as far as I could tell, in quantum theory and operator theory.

Aspray: Lie algebras.

Tucker: Yes. So I doubt that he had any impact at all on logic.

Aspray: Shall we turn to statistics?

Tucker: Yes. Statistics got started very slowly in the 1930s. Wilks came as an instructor in 1933. Before that there had not been any interest that I know of in statistics within the mathematics department. This is not something that comes out of the Veblen tradition, but Eisenhart does have a hand in it because it was Eisenhart's doing that brought Wilks to Princeton. I remember this from being a colleague of Sam's, of about the same age—indeed we were within a few months of one another in age. I saw that he found very little sympathy for his field with the other members of the department. H.P. Robertson, because of statistical aspects of mathematical physics, was sympathetic, but the main support came from Eisenhart. Now I am not at all sure what made Eisenhart interested in statistics. Of course his son Churchill became a statistician, and I guess Churchill was an undergraduate about the time that Wilks came. So I think that it was not Churchill who created his father's interest in statistics; perhaps it was the other way around.

I think that Eisenhart was most interested in what he regarded as the general well-being of the University. Up until the time that Wilks came, he had been the Dean of the Faculty, and he had encouraged the development of statistics within the economics department. I think he came to feel that really to get statistics into the Princeton picture he needed to give some support to it from within the mathematics department. He was the chairman of the mathematics department. For the most part he didn't throw his weight around, but I think that he did in bringing Wilks here. Some sort of a treaty was made with the economics department: Professor Duncan was to do the teaching of statistics, and the mathematics department was not to compete for this.

So Wilks was here for about three years before he had a chance to teach any statistics. He really served as another one of the staff in analysis. But he finally did get going with statistics courses—at the upperclass level and at the graduate level. And the first of his Ph.D.s, Joseph Daly, completed a thesis in about 1939 with Wilks. There also was the Eisenhart influence, because it's my recollection that Daly came to Princeton with the idea of doing a thesis with Eisenhart in differential geometry. I don't know whether you were able to find out anything like that from Joe when you interviewed him.

Aspray: I think he mentioned that.

Tucker: I might give an indication of the problems that Sam Wilks had. Lefschetz, who was the research professor in the department and who represented the department on the university research committee, would never agree to using any of the math-research funds for research in statistics. This money was used to provide research assistance (thus also supporting graduate students) and to bring visitors. It was Lefschetz's feeling, which he maintained throughout his active days in the department, that statistics was not a proper part of mathematics. Now Dean Eisenhart did not feel that way. I myself did not feel that way, even though I was very close to Lefschetz. He knew that I disagreed with him, and that was that. I gave all the moral support I could to Wilks, because I did feel that to develop a strong program in mathematical statistics at Princeton in the mathematics department was an important thing to do.

Aspray: Wilks went where for his research money?

Tucker: He would get some from other sources. Bell Labs at that time was quite interested in mathematical statistics; this was in the early days of quality control. The man who was the well known mathematician at Bell Labs—I can't think of his name right now [Thornton Fry. A.T.]—kept in close contact with Princeton and had a great deal of interest in probability. Indeed when I was a graduate student he gave, at the expense of Bell Labs, a course in probability. He directed the course toward the theory of real variables, but it was of course in probability as well as real analysis. So Bell Labs was one source of funds. Another was the General Education Board. This was before the days of government funds. After World War II Wilks had a large project from the Office of Naval Research, and he used that project to support graduate students and visitors.

There was a core of very good students, graduate students, who worked with Wilks. They came as graduate students in mathematics, but they ended up doing theses in statistics with Wilks. An outstanding example is Fred Mosteller.

Starting in 1939, Sam Wilks had a steady run of graduate students doing their Ph.D.s with him in statistics. First was Joseph Daly in 1939, and then in 1940 there were George Brown and Alexander Mood, who hit it off very well with Sam because Wilks and Mood were both from Texas. Then in 1944, Will Dixon and then his two outstanding students in 1945 T.W. Anderson, the leading statistician at Stanford, and Fred Mosteller, the leading statistician at Harvard. Both Anderson and Mosteller would have got their doctorates earlier if they had not been involved in wartime projects with Sam. He ran two or three wartime projects, and by 1945 when Anderson and Mosteller took their Ph.D.s, mathematical statistics was completely established at Princeton. That, by the way, was the year that Dean Eisenhart retired, so he had the pleasure of seeing his efforts bear fruit.

Aspray: Is it correct that Princeton with Wilks and Berkeley with [Jerzy] Neyman were the two important centers of mathematical statistics in the U.S.?

Tucker: One should add to that the group at Columbia, with Abraham Wald and Harold Hotelling. That group dissipated because Wald died and Hotelling moved to North Carolina. Of course the Berkeley group has continued strong, as has the Princeton group, although I feel that the Princeton group in statistics was strongest in the 1940s. The groundwork for that, of course, lay in the 1930s.

I guess we now have covered the main areas of math research active in the 1930s, with the exception of mathematical physics. And I think that we ought to consider that separetely, because it was much influenced by developments at the Institute for Advanced Study with the coming of Einstein and the people associated with Einstein.