ALBERT TUCKER

CONVERSATION WITH ALBERT LEWIS

This is an interview of Albert Tucker on 9 April 1979. The interviewer is Albert Lewis, archivist, University of Texas.

Lewis: Before we really get into things, there are questions that I have as an historian about papers. You talked about the departmental papers. They're still in some basement there?

Tucker: Yes.

Lewis: Which is good news, because I just today talked with Mr. [Earle] Coleman, the archivist, and of course he has nothing of the departmental records. So that's very nice to hear. By the way, he said that he would like to have a copy of anything that you produced to put in the archives. They don't have many tapes, but they will take care of them.

Tucker: Do they have facilities for copying tapes?

Lewis: Probably not. There are commercial firms that might be able to do that. I know at Austin if you have a television film, they have facilities to do it. Perhaps Princeton does, I don't know.

But the question I wanted to ask as a historian is about the papers of some of these individuals, because people have asked me to ask you. For example, I know people who are anxious to find some of [James W.] Alexander's letters and manuscripts. Would you have any idea?

Tucker: Well, I had an inquiry from someone at Amherst.

Lewis: Yes, that's one of the people who talked to me.

Tucker: He actually came to see me, and I had a very pleasant chat with him. This was before I had the tape recorder. In my office over at Fine Hall. The only lead that I was able to suggest was his family. Last summer Alexander's daughter rented a neighbor's house here, and we saw her around. It was not until after she had left that Professor [Stanley] Howard next door just happened to remark, "You know that was J.W. Alexander's daughter who was in that house." These houses around here, there are eleven of them, and in all of them are retired members of the faculty or widows. I'm actually the youngest of the householders around here. We're a corporation. Princeton University Faculty Housing Corporation.

Lewis: How long have you lived in this house?

Tucker: I've lived here since 1970. But the house has existed since 1954. It was occupied earlier by a professor of geology, Paul McClintock, who died in 1970. Lefschetz moved into the house down here at 11 Lake Lane in 1954. He had retired from the University in 1953. They let him stay on in the rented house he had for a year until that house was finished, and then he lived there. His widow died just last September. So the house is now vacant. It has not yet been reoccupied.

Lewis: I see. We'll get back to Alexander. Did the Amherst person succeed?

Tucker: He got in touch with the daughter, or at least obtained information on how to get in touch with the daughter and was going to do that, but I have not heard at all as to with what results.

Alexander in his later years was pretty much a recluse. He had independent means. He came from an old Princeton family. There is Alexander Street, Alexander Hall, 'Alexander' all over Princeton. I think a grandfather of his was once the president of the Princeton Theological Seminary. He's the only person I know of who held four degrees from Princeton. Bachelor's, master's, Ph.D., and Honorary Doctor of Science, which was awarded him in 1946 at the time of the Princeton Bicentennial. He was my professor when I was a graduate student at the University. I had great admiration for him, but realized at that time that he was quite different from the run of the mill professor because he had a nice home over in the west end of town. Graduate students were invited to drop in for Sunday afternoon tea there. There were servants in the home, you know. It was very clearly a home of affluence.

Alexander traveled a great deal. He was a remarkable mountain climber. He had many first climbs to his credit in the Alps and also in the Rockies. My good friend Don Spencer, who grew up in Boulder, Colorado, said that he was acquainted with the name of Alexander, in 'Alexander's Chimney', long before he knew that Alexander was a mathematician, not just a celebrated mountain climber.

Alexander went, as I told you, to the Institute for Advanced Study in 1933, from the University. But even at the University he had taught only part time and accepted only a half salary. At the Institute he started out on a half salary, but after a few years he asked to be relieved of his professorship, and he was made a so-called permanent member with no stipend. He had an office at the Institute which he came to from time to time, but he really didn't have very much to do with mathematicians.

In 1953 when Lefschetz retired and we had a symposium in Lefschetz's honor, I called up Alexander and gave him a special invitation to attend as much of the thing as possible, but to please come if he would to the dinner when we were going to make the presentation to Lefschetz. I knew that Lefschetz had a special regard for Alexander. He felt that it was Alexander who had become aware of his work and had been the one who had insisted on Lefschetz being brought to Princeton from the University of Kansas, where he was.

Lewis: That's interesting.

Tucker: At the same time, Lefschetz hated Veblen, but he felt that Alexander had been his friend and that it was through Alexander that he had become recognized, at least as far as American mathematics was concerned. So Alexander said, "Well, I would like to come. But I don't go anywhere where there's a crowd." Now I suppose ... there's a word for this, agoraphobia, the fear of crowds?

Lewis: Yes.

Tucker: And he said, "Let me think about it." He said, "I want to come." Then he called about a week beforehand. I didn't talk to him myself. He simply left a message, and it was sorry, he couldn't come.

Now on one occasion after that he did come to Fine Hall, the old Fine Hall. My colleague Ralph Fox had worked very much in things that Alexander had worked in, especially in the theory of knots, which is something that Alexander made important contributions to. And Fox had written a letter to Alexander—he knew Alexander lived here in Princeton—asking him for some bibliographical information. Weeks went by, and he had no answer. Finally one day Alexander showed up at Fine Hall asking for Fox. He had come to give Fox the information word of mouth rather than by writing him a letter. That was the only time after 1950 that anyone that I knew of saw him. He died along about 1970. Now, I have made inquiries because many people are interested. I had an inquiry from somebody in England.

Lewis: Yes, I know this person too.

Tucker: His inquiry didn't come directly to me, but it came finally—I'm sort of the final place where these things come. I wrote to him that I did not know of any organized files of Alexander. It's a great pity if there are none, because in his younger days Alexander kept files, and most of the papers he wrote he never published. In a

topology seminar in the 1930s some young post-doctoral mathematician would talk about something, and Alexander would be present. When it came time for questions at the end, Alexander would say to the speaker, "Now, have you applied your technique to such and such a question?" Of course the young man, who had just barely done what he had done, would say No, he didn't know what that would yield. Then sort of half talking to himself, Alexander would say, "Oh, I remember that one time I got as far as you did, but I felt that unless I could do so and so that it wasn't worth publishing." And the poor young man. But Alexander didn't mean it that way; it was just his regret that this young man had not climbed the mountain.

He was a very kindly and courtly person. He was very much at home in France, and Chemonix was his favorite place to go summers mountain climbing and for the sort of haughty French society that went on there. His wife was French and quite socially inclined.

Lewis: You mentioned that you are really talking about the Princeton school of topologists. I'm not sure, you have a colloquium publication?

Tucker: Not I. Lefschetz has two colloquium publications. The first one simply called *Topology*, and indeed that's the origin of the word topology. Before that it had been called analysis situs. Veblen had written a colloquium publication around 1920 with the title *Analysis Situs*. So when Lefschetz was writing a colloquium publication he felt that he had to get another name for it. He would say, "I like snappy titles." Even though his background was French he went to the German where [Johann] Listing had written a book—Listing was a student of Gauss and a contemporary of Riemann—on "Topologie". So in German the word 'Topologie' was to some extent established. Lefschetz decided to turn that into an English word, 'topology', and use that as the title of his colloquium volume which was published around 1931. I've never seen anything catch on as fast as that did. Of course it's fairly obvious. You know, you have 'topology', 'topological', 'topologist', and so on. Try to do that with 'analysis situs'.

Lewis: I wanted to get your reaction to R.L. Moore.

Tucker: The infamous or famous Robert Lee Moore. He was working in analysis situs until Lefschetz introduced the term topology, and then he proceeded very reluctantly to adopt it.

Lewis: Even in his colloquium publication in 1932 he calls it point-set theory. I think he must still have had a reluctance to call it topology.

Tucker: Oh yes.

Lewis: He never at Texas referred to it as topology in the course descriptions.

Tucker: I see.

Lewis: It was "point-set theory".

Tucker: Of course he disowned some of his students, like R.L. Wilder, but G.T. Whyburn and others of his students certainly adopted the term topology, and as far as I knew Whyburn never fell from grace.

Lewis: I'm surprised you would say Wilder did as well.

Tucker: Oh, I have this directly from Wilder. Wilder held a Guggenheim Fellowship at Austin in the year 1940-41. He has told me that in that year that he was at Austin, R.L. Moore did not at any time talk mathematics with him. Moore did not invite Wilder to give a talk; he completely ostracized Wilder mathematically. At the same time Mr. and Mrs. Moore would have the Wilders to dinner at their place and be very friendly with them socially, but Wilder told me that he was cut off as far as mathematics was concerned.

Lewis: Well, that's interesting.

Tucker: I visited Austin in January 1941. There had been a meeting at the American Mathematical Society at Baton Rouge, and from there I went on to Austin. I had a friend at that time at Austin by the name of E.W. Titt. I knew R.L. Wilder, and while there I called on the Wilders. As a matter of fact they had a party while I was there and invited people from both departments. I was told that that was the only occasion during that whole year that people from both departments had been at the same party. Furthermore the Moores didn't come to the party, but they asked the Wilders to bring me around to tea the following day, and I had tea with the Moores and the Wilders, but the conversation was all nonmathematical.

Lewis: Very interesting.

Tucker: Moore had been here at Princeton when he was visiting lecturer of the American Mathematical Society. That would have been sometime in the year 1931-32.

Lewis: But he was here earlier as an instructor?

Tucker: Oh yes, but that was before my time. Harry M. Gehman has told me about R.L. Moore spending an unhappy year at Princeton.

Lewis: Yes, he seemed to have something against Princeton. I'd always known that he felt that Lefschetz was competing with him sometimes for the same students, possibly Gehman. Moore knew about Gehman, who was thinking of going to study with Moore.

I didn't know about the antagonism between Veblen and Lefschetz. Veblen was officially Moore's doctoral supervisor at Chicago, so all of these connections begin to make some sense perhaps. Because Moore, I think, stayed on good terms with Veblen and Veblen did not really like Lefschetz, this would be further reason for us to separate Moore from Lefschetz.

Tucker: Well, Lefschetz was a very rough person, in the sense that he spoke rather roughly. You see I was a Ph.D. of Lefschetz; indeed I was his man Friday for years and years while he was chairman of the department. So I really regard Lefschetz as my mathematical father.

I had the very good fortune my first year as a graduate student to stand up to Lefschetz. I criticized the way he was proving something in a graduate course, and he sarcastically invited me to come to the board and prove it the way I thought it should be proved. Which I proceeded to do. I finished the proof, and then he ridiculed this performance. Class came to an end. Two days later, when there was the next class, he said that he wanted to go back over this proof that there had been criticism of. He proceeded to give exactly the proof that I had given and made quite a hit. Everybody in the class knew it was my proof, but he never admitted this in any way. Indeed I remember saying when he was getting rather old—I said this to one of my colleagues one day—"You know, Lefschetz is slipping. Today I won an argument from him." I said, "Usually I have to wait several days before I'd known I won an argument with him, but today he agreed to my point. This very day."

I remember the time when Moore came to give his visiting lectures. I think just for sheer meanness, Moore scheduled the first one of his lectures for Saturday evening, and then they went on Monday afternoon and Tuesday afternoon. Well, Lefschetz and Alexander went around to all of the graduate students and said "This is one time when your attendance is required. You have no option. You must be there." So the room was full. Lefschetz accorded Moore the same show of interest that he would any other speaker, namely by asking questions. Also Lefschetz did this partly to sort of help the graduate students. Moore started out by writing his axioms on the board. Something like five minutes went by, and there was no sound except this chalk on the blackboard.

Well, Lefschetz was very, very restless, and he finally broke in with a question. Moore turned around slowly and looked at Lefschetz and said, "If you would read what I have written on the board, you wouldn't need to ask your question." You could tell when Lefschetz was getting angry because the back of his neck would get pink. Well, Nathan Jacobson and Bob Walker and I, who were sitting together—we were close friends—were right behind Lefschetz, and we decided to keep score. Lefschetz repeatedly asked questions, which we knew was just Lefschetz's style, but every one of these Moore regarded somehow as being a planned barb, so he would come back very hard in some scornful way. Alexander actually tried to pitch in and help Lefschetz a bit, but on points Moore won easily.

At the next talk Lefschetz did not say a word. Again we sat where we could watch the back of his neck, and it would get red and then the color would recede, but he didn't say a word. As far as I could judge, everybody went out of his way to show interest and concern and respect towards Moore. But afterwards at other places he went, I understand he made very caustic remarks about the way he'd been

treated, especially by Professor Lefschetz, at Princeton. But this heckling was just a standard thing with Lefschetz. There's a verse that was composed back in those days by some of the young mathematicians at Princeton: "Here's to Papa, Solomon L.,/ Unpredictable as hell,/ When laid at last beneath the sod,/ He'll start right in and heckle god."

Lewis: What about his papers? We have to get to that eventually.

Tucker: They're all at Brown University. Brown University established a Lefschetz Center for Dynamic Systems, or something like that, in their School of Applied Mathematics. Indeed, Lefschetz served as a consultant to Brown in his later years. He worked very closely with [Joseph] LaSalle there at Brown, and he bequeathed his papers and library to Brown University.

Lewis: Do you know if these consisted mainly of letters, correspondence, or manuscripts of various kinds?

Tucker: I don't know. I have checked with Deane Montgomery at the Institute, and he says that they have no file on J.W. Alexander.

Lewis: Yes, the only papers I know of at the Institute are those of Goedel and Einstein.

Tucker: Yes.

Lewis: There may be some others I don't know of. Von Neumann's are at the Library of Congress, I think.

Tucker: Yes. They were at the Institute for a long time while [A.H.] Taub was editing the collected works. They were remarkable, because von Neumann kept almost a diary of papers. It wasn't in the form of a diary, but whenever he had an idea, he tried to write it out in a rough form. I was called in when the collected works were being edited to look at some things that had to do with the theory of games that had not appeared in any publication. The question was whether anything should be done about these. There were a couple of them that I helped to put some touches on, so that they could appear in the collected works.

Lewis: If I could change the topic a little, what about Marston Morse's papers, which also I've been asked about? I understand that Mrs. Morse is still living here in Princeton. I'm told if I call her up she would probably have be willing to talk about this matter, because she is very interested in preserving the things that he was associated with. Do you happen to know if that matter of ^preserving his papers has been taken up yet?

Tucker: No, I don't. She has moved to another home, so I would think that any things that were in his study at home, that something would have had to have been done about them. I would guess that Marston would have kept things pretty thoroughly. It's my estimate of

his character. I think he had the feeling that anything he did was pretty important, and often it was.

Lewis: Yes, there was an interesting to-do over the *Mathematical Intelligencer* item about Marston Morse, a memorial article about him, which tended to take this kind of critical edge. I've forgotten who wrote it. You didn't write it?

Tucker: No.

Lewis: I didn't take it as being critical in the sense of trying to be mean or biting, but some people apparently took it this way, and there was a counter-article published to try to point out what they felt were the virtues of Marston Morse that had not been mentioned in the article. The overall impression was one of an interesting character that people would get different views about.

Tucker: Well, I had a great deal of respect for Marston. He had been my post-doctoral supervisor at Harvard in the spring of 1933, and this was when he was busy writing his *Calculus of Variations in the Large.* I got my Ph.D. in June of 1932. I'd been at Cambridge, England in the fall, and I arrived in Boston about December 1932. I was there until June 1932 with Marston as my supervisor.

Well, I did not get any help or inspiration from Marston. He got a great deal of help from me, because he needed to know something about the singular homology theory that Lefschetz had been developing. He needed this for certain technical purposes in his *Calculus of Variations in the Large.* I met with him at least once a week. He was living as a bachelor at that time in one of the new houses at Harvard, Eliot House, and I would go there at least once a week and spend two or three hours. I would attempt to tell him about what I was trying to do and get some reaction from him, but he would change the conversation to the calculus of variations in the large. He was picking my brains. Well, he acknowledges this very handsomely in the preface to *Calculus of Variations in the Large.* He says I contributed to his knowledge of certain matters. He also had me offered a Peirce Instructorship at Harvard. It was in competition with that Peirce Instructorship that I was offered a research instructorship at Princeton, which later became the Fine Instructorship. At that time it had no name; it was something that Eisenhart and Lefschetz cooked up.

I had no hesitation whatsoever in accepting the Princeton offer, rather than the Harvard offer, because I liked the atmosphere at Princeton, particularly this Fine Hall business that was starting to develop. At that time there was no mathematics headquarters at Harvard. The various professors had offices. Birkhoff had his office in the Widener Library, Morse as I said was in Eliot House, and [Julian Lowell] Coolidge was Master at Lowell House. There was just no place where the mathematicians got together, except once a week when there was a colloquium. Otherwise there was just no mathematical life. Also I liked Lefschetz and Eisenhart better than I did Morse and Birkhoff. They were the ones that interested me of the people at Harvard.

When I told Morse that I was accepting the Princeton offer, he said, "Tucker, you're a fool. Don't you realize that if you just play hard to get, Princeton will up the ante." Well, this just shocked me. You know, I'd had a rather sheltered life. My father was a Methodist minister, and this way of looking at things that Morse showed went completely against the grain. I realize in retrospect that in a sense he was giving me very good advice, but at the time it was not advice that I could tolerate.

Then, of course, Morse came to Princeton, to the Institute for Advanced Study. In his first period here he was a bachelor—a very eligible bachelor—and he led quite a gay social life, going to dances and such. It was a matter of considerable gossip as to who the woman was that he was taking to this dance or that dance. Then he met Louise, I think on a boat returning from Europe. That was the end of that bachelor life. Then they set up, I suppose you would call it, a mathematical salon here in Princeton, no one else I think among the Institute professors entertained in the way they did. But I always had the feeling that for any person who worked with Morse, it was always all or nothing. You either became his henchman without question, or you didn't have any lasting relations. I managed to keep on fairly good terms, that's always what I tried to do with everybody, whether I really liked them or not.

I got along beautifully with Veblen. He liked to pull strings. Even after he moved out to Fuld Hall and was no longer in Fine Hall, he would come around. He would have some thing that he was trying to get accomplished without him appearing. He liked to do things behind the scenes, but I would never take the bait. You know, I could talk to him very easily, and then I would see that he was getting ready to, in an indirect way, enlist my support in something. Because he did this in an indirect way—he never made it in a direct request—I was always able to act as though I didn't understand what he was after. My loyalty, of course, was particularly to Lefschetz, and knowing the animosity that there was I was on my guard. But this didn't stop him from having friendly relations.

After Lefschetz became chairman of the department in 1945, for about five years Bochner never spoke to Lefschetz. Indeed, if Lefschetz came into the room where Bochner was, Bochner would leave. Any dealings that the chairman of the department had to have with Bochner were done through me.

Lewis: Did you know Mrs. Veblen, by any chance?

Tucker: Certainly, yes.

Lewis: You knew then that she was the daughter of the English physicist?

Tucker: Richardson, yes.

Lewis: We happen to have all of his papers at Austin, and I was intrigued with some of her letters to her father.

Tucker: Yes, I knew her.

Lewis: She seemed to keep up a good garden. She was always talking about her garden.

Tucker: The Veblens had a house over on Battle Road, which is so called because of the Princeton Battlefield, the site of the Battle of Princeton shortly following Washington's crossing of the Delaware. Indeed the Institute for Advanced Study is built over on the edge of the Battlefield Park. Well, Veblen had a house there when I first came to Princeton. It seemed rather out of town at that time. And of course that's why he liked it, because it was out of town. His hobby was cutting wood.

Lewis: I seem to have heard that. It still seems strange.

Tucker: So when it started to build up around where he lived, the Veblens got another home, and to make sure of things they bought a lot of property around there. They left that property to the township as a park. So there's something up there that's known as Veblen's Woods.

Lewis: He didn't cut all the trees down, did he?

Tucker: No, no.

Lewis: It seems to me that there was another name that you mentioned at the beginning here. I don't remember what it is now. Michael Mahoney mentioned Church, and I wouldn't have remembered him. He is still living?

Tucker: He is still living. He's still listed as being on the faculty of the University of California at Los Angeles.

Lewis: Yes, that's what I thought. But he was at Princeton then?

Tucker: He was a Princeton undergraduate and graduate student. He did his Ph.D. with Veblen and was a protege of Veblen's. He did his Ph.D. along about 1926, and then he had post-doctoral fellowships; I think the first year he was at Harvard, and the next year at Goettingen and Amsterdam. He was appointed assistant professor at Princeton in 1929.

Alonzo retired early from Princeton and took a position at the University of California at Los Angeles, because the University of California at Los Angeles agreed to take responsibility for the *Journal of Symbolic Logic*, which had been very much his baby. I had tried very hard during my chairmanship to get Princeton University to take on responsibility for the *Journal for Symbolic Logic*, but I didn't succeed. But the philosophy department and mathematics department

together at UCLA succeeded in persuading the administration there to underwrite the *Journal for Symbolic Logic,* and to that end they offered Church a professorship. He was within about two years of retirement here at Princeton. He was quite prepared to resign, but I persuaded the administration to at least retire him so that he could carry the title of professor emeritus. I felt that he was rather unique in the fact that he had been a Princeton undergraduate, a Princeton graduate student, and had spent his entire career at Princeton up to that point.

I had tremendous respect and admiration for Church. But he was completely oblivious of everything that went on in the world except in mathematical logic. He had a very devoted wife who looked after him. She unfortunately died about three years ago. I don't know who's looking after him now.

Lewis: Yes.

Tucker: He would be an ideal one to get recollections from, except for the fact that he seemed so oblivious of what was happening except in his own bailiwick. Now Bochner, who came here in 1933 from Europe, was very much interested in history, as you probably know. He has done one article that was about not particularly Princeton, but American mathematics as seen in the '30s by someone who was not American. He made comparisons between American mathematicians and European mathematicians.

Lewis: That's interesting. In the case of Church, how many years has he been at Los Angeles?

Tucker: He's been something like twelve years at UCLA.

Lewis: I'm wondering if he's considered leaving his papers and where they might be deposited. When a person who had made that kind of shift you know, been at like Princeton for a long time and then goes someplace else, it raises sometimes problems.

Tucker: We had a remarkable secretary in the mathematics department at Princeton. She was Agnes Fleming, and then her married name is Agnes Henry. She retired about six or seven years ago and with her husband went to live in the Carribean. She started with the mathematics department in 1931 at the age of 18, and was with the department until she retired. She was very much interested in people and had a quite remarkable feel for delicate situations and how to best deal with them. Perhaps Agnes could be got to make some recollections.

Lewis: Yes, there is a wealth of possible things to do here.

Tucker: As an example of something, here is a record of the bicentennial conference in mathematics, held as part of celebration of the 200th anniversary of Princeton University. There is this picture of people, and there's a list of names.

Lewis: What brought these people together here?

Tucker: They were invited.

Lewis: Do they all have some connection with Princeton or not?

Tucker: No, they were invited from all over the world.   This was an international conference on mathematics.

Lewis: Very interesting, almost every one of them seems to be a well known name.   I've never seen this before.   This is a really interesting gathering.

Tucker: It was written up by John Tukey.   He acted as the reporter for this.   It was the first international mathematical conference that was held after World War II.

Some of the historical matters that I interest myself in are ones that I have a research or teaching interest in.   Through this geometry course, for example, I had to find out for myself what the origin was of the conic sections.

Lewis: That's a major task.

Tucker: Well, there's only one place to start with this, if you should ever do it, and that's O. Neugebauer.

Lewis: Yes, that's just what I was just going to say.   I've done it, and that's where I've started.   There are still some debates, but he always seems to have the last word on the subject.

Tucker: He of course states it very cautiously as a conjecture, but to me the circumstantial evidence is very convincing that the conics came from the geometry of the sundial.   I use this, as it's almost worth a week in my geometric-concepts course.

I cite this as an example of the wide interest that I have.   I've been very much interested in matrices and linear algebra, and this took me to [Hermann G.] Grassmann.   Here, I found the best information I could easily find was E.T. Bell's *Development of Mathematics*.   I had known E.T. Bell.   I'd been at Cal Tech for a term in the spring of 1941, just following the visit to Austin I mentioned earlier.   Then from January until June I was living at the Atheneum at Cal Tech.   E.T. Bell was getting his meals at the Atheneum because his wife, Toby, had died just shortly before that.   He was terribly lonely and didn't want to go home to the empty house, and in the evening after dinner, I would get in conversation with him or he would get in conversation with me, and it would be peraps 1:00 in the morning when we would stop.

I learned much from these conversations that we had, about E.T. Bell and a little bit about everything else.   So although E.T. Bell is regarded, certainly in terms of things like *Men of Mathematics*, as much more interested in color than in history, nevertheless he had a very keen appreciation of the history of algebra.

It was through Bell that I learned that Cayley was anticipated, that the things that Gibbs called dyads were the means by which Grassmann developed the notion of linear mapping. He took these primitive products and then combined them, took linear combinations of them. Of course, in forming the product of two vectors if instead of multiplying rows into columns we multiply columns into rows, this makes a matrix. Now you multiply the i-th column of the first matrix into the i-th row of the second matrix, and this creates a matrix. Now you add all of these matrices together for i ranging from 1 to how many columns there are, which is the same as the number of rows of the second matrix. This is another way of defining the product of those two matrices. Well, this is the way that Grassmann worked; somehow or other it was an attempt to stick with vectors. I think the great accomplishment of Cayley was that he was willing to think of an array as an entity. So Cayley's accomplishment was very great, but in something like 1843, 15 years before the great paper of Cayley in 1858 was done, Grassmann had been doing the same thing, accomplishing the same purpose by these primitive matrices that he had—he called them open products or something like that, or indeterminate products.

I happened to run into Professor Gillispie a couple of years ago just when I was full of this enthusiasm of my discovery about how Grassmann had in a certain sense anticipated Cayley. I asked him, "Do you have a good article on Grassmann in the encyclopedia [*The Dictionary of Scientific Biography*] that you're editing?" "Well," he said, "it's strange that you should ask me that, because that was one of the articles that we had to postpone to a catchall volume, and I just recently got the proof sheets for it." He said, "Would you like to look at them?" I said, "Yes, I would." Well, I found that the article was incompetently written. Incompetent in the sense that the person who wrote it was just doing a job. He didn't have any feel at all for what he was writing about, and—he was a German—he had read what reference articles there were about Grassmann in German and had proceeded to write his thing. There's no reference to Gibbs. Do you know about this article?

Lewis: Yes. Go ahead. You're confirming what I think about it. I'm interested to get a mathematician's opinion about that.

Tucker: It was very inadequate. It didn't attempt to deal with Grassmann versus Hamilton, nor with Grassmann versus Cayley. I'm using 'versus' here in the sense of comparison, and I made this criticism after I simply said that I thought it was inadequate. Then Gillispie asked me for some specific criticisms that he could send to the author, so I gave specific criticism and also gave some references. You know there is just a superb paper of Gibbs—it was his retiring address, I think as president of the AAAS, something like that—on multiple algebra.

Lewis: I am not acquainted with that. It must be the one where he talks about Grassmann and about the fact that he discovered that he had been anticipated by him only after he had done his own work.

Tucker: Yes, he became an expert on Grassmann.

Lewis: Well, that's fascinating.

Tucker: This too is my interest in history. When I get interested in a particular thing, then I go back to try to find the roots of it.

Lewis: Well, that's exactly why I did my dissertation about Grassmann's Ausdehnungslehre. I had been studying classical tensor analysis at Austin with Homer Craig, I don't know if you know him or not.

Tucker: I do.

Lewis: Grassmann was one of his heroes, someone whose mathematics was not recognized and who turned to philology, an interesting man as well as being one of the geniuses of the century. So when I turned to history of mathematics I was curious to see exactly how Grassmann's work fitted in. But I was a little late in the Grassmann field to get a chance to write that article. If it had been a couple of years later, I think with the connections that I had, I could have done it.

But the author of the article I met in Hamburg. He was in after he wrote the article, and he had really turned away from history of mathematics. He said he would send me the proofsheets when he got them. He was very slow about it, and the exposition in those proofsheets seemed to me to be muddled, even in the notation. But he never even asked me for any corrections or anything, so I just sort of noted them. Only this past week have I seen the printed version in that volume.

Tucker: Yes, I haven't seen the final version. All I saw was the version that I criticized, so I'm curious to see how they differ. But apparently he was working with a student, and I can imagine him using the German technique of giving it to a student to do and just passing slight judgment on it.

Tucker: As a matter of fact, that was what Gillispie conjectured.

Lewis: Did he ask you to write anything for the *Dictionary of Scientific Biography*?

Tucker: No, no. And there wouldn't have been any use of asking me because I write only what I absolutely have to write. I don't like writing.

Lewis: What sort of things do you absolutely have to write then?

Tucker: Things that, if I don't write, I'm afraid they'll be lost.

Lewis: Whereas biographical articles and so forth could be done by other people presumably?

Tucker: You know if a promising student would with enthusiasm attempt to do a history of Princeton mathematics or of a period, I would

work with that person gladly. But when I sit down with a blank piece of paper in front of me, I think of all sorts of things that I would rather do.

Lewis: Well, yes, so do we all.

Tucker: But you can, I'm sure, tell that I'm enjoying this conversation with you greatly. So if whenever I have the opportunity to work on a personal basis with someone who is interested in the job, not just someone who is doing it to make some dollars, I would be able to spend hours and hours, and I'm interested wherever possible in trying to pin things down. I'm not going to be happy until I have pinned down exactly when it was H.S. Vandiver was here at Princeton and what he was doing.

Lewis: Yes, why he was here.

Tucker: In other words, two weeks ago I couldn't have cared less about this, but now that I know that there is somebody who is interested in the answer to this question and that I'm probably someone who can trace it down, I've looked up the son in *Who's Who* and he was born in 1925 I think. So if 1934 is the correct date, he would have been nine years old when he was living on Mercer Street. It's then clear that he can't be expected, just from his childhood memories, to know exactly what what his father was up to in Princeton.

Lewis: And the son has not followed his father's mathematical career.

Tucker: His son is an historian, isn't he?

Lewis: Oh yes, interested in Civil War history. He's admitted to me that he really was not aware of the stature of his father until I had cataloged his father's papers, which we had at Austin, and sent him the catalog. He then, apparently for the first time, reflected upon what his father was doing as a historical figure. That must have meant something to him, for the first time to have thought about this. I'm making a note about this question, because there is somebody still living in Austin who has a good memory for these sorts of things. That's H.J. Ettlinger.

Tucker: Oh yes, I remember him.

Lewis: He's still going strong. I think 89 years old. His wife is suffering somewhat, but he was there all of that time and knew the Vandivers well. He probably could explain that if it isn't in our papers.

Tucker: There were stormy periods, weren't there, between Vandiver and [R.L.] Moore?

Lewis: Yes. At one time apparently they did get along well, but then there are stories which I have yet to try to unravel. One of the stories has to do with the creation of the separate pure and applied

mathematics departments, and that Vandiver joined the applied mathematics department.

Tucker: Ironically.

Lewis: He was rather proud of the fact that he did that, because he would have nothing to do with Moore. But exactly how this happened is still a puzzling matter. You know Lucille Whyburn of course.

Tucker: Yes.

Lewis: She's been putting a lot of effort into answering questions like that about Moore and really has not come up with any real good answer about what was happening there. The people who were there then tend not to explain things very well.

Tucker: I've heard so many stories about Moore over the years, and my feeling for being in the rival camp is, of course, often to think the worst of Moore. But I think one can say without shadow of doubt that no American mathematician trained more successful students than R.L. Moore.

Lewis: Yes, that seems to be an incontrovertible fact. It still amazes me, what he was able to do.

Tucker: But he accomplished it in a rather Spartan fashion: it was sink or swim. He had many who started as students with him that one has never heard of. Even though the number of successful students is large, perhaps his batting average wasn't so good compared with Birkhoff, say. But of course it was a case where Birkhoff was dealing with a much smaller group of students, but probably a much more highly selective group of students. Let me tell you one or two of these stories to see if you have heard them. This is one that I got from a man at Rochester, Young, ..

Lewis: Gail Young?

Tucker: Gail Young, yes. Well, Gail Young told me this story of R.L. Moore. One summer R.L. Moore was teaching in summer school, a course in real variable. Many of the students in the course were secondary-school teachers who were trying to get a master's degree or something like this. He was following the customary Moore technique of giving axioms for the real number system and then theroems to be proved by the students, and he would tuck in an occasional ringer. So the day came to take up one of these ringers. The students had been asked to prove that in any set of real numbers there is a greatest. He asked for someone to go up to the board to prove this, and there were no takers. Have you heard this story?

Lewis: No.

Tucker: Finally there was someone who was clearly a schoolmarm, who held up her hand timidly and said, "Professor Moore, I have a

counterexample." And Moore beamed right at her to present her counterexample. She said, "Take a set of two real numbers. There is a greater, but there isn't a greatest."

Lewis: That's the sort of story I think Moore would delight in telling too, yes.

Tucker: And you know R.L. Moore taught at Northwestern for a year.

Lewis: Yes.

Tucker: This was just after he was married. I was told the story by [E.J.] Moulton, who was chairman of mathematics department at Northwestern. Two stories as a matter of fact. They went down to meet Moore, who had been married just a few days before that, and took him to Northwestern. Towards the end of the year word came to the chairman of the department that Moore was toting a gun. So the chairman involved Moore in casual conversation and found that, yes, he was carrying a gun and that in one of his classes, a fairly large class, he had come to feel that when his back was turned the students were making fun of him. So what he planned to do was, he was going to whirl around and catch one of the students making fun of him and he was going to shoot. Moore assured Moulton that he was an expert shot and that he would miss, but he would come close. He felt that this would end the matter. Well, the chairman of the department discovered that that room where the class met had to be renovated and that no other room was available on campus, so it was for these reasons necessary to cancel the last two or three classes in this course.

Now this is no second hand story. This is a story that I got directly from Moulton.

Lewis: I'd never heard either of them before. I must say that most of the new ones I hear are just clearly made up ones of various kinds, but there is interesting. Before I forget it, I must mention William Duren.

Tucker: Oh, I know him. Bill is a great friend of mine.

Lewis: Well, he came to visit us at Austin several weeks ago. He was travelling through Texas, the southern part, as opposed to the northern one where you got stuck, and they stopped by because Lucille Whyburn insisted that they do so. But I think they enjoyed it, talking about our mutual projects. He too has topics in the history of mathematics which he's taking on.