

An Interview with

ALBERT W. TUCKER

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Conducted by William Aspray

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Abstract

Tucker, a professor of mathematics at Princeton University from the 1930s until the 1970s, describes mathematics in Princeton in the late 1930s and during the years of the second world war. Topics include: personnel and personnel changes at the Princeton University Mathematics Department and the Institute for Advanced Study School of Mathematics; Solomon Lefschetz as the leader of the mathematical research program in the university; the later careers of James Alexander and Oswald Veblen; active mathematical research areas in Princeton; the move of the Institute for Advanced Study to Fuld Hall in 1939; the impact of the physical separation of the Institute and the University mathematics programs; teaching of mathematics to military personnel during the war; and mathematical contributions to the Fire Control Project located in Princeton during the war and to war-time calculation. Tucker also recounts his own career during this period, including his research, his war-time teaching of mathematics and use of the slide rule, and his suggestions for improvements in calculating aids for the military.

ALBERT W. TUCKER INTERVIEW

DATE: 8 May 1986

INTERVIEWER: William Aspray

LOCATION: Princeton, NJ

ASPRAY: This is an interview with Albert Tucker on the 8th of May 1986 in Princeton, New Jersey. The interviewer is William Aspray of the Babbage Institute. Let's begin by summarizing the state of mathematics in Princeton in the late 1930s before the war began. First, looking at the University and then the Institute; although there's still, I suppose, some close relationship between them until the Institute moves to its new home. I'll let you begin wherever you'd like.

TUCKER: Yes. At the end of the academic year 1937-38, two members of the Department of Mathematics resigned to go elsewhere. These were T.Y. Thomas, who had been at Princeton since the early 1920s, when he had been a graduate student working with Veblen and Eisenhart. He left Princeton to take a professorship at the University of California at Los Angeles.

ASPRAY: The reason for his leaving, as far as you can tell?

TUCKER: I think that Thomas felt that he was not regarded as the kingpin that he felt he should be. Lefschetz, the Fine Professor, the research professor, did not particularly like Thomas. So I think Thomas had chips on his shoulder. Also, I think that chairman Hedrick at UCLA predicted that if Thomas went to UCLA, in particular went somewhere other than Princeton, that he would soon be elected to the National Academy of Sciences. Whereas at Princeton, there were already members of the department who were members of the National Academy of Sciences, and it was not too likely that someone else from the department would be elected in the near future. Incidentally, this prediction by Professor Hedrick was actually born out by subsequent events.

ASPRAY: Was he treated with the kind of honors that he had hoped to get by moving to UCLA?

TUCKER: Yes. He became the star of the mathematics department at UCLA. Also, Thomas was rather abrasive as a colleague. No one here liked him too well and he was aware of that. So, I think there was a general sigh of relief when Thomas left, although he definitely seemed like a heavy loss. The other resignation was Morris Knebelman, who had been around from about 1928 when he had taken his Princeton Ph.D. with Eisenhart. Before he ever came to Princeton, he had taught at Lehigh and had been concerned with engineering courses. He was given the assignment in the mathematics department of teaching the engineering mechanics that was taught to certain junior engineers. He had done a very good job on the teaching side, but he had not taught graduate courses or contributed to the graduate work. He left to be chairman of the department of mathematics at Washington State University in Washington.

ASPRAY: He also had not done research. Is that correct?

TUCKER: Oh, he had published an occasional paper. He was really a very good mathematician, but not quite in the league with Thomas and others. Thomas was a research loss, Knebelman was not. It did create a problem for the department, though, in getting the engineering mechanics courses taken care of. H.P. Robertson, much to his unhappiness, was put to doing some of that work. Other people helped out. I even took a turn at teaching the engineering mechanics somewhat later on. So Knebelman was missed very much from that point of view.

ASPRAY: All right.

TUCKER: In 1939, Professor William Gillespie, who was the oldest member of the department, retired. He had come, I think in 1897, after getting a Ph.D. at the University of Chicago. He had been at Princeton for 42 years. Oh, such a fine figure of a man: over six feet tall; his hair had become grey, but he was certainly, by far, the most imposing person in the department. He also had been for umpteen years the master in residence of the graduate college. He was a bachelor like Wedderburn. He handled very beautifully the duties of master in residence, which were not heavy, but involved such things as saying grace at dinner, adjudicating any quarrels that might arise among graduate

students, or disciplinary problems. There always were problems at the Graduate College about women visitors. There was a very definite rule that there were no women visitors allowed after dark. Incidentally as a sort of joke, I tell you that on one occasion there was a notice posted up, over the signature of Dean Eisenhart, who then was dean of the Graduate College -- that no woman was to be seen leaving the Graduate College after dark. The graduate students took this to mean that any woman who was there after dark had to remain. Well, Gillespie handled his duties very well. He also taught the freshman course in mathematics for students who were not going to major in engineering or the sciences: the easier course that started with algebra and trigonometry, and went on from there.

ASPRAY: Were there other retirements in the late 1930s?

TUCKER: There were no other retirements, except I should add that Wedderburn's health became less good. Although he did not retire until 1945, he was for most purposes no longer an effective member of the department. He would teach his graduate course in the theory of matrices every year. Graduate students were always told that that was the course that they should take. But really, his style of lecturing was dull. He didn't read from his book, but he might just as well have read from it. And he could not tolerate questions of any sort. He did have one or two Ph.D. students... Ernst Snapper is one that he had right at the end of the 1930s and beginning of the 1940s --also Merrill Flood and George Garrison. The department was very active at that time: Bohnenblust and Bochner in analysis. Dean Eisenhart was still quite active in thesis supervision in the area of differential geometry. Then Lefschetz, of course, in topology. And a new appointment, I guess it was in 1939, Claude Chevalley bolstered the situation in algebra that was withering because of Wedderburn's age. Chevalley of course, being an original member of Bourbaki, had a rather radical point of view that interested many students. There still was a great deal of influence on the work in Fine Hall from people at the Institute, especially Weyl and von Neumann and Marston Morse -- although Marston Morse was more inclined to gather a small group around him, mainly young post-doctoral people, than deal very much with the graduate students in Fine Hall. Also, Robertson was quite active in the department. Wigner had been appointed in 1938 to the Jones chair, which had really not been occupied since the one year that Hermann Weyl had had it in 1928-29. But Wigner, who had been away for about two years at the University of Wisconsin, returned as the Jones Professor, which had an office reserved in Fine Hall for. During this interim when there had been no Jones

professor, the office had been occupied by Einstein. Wigner, who has always regarded himself as a theoretical physicist rather than a mathematical physicist (and indeed, in recent years has not occupied an office in [the new] Fine Hall), was quite happy to occupy an office in the original Fine Hall because it had been Einstein's office. Of course, Einstein also regarded himself as a theoretical physicist rather than as a mathematical physicist. The world, perhaps, is inclined to speak about him as a mathematician, but he never thought of himself in those terms.

ASPRAY: The heart of the graduate program, many people have mentioned to me, was in the hands of Lefschetz in some ways. Could you elaborate on that?

TUCKER: Well, Lefschetz, as the research professor, the Fine Professor, as had been Veblen before, regarded as his responsibility the graduate program and the research program in the department. It has always been the attitude that the graduate program and the research program were inseparable, that the theses were written as part of the research program, either independently or with some member of the faculty who was doing research in the area. So, Lefschetz was the drill master, the mother superior, the "great white father" (Tompkins), whatever you want to call him, of the graduate students. He tried at all times to have some impression as to how each student was doing. If he found that there was somebody that he didn't have a tab on, then he immediately set out to get that. He did this by talking to the students and did this by talking to other members of the faculty about students. He formed his impressions very quickly, probably much too quickly. I don't think that there was ever a student that he thought highly of that didn't deserve that. But I think that there were students of whom he thought badly that did not deserve it. But anyway, he really ran the graduate program and, indirectly, the research program. At the same time, of course, he was the editor of the Annals of Mathematics. Von Neumann was also an editor, but von Neumann did not give the Annals the attention that Lefschetz did. Von Neumann, essentially, only did the specific things that he was asked to do. Or if a paper was sent to him by someone, I think he felt that meant he should handle it. Towards the end of the 1930s, I think it would probably be about 1938, Bohnenblust was also made an editor of the Annals. For many years before that Bohnenblust had been doing much of the work in the editing -- after manuscripts were accepted: deciding what should go in a certain issue, sending the manuscripts off to the printer, looking after the proof sheets, and all this business -- which nowadays is done by some able secretary, but then it was done by a junior member of the

department. Indeed, as I said before, I actually participated to some extent in the editing, in handling manuscripts until they were accepted or rejected. So, through the Annals, through being in charge of the graduate students, through being the Fine Professor (and that carried membership on the university research committee, which decided on the use of the research funds), in all of these ways, Lefschetz was supervising and superintending the graduate and research program.

ASPRAY: How would you judge the overall results to be?

TUCKER: Excellent. It was great. But at the same time, it wasn't exactly democratic. If Lefschetz had been a despot (as Thomas might have been, given the chance), things could have gone quite badly. But Lefschetz's intuition and judgement of people and mathematical ideas were stupendous. There were mistakes, and some of those mistakes I myself feel badly about, but the overall results were extremely good. Indeed, it was Lefschetz who kept up the momentum in the department, that kept the departures to the Institute for Advanced Study from damaging the department.

ASPRAY: When you mentioned the people who were especially active at the Institute, you mentioned neither Veblen nor Alexander. What was their productivity like in the late 1930s?

TUCKER: Alexander was working on some ideas of his own -- things that were called "gratings". Essentially, he was trying to build a system of combinatorial topology that would fit into the rectangular structure of coordinates -- a Cartesian board, rather than the triangular or simplicial structure that the subject had taken on from the time of L.E.J. Brouwer. Brouwer had shown that any continuous compact mapping could be approximated as closely as desired by a simplicial mapping. It was this simplicial structure of topology that was the main point of Lefschetz's work in topology. So that the standard object that I and other topologists of my time worked with was the so-called "simplicial complex", a sort of a generalization of an arbitrary polyhedron with triangular faces.

ASPRAY: Right.

TUCKER: Because any other polyhedron can be broken up into triangles. But a triangular structure does not coordinatize nicely. There are barycentric coordinates and so on that will do it, but these are very clumsy compared with Cartesian coordinates, particularly if you are going to use combinatorial structure to get at results in analytic things. Manifolds that are defined by systems of equations (at least locally) do not lend themselves conveniently to this triangular structure. So there was a certain dichotomy between the structure that topologists found it convenient to use in their topology and the structure that the analysts want in order to get results in analysis.

TAPE 1/SIDE 2

TUCKER: Jimmy Alexander was trying to develop something that would do what the simplicial structure did and at the same time would fit with what an analyst needed. He didn't succeed in carrying this to a successful conclusion.

ASPRAY: At all?

TUCKER: Well, he would get a certain distance and he would become unhappy, discontented, with what he had and put it aside. Then he would come back to it and make another attempt. He also became a bit of a recluse. I think this is the word, isn't it, agoraphobic, people who are made nervous by being in a crowd of people.

ASPRAY: Agoraphobic?

TUCKER: Agoraphobic. Yes, that's right, agoraphobic. Indeed, I remember when Lefschetz was retiring in 1953. I called Alexander to ask whether there was any way we could arrange to have him come to the retirement dinner for Lefschetz. He told me that he would consult with his doctor and that he would very much like to come. He felt great admiration and affection for Lefschetz. But that he was not sure whether he could. He called back. I did not take the call, but he called back and left word that the doctor said "no". Now I don't know anything more about it than that, but I think that this phobia was beginning in the late 1930s. When a seminar taught by Alexander would be

announced, everybody would be there. But I don't think he worked with students, either University or Institute. Leo Zippen may be an exception to this. Also Henry Wallman. This is something that could be checked by consulting with Deane Montgomery.

Veblen was also into a number of things, extensions of differential geometry. He actually wrote a book in German, Projective Relativity. Then he worked on things that were called "spinors". But none of these things that he was doing then have been followed up, at least in mathematics. They may very well have been absorbed into physical theories that I don't know about.

ASPRAY: What about Veblen's role as an active, if informal administrator at the Institute?

TUCKER: Oh, he was the head of the School of Mathematics; not in name, but in fact. He was the one who had the principal say in the people that would be accepted each year as visitors, and those to receive stipends of some sort. He was constantly being consulted on matters of mathematical politics; still very active in mathematical organizations, especially the International Congress; and still working on the placement of refugees. But he was not exerting a strong influence on the mathematical research that was going on.

ASPRAY: Training students at all?

TUCKER: I do not think so. J.W. Givens and J.L. Vanderslice worked with him in the mid-1930s. But after, say, 1936 or 1937, I do not think that there were students who worked with him in the same sense that, say, Givens had worked with him.

ASPRAY: Rounding out questions of personnel in the late 1930s, there were some appointments to young faculty members, weren't there?

TUCKER: Well, the Fine Instructorships were set up in the late 1930s. But there were also excellent young men as

instructors, such as A.P. Morse and C.B. Tompkins. The Fine Instructorships were supposed to match the Pierce instructorships that had been at Harvard for a long time. Harvard usually had one or two Pierce instructors each year. They usually stayed on for a second year, occasionally for a third year, and were appointed a year at a time. These appointments, because they had a name attached to them, were regarded as rather fancy. They involved half-time teaching, as compared with a regular instructor. It would be, say, six hours a week instead of twelve hours a week. Usually of the six hours, one would be an advanced course of some kind. We set that up along about 1938 or 1939. Also there were visiting lecturers such as J.L. Synge and H.S. Vandiver. The first holder of a Fine instructorship was Steenrod. J.W. Tukey was another early holder of a Fine instructorship. Brockway McMillan was another. Brockway McMillan was an MIT Ph.D. of Norbert Wiener. He was extremely promising and ended up at Bell Telephone Laboratories, where he became one of the principal leaders and administrators, as far as mathematics and applied mathematics were concerned. Then there was a man from Columbia -- Strodt. He had been a student of J.F. Ritt at Columbia and had seemed very promising. These were all people who would ordinarily be called assistant professors, but were called Fine Instructors. Half salary was paid from instructional funds and half salary was paid from the research fund. These definitely strengthened the research side of the department. The first few appointments were extremely good, e.g. Steenrod. Another acquisition at that time was Claude Chevalley. He had come over on some sort of postdoctoral fellowship in 1938-39, perhaps a Rockefeller fellowship, and was caught here by the beginning of the war in Europe. So he was appointed. I think he was first called "lecturer" and was promoted to assistant professor and went on up the ladder very quickly. He went on leave of absence in 1946, on sabbatical leave to go to France, and to everybody's disgust at Princeton, while he was away he accepted a professorship with Columbia and did not return to Princeton. It's always been regarded that there was a sort of general rule that you came back for at least one year after going on a leave of absence. The simple fact was with Chevalley that when he had been living in Princeton, he had spent as much time as he could in New York because then and perhaps still now there is a very active French-speaking community in New York. He wanted to participate in that, that was his life outside of his mathematics. So, when he was offered a position at Columbia, he jumped at it; because he was to be at Columbia and in the French community at one and the same time. But Lefschetz and others felt very unhappy about this. So Chevalley, who was an algebraist and an algebraic geometer, was here for the interregnum between Wedderburn and Artin. Artin came in 1946. But that period was covered very nicely, as far as algebra was

concerned, by Chevalley. Now I think this completes the coverage of the staff changes that took place in the 1930s and the 1940s.

ASPRAY: In the late 1930s, the areas of most active research in the wider Princeton community were what?

TUCKER: Well, of course, topology. Even though Alexander and Veblen were no longer really active, they were magnets that drew people from all over the world to this center of topology. But it was then the younger people who supplied the community. Neither Fox nor Steenrod were here during the war period but returned after the war. Steenrod came back in 1947. I have forgotten now when it was Steenrod went to the University of Chicago. I guess it was 1940, or maybe 1939. Of course, Tukey was still a topologist. When he served as Fine Instructor, he was appointed as a topologist, not as the statistician or a numerical analyst. He later became a numerical analyst and that is what he is known for.

ASPRAY: And yourself?

TUCKER: And myself, yes. But there were many people passing through. I just do not think of names at the moment, but there was a weekly topology seminar, not during the summer, but during the regular year. And I am sure that there would normally be an attendance of something like twenty people.

ASPRAY: Other areas of active research?

TUCKER: Well, of course, mathematical statistics.

ASPRAY: Under the guidance of Wilks?

TUCKER: Under the guidance of Wilks. This was actually the area that was most active in the department during the war. Wilks had had his first Ph.D. student in, I think it was, 1939 -- Joe Daley. Then the following year he had had

George Brown and Alex Mood; and later on, Ted Anderson and Fred Mosteller; and then Will Dixon. There was probably a larger active group then in mathematical statistics than before or after. Wilks was the one person who was heavily involved in wartime consulting, who was able to use Princeton as a base for that, to maintain his contact with graduate students working on their theses, and at the same time to involve these students in some of the projects that Wilks was into. There were several of these. There was a quite large statistical research group set up at Columbia. And Wilks was involved with that group without ever being a member of the group. At the same time, I am sure that he spent on the average of about one day a week in Washington. He was advising many government agencies, even those of size -- the Defense Department. For example, I know that in preparation for the invasion of Normandy he did a statistical study of various systems for meteorological prediction, because it was going to be very, very, very important to pick the right day for crossing the channel. He did the statistical study in advance of that. I happen to know that it was not a very successful study. At that time there was no reasonably sure way of predicting tomorrow's weather. The standard that Sam used for his comparisons was to predict that tomorrow's weather will be identical to today's. He found that no system of weather forecast that he examined was better. (laugh) Well, I mention this just as an example to show you the variety of things that Sam was involved in. He was a terribly hard worker and he expected the same of his students and the people with whom he worked. You can find that in interviews that we have with his students [in the Princeton Mathematical Community of the 1930s Oral History Project].

ASPRAY: Are there any other areas that stand out in the late 1930s?

TUCKER: Mathematical physics was very active at Princeton until the war began. But after the war began, the Princeton physicists were involved at other places and there was very little, as far as I know, of the wartime research in physics that went on at Princeton.

ASPRAY: What areas were the people working in in mathematical physics? What kinds of topics, problems?

TUCKER: Well, especially in quantum theory. That was the principal interest. But the quantum theory involved so

many things that that is a great deal. Robertson had a special interest in cosmology, that is in the structure of the universe. Ultimately, he left Princeton to go to Cal Tech to be a professor of cosmology, to be connected with the Mount Palomar Observatory that was expected to open up new vistas in the subject of cosmology.

ASPRAY: He left when?

TUCKER: At the end of 1946-47. In the late 1930s, I would say his main academic interest was in cosmology. It was Wigner and Wheeler who seemed to attract the students. Along about then Wigner had two students who later were Nobel Prize winners, and Wheeler had Feynman who was also a Nobel Prize winner. But those were all graduate students in physics. I have explained a graduate student in mathematical physics could be a graduate student in mathematics or in physics. Bardeen was a graduate student in mathematics. But these others who worked in the late 1930s and early 1940s at Princeton, such as Feynman, were graduate students in physics.

TAPE 2/SIDE 1

TUCKER: I guess I should say something about mathematical logic. In the late 1930s Church did not seem to have correspondingly great graduate students to those that he had had earlier, Kleene and Rosser and Turing, in the mid-1930s. They did not have counterparts in the late 1930s. At that time Church started out to write a book on what was to be a compendium on mathematical logic. As far as I know, it was never done. There was something that came out in the Annals Studies Introduction to Mathematic Logic that was the start of his big project. He was very much engaged in the editing of the journal Symbolic Logic, which he had taken on somewhere around the mid 1930s, practically taking over more and more and more jobs. Incidentally, when we were talking about Wilks, he should have been mentioned as editor of the Annals of Math Stat. That was another of many things he was doing in the late 1930s and early 1940s. He involved his students in that as well. So there really was a very active, effective workshop in mathematical statistics.

ASPRAY: Can you think of any other things?

TUCKER: I've not said very much about analysis. I guess it is because I had no feel for that. But certainly Bochner was very active in writing. He was probably the most active member of the department, publishing four or five papers a year, often joint papers with someone who was at the Institute for the year. He had several Ph.D. students at this time. Bohnenblust was active in analysis, but more as a teacher. His graduate course was regarded without question as the best graduate course in the department. Every graduate student took that course and used that course to prepare for the prelims. Also Bohnenblust was doing a lot of work on the Annals of Mathematics; but he was not publishing much himself. I tried to persuade him to write up the mimeographed notes of his course for publication in some form. But he could never seem to bring himself to do this. These notes were treasured by graduate students. I have heard many cases of there being individual photocopies being made of them. They were so highly prized.

ASPRAY: Let's turn to a rather different subject. In 1939, I believe it was, the Institute opened Fuld Hall for the School of Mathematics. Can you describe the circumstances surrounding that and the attitude of the mathematicians, both at the School and at the University?

TUCKER: I think there was a great deal of sadness about the move. Veblen had played a role in planning Fuld Hall. He had worked with the architects building Fuld Hall, just as he had worked with the architects on the building of Fine Hall. And there are many ways in which Fuld Hall resembles Fine Hall. Originally, the library was on an upper floor of Fuld Hall, just as with the original Fine Hall. Now there is a separate Institute library. The offices have blackboards and fireplaces. There were different architects, and so the wood paneling is a light color in Fuld Hall, as compared to the dark color in the original Fine Hall. But I think there was a great deal of nostalgia for Veblen and Alexander in the move to Fuld. I don't think it made much difference to von Neumann and Weyl, because at Fuld they had first-rate offices, whereas they had some of the second-rate offices in Fine Hall. Veblen and Alexander had top-notch offices in Fine, but Weyl and von Neumann had ordinary offices. But to neither of them was the office quite the same as to Veblen and Alexander. Von Neumann did his work wherever he had the happened to be, and Weyl liked to work at home. Morse had really not been around Fine very long; and I think he also appreciated the fact he got a grander office at Fuld Hall. Things had become very crowded in Fine Hall, and most of the people who

had just come to visit for a year or two at the Institute simply did not get any office space at all. But that changed when they moved to Fuld Hall. That was the overriding argument for making the move. The University was not at all encouraging the School of Mathematics to move out of Fine Hall. They would have been welcome to stay. On the other hand, there was a certain pleasure when the Instituteters left, because this made more room. Advanced graduate students could share an office, so there was more space to rattle around for the mathematics department.

ASPRAY: Did the department lobby one way or the other with the University administration?

TUCKER: Not to my knowledge. By the way, to ease the pain, the Institute kept two or three offices for its people after the move. So I think that, say, Veblen and Alexander and perhaps somebody else had an office in Fine Hall that they could come to and leave their things if they were over that way; and the same way Weyl and von Neumann and Morse. There were no classrooms in Fuld Hall (with one exception: there was one room that could be used as a classroom or lecture room, but it was not a very congenial room. It was rather dark and the seats rather crowded)... So, as a result, seminars and courses still went on, given by Institute people at Fine Hall.

ASPRAY: Did the move diminish the number of courses?

TUCKER: I don't think so. I think Weyl gave a course every year -- in the large lecture room at Fine Hall. This continued on through the 1940s. Morse would give a course. When I say he gave a course every year, it may have been only for one term. The Institute had very short terms. The Institute did not open at all until about the first of October. And it closed its first term well before Christmas. Then the second term started about the middle of January and ran until about the middle of April. So, when Weyl or von Neumann or Morse would give a course, they would probably make it only within one of the Institute terms. But it was not a university course. If you were giving a university course that was issued in the graduate catalog, you were obliged to give it during the university terms.

ASPRAY: Was there less contact after they moved?

TUCKER: Well, I think that one thing had to do with the Common Room. I think that the Common Room was less exciting. There was not quite the high degree of activity. I am not referring just to teatime. I am referring to all hours. Another point to be made is that the Institute had no real library at that time. They were starting to build up a library, but they really did not have the back periodicals. They only had recent periodicals. And they did not have the collected works. So, the mathematicians came to Fine Hall to make use of the library. It was, in a certain sense, a situation where there was a Fine Hall annex that provided office space out at the Institute, which was the official headquarters for the School of Mathematics. That's where the secretary was located and so on. But the mathematical life was still centered in Fine Hall.

ASPRAY: Did that change over time? Did two separate centers of mathematical life develop?

TUCKER: Oh yes, that has changed over time. So that now there are very definitely two centers. The Institute library is an excellent working library. I do not think it is yet as complete as the Fine Hall library, but it's got 95%, I would suppose, of what the people who are working in there need -- and in very nice facilities. Another thing, as you know, the Institute has a very fine cafeteria, which is something that Fine Hall lacks. There seems to be an abundance of -- not an overabundance, but an abundance of office space that's mostly shared by the people who come for a year. And then, over there there's housing. So I think now it is only something special happening at Fine Hall that draws people to it: a special lecture, a special seminar. Or there may be somebody who is really interested in following a course that Thurston, say, is giving. But it's the exception now.

ASPRAY: When did those changes occur? Were they gradual?

TUCKER: Yes. It's only gradually that the Institute has built up a good library. And also, it's only fairly recently that these additional buildings were built at the Institute to provide the library facilities, the cafeteria facilities, and so on.

ASPRAY: Might it also be as new appointments were made at the Institute, who didn't have the tradition of close ties to the University that the situation changed?

TUCKER: Oh yes, that's right. I do not think there were any new appointments made at the Institute until about 1950. This is something I meant to check on. Kurt Godel was here regularly from 1940 on, becoming a professor 1953; and C.L. Liegel from 1940 to 1951. I know that Montgomery was made a professor about 1948; Atle Selberg in 1949. I do not know exactly when Hassler Whitney came, but I think it would be around that time (1952, to be precise). So that somewhere around 1950 there were new top people starting to come to the Institute. Von Neumann died in 1957 and was involved almost completely with the Atomic Energy Commission in the years that preceded that, so that his position was essentially vacant from 1950. Veblen had retired in the late 1950s. Alexander had resigned in 1947, because he felt that he was no longer functioning as a professor should. And he was independently wealthy, so...

ASPRAY: Let's change topics somewhat and trace your career from the late 1930s through the wartime years. Just begin wherever you choose.

TUCKER: I had my first leave of absence in 1940-41. When I started on that leave of absence, my main intention was to write a book about the origins of topology. I was interested in the origins of those things, generally; and I felt that what was customarily said about its origins, referring to Cantor or point-set topology, and referring to Poincare for the combinatorial or algebraic topology. So, from a point of view of combinatorial topology, things began in 1895. But when you read these masterful papers of Poincare on analysis situs you naturally asked yourself, 'Well, what got Poincare into this?'; because he presents it as *fait accompli*. I had looked a bit at things that seemed to have happened earlier, and I saw that there was a great deal that had happened before 1895. So I hoped with the leave of absence to find out something of this. I did not get very far in the fall when I was at Northwestern University. Then, after a vacation period, the mathematics meetings were held at Baton Rouge; and after that a vacation in Mexico, ending up at Cal Tech, where I was for the rest of the year. Incidentally, on the trip to Mexico we stopped at Austin and I was entertained very nicely there by R.L. Wilder. Austin was R.L. Moore's place. He refused to attend the Wilder's party, because there were members of the Applied Mathematics Department who were invited to the Wilder's party, as well as R.L. Moore's department. The following day I was at tea at the Moore's. Mrs. Moore

thought very highly of Wilder and she was apologetic that her husband declined the Wilder invitation, so she invited the Wilders and their guests to tea. Mrs. Moore said that it might very well be that her husband would not appear. He showed up and there was very pleasant chit-chat, but no mathematics was discussed. This was typical of R.L. Moore. Wilder told me that that whole year that he was visiting Austin, he was never once invited to give a talk in the seminar that R.L. Moore ran; because Wilder had blended the Moore topology with some of the Princeton topology. This was heresy as far as R.L. Moore was concerned. Anyway, at Cal Tech I had the tremendous good fortune to meet Eric Temple Bell.

ASPRAY: You were there the second term?

TUCKER: February through June.

TAPE 2/SIDE 2

TUCKER: E.T. Bell's wife, Toby (who had collaborated with him in his writing, to the extent of typing, proofreading, and that sort of thing), had died a few months before. He was very much at a loose end. He was taking his meals (not breakfast; but lunch and dinner) at the faculty club, the so-called Athenaeum, where I was living. In the evening, after dinner, he did not want to go home. He would stay around in the lounge talking to anybody. So night after night, I talked with him about the history of mathematics; and particularly about what I was interested in -- namely, the source ideas of topology. Sometimes I guess we would talk until midnight. I would walk him home -- a couple of blocks away. He was a person who had a very keen sense of things. He was not content in dealing with historical matters to just give a chronology. But he was interested in what led people to their ideas. Although he was an algebraist and a number theorist and claimed he knew nothing whatsoever about topology, he talked very sensibly about the things which I was interested in and gave me many leads on places to look for what I was seeking. But in the course of doing this, I had to restrain myself from giving him a lecture on topology before Poincare, and indeed topology before 1895; because Poincare in the 1880s wrote some papers that showed you the motivation that he had in what he wrote in 1895. But in the course of this, Bell drew my attention to the fact that the first chapter of

Maxwell's Electricity and Magnetism, Volume 1, which is devoted to mathematical preliminaries, actually has quite a bit of topology in it -- in particular, having to do with problems of fluid flow. Of course in flow, unless it's just a straight flow in a river, there are eddies and phenomena of the sort that are very important in hydrodynamics and aerodynamics and so forth. I found stated in Maxwell, reference given to a paper of Lord Kelvin, a theorem that I saw could be turned into a topological theorem on manifolds with regular boundaries. That was very much like work that was being done in England by W.Y.D. Hodge on harmonic integrals. To some extent it was my differential geometry background that was coming in. But it was primarily the topology. This, then, turned me aside from the history itself; and I wrote up a paper, just a summary, of my ideas on these matters. Along about June I sent this paper off to Lefschetz. It was, I think, about five or six typed pages. I sent this paper off to Lefschetz to submit for me to the Proceedings of the National Academy of Sciences. At that time, the only way to get quick publications of mathematical results was to get it published in the Proceedings of the National Academy of Sciences. Now the American Mathematical Society has its own journal for this purpose; but that was the way it was done at that time. Well, it turned out that Lefschetz was having a quarrel with the editor of the Proceedings, who had come to the conclusion that there were too many papers in mathematics being submitted. (There was a time when about a quarter of the papers published in the Proceedings of the National Academy of Sciences were mathematics.) So Lefschetz transferred my paper, without saying anything to me, and put it in as a paper to be published in the Annals. He was editor. He accepted it and it went into the hopper. Well, I didn't find out what had happened until I came back to Princeton in the fall. I didn't even find it out immediately, it was in the middle of the fall. I was very upset because it was not in a suitable style to be published in the Annals of Mathematics. It was in the abbreviated style that was appropriate for the other Proceedings. So I withdrew the paper, intending to develop it more properly. But no sooner had I returned to Princeton in September, than I became involved in war work. Merrill Flood, John Tukey were already involved in this Fire Control Project. I had not been in Princeton more than a couple of days when Merrill Flood came and said he needed my help. And I agreed. Of course, I checked with Dean Eisenhart. He approved the idea.

ASPRAY: Can you describe the project to me? Its origins, its objectives?

TUCKER: The project was designed to study optical range-finders and height-finders. This was just before the days of radar. So, rangefinders for artillery fire, or naval fire, and height finders, and anti-aircraft artillery were very important. In most artillery fire, the observer directing the fire is able to pinpoint the target as far as azimuth is concerned. The difficulty is the distance, because the distance determines the elevation of the gun for the parabolic trajectory. The rangefinder quickly becomes the crucial thing in the artillery fire. Our project was aimed at getting better results. This meant improving the mechanics of the height- or range-finder. Also, it involved trying to find out who made good observers. Some operators were much better than others. I'm not now talking about the speed. I am simply talking about their ability with the combination of their eyes and the machine. So, in our project we had psychologists working because it was the man-machine combination that we were really working on.

ASPRAY: Why mathematicians?

TUCKER: Because this was something like the work at Aberdeen -- where Veblen and others were involved. This carried over from World War I, where the mathematicians had worked on and contributed to work on ballistic problems. Since range-finding seemed to be some sort of a ballistic problem, mathematicians were brought in. Actually it was, I think, statisticians who proved the most useful in the project. My work in the project was largely administrative. Flood was the director of the project, and I was the associate director. The reason Flood wanted an associate director was that he wanted to be free to travel. He went around visiting military establishments and picking up leads, information, and so on. I was stuck here at Princeton, teaching. But when I was not teaching I was in my office over at 20 Nassau Street running the project. This meant, particularly, looking at all the reports that were written and making sure that those reports were in a form that would be readable by army and navy officers. In other words, to make sure that the technicalities were well explained.

ASPRAY: This was a joint army -navy project?

TUCKER: We were actually working under what was called the National Defense Research Committee, and then the Office of Scientific Research and Development, which had Vannevar Bush at the head. We were working as civilians.

But our end product was to be consumed particularly by the coast artillery. They were interested in the height-finders. This was at a time when they thought the United States might be attacked. The coast artillery had an establishment down around Norfolk, Virginia. We had some of our people working down there. One of the things that we were doing was running tests that were designed to determine accuracy of observations. An airplane would draw a drone across the sky. On the ground there would be a number of photo theodolites tracking the target by spherical trigonometry, to work out the position of the target; viewed from two or three photo theodolite stations. The computation on these test runs was done here at Princeton on so-called Hollerith punched-card machines. The data would be shipped up here by courier, would be analyzed here, and a couple of weeks later the results would be sent back. This wasn't very satisfactory, because it would be nice in your test tomorrow to know how things would come out today. Here was actually where I played a role, because they wanted four-figure accuracy in the trigonometric computations. They didn't need the five-figure accuracy that would be got using trigonometric tables and full punched-card computation. They wanted four figure accuracy. A slide rule, if properly used, would give three-figure accuracy. I made the very simple suggestion to have a slide rule ten feet long, and so such a slide rule was built.

ASPRAY: Telescopic style, or was it actually linear?

TUCKER: It was actually linear. It took a crew of enlisted men to operate. But it got the four-figure accuracy that was needed. Then they thought they would like another. Was there any way of getting a hundred foot slide rule? So I came up with an idea for that, to put it on movie film and reel it past a viewing position and then you could lock the two wheels together so the film would go together in the position where you wanted to read off the result.

ASPRAY: Very clever.

TUCKER: So perhaps a mathematician can help. Another story from that time involves George Brown. He was working on a navy report in which he learned that the naval doctrine, as of that time, on long-range gunnery (you know, these battleships at that time would send shells many miles) was to try, if possible, on the first two shots, to establish a bracket. So if your first shot was short, you tried to make the next shot long. Of course, this meant that

you were aiming to overshoot. Then when you had established the bracket, you proceeded by repeated bisection. This indicated that, under ordinary conditions, that statistically it would take about six shots before you would hit the target, in terms of what was known of the accuracy. George felt that this business of overshooting to establish the bracket was not right. So, he tried experimentally and did a computational experiment in which the aim was to establish a bracket half of the time. In other words, to shoot in such a way that you thought you had a 50% chance of overshooting, and a 50% chance of undershooting. This is a reasonable idea. He proceeded to test this out. If you don't establish a bracket, then again each time you aim with the objective of a 50% probability of getting the bracket. You do this even when you are bisecting. He found that he would hit the target in five rounds on the average. So this report was written up and sent in. Finally, it found its way to the desk of Chief of the Bureau of Ordnance. He immediately got on the telephone and talked to Flood and said, "Come down here immediately!" So Flood and I took the next train to Washington. We went to see the Chief of Bureau of Ordnance. He said, "This is very interesting, but have you tried it out?" We said, "Of course, but unfortunately we didn't have the facilities to try it out." We hoped that he would try it out. "Oh," he said, "We will." We never heard directly anything further about it, but we did find out after the war that the doctrine had changed. Brown tried to give a theoretical proof of this, but did not succeed. So the only proof we had is empirical computation. It was that project that turned Tukey into a statistician. When he started on that project, he was a topologist. So, he really feels, as I think was said in the interview that we had with him [Princeton Mathematical Community in the 1930s Oral History Project], that it was that project and in particular, this experienced statistician, Charles Windsor, who was a member of the project, that really made him feel that statistics was the one thing that he wanted to devote himself to.

Well, my paper for the Annals did not ever get written. I was heavily involved in teaching. As soon as Pearl Harbor came, the University instituted some more applied courses. This was mainly motivational, to give students who were trying to go on with their studies the feeling that they were getting their hands dirty at some point. One of the applied courses was one that I gave in graphical means of computation. This was slide rules, nomograms, and such things. I had great fun with that course and I think the students did, too.

TAPE 3/SIDE 1

TUCKER: One thing that we designed in that course that had not been done before was a slide rule to be used by the field artillery. The ROTC at Princeton at the time was field artillery. The field artillery had a standard operational problem in which there was an observer and there was the artillery battery, and there was the target. These formed a triangle, but the triangle was a very long, thin one, reaching to the target. There had to be hand computation done right on the spot. As perhaps you know, the artillery uses mils as a measure. All the existing slide rules were in degrees, so there was this so-called "short base problem" that the artillery had of working out the distance from the gun to the target. You knew how far the observer was from the gun. You could measure base angles. The question was to do a bit of right triangle computation here in split seconds, so to speak. A slide rule was designed by the class for this purpose -- a quite simple thing. It just had two scales, a fixed scale and a sliding scale. One of the men among the ROTC officers was very much interested in it. He asked permission to send it in to higher authorities. Finally, well after the war was over, it finally came back to ROTC here, which reported to me, that this was a very, very helpful suggestion, but this problem was no longer of importance, because of radar.

ASPRAY: Right.

TUCKER: So, in addition to doing regular teaching I was doing special teaching and also the fire control work.

ASPRAY: Did that continue the whole war?

TUCKER: The fire control work wound up some time, I think, in 1944. By then radar had proved itself. Indeed, we discovered that the radar testing system used was that we had set up with the theodolites. We had, in addition to working on improving the height-finders and range-finders, we also improved the phototheodolites. There, again, one of the improvements in the phototheodolites was an idea of mine. The phototheodolites were graduated in mils. Well, you got two readings: the reading in azimuth -- horizontal, and a reading of elevation -- vertical. There was no problem particularly in the elevation reading, because it ran all the way from zero to 1600. (There are 6400 mils in a circumference.) A mil is an approximation to a thousandth of a radian. But there should be then, not 6400 mils in 360

degrees, but less than that. It should be got by multiplying pi by two thousand. But it's rounded up to 6400 mils. That's all a right angle is -- 1600 mils. But the trouble was with azimuth because sometimes when you would be tracking a target, you would go all the way around and be in a second revolution. The counter ran up to 10,000 mils and went to zero. There is no connection between 10,000 mils and 6400. So we had a great deal of trouble; sometimes the whole experiment would simply be lost because this had happened, and the crew that was doing it had not noted down the fact that they had gone through 10,000 mils. My solution...

ASPRAY: Counter that ends at 6400?

TUCKER: Put in two octal counters and two decimal counters.

ASPRAY: Oh, sure.

TUCKER: So it will count up to 800 mils, at which point it will go to zero and one will appear in the next place. It goes up to eight... What a lucky thing -- 64 is a perfect square. This just solved the problem beautifully. It had an additional value, that when you're using trigonometric tables, you need only the trigonometric tables from zero to 45 degrees, in other words from zero to 800 mils. So you can simply use that last octal digit to tell you which 45 degree segment you are in. This will give you the simple transformation on the trigonometric functions. So, this very simple suggestion just seems trivial. But it solved the problem beautifully. So, although I was not supposed to be doing any of the research in the project, just supervision, I also contributed some ideas.

The real confusion on the campus began the first of April, 1943 when the first group of army trainees arrived. These were 18 year-olds, just out of high school, mainly from the New York City area. We had about 800 of them. The army prescribed the things that they were to be taught in terms of subjects, and had referred to a few textbooks for each course; or about five textbooks mentioned and a number of chapters in those books to serve as a basis for the course. Most of the army specialized training program elsewhere did not begin until July, but we had an experimental group that came in April. In the first term; it was pretty much algebra and trigonometry. In the second term it was

analytic geometry. In the third quarter it was differential calculus; and the fourth quarter it was integral calculus; and the fifth quarter the students learned about differential equations. We taught it just about the way we taught our ordinary undergraduates -- in sections of about 20. But the students were in uniform and they marched into class and marched out. That went pretty well. But then we got the larger group on the first of July and continued in the months ahead. Things became more difficult because then the army specified that there was to be three lectures a week and three class periods a week. Saturday was a day just like any other. We started to do the lecturing. Because the army also had put cost limitations on things, we tried to figure out a schedule so we would break even on the number of teaching hours we used and the amount that the army would pay. This involved teaching the lectures in very large groups, very large for Princeton; you know, perhaps 200. I did some of this lecturing. I think Deane Montgomery did some of the lecturing. There was someone named Henry Sheffe who was around at that time; a man who was converting from a previous mathematical experience into working in statistics. Teaching those large lectures was, I think, the hardest work I have ever done in my life.

ASPRAY: Why is that?

TUCKER: I can understand how a performer, a concert pianist, or someone like that is completely exhausted by a performance. I carefully prepared what I was going to say and worked out the examples and so on. But I don't think we had vugraphs and that sort of thing. I often had to lecture in a lecture room that did not have proper blackboards; it just had portable ones. Of course, I had to speak very loudly. And it was the utter concentration that was required to be conscious of the audience, to be conscious of what you were saying. I never had done anything like it. Then after about the third term of army teaching (each of three months), Dean Root, who was supervising the whole thing for the University, called me in and said that the scores (they were taking standardized tests at the end of each quarter) in mathematics had fallen off, as compared to the first quarter. He asked me if I thought that there was any reason I could give for this. And I said, well, as far as I could tell, everything was the same. The same people were involved in the teaching, so there was no important change except this use of lectures. He said, "Well, let's go back to the sections. They were all that way." I said, "We'll exceed the allowable costs." He said, "It doesn't matter. Our overall instructions is to do it the best we can." So we went back to the sections and our scores went up again. Now

this was not a controlled experiment, but I'm against teaching the in large groups. The sort of mathematics that we were trying to teach was much better done in sections than in lectures.

I guess that I was also attending some seminars in Fine Hall. One incident that sticks in my mind. There was a graduate student by the name of John Denby-Wilkes whose father was an officer in the French navy and whose mother was American. Because of his French connection, he had high regard for Chevalley. But this was a topology seminar, a sort of junior seminar; an expository seminar rather than a research seminar. He was giving a talk, which I had apparently helped him prepare, about the topology of surfaces. He was using the so-called "cut and paste" methods of combinatorial topology, in which he thought of his surfaces as cut up into pieces. The overall structure of the surface could be described by how pieces fit together. Chevalley was present in the audience, spoke sarcastically at the end about this cut and paste. He made some remark that I later saw attributed to another member of the Bourbaki group, with regard to Riemann surfaces: "to cut a Riemann surface is to kill it." John Denby-Wilkes was terribly upset by this criticism. He got all red and made some sharp retort to Chevalley, and then he just walked out of the room. He was not a very successful graduate student, although he got a doctor's degree elsewhere and is in the Combined Membership List of the Society, teaching at Virginia Commonwealth University. I rather liked him. I thought he was quite spirited. But for his idol to have made this cutting remark (sic) was just too much for him. Of course, there must have been other things going on that I was partially involved in. But it is the wartime teaching and the fire control research project that stand out in my mind.

ASPRAY: And graduate instruction?

TUCKER: Oh, yes; there was graduate instruction, because you look here at the list of doctorates. Although it's a bit sparse from 1943 to 1946, in all the other years in that period there were close to ten a year.

ASPRAY: Were you doing any graduate teaching?

TUCKER: I don't remember. I don't think so. I really don't think so, because the demands came first for the army. I

also taught a course for the navy. This was a 4-month course for naval officers who were sent here before they went on to MIT to study radar. Here they were getting a preliminary sort of brushup course. It was my job to teach the brushup mathematics. These were all supposed to be graduates of the Naval Academy, or the equivalent at an engineering school. Among other things, I was teaching them a refresher course on the slide rule. One of the naval officers, who was there as a student, was a man by the name of Bland, who was named as one of the authors of the manual that came with the sliderule that we would be using. He was an instructor at the Naval Academy who had gone into service and was sent along. This was a strange experience that I had in teaching a man who had helped author the manual for the slide rule on how to use it. But fortunately, because of the course I had given earlier on graphical computation, I had discovered a way of solving one of the standard cases of a spherical triangle, using something that's called the haversine formula. I had found a way of doing that on the slide rule. As well as teaching them, I also had to supervise study periods... At one of the study periods I took out a slide rule and remarked that it must seem rather boring for him to be brushing up on the sliderule. Then I said, "By the way, do you know a method for using the haversine formula for spherical triangles?" He sort of said, "Well, I don't think you can. The haversine formula doesn't lend itself to the scales on the slide rule." So I said, "Well, let me show you." He had been making some cracks before that to this fellow officers about my teaching. I don't think that there were any more cracks. At the very end, before normal teaching resumed, I worked for von Neumann for about four months.

ASPRAY: What did you do?

TUCKER: He had a project in which Montgomery and Bargmann were also involved. It was a project in numerical analysis. Von Neumann was particularly interested in attacking problems in partial differential equations for purposes of meteorology etc. He thought that partial differential equations were the main challenge. The idea was to try to develop new approaches to computation in several variables. The computational procedures for ordinary differential equations were pretty thoroughly developed at the time. But very little had been done on approximation with partial differential equations. A lot had been done in theory, but not in actual computation. My job, as I was supposed to be a topologist, was to see what I could come up with that would generalize finite differences to two or three dimensions. With regular finite differences you're working with functions of a single variable, and you are

considering the endpoints of successive intervals. Now you go to a function of two variables, and then your domain is a plane. There is the obvious way of breaking this up into rectangles. But there are other ways, many good ways, as I said earlier, combinatorial topologists use triangles. So he had me playing with that. But I really got nowhere in the four months. At that point my teaching duties called me back to the University.

END OF INTERVIEW