JAMES WALLACE GIVENS, ABRAHAM H. TAUB,

and ANGUS E. TAYLOR

(also LEON HENKIN, with ALBERT TUCKER)

This is an interview session on 18 May 1984 at the Berkeley campus of the University of California. Interview subjects are Wallace Givens, Abraham Taub, and Angus Taylor with, as they say in certain parts of California, a guest appearance by Leon Henkin. The interviewers are Albert Tucker and William Aspray.

Aspray: Since you can be here for only a short time, Mr. Henkin, would you say a little bit about how you were attracted to Princeton and some of your experiences?

Henkin: Well, I was an undergraduate at Columbia, Class of '41. I was interested both in philosophy and mathematics. The only person on the faculty at Columbia interested in modern logic was Ernest Nagel, the famous philosopher of science. There was no one in the mathematics department at all. Still, I was attracted more to the mathematical side of the subject. I remember that in my senior year F.J. Murray, who had surely been at Princeton in the '30s ...

Tucker: Oh, yes.

Henkin: ... collaborated with von Neumann. He got hold of a copy of Goedel's recently published booklet in the *Annals of Mathematics* series on the consistency of the continuum hypothesis. He thought it would be good if a few people just went through it, studied it, found out what the concepts were. There was no colleague of his in the math department interested, but he knew that I, an undergraduate student, had some interest in this area. So he invited me to work through it with him. That was really all my contact with mathematical logic during my Columbia years except for a single lecture by a refugee from Europe who appeared one day, named Alfred Tarski. Alfred was to bring me to Berkeley many years later.

Anyway, when it came time for me to leave my undergraduate years, I asked people around me for advice on where to go, and the names of Harvard, Princeton, and Columbia came up. Those are the places to which I made application. I was accepted at all places, but Harvard did not offer any financial help. Both Princeton and Columbia did. My Columbia professor said, "Well, you've been around here. You know, you've learned from us. Here is this exceptional logician, Alonzo Church, at Princeton. Why don't you go there?" So I did. Happily ever after, as they say.

When I arrived at Princeton and began taking a variety of courses, I saw how very meager was the mathematics instruction I had received at Columbia. Nowadays we recognize Columbia as quite a mathematical center, but in those days there were relatively few well-known mathematicians there and relatively few advanced courses. I did have an elementary differential equations course with Joseph Fels Ritt. I studied complex analysis and probability with Bernard Koopman.

Tucker: Paul A. Smith?

Henkin: No, I did not study with Smith.

Taylor: Did E.R. Lorch teach there or only at Barnard?

Henkin: He was separated off from us. He was at Barnard. Smith arrived just as I was leaving. At any rate I quickly found myself fascinated with all there was to learn at Princeton. But still I took, I considered, a relatively leisurely pace until December 7, 1941. I remember clearly coming into the Fine Hall common room. We gathered every afternoon for tea. It was a Sunday, but still we gathered there and learned about America's plunge into World War II. As a matter of fact I remember meeting Mrs. Eisenhart on the street the next day, and she asked me what did I think of it all. The reality was that I was afraid, I was afraid of what was going to happen to me and to the world. And I tried, innocent as I was, to express those ideas, but that's not what she wanted to hear. She quickly told me what I was supposed to say, "We must all do our duty and get on with it." Very quickly everyone in America was talking that way.

I also remember that I had a lecture by Hermann Weyl that same morning, Monday the 8th. At lunch a little while ago Al Tucker was telling us how in these days the professors at the Institute for Advanced Study have ceased giving regular courses. Even in that year Weyl was giving the course, and I have a notebook full of the details in my office right in this building.

Tucker: What was the subject of the course?

Henkin: I think it was classical groups.

Tucker: Yes, which he had already published.

Henkin: Yes, I have more to say on the subject. So he came to the front of the room at the beginning of the hour. It was 9:00. He said, "I know that all of you are very excited and upset and cannot let go of these great world events that have engulfed us. But," he said, "I've learned from my experience that in the most tempestuous of times, there is a great value in giving some of your attention and your energy to your continuing work. Therefore," he said, "I am just going to give the regular lecture now that I planned with you last week." So he did, and I think there is something of real value in those opening remarks.

Aspray: Do you remember particular people that stand out, or incidents?

Henkin: I had many, many great teachers. Certainly Emil Artin was an outstanding lecturer. It was easy to understand mathematics when he spoke. And yet years later, I found that much of the mathematics that I had learned in his courses had somehow slipped away. And I analyzed it as being related to his amazing technique of explication. He made things so clear that you didn't have to struggle to get hold of the ideas.

Tucker: Struggling helps you get ahold of the ideas.

Henkin: Yes. That effortless way in which the ideas came made them too easy to slip away. I probably learned more densely packed material from what we called the "baby seminar", in point set topology conducted by Arthur Stone. I learned more because he made us do all the work. He told us to find the definitions and find the theorems and find the proofs.

Tucker: This is something that I think has come up before in our taping. That is that the graduate students were very much encouraged to form informal seminars where they would teach one another. No attempt was made in the course offerings to cover all the things that a graduate student might need for the general examination. If graduate students were to complain that there was no complex variable being given this year, they were just told to go and form a "baby seminar", as it was called.

Henkin: I do remember one particular encounter with Professor Lefschetz, who was then the chairman of the department. He, in connection with this "baby seminar" in point-set topology, became inquisitive about these new babies that had arrived on the doorstep. So he sat in. It happened that I was giving my solution to one of the problems that Arthur Stone has set to me before, and being a logician I wanted to make all the details very clear and Lefschetz became impatient. As I got into some of those details he said, "Well, that's all obvious. Just go on toward the end." I was a very brash young man. I said, "Professor Lefschetz, it may be obvious to you, but I have come from an environment where a proof requires us to give all the details." And I just went ahead. I think although he was a little taken aback, that little exchange between us really was an entree into a lasting and solidly friendly relation between the two of us.

Givens: *Could I* add something to that. I had a similar incident in Lefschetz' topology class. He proved something one day, and the next day I looked at it and saw there was an error. Then the next time I said, "Professor Lefschetz, I think this theorem is incorrect." He brushed me off, but I persisted. I had even learned the term 'gegen Beispiel'. So I said I had a "gegen Beispiel" and that brought him up short. And he listened and agreed that there was indeed an error and sent out for Tucker to correct the mistake. I always thought that that was one of the reasons that I got a JSK fellowship the next year.

Tucker: Oh, I'm sure. Nothing impressed Lefschetz so much as people who stood up to him. That was my experience.

Givens: He was a very nice person.

Tucker: I made my good relations with him by standing up to him in the first course I took from him. It was very much like Wallace has described. He had no respect at all for the people who were meek and accepted his lashing.

Henkin: Well, let me give just one more anecdote which relates to a course of Professor Tucker's. I was by then a post-doc, and I had gotten very interested in dancing. I did folk dancing and ballet dancing and modern dancing, and when I wasn't dancing I was squirming. This gave Al an idea. He was teaching a course in some kind of topology, and it had come to the end of the term. Like every great teacher he wanted some dramatic incident to imprint the course on the minds of the young students. And so he connived with me to play a role.

So, by arrangement, I sat in. I think it was about the last lecture of the course. I sat in, placing myself right near the front where I could be seen. As the lecture flowed from Professor Tucker I began to squirm, first slightly and then more and more vigorously. I began scratching myself, and of course pretty soon everybody looked with amazement. Most of them didn't know who I was, or what was I doing, or why was I misbehaving so wildly. Finally Al turned to me saying, "Dr. Henkin, what is the matter with you?" I said, "Well, Professor Tucker, my vest is very uncomfortable." And he said, "Well then take it off." Whereupon I stood up and removed my vest without taking off my jacket, which was the whole point of the little skit.

Aspray: I see. Mr. Taylor, I know more about the backgrounds of some of the other participants today. Could you tell me something about the occasion of your coming to Princeton?

Taylor: Well, I got my degree at Cal Tech in 1936, and I wanted to get married. I was engaged. I had applied to Princeton, to the Institute rather, for a post-doctoral fellowship. I had applied for a Peirce Instructorship (at Harvard), and I had also arranged with friends to nominate me for a position in the Society of Fellows at Harvard. My teacher who would probably have stood me best at Harvard, W.F. Osgood, was in China at the time, and I was pretty soon notified that I wasn't likely to get a thing like that. I was still wondering what was going to happen when [Robert A.] Millikan offered me a job at Cal Tech. I took it because I wanted something certain, and I cancelled my applications. One day H.P. Robertson, who was at Cal Tech at that time, told me that he'd heard from Veblen, and Veblen wanted to know if Millikan couldn't possibly release me, because he apparently thought that I stood a very good chance of getting a fellowship at the Institute. Well, there were cross-currents, I think, at Cal Tech at that time. I don't think Bell wanted me to stay at Cal Tech very much, and he had very much encouraged the idea that I ought to go to Princeton. But nothing ever came of it. I didn't want to resign at that point without the certainty of going to Princeton.

So I spent a year at Cal Tech, and then I was encouraged to apply to Princeton again, which I did. I think Veblen came out that year and more or less told me that I ought to apply. He seemed to hold it against Millikan that Millikan had offered me the job in the first place. At that time I was interested in some of the things Salomon Bochner was doing, and so I applied and got the fellowship at Princeton. It was the fellowship of the National Research Council, not the Institute for Advanced Study. So I was a National Research Council Fellow, and I worked with Bochner. We wrote two papers together, and I published some other papers. I had a very good year and was appointed for a second year. My wife was pregnant and was threatened with a miscarriage, but it didn't occur.

Then I got an offer from UCLA. Well, I wanted ultimately to wind up on the West Coast, so I was very seriously interested in it, but I asked UCLA if they wouldn't hold it open for a year because I had already accepted my second-year appointment. But they said, "No, we couldn't do that. The University of California doesn't work that way." So I talked to Veblen and Eisenhart both about what I ought to do. I remember they said, "Taylor, all the mathematics that's really worth doing, you know, goes on on the Atlantic Coast. What the hell do you want to go to California for?" Anyway, I resigned my fellowship and went to UCLA. But while I was in Princeton I had a very useful time. My association with Bochner was close, but actually I was doing a good many things that were quite independent of him. He did get some ideas about things and wanted me essentially to write his papers for him. I suppose I should put it that way because he supplied about three-fourths of the ideas, told me what the theorems were, and asked me to prove them and write them up. We got two papers written that way.

Aspray: We haven't had many comments about Bochner. Would you like to talk about Bochner the man for a few minutes?

Taylor: Well, Bochner was an interesting person to me. I think I had read his doctoral thesis, which was published in German. I think it was on orthogonal systems of analytic functions, or something like that.

I was always interested in analytic functions. But it turned out he had passed beyond that particular sort of thing when I got to Princeton.

I remember my first meeting with Bochner. We rented a flat that he had lived in; he had just gotten married. We got to Princeton in the fall of '37. Bochner had been living alone in a second storey of a house where the family that owned the house lived on the first and the third floors. But he had recently gotten married and had moved somewhere else. We moved into that flat, and he came over there to see us very soon after we got to Princeton.

I remember thinking that Bochner was a very abrupt and kind of a cynical person, judging by his manner in general. When I started seeing him and talking about things, I would suggest something that I was doing and he would say, "That's too difficult. You can't do that." He was always, I would say, slightly cynical, and badgering in his manner. I don't know if this jibes with your impression of him or not, Al, but he always seemed to me to be jovial with a kind of a twist to his manner.

Tucker: I felt that it was a certain ill-at-ease feeling that he had which he was trying to cover up by his manner.

Taylor: I found as a matter of fact, he wasn't hard on me or anything, but he sometimes said things that would set me back a little until I thought, "Well, maybe he doesn't mean it in any personal way." I remember once, along in the late fall or early winter, I discovered something that was, I thought, astonishing. It amounted to this. Consider the application an operator-valued function of a complex variable to a vector. It maps one vector into another vector in a Banach space. If that vector-valued function is analytic, then the operator function itself is analytic in the uniform topology. In other words, strong analyticity is the same as uniform analyticity. I couldn't believe this at first, but it was true. I told Bochner, and he said, "Oh that can't be true. You made a mistake."

In fact it was true, and when I gave this paper at a meeting at Columbia in January, [Nelson] Dunford asked me a question, "What do you think about weak analyticity and strong analyticity?" I said, "Well, I haven't really thought about that." It turned out that Dunford had showed that weak analyticity is equal to strong analyticity. It's the same principle, it depends on the Baire category theorem basically. So I found my relationship with Bochner was profitable.

I read his book on Fourier series and learned a lot from that. One of the things that astonished me at Princeton was that I wanted to learn some topology. I read Sierpinski's book on point-set topology by myself. But I wanted to learn some combinatorial topology, so I went to a series of lectures that James Alexander was giving. Well, it wasn't a basic course. He was intent on using some new ideas that he had. Each time that he started he would explain carefully what the axioms were. It was an abstract basis for the thing.

Tucker: Was this on gratings?

Taylor: Well, I can't remember in detail, but every time we came to the next lecture he started it all over again. He said that he had improved it a great deal, and he wanted to start all over again. That went on for weeks.

Tucker: Well, he typically did that. And after a while the notice would go up that Professor Alexander would not meet his course that day. The meetings would get fewer and fewer, and finally a notice would go up that Professor Alexander would not meet his course until further notice. There would be no further notice.

Taylor: That really astonished me because I had heard a great deal about Alexander and about things that had made him famous. But that was the situation.

Tucker: Well, he was such a perfectionist.

Givens: I remember one spring, early spring I guess, when it seemed that he was going off for a skiing vacation in the middle of the course. Now Veblen once made a comment about Alexander, a quiet comment, he wasn't talking a lot of details, but he said, "Alexander really did some very good work once." He didn't talk about current work, or anything else, but he more or less defended the fact that he was on the faculty, by saying he had done first-class work.

Tucker: Well, Alexander was a protege of Veblen. And it was with Alexander that Veblen got involved in analysis situs and finally did a colloquium volume on analysis situs about 1920.

Givens: Was Alexander more the originator of that subject than Veblen?

Tucker: Veblen got interested in analysis situs through the second volume of his projective geometry.

Givens: I see.

Tucker: The first volume is very well known, but the second volume is much less so. He got tangled up in the second volume on questions of orientation. In the projective plane clockwise becomes counterclockwise as you pass through the line at infinity, but in projective 3-space right- and left-handed frames remain unchanged as you pass through the plane at infinity. If you read in the second volume of projective geometry, you will see the knots that Veblen got himself into. That was what launched Veblen into analysis situs. Then at about that time he had Alexander as a student. Alexander then took off from this somewhat clumsy start that Veblen had made.

Givens: There was a story about Veblen's giving a popular lecture on something to do with topology. He took slips of paper and twisted them and fastened them together and took scissors to cut them apart, and he had predicted the result incorrectly. Someone in the audience like Lefschetz was trying to straighten him out on it. On the other hand, I'd always had the impression that Veblen really had quite a lot to do with the foundations of topology.

Tucker: Oh, absolutely. He should be credited as the person who began the Princeton school in topology.

Givens: This is interesting to me because in the years that I was associated with Veblen I don't recall him ever discussing anything whatsoever about topology with me.

Tucker: Yes, but Veblen went through all of geometry. He was always looking for something new. I guess the last thing that he got involved in was spinors, wasn't it? He started with his thesis on axioms for plane Euclidean geometry. This was at the University of Chicago, with E.H. Moore. And the next year he actually supervised the thesis of R.L. Moore on another set of postulates, but then he decided that projective geometry was more embracing than Euclidean geometry. Then from projective geometry he got into analysis situs, and then with the advent of general relativity he became interested in the geometry related to general relativity. He was in that stage when he had T.Y. Thomas and J.M. Thomas as students. Later on he got into some foundations of differential geometry with J.H.C. Whitehead.

Givens: I think we've gotten away from Bochner, and I would just want to get in one brief story that might throw a little different light on Bochner as a person and also on the nature of the work at Princeton in those days. I don't remember examinations at Princeton with the exception of the final oral-examination. A few people had the great distinction to take two final orals, in mathematics and in physics. Taub was an example. All of us were very impressed with that. We were in some awe of that.

I had little association with Bochner. I suppose I attended some of his lectures, but I don't remember much about them. At any rate, I remember him being on my final oral-examination. I don't remember any stress in the five years I was at Princeton, but I suppose the oral examination would come as close as anything. Anyhow, I saw Bochner shortly before the examination started, and a few casual words were exchanged in the course of which he said, "Oh, these examinations, the first five minutes the committee finds out whether the candidate thinks like a mathematician, the rest of the hour is spent at convincing him that you have found out." I think that's pretty close to being verbatim. I was rather reassured.

Taylor: Well, Bochner and I maintained an association for a good many years actually, although very sporadically. I was invited back when they had some kind of an affair [a symposium to honor Bochner on his 70th birthday, held April 1-3, 1969]; they published a volume in his honor, and I have a paper in that volume [*Problems in Analysis*, ed. R.C. Gunning, Princeton University Press, 1970]. By that time I'd become interested in history of mathematics to some extent, and Bochner, after he went to Rice at least, was seriously interested in that.

Tucker: Oh, he was interested even before he went to Rice.

Taylor: Oh, I didn't know that.

Tucker: He was involved in the Program in History of Science here.

Taylor: I know that he and Charles Gillispie worked together. I know that Gillispie had a high regard for Bochner.

Tucker: Oh, very.

Taylor: Another thing I remember of some interest was that there was a kind of a current-research seminar in which people were invited to report on current publications. It seemed to be partly Institute people and partly University people.

Tucker: Yes.

Taylor: I remember Bochner asked me to read and report on some of the work of J. Schauder, the Polish mathematician, from which I learned a great deal actually. But one of the things that I remember about that seminar in general was that almost every time after things had gone sort of toward the end of the hour, Weyl would have to get up and explain it all in much simpler terms, clarify the whole thing for everybody. That was good, as a matter of fact, but it was my impression that he just couldn't resist trying to put it in his own terms.

Givens: Was this a certain sort of a current literature? I remember that Weyl protested on one occasion that there was quite a lot of good mathematics being done which wasn't being looked at at Princeton. That may have been one of the reasons for his later establishing a current-literature seminar.

Tucker: Yes.

Taylor: This was 1937-38, about the time that Bourbaki was coming onto the horizon. There was a young graduate student there with the name of John Tukey. Tukey and Ralph Boas and Frank Smithies, an Englishman, were there that same year. All of us got very interested in what kind of things were coming out in Bourbaki. We learned, you know, where the name came from, and who the people really were to some extent. We got together regularly to talk about compactness of the unit sphere in a Banach space with the weak topology and that sort of thing.

Tucker: It was out of this group that the lion-hunting article came.

Taylor: I guess, I don't remember who wrote that now.

Tucker: Well, it was written under a pseudonym, H. Petard.

Taylor: That's right, I'd forgotten about that. Probably it was Tukey, wasn't it?

Tucker: Oh, it was a number of them but Tukey was certainly influential.

Taylor: I think I had left Princeton by the time that actually was published. Tukey had not yet become especially interested in statistics at that time.

Tucker: Oh no, that happened during the war, due to the war work that he was involved in.

Taylor: Bochner asked me to teach his course in differential equations a couple of times when he wanted to do something else, which I did. Some of the people who were at Princeton or the Institute that year, whom I got to know just informally, not really mathematically, were interesting. Aurel Wintner was there, a very strange man. He was visiting, on leave from Johns Hopkins I think. He and his wife had had their first child. He couldn't drive a car, she had to drive the car. She would occasionally ask him to hold the baby, and you would have thought that the baby was something breakable; it made him exceedingly nervous. He didn't seem to know how to hold a baby or anything.

He was a very pleasant and nice man, but he was extremely nervous. Let's see, who else interesting was there that year? Hlavaty, the Czech, was a man I found interesting. He was a differential geometer, Al, wasn't he?

Tucker: That's right.

Taylor: Hlavaty told a story once that I thought was very funny. He had taken the train to go up to New York from Princeton Junction, and he heard the conductor going through the car saying, "Newark, Newark". He thought it was "New York" and got off the train.

Tucker: We had another Czech around at about that same time by the name of Cech.

Taub: He was earlier.

Taylor: I don't think I met him.

Tucker: "Eduard Cech, by God's grace,/ Is the only man on earth to trace/ The sordid and dreary cohomology theory/ Of a sub-normal bicompact space."

Taylor: I mentioned most of the people that are worth taking the time for here, I suppose. Charles Morrey was there. Morrey and Tony Morse both wound up here in Berkeley, and both died fairly recently, Morrey just a very short time ago. But it was a very worthwhile year in my career.

Aspray: Professor Taub, would you tell us something about your going to Princeton? Why you chose to go there, and the occasion of it?

Taub: Well, I'd taken an undergraduate degree in Chicago in '31, and I really didn't know much about the academic world. After I got my degree, I wrote and asked if could I come to Princeton. I didn't hear anything until it came time for classes to start. So I wrote to the graduate school and said, "What about it? Am I admitted or not?" They said, "Yes, you can come." But by that time I was just one of the supernumeraries, I guess. There was the quota at that time, and I guess Eisenhart had been away for the summer and didn't answer letters that came after he'd gone on his vacation.

So I got there about in '31, shortly after Fine Hall was open. I was little overwhelmed by how nice a building it was. I wasn't aware that Veblen had spent, what, over two years designing it and arguing with everybody about every detail in it. I had become interested in relativity when I was at Chicago, so I knew what courses I wanted to take. I quickly realized that H.P. Robertson was the man to get in touch with. So I got acquainted with him and went to his courses. I've been trying to remember the people that were graduate students at the time. We've mentioned some of them. You had just gotten your degree I think, in '30 or '31?

Tucker: No, I didn't get it until '32. So the year that you arrived was my last year working on my thesis.

Taub: One person that we haven't mentioned that was there at the time was John Vanderslice, who was sort of a noteable character. I don't know what's happened to him, do you?

Tucker: Oh, he's dead.

Givens: I think he committed suicide.

Tucker: Yes. He said at the time he was a graduate student that he was going to commit suicide when he got to be thirty because he felt that a mathematician was done at age thirty.

Givens: He was one of the rather colorful figures. I remember that he on one occasion—I don't think it was because of any lack of money—he decided to try a diet consisting of blackstrap molasses and milk, being possibly healthful.

Tucker: Do you remember the occasion when in Fine Hall—this was in the evening one time—there was a musical contest? There were various people doing various things. Henry Whitehead sang a bawdy British ballad, and Vanderslice whistled a movement from a Tschaikovsky symphony, and Dwight Marfield did a tap dance.

Taub: He was the guy whose head was very peculiar.

Tucker: Yes, very pointed head.

Taub: He always looked as if he was wearing a cap, but that was his head.

Aspray: May I ask you, Professor Taub, if you got any advice from the people at Chicago about where to go to school?

Taub: No.

Givens: Could I interrupt though and say I think Taub's a little too modest about the considerable distinction that got him to Princeton. Henry Wallman once claimed to have looked into the matter while we were graduate students. He said that one in three of those who applied for entrance into the graduate school in mathematics was admitted, and one in three of those who were admitted stayed to get the Ph.D. Now those are obviously inexact figures but that's what Henry Wallman believed to be true.

Tucker: Well, I would agree with roughly the first figure, but not with the second figure. No, we had an exceptionally good record of people finishing.

Taub: There were a certain number who got a master's degree to leave.

Tucker: Yes.

Taub: There's one man, Frank Cubello, who never finished.

Tucker: No, he got a job as actuary with Prudential in Newark.

Taub: Yes, but he was there that first year. Well, at Chicago I had taken a degree both in math and physics, and I tried to do the same at Princeton. But before I got my degree with Robertson I'd gotten interested in the kind of things Veblen was doing. And the stories about Alexander starting a seminar and not finishing and not getting very far at the end of one day go for Veblen too, I think. But before I got my degree I worked out some things with Veblen, and then I went back to H.P. Robertson and got my degree with him, working at some problems in cosmology.

Aspray: Veblen's interest in mathematical physics was through his interest in geometry and geometrical relations in physics?

Taub: Well, there were two things to study in those days in mathematical physics, either quantum theory or relativity.

Givens: Aren't they still?

Taub: Well, people had been trying for years, to meld them Wallace, but many of those people had just as well not done anything. At any rate, since I had been more interested in relativity when I came, I sort of leaned toward that side of things, although I did study some of the quantum theory there. In fact my thesis was on solving Dirac's equation in cosmological spaces, which was a mixture of both.

Tucker: With E.U. Condon the second reader.

Taub: Yes. It happened, and I've been stuck in that sort of thing since, although during the war and later I got to doing some other things.

Tucker: Well, you came back around 1940 in the physics department.

Taub: I came back in '40-'41 in hopes of finishing a book with Veblen, but we didn't. That summer Wallace and his family and I and my family went up to Maine to be close to Veblen, because we thought we might even be able to finish it then, but we didn't. Then in March '42, I came back to work at Princeton doing some classified work, or working in projects connected with the war effort. At the end of that time I went back to Washington for one year, and then came back to Princeton, to the Institute, on a Guggenheim. Then I switched to the University of Illinois and worked in computers until about '64 at Illinois.

Tucker: What got you into computers?

Taub: Well, two things. One was I got Louie Ridenour. Well, what really got me into it in the first place was that during the war von Neumann and I were worried about what was called Mach reflection. So we tried to get ways of solving equations of hydrodynamics. Of course with that area there were some problems that one could reduce to algebraic problems describing reflecting waves. But these involved solving whole systems of polynomial equations of high degree. So I arranged to get on the ENIAC, and Goldstine's wife was helpful in coding the ENIAC to solve the equations. That was my introduction to computers, but I must say it was sort of done, in one word, remotely. I mean Adelle Goldstine really did it; I just told her what I wanted done.

Givens: What enabled you to get out, sheer determination? You once told me you were very much happier once you got out of there.

Taub: Well, at that time Louis Ridenour, who was actually an undergraduate at Chicago at the same time I was—we were friends from those days—came to Princeton, presumeably because Enrico Fermi was supposed to come to the Institute. The Institute thought that they ought to provide Fermi with an experimentalist as an assistant.

Taylor: This was after Ridenour got his degree at Cal Tech? [I'm pretty sure that Louis Ridenour was a grad student or post-doc at Cal Tech some time in the period '33-'36. He *was* called Louie. A.E.T.]

Taub: Yes, it was just after. It turned out that Fermi did not come to Princeton. But Louis was there and was in the physics department. We saw quite a bit of each other in those days, and then after the war he became Dean of the Graduate School at the University of Illinois.

They thought they needed a computer, and in fact they started some efforts to get one there. Louis looked at these and decided that he wasn't quite sure that that was the way they ought to go. So he said, "Why don't you come to Illinois and see what ought to be done about computers? You published a paper involving computers, so you must know something about them." I thought about this proposal and wrote to the people back at Washington about it, and before I knew it they said, "Gee, we can't compete with them. Good luck." In other words, Winger accepted my resignation before it was tendered. So there I was in computers.

Tucker: That gives you something in common with Al Hague.

Taub: So that's where I was, and I worked with computers until I came here to Berkeley in '64 and worked both in the mathematics department and the computer center. In '68 I thought I had done enough for the computer center, so I came back to the mathematics department and spent my time doing relativity theory. It was a great game.

Aspray: I'd like to trace sort of your early history, because the two of you have many things in common.

Taub: Indeed.

Givens: I take it you'd like to hear more or less why I went to Princeton.

Aspray: How you came to Princeton.

Givens: I think maybe its illustrative of the spirit of the time. The reason I went to Princeton was a single sentence said to me by a professor in mathematics at the University of Virginia. Let me give you a little background. In 1928 I graduated at age 17 from a small college from which my parents had graduated in 1908, soon after the founding of the college. While I was there the college was accredited. I majored in physics because there weren't enough mathematics courses offered for a major in mathematics. It wasn't such a bad idea, the professor of physics was a very good man, and he got me a fellowship at the University of Kentucky where they had never given a Ph.D. in mathematics. But they gave me an assistantship in physics, and I went there for a year. Partly because they had not given a Ph.D. and partly for personal reasons. (I thought incorrectly that I was going to be visiting Lynchburg College to see a female student still an undergraduate there.)

Anyway, I went to the University of Virginia after one year at Kentucky. I entered the physics department without financial support. My parents were being highly supportive, both were teachers. At one time my father left the teaching profession to earn a living. Then after a year in physics and a brush with experimental physics under a man named Beams, who I guess was distinguished, but whose idea of lecturing on Page's theoretical physics was to insist that the students keep the book closed while he kept his open and copied on the blackboard. So I had a fellowship in physics when I took a master's degree under Ben Zion Linfield, a remarkable man. He's not listed in record books. He had a Ph.D. from Harvard after coming to the United States as a young boy in his teens, got off a cattle boat or something.

Aspray: From where?

Givens: I don't know, Poland possibly, somewhere in Europe. Ben Zion Linfield was his name. This is a little digression, but it does give the spirit of the times and what could happen in the United States in those days. He got to Harvard very quickly. I think he didn't know English when he got off the boat. In a short number of years he had a Ph.D. in mathematics at Harvard and came to Virginia. I think partly as a result of personal tragedies he never really did a lot, but I've always been grateful to him for starting me off with some knowledge. A remarkable man.

Okay, I obtained a master's degree in mathematics while I had a fellowship in the physics department. Things were that casual at the University of Virginia. I was in no pain and stayed on for another year in mathematics. A man named Echols was one of the early editors of the *Annals of Mathematics*, when that was published at the University of Virginia. He was old and ailing, and he had a course in complex variables but he wasn't able to meet the classes. I had had some work in complex variables at the University of Kentucky, but I was taking the course because there were few choices. So I took over and conducted the class for him, although I was a student in it. Then later, perhaps more or less as a consequence of that, I talked with him—just a casual conversation—but I think I remember almost verbatim what he said. He said, "I would rather have a good recommendation from a professor at Princeton than a Ph.D. from the University of Virginia." And he'd been there for 50 years. So I applied to Princeton.

I didn't know much about Princeton. The communication in those days was not like it is now with television. I had gotten acquainted with the *New York Times* as an undergraduate but, never mind, I didn't know a thing about Princeton really. But I promptly wrote to Princeton and was duly admitted. My family supported me for a year. Then I got a JSK Fellowship for the second year, and for the third, fourth, and fifth years I was paid by the Institute for Advanced Study as Veblen's assistant.

Aspray: What does 'JSK' stand for?

Givens: I don't know. When looking over my old files, which should have been thrown away decades ago, I found a letter which I wrote someone and the polite reply was that no more was to be known about it. It was not public knowledge as to who had given the money.

Tucker: It was given by an anonymous donor, and I have no idea what 'JSK' stands for.

Givens: Let's see now, the reason I started to work with Veblen is that ..., but let me go back a moment. A few weeks after I first arrived at Princeton in the fall of 1932, the graduate students were called into Lefschetz' office. I have a high regard for Lefschetz, so please don't misunderstand this. He made a graceful and informative talk as to the what the graduate students were expected to do and what was available and so on. That was fine. At the close of this he asked for questions. I'd had four years of graduate study, although I had started with an undergraduate degree from a small college. Anyhow, I asked him how one got started on writing a dissertation. I shall never forget, he was very polite about it but was quite firm in saying, "Once you stay around a year or two, these things come naturally." I don't think he had the faintest idea that I had graduate work before I came to Princeton. I don't think he looked into it at all. He may have, but there was no indication of it.

Tucker: Thoroughness was not a characteristic of Lefschetz.

Givens: He was indifferent to that matter. The fact was that he was firmly in control of his scientific and scholarly activities. Well, let me just interpolate an anecdote which I heard. Lefschetz was supposed to have been on a trip somewhere, accompanied by a young mathematician. He had mechanical hands because he had lost both of his hands in an accident ...

Tucker: An accident in East Pittsburgh, Pennsylvania, working for the Westinghouse Electric Company.

Givens: At any rate, the younger mathematician asked if he could do anything for Lefschetz while he was on the trip. And Lefschetz said, well, had he gotten his tie tied quite correctly and perfectly. He was so self-sufficient with his mechanical hands you wouldn't believe it. That was a certain inspiration too, because you saw what you could do if you tried. He did have to carry his own chalk when he lectured in Russia. They had lump chalk and he needed stick chalk to fit into his hand.

Okay, to get on with things, I think Lefschetz may have had, well excuse me, I know quite explicitly that Lefschetz had a great deal to do with my eventual work with Veblen. One day at tea in the afternoon in Fine Hall, Lefschetz spoke to me abruptly, as was his way, and said, "Go see Veblen. He wants someone to work with him." That's the first contact I'd had with Veblen of any sort. I went to see Veblen, and he had a complicated identity, all in tensor notation. I guess he wanted to see if I could read the formula. He asked me to check whether it was correct or not. I did, and it wasn't. I corrected it and from then on I worked with Veblen. This was in the spring, during my second academic year at Princeton. The first year passed pleasantly with the seminar with Lefschetz and attending lectures, but there was nothing very notable I suppose.

The second year I started working with Veblen. My association with him will not I think be quite completely understood unless I explain. You can be an assistant to someone who's busy, and he sees you occasionally and says "Well, what have you been doing?" I think that's a splendid arrangement for many people under many circumstances. But this was not the way it was with Veblen. I think it was only after the second year that I got a desk in a shared office, shared at least part of the time with Wilcox.

Tucker: Roy Wilcox.

Givens: He shared with me the office next door to Veblen's corner office in Fine Hall. The standard way of working—Taub can correct me if my memory fails in some of these matters—was that Veblen would come in in the morning and do whatever he wished to do in regard to his correspondence or whatever. Then he'd step outside his office and knock on the door immediately adjacent, and I would respond. I was waiting. I would go into his office and spend some hours. My memory is that this happened pretty regularly, five days a week for more than three years, I guess.

Taub: He would come back in the evenings and half expect you there.

Givens: He was not a harsh taskmaster or a demanding taskmaster. He was a demanding taskmaster in some ways, but he was not lacking in consideration. For example, he never pressed me into service for weekend forays to provide walking paths along the Princeton canal. He must have gathered that I wasn't enthusiastic about such activities and that there were social activities which I preferred. While he was quite prepared to press Dirac into such matters—Dirac probably being quite willing—I never had to do that. I always thought he showed a great deal of consideration in that.

Taub: Let me just interject. There was one time when he came in on a Monday morning with bandages around his wrists. It turned that he had run into a patch of poison ivy or poison oak.

Givens: I was about to say something that might be related to that. It may be that I told him that I was highly allergic to poison ivy. In fact, many years later I was in bed with an attack as a result of clearing a camping site for my son.

Taub: He was shamefaced about that, because he either was immune or he knew better than to get caught.

Givens: He would have expected to know perfectly what to do. He was a perfectionist in that sort of thing. He accepted responsibility. He was that kind of person.

For the years that we are concerned with here, the most significant scientific remark I suppose I can make is that Veblen and von Neumann conducted a seminar jointly. Von Neumann must have done it out of courtesy to Veblen. I don't think that he could possibly have really been interested in doing it. But Veblen, I think, had a good deal to do with bringing von Neumann from Europe in the first place. Anyway, there was a seminar conducted by Veblen and von Neumann. I do not recall von Neumann at any time speaking in it. It didn't attract many people. This gives an idea as to what eventually happened as to the documentation of the seminar. There still exist bound mimeograph copies of the results. I've often thought in the intervening decades that Taub would quite rightly think that he gave 60 or 70 percent of the lectures. I'm quite sure that I gave more than 50 percent of the lectures, and Veblen would probably have laughed and thought that we were overly ambitious and yet he told us both, or at least me, what to say and prepared the lectures himself. At any rate, the lectures went on three times a week for, what, two years. My memory isn't clear about the duration of it, although the records would show that '35-'37, I suppose. Is that about right, Taub?

Taub: Well, I think it was about '35. I don't know what happened in '36-'37 because I went off, you see, to Washington.

Givens: You know, I'd forgotten that.

Taub: I wasn't there for the second year.

Aspray: Was Veblen with the University at the beginning of the Thirties, or at the Institute?

Tucker: No, Veblen was at the University at the beginning of the Thirties. He moved to the Institute in beginning of the year 1932-33. He was with the Institute for a year before the Institute really got started.

Givens: There is a record for 1930-54: "The Institute for Advanced Study. Publications of Members 1930-1954" (Princeton, New Jersey; 1955). [Some misstatements have been corrected in proof after a more careful reading of relevant lists in this volume. J.W.G.] It contains lists of the appointments to the Institute for Advanced Study. The first appointments (in 1930) were were Frank Aydelotte (secretary and later Director), Abraham Flexner (Director), and several trustees, including of course Louis Bamberger and Mrs. Felix Fuld. Veblen's appointment is listed here as 1932. Einstein's was 1933. I stand on the record as I read it, but I don't claim that the record may not be inaccurate.

Taub: Well, my memory is that Aydelotte didn't show at all in that period.

Tucker: I bet that this is a misprint.

Givens: It could well be. I would be delighted to learn that, because I thought it was an absurdity the first time I read it.

Aspray: I'm almost certain that what happened was that Aydelotte was brought in as an administrative replacement when Flexner decided to retire. He'd been at Swarthmore until that time.

Givens: Hopefully there is someplace that that can be clarified. ["Secretary of the Trustees 1930-36" is the listing on page 269 of the reference. J.W.G.]

Taub: You know, I said one remark over and over again to myself after looking back at those years of our struggles with Veblen to get something out. I think our problem, particularly mine, was that I tried to give him something what he wanted and he didn't know what he wanted. He couldn't say what he wanted. Over and over again we wrote one version after another, repeatedly, and never got anywhere, simply because he never knew what he really wanted. I didn't have enough guts to stand up to him and say, "Look, this is it."

Givens: Neither one of us ever really came to grips with this great man in that sense. On the other hand, the fact that he finally asked that one of the many repetitions of typing of the manuscript be labeled printer's copy was his recognition of the fact that the troops were about to rebel. Certainly he was conscious about it, let's be a little bit blunt about this. Veblen after all had done great work. People forget that there is one accomplishment which is so rare in mathematics one hardly identifies it. That is to complete a subject so you don't study it anymore. That's what Veblen did for synthetic projective geometry.

In my long years of close association with Veblen, I think there was only one occasion when he really said something complimentary of his own work. He wasn't that kind of person. But the one time he did. He said, "When *we* did that," and he was referring to his coauthor [J.W.] Young, "they didn't think it could be done." You see people knew very well that you could start with numbers and create geometry. But it is something else to believe deeply that geometry can be used to create numbers, to get rid of the discrepancy between the two subjects. This was what Veblen did. I've always valued that very highly.

Tucker: Yes.

Givens: Incidentally, the Veblen and Young book. I don't know much about Young, except that I think that he gave up before the first volume was anywhere near completion.

Tucker: He didn't participate in the second volume.

Givens: And Veblen would emphasize the word 'we', instead of 'I', when he said, "We were seven years on the first volume." Then he would pause a moment and say, "That was when I was young." So you see when we were working with Veblen he really couldn't originate anything of great magnitude. Now let's not be coy about this. The thing we were trying to do, though we never talked about it in this language—Veblen would have dismissed it as being too majestic—was to get a mathematical foundation for relativity which would unite it with quantum theory. Einstein was working on the unified field theory. We never spoke of unified field theory. We did not want to be guilty of *lese majesty*! Taub was the physicist. I was a member of the American

Physical Society starting in 1929, and in 1934 I got a letter which amused me greatly. I'd been named a Fellow because I published a couple of pages in the *Proceedings of the National Academy,* but that's the end of my contact with physics. I'm not a physicist.

Taub: They didn't take it away from you.

Givens: They never took it back, I guess, but I was hugely amused. We simply weren't physicists. Taub was, and he would perhaps have done us all a service if he'd said, "Get the hell out of the subject, because you can't do that, it takes more physics than you've got."

Anyhow, the basic concepts of the subject were essentially to try to do linear algebra as a foundation for the geometry. With Veblen it had to be geometry, but it was linear algebra and matrix algebra and at a level which was just too low for the ambitious attempt. The work then went on. Von Neumann sat in the back row and did his own work. I suppose he didn't waste too much time. In spite of the fact that he was there, he went on about his own work. He was also incredibly informed I think, as to what went on. I had even in those days the sense to realize that he probably knew more about what was going on in the lectures than the lecturer and didn't need to listen very much.

May I get in one anecdote. I have always appreciated this as an excuse for my own career in a way; after all there was no place to go but down. I was lecturing on some correspondence between groups of matrices. The correspondence was such that plus or minus A corresponded with plus or minus A prime. I had these on the board and so on. Von Neumann with a quite unusual interruption said that he knew that this two-to-two correspondence between the groups could not be sharpened. He even knew how to prove it, but the methods used were infinitesimal and had to do with the infinitesimal group. He thought that was inappropriate. Well, von Neumann and Veblen discussed the matter quietly in the back of the room and—I was a graduate student—I kept my mouth shut while this went on. When they stopped I called attention to the fact that I had matrices A and B which commuted and that they corresponded with matrices A prime and B prime which anticommuted. Then I hoped someone would say, "Oh, that's very nice." But no one said a word. I waited and then went on, but I always thought that was a very good incident for a graduate student faced with such extremely weighty people. More importantly, the "obvious" is not at always obvious to "the best and the brightest".

By the way, I think in our discussion so far we have missed something in regard to those days. Let me go back a minute. When Hermann Weyl left Goettingen—the dates are easy to find—the local newspaper in Goettingen published an editorial calling on the government to make every possible attempt to reverse the decision of Hermann Weyl to leave to go to Princeton. I suppose that everyone will be aware that this was because of the influence of Hitler's terror.

Tucker: Yes.

Givens: But the editorial called on the government to try to keep him there and gave as the reason that Goettingen had been for a century a center of the mathematical world and that distinction was being lost.

Aspray: What year was that?

Taylor: Well, it was before '33.

Givens: I would have to refresh my memory to be certain of the year in which Weyl came to ...

Tucker: Weyl came to Princeton in 1933.

Givens: Well then, it was just before that.

Tucker: He'd been there, of course, earlier on, in the year '28-'29.

Givens: If anyone had a copy of that editorial I think it would be a very nice thing to put in the record.

Taylor: In the Goettingen newspaper?

Givens: Yes. Now someplace in the intervening decades, it could have been while I spent almost all of 1974 and a couple of months of 1975 at Munich, as an awardee of the Alexander von Humboldt Stiftung. It could have been then. I am quite indefinite as to when this conversation took place, but some European mathematician—I don't remember who it was—spoke about those days and the discussions that took place in Europe at that time. He said that the debate was over whether Veblen or George Birkhoff was the greater mathematician. Veblen once remarked quietly, with amusement, that when he and Birkhoff used to go from Princeton up to New York for mathematical meetings he had to dissuade Birkhoff from presenting a solution to the 4-color problem. This was uncharacteristic of Veblen but he made such a remark. So anyway, this was the debate in Europe as to which was the greater mathematician. A rather absurd discussion.

It was in the same type of conversation that the remark was made that Europeans recognized that the signal of the change that was taking place in the world was when von Neumann left Europe to go to the New World. This was a signal that the center of power in mathematics had moved. Von Neumann was of course not flamboyant, but I heard someplace that he was quite a playboy in his early days.

Tucker: Oh yes.

Taylor: What was von Neumann's official status in Europe before he came to Princeton. Was he a professor?

Givens: That I don't know.

Taylor: I'm curious to know whether he really had a professorship or not.

Givens: I guess he was young enough that that would have been very difficult in the rigid system in Europe in those days. Isn't that true?

Tucker: I don't think it could have been professor.

Aspray: I think he was a Docent.

Givens: Is it true that the professorships in mathematics at Princeton were not all that easily had, and that the money for them not all that easily divided, and that von Neumann and Wigner shared a professorship offsetting their visits to the University?

Tucker: That's right.

Tucker: But Wigner's appointment in Berlin was at the Technische Hochschule and von Neumann's was at the University.

Givens: I see.

Tucker: Because in the interview we had with Wigner he said this, and he wasn't making any mistake. It surprised me.

Taub: The story I heard years ago was that Weyl came for the year '27-'28 or '28-'29, and there was an attempt to keep him at Princeton but he refused. I expect because he had expectations for the Goettingen job then. I think he came from Zurich originally.

Tucker: That's right. That's how Bohnenblust came in. Bohnenblust came as Weyl's assistant.

Taub: Then Weyl was asked who he would recommend. He recommended von Neumann and said, "That's the man you ought to get." So they tried to get von Neumann. Von Neumann would come for only half of the year, and he also, I think, made the stipulation that Wigner get the other half.

Tucker: Yes, I don't know that to be a fact, but it seems to me that it fits with what I know.

Taub: Well that's the picture I'd always had of the thing.

Givens: Well, the shift of power from the old to the new was perhaps due to Hitler.

Taub: Oh yes.

Givens: I said 'perhaps' only because there are many causes for Hitler and for the turbulence in the world.

Tucker: Well, there were two things. A shift came because of Hitler, no doubt about it. But that was in a sense just the straw that broke the camel's back. I think the movement was occurring already.

Givens: Aren't you saying that there were forces at work in Europe which allowed a reasonably sensible people to commit such atrocities?

Tucker: Yes.

Givens: One other thing I remember in those days was von Neumann. I was reminded of an anecdote because I was reading the *New York Times* when it arrived last night at 10:00. We now get the *New York Times*, here, delivered at 10:00 in the evening.

Taub: You don't have to get the night edition by going to Times Square to pick it up.

Givens: No, that's right. I lived in New York for a while, and used to walk up to get the Sunday edition Saturday night. Anyway, it was while we were at Princeton that Roosevelt closed the banks. We were graduate students in the Graduate College, and young males kidding one another, one way or another. Rosser one morning said he had to stop by the bank to cash a check. He needed some money. He was roundly abused and made fun of because the banks had been closed for some days. And he didn't know that. He was unaware of the environment and didn't need to be aware of it. Now none of us was particularly inconvenienced. I certainly wasn't. My room and board was paid for, and I didn't need money.

Taub: Well, there was always Tucker that you could go to to get some money if you were really strapped in those days.

Givens: I didn't know that. I missed that source.

Taub: I don't know how many times he bailed us out.

Givens: Well, I was fortunate. Now the thing that makes me remember that is that the lead column in the *New York Times* today is about the run on the Illinois bank. The times have a way of repeating themselves. And I tell you that I had hoped to make a little more preparation for this discussion, but I was so fascinated by reading what was going on in the Iran/Iraq business and the political scene and the bankrupcy and the $700 billion overhead and debts and so on. But that's enough of that. I'm just trying to say that those days were different from what one thinks of them, unless one remembers the economic chaos—if it's not too strong a word—of that day and the organized complexity of the present day are rather different.

For example when Al sent me the list of people on the faculty at Princeton in those days, I was a little taken aback when I realized that the list of only the faculty paid for by Princeton University would not have given anything like the flavor of the times that was provided by the Princeton faculty plus the faculty of the Institute for Advanced Study. Now I am reminded of this particularly because once Veblen—I think this would be the spring of 1955 when I was back at the Institute for Advanced Study for four or five months on much the same project as Taub has previously described, a useless project—Veblen remarked that the mathematics faculty at Princeton was really very distinguished. He said it was much better than "when I was a member of it". I think you [Tucker] should take some acknowledgment of the compliment.

Aspray: I'd like to ask a general question about the mathematical physics community. Who was involved, and who were the major figures? What was the relationship to the physics department at the time? What was the underlying interaction?

Givens: Surely we can start with Einstein. May I get in one anecdote while I am so bubbly with this? I think it has mathematical content to it. Veblen was a very subtle man in some ways. He was simple and straightforward, and you thought he was an Iowa farmer from his general demeanor, but he was a good deal more sophisticated than that. He once told me, "You should stay around and see how these things are done." I think maybe he saw that I would be a better administrator than I was a mathematician.

Anyway, he had been trying to persuade me that in the metric for general relativity the signature of the quadratic form was quite clearly three minuses and a plus rather than three pluses and a minus, just a change in sign because it's the foundation of the concept of causality and no other signature will do for that. It really should be called a causality metric rather than a gravitational metric, but after all it was done by a physicist instead of a logician or a mathematician. Anyhow, Veblen had been trying to persuade me that it made a difference which you used, three minuses and a plus, or its negative, three pluses and a minus. Well, he was much too good a mathematician in every respect to tell me authoritatively. That was not the nature of the relationship. Veblen wasn't that kind of a person. He didn't do that to graduate students, and he didn't do it to me. But he was not without guile.

The occasion was that I was in my office waiting for the usual morning call to go into Veblen's office and talk. No one came. Veblen didn't knock, and I guess it was getting along towards lunch, so I thought I had better see what was going on. I stepped out my door and knocked on Veblen's door, and Veblen said come in and I went in. I saw what the difficulty was. He had been having a conversation with Einstein. Well, I'd met Einstein—his office was two or three doors down the hall—but I never knocked on Einstein's office because I had too much respect for his privacy and his time.

Anyway, on this occasion Veblen took the opportunity to fire a big gun on this little question of the signature. Well, both of us knew perfectly well what was going on. I don't know what the subject of the conversation with Einstein had been about. They both agreed that they were concluding it, and Einstein was about to leave. So Veblen said, "Professor Einstein, perhaps you'll decide *ex cathedra* a little question for us in regard to the signature of the metric." Well, Einstein laughed, quite a hearty laugh; he rumbled in laughter I think would be an appropriate way to describe it. He was flattered a little; he enjoyed it. He understood the question (and its phrasing!) and remarked quietly with some answer. This was more or less the end of the conversation and Einstein left, and I had a quiet, brief conversation with Veblen.

Now the story doesn't quite end there. Someone is supposed to ask which signature Einstein chose. Well, as a matter of fact, I don't remember, but the nature of the work at that time was of the following character. Einstein didn't give his reasons, so why did it matter which he said. That was the way things were done at Princeton in those days. Actually of course the question is easily answered by looking in Einstein's little book called *Relativity*, and I think it's three minuses and a plus. I think that's what he said, but I can't even be absolutely sure of that. But as I point out, I don't really think it matters very much. At least I wasn't convinced, even as a graduate student that it mattered very much.

Veblen wrote in German. I didn't know he knew that much German.

Taub: You know who his assistant was?

Givens: No.

Taub: Fritz John.

Givens: Oh really. He was the one who wrote the German in that monograph on projective relativity?

Taub: Well, he certainly struggled with it. He didn't understand a word, but he knew the German.

Givens: Okay, that I never knew. Anyway Veblen was in Germany for a year and in elegant devotion to scholarship wrote a book, wrote it in German, a monograph on projective relativity theory. Now in my view, and I ask for comments, one deep difficulty in relativity, not perhaps talked about quite so much, is that there is no quantization of the relativistic interval. Hence there is no normalization. Hence one can multiply everything by a scalar. One can have a ratio of intervals, but not an absolute interval. There being no absolute interval and only the ratio of distances I suppose goes back to Euclid. Did Euclid ever discuss absolute distance as opposed to the ratio of distances? It seems to me that one ought to make some use of this arbitrariness in the metric. There is an arbitrariness, but one we should make use of. There were various efforts to introduce a projective metric and thus make use of the additional flexibility to makes some approaches to quantum theory, in other words to develop the theory further in some direction.

I think one of the great difficulties in the history of this whole subject is that perhaps the wrong decision was made at that time. One should avoid the use of the metric; one should use something which determines the metric only to within an arbitrary scalar. In other words, the metric is determined only in that sense. So I've been wondering at least in recent decades if one of the troubles with the work that Veblen and Taub and I were attempting to do was that in a way it was a little premature. There should have been a little more available to work with, in particular, the invention by Jordan, von Neumann, and Wigner of the Jordan algebras.

Taub: You'll be interested, Wallace, in knowing that the current theory, the so-called Yang-Mills field theory has been geometrized in the sense of using fiber bundles over the coordinates of a continuum. In doing this the projective relativity comes back as taking the Maxwell field, looked upon as a Yang-Mills field with the unitary group as the gauge group in the field. Now, other Yang-Mills fields give a generalization of projective relativity, or a generalization of the Klein-Kaluza theory. People are worried about different things that happen to the algebraic properties and so on.

Givens: Well, I think you got a letter once from Dirac.

Taub: It was Dirac's assistant Halperin.

Givens: I got one at the same time and didn't do anything about it. I think you wrote back, and he said there was some interest in that work.

Aspray: You started to list the mathematical physics community, and you talked about Einstein and von Neumann.

Givens: There were so many visitors of such eminence that it's really hard to say something about them. One thing, though, I would like to bring up, and Taub can perhaps add to, a man named Lemaitre visited Princeton during the five years that I was there. I don't know the exact dates. He showed up in clerical garb, and he was the originator of what is now talked about a great deal, the Big Bang. I don't know that Abbe Lemaitre and Einstein ever met. I assume they did since he came to the small group, and he was working on cosmological theories. I remember someone—I don't know whether I was actually present or merely heard it—asked Lemaitre if a conflict seemed to arise between his views in physics and his theological views, what he would do. He said very straightforwardly and clearly that he would abandon the physics. Now quite unrelated to this actual visit by Lemaitre, there was a conversation at the high table in Procter Hall. Let's see, I ate in Procter Hall for three years. Wasn't that about the time you were there?

Taub: Well, I had a one year's tenure in Procter Hall.

Givens: You had good sense enough not to like the food.

Taub: Eisenhart suggested that both Procter Hall and I would be happier if I left the Graduate College.

Givens: That I didn't know. Anyhow, the incident I want to very briefly recount took place on the one and only occasion in my memory of Einstein visiting for dinner. A man named [William] Gillespie was the usual Master in Residence. Latin grace was said to start the proceedings, and the graduate students were obliged to wear some sort of a black gown consisting of a scrap of black cloth which could be used as a baseball plate in the spring.

Anyhow, Einstein was going in to dinner. Eisenhart was not usually there, but was Einstein's host and was presiding in place of Gillespie. Well, maybe I wasn't a very good graduate student but I knew an opportunity when I saw one. And so I shoved the other graduate students aside enough to get, not next to Einstein but with one other graduate student in between. There was Eisenhart, Einstein, the graduate student, and I. I was able to hear better in those days, and I heard the conversation. This was about the time the books by Sir James Jeans appeared, which had to do with the basic cosmological structure, and Eisenhart inquired of Einstein what he thought about these books by Jeans. The conversation by the way was in English, which made it a great deal easier for me to follow.

Anyway, Einstein said that he thought the books were very good. He even repeated this, they were very good books indeed, but that he had one conern. Well, Eisenhart knew an opportunity too. So he asked Einstein what the concern was. Einstein spoke quietly and slowly, but I thought with some conviction. He said that he felt that the general public might well misunderstand. He said, "though we know a great deal and we know it with considerable certainty, these matters are very problematical"—I don't remember the exact word, 'speculative' I think is the word he used. At any rate I never read this stuff about black-hole theory and the origin of the universe without wondering whether this view of things is certain. For example, the red shift is surely a fact and is an important one, but does it really have only the explanation which is usually given?

Taub: The answer is no, it doesn't.

Givens: The second one is do physicists really appreciate fully the effect of the real-number system on their work? I don't know any nonstandard analysis, but I do know something about the existence of quite a large number of fields, and I know something about the difference of opinion between people like Birkhoff and MacLane as to how much the real-number system needs to be emphasized in the foundational work and teaching of mathematics. So that would be another thing. The remaining thing is if there is no matter, there is no causality, because the grand mystery of physics is the connection between causality and the phenomena of matter, gravitation. Well, if there's no matter and you make these predictions about what happened at the beginning, it looks a little absurd.

Taub: I think the attitude of many physicists, and the only one they can live with, is the view of that the arena in which physics takes place can be regarded as Minkowski spacetime for a whole host of phenomena that they can study and deal with and are very comfortable with.

Givens: But is that special relativity or general relativity?

Taub: Special.

Givens: Ah, that's easy.

Taub: Now the general theory—this is where the problems of matter that you're worried about come in—and quantum theory are two disparate things. Still, the only explanation of general relativity that one has on the classical level is one in which there is a deep connection between the measurements of time and distance and the metric which you introduce in the space. Now if you're going to quantize the gravitational field, what in the hell are you going to do about the notion of the measurement of time or distance? I mean you can say that the probability of time is such and such, but ...

Givens: There is a simpler answer, and this was discussed by people like von Neumann and Pauli. I remember a conversation I *think* it was between von Neumann and Pauli. They were talking about the difficulties of the mathematical foundations of physics and thought that the mathematics seemed to contain a lot of difficulties which really weren't inherent to the physical situation. One had the infinite phenomena of the infinite limits and the infinite dimensional spaces and what not. I listened with an avid interest, but it's too long ago to remember any details, but the nature of the discussion was pretty clear. They were uneasy about the fact that the physical predictions are based upon mathematics which seems to have unnecessary complexities in certain respects. I suppose that is still true.

Taub: Oh yes. It's even worse than that now.

Givens: Thank you, you comfort me.

Taub: I mean the picture the physicists have of matter in which large numbers of elementary particles occur ...

Givens: It's very daring.

Aspray: May I ...

Tucker: Go ahead, we're both wanting to get back to ...

Aspray: Right, I wanted to turn this back to Princeton in the '30s, and there's a connection here. If you came in as a young Ph.D. or as a graduate student and wanted to study mathematical physics, what would you be told to study or what would you be encouraged to study? How much mathematics? What? How much physics? What?

Taub: You take the exams in physics, and you take the exams in math.

Givens: That's what Taub did. That's the way it should be done.

Taub: There were people who had joint appointments in math and physics. That was E.U. Condon and H.P. Robertson. Then, in addition Wigner came, but he came originally I think only in the mathematics department.

Aspray: Wigner and von Neumann had a joint appointment.

Tucker: That was joint mathematics-department/physics-department.

Givens: Were things a little bit casual about such joint appointments in those days?

Aspray: This was a named chair, the Jones Professorship, that was specifically for mathematical physics.

Givens: The buildings of course were connected. Something else I should probably mention here is that Hermann Weyl knew one hell of a lot of physics as well as mathematics. The scene in the last 50 years in mathematics has suggested to me that I ought to write a paper, which I shall never write, but I have a wonderful title for it: "The Best and the Brightest". [The allusion to the book about the Vietnam war is intended to be ludricrous. J.W.G.]

Taylor: The divergence of mathematics and physics became very great in the '50s. I don't remember now that I can say how one thought about these things in the middle and late '30s. I was in graduate school from '33 to '36 at Cal Tech, and there the physics students and the math students were expected to mix and all that, but the math students were expected to learn analytic dynamics and quantum theory. Basically that's what they were expected to know.

Taub: [Paul S.] Epstein was there teaching.

Taylor: That's right. But Millikan for example had no interest in theoretical mathematics at all. He only wanted mathematicians at Cal Tech to teach and to help the physicists. He really wasn't interested in building a strong math department. But after World War II pure mathematics became very snobbish toward physics.

Taub: This is not anything new with the '30s, but you see the growth of American mathematics started in what I would call relatively new mathematical fields. I mean topology, abstract algebra, and so on. The strength of American mathematics in those fields was much greater than in analysis. There were very simple reasons for it. One was that the only way the young mathematicians could make a big dent in the subject was to go and start something new. If you had a subject where there are two theorems and you've proved another one, you've made a 50 percent improvement in the field. And you've made a name for yourself.

Givens: Does that come from R.L. Moore's insistence on everyone originating his own subject?

Taub: But I think that most of the mathematicians trained in the United States were trained in these newer fields. Now with these newer fields there wasn't nearly as much contact between physics and mathematics as there was between analysis and physics. So this divergence between mathematics and physics in this country in the '50s, I think, is explained by just that phenomenon.

Givens: Very interesting. I've never heard it as well put—what's happened in American mathematics in the last 50 years. It is now very different from what it was in the days at Princeton when the Institute people came with that deep connection with the physics, such as von Neumann's mathematical quantum theory.

Taylor: Even in analysis, the Hilbert space really originated as an attempt to explain quantum mechanics. But [M.H.] Stone seemed to have sort of taken it into the pure realm. Most of the functional analysis that developed in this country for a long time was pretty much isolated.

Taub: It is still so. There are many functional analysts that don't know beans about operator theory.

Givens: That was one of the great constants I ran across in my notes, my mimeographed notes on operator theory, in looking over the material in the last few days.

Tucker: There is a question that I wanted to ask. When we were interviewing Wigner we found that he spoke with some bitterness of being—I've forgotten the word he used ...

Aspray: Not appreciated.

Tucker: Well, and also that he was told to leave when he went to Wisconsin.

Givens: For heavens sakes. He has a right to.

Tucker: Do you know the background for that?

Taub: I don't know the background except that by that time von Neumann had gone to the Institute I guess there was a feeling that he had been nurtured by von Neumann and that he was always in von Neumann's shadow at Princeton. And when von Neumann left Princeton they decided that, although he had tenure, they weren't going to treat him as well as he expected to be treated. So he went to Wisconsin.

Tucker: Do you know how Robertson felt about Wigner? Was there any animosity there?

Givens: I was about to ask that. I think we ought to talk about Robertson a little bit.

Taub: Well, I don't know that there was any animosity. I think there was a certain divergence of fields of interest.

Tucker: Yes.

Taub: Well, Condon too was not too comfortable at Princeton toward the end. I forget what Condon's history was. I guess he didn't leave Princeton before the war.

Tucker: No, he left about 1940 I think. [Actually 1937. A.T.]

Taub: Then he was involved with some war projects and went into the Bureau of Standards after that. And went to Colorado after that. I would guess that there was a feeling on the part of Condon and Robertson that Princeton could do better than Wigner at the time. There is nothing overt that I can put my finger on.

Tucker: But apparently it came up for a vote, and Wigner was rejected.

Givens: Well, to come back to Veblen's influence in founding the Institute, it was a great compliment to Princeton that Veblen did it. But when he moved out of the University, it was rather a blow to the University I think. My feeling is that if you look at the list, it took the Institute to make that a great place and not the University alone.

Tucker: That's right. And apparently from the things I heard Veblen made some incautious remarks about the fact that it was the death knell of Princeton mathematics department.

Givens: He never confided that in me.

Tucker: And this put Lefschetz on his mettle that he was going to see that Princeton mathematics department didn't die.

Taub: I have a feeling that Bochner felt very badly that he had not been selected among the group to go to the Institute. He felt that he was the same caliber as some of the people there and was very put out. 'Put out' is a mild expression for his feelings.

Taylor: If so, he never confided that to me. I wanted to ask a question about the two European mathematicians: Kerekjarto and Alexandroff. They were both at Princeton in the town at least at this period. I'm not sure exactly what time. In my studies of [Rene Maurice] Frechet I've come across a number of letters from both Alexandroff and Kerekjarto to Frechet, and some of them were written from Princeton.

Tucker: Alexandroff and Heinz Hopf were both at Princeton as International Education Fellows in 1927-28. I don't think Kerekjarto was ever at Princeton.

Taylor: Yes he was. I have copies of letters that were written from him to Kerekjarto from Princeton.

Taub: That was before your time, Al.

Tucker: Maybe it was before my time, but I've been doing my homework and I've never heard of Kerekjarto in Princeton.

Taylor: Well, I'm pretty sure I'm right. I'll have to check my notes again because I spent a spring in Paris some years ago looking at the papers left by Frechet. [I did check on this. I was right. A.E.T.] And I came across all these letters, and I have photocopies of the Alexandroff letters. I don't have photocopies of the Kerekjarto letters. But Kerekjarto intended to write two volumes. He wrote only one, and he has this picture, which is supposed to be a picture of Bessel Hagen. The name is in the index, but the name doesn't appear on the page, only his picture.

Taub: A caricature of his picture.

Tucker: Yes, a caricature.

Taylor: But they were brought there before the Institute came into existence, isn't that true?

Tucker: The charter of the Institute was in 1930, but it didn't start until 1933. But I wish you'd check this Kerekjarto bit, since Kerekjarto was a topologist, I feel that if I didn't know about that, then there's something wrong with me.

Givens: Your mention of the founding of the Institute reminded me of something that I would like to get in. There was a talk given on I think Wednesday evenings at the Graduate College quite regularly. These talks were of a general sort, from quite varied people, a professor of English for example. Flexner gave a talk on one occasion of the founding of the Institute, one of these Wednesday evening talks. I wonder if anything has survived in the way of a statement by Flexner about this.

Tucker: Flexner wrote an autobiography.

Givens: This I have never seen.

Tucker: And what's the title ... *I Remember*. And he tells a lot about how the Institute came to be founded.

Givens: In a conversation with Bamberger and his sister he felt obliged to ask Bamberger for money so that he could move on with this. Bamberger asked him how much, and he gulped and said, "Two million." And Bamberger wrote out a check for the amount.

Tucker: Well, you'll find this in print in his autobiography, *I Remember*.

Taub: Well, I think I've just remembered some story that will illustrate Veblen's character and manner of operating. Veblen was a trustee of the Institute for Advanced Study for many years. Now after he retired he still kept his trusteeship. His appointment ran out, but that didn't matter to him. He went to the trustee meetings anyway. And he just kept going as long as he wanted to. And von Neumann then said, "Imagine this. Here the trustees are, men who rob orphans and widows, for years being able to handle anything, but they can't get this guy not to come to their meetings and tell them what to do about the Institute."

Givens: I have a supportive story which is directly connected to this. It took place I'm sure much earlier because it was when I was Veblen's assistant. Hermann Weyl was also a trustee, and Weyl resigned on, I think, the stated grounds that it was a conflict of interest being a professor. And Veblen said, "Why, this goes on all the time in industry. I didn't resign. Of course, I don't have a vote, but most things are decided by consensus anyway. And I can talk."

Aspray: I have a question I'd like to ask about Veblen. Nathan Reingold has done a study of mathematics between the two world wars, and I wrote to him to tell him about this project and he wrote back to me and said, "There's one thing that I'd like to know. That is that several people when I was doing my research had told me that Veblen had a feeling that certain positions that were out there available for new Ph.D.s should just go for mathematicians and other ones should go for mathematical physicists. And in fact there was a case where a mathematical physicist was not recommended for a job because Veblen thought the department would grow better if there was a mathematician in it." Do either of you have anything to say?

Taub: Well, I would say this. Veblen's attitude about mathematics and mathematics departments was created in the days that mathematics departments were wholly considered as service departments in the university. He therefore always took the attitude that anything to destroy this notion was to be done in any university mathematics department. He felt that it was very important to bolster what might be called the mathematical activities in contrast to service activities. And so it would be quite in keeping with his ideas that he would feel that mathematics was important and that mathematicians should fight against the notion that all that was needed was a service department.

Givens: I think that probably strikes to the heart of the matter. Let me, however, supplement it by a remark which was made, certainly, much later, but really illustrates the point. Not earlier than I think 1955 and possibly a little later there was a conversation between Oswald Veblen, Mrs. Veblen, and myself. I was somehow with the two of them. It was characteristic that Mrs. Veblen was not as restrained in her speech as Oswald was. He would smile sometimes and not comment. [As once when Mrs. Veblen said of a mathematician who had just been named in the conversation: "Oh, Oswald, is he the man who publishes too much?" (That mathematician used to say that each page published was worth a hundred dollars to the instructor publishing it.) J.W.G.] On this occasion the conversation had to do with the establishing at the Institute for Advanced Study and the building of the computer there, and Mrs. Veblen said, "Oswald, you never did want that computer at the Institute did you? You just thought that if Johnny wanted it, he should have it." I direct this remark to those who don't understand the way to administer scientific departments. Veblen was a superb administrator.

Von Neumann once made a comment about Veblen, which is related to what I've just been saying. He said, "Veblen is often correct, very often correct, but when he is wrong he is so very, very wrong." And he said this with a passion which suggested that he'd had to put up with some of it.

Aspray: I think there are supporting documents in the von Neumann papers at the Library of Congress that indicate that Veblen had this attitude about the machine. I can't swear to that because when I was looking at the papers I was interested in von Neumann and didn't care at all about Veblen, but I think I remember that as being there.

Givens: Von Neumann appreciated Veblen alright. I don't think there was a lack of appreciation. After all, his courtesy in coming to their joint seminar was beyond the call of duty. I was back at the Institute in the spring of '55. I had been at N.Y.U. for a period writing codes for the UNIVAC, yet Veblen and I never really discussed computing. But he was just too courteous, he wouldn't have said much of anything, but he never showed any enthusiasm for the computing.

Tucker: Despite his Aberdeen background.

Givens: That is an interesting point, because you know that in the first world war he had a very considerable part in the computation of ordnance tables, at Aberdeen. He was not by any means a person who didn't understand the need for doing what was necessary to win a war. This was not at all the case.

Taub: But his main role even then was getting people to come to Aberdeen.

Givens: I'm sure you're right. I don't think he did any arithmetic.

Taub: And placing people at various places.

Givens: Sure, but that would not have been inconsistent with his views that you got the job done with the means that were appropriate to getting the job done.

Tucker: I always felt that in some sense Veblen was like a very astute political boss. That he worked by indirection mainly.

Givens: That's entirely consistent with his remark to me to stay around and see how these things are done.

Taub: My view of the matter is that from the early days of American mathematics Birkhoff, Veblen, and Bliss determined everything about American mathematics. They determined who got jobs where, they determined your getting a fellowship from National Research, ...

Taylor: What about E.H. Moore?

Taub: Well, he was out by that time. I guess I'm talking about the politics of American mathematics rather than the creation of American mathematics. I guess E.H. Moore never cared at all about the politics after he got himself comfortable.

Tucker: I judged also that in the '30s Griffith Evans was ...

Taub: Well, he began coming because when he went to Rice he had an opportunity to get people, and then he came here and built the place.

Taylor: I agree with you. Of course, Moore was only a remote name to me. I've heard a lot about him since.

Taub: There were other people who were sort of secondary. I mean, I would say those were the generals. Then there were a lot of other people. Richardson.

Aspray: Howard Richards was the one I was thinking of.

Taylor: When Veblen said to me that all the good mathematics was done on the Atlantic Coast, he said except for Chicago.

Taub: Well, that's because he got his degree there.

Givens: R.L. Moore was, I think, the first of Veblen's Ph.D. candidates, although it's a little questionable whether he did his dissertation with Veblen, and I was almost surely the last, because in fact Veblen couldn't sign my thesis although I'd done all my work with him—he wasn't a member of the faculty any longer. He pressed J.H.M. Wedderburn into service.

My contact with Wedderburn was of the following sort. I attended his lectures. There were no final exams or anything. You just signed up, and the faculty received a list of the students who had signed up for the course. They signed their name to the list and sent it back to the registrar, and that was the end of their participation with the students unless the students asked a question or something of the sort.

Tucker: You know, I actually have a record book in which at the beginning of each term I got the signature from the professor. The name of the course is there in my handwriting and it is signed and then at the end of the course it is signed again. Do you have one of those?

Taub: I had one but I don't know what has happened to it.

Tucker: I still got mine. I showed it to Bill.

Givens: Is it true that you were on leave or something at some later date, and someone took over the direction of the department and instituted examinations, and one graduate student left in high indignation and went to Columbia over the fact that he was being examined in the course? That's probably apochryphal.

Tucker: Well, there was a short time when due to Chevalley the general examination was written as well as oral.

Givens: I see.

Tucker: But then Chevalley went off to Columbia, and as soon as he left the whole matter was dropped.

Taylor: Why did T.Y. Thomas leave Princeton to go to UCLA? I mean I had a good reason, because I thought I wanted to live in the West, but I never understood why T.Y. Thomas left.

Taub: Lefschetz.

Tucker: Lefschetz, yes.

Taylor: Well, they had just started the Ph.D. program at UCLA. The first Ph.D. at UCLA was granted in '47.

Tucker: There was also, I think, another point. And that is ... wasn't [E.R.] Hedrick the man?

Taylor: Hedrick was the head of the campus. [His title was provost. A.E.T.]

Tucker: Yes, well Hedrick persuaded T.Y. Thomas that he'd get to the National Academy quicker if he wasn't at Princeton.

Taylor: I thought he was already in the National Academy when he came to UCLA.

Tucker: No.

Taylor: He became very soon thereafter.

Taylor: I think that there was only one member of the National Academy other than T.Y. Thomas at UCLA for a few years.

Tucker: Yes.

Aspray: You both said Lefschetz. Do you want to explain that to me?

Taub: Well, Lefschetz never thought very highly of T.Y. Thomas' mathematics, the kind of things he did. I remember his coming in one day saying, "I just heard a click. T.Y. Thomas proved another theorem."

Taub: Also their personalities ...

Taylor: Well, Thomas was a difficult man to figure out, I always thought.

Taub: Of course, Lefschetz was too.

Taylor: But they were very different, my heavens. By the way, you spoke about his mechanical hands. Were those fingers moveable? I don't think they were.

Tucker: Yes.

Taylor: Were they?

Givens: Slightly. He could put a piece of chalk in between his fingers and push.

Taylor: The chalk was always lined up on the edge of the table.

Tucker: Yes, but he pressed something here that opened up the fingers, and put the chalk in, and then pressed something and it tightened down.

Taylor: They were always gloved, that's all I knew about them. You say the accident occurred in Pittsburgh?

Tucker: It occurred at the Westinghouse laboratories in East Pittsburgh, I think in 1909.

Taylor: But he wasn't a mathematician then.

Tucker: He trained as an electrical engineer in Paris, and he came to the United States. His first job was for the Baldwin Locomotive Works outside Philadelphia. One time when I was going with him on the train between Princeton and Washington, he pointed to that and said that's where he'd had his first job.

Taylor: Where did he become a mathematician?

Tucker: He went to Clark University and took his Ph.D. there. Clark University along with Hopkins and Chicago were the places where you could get a doctor's degree in those days.

Taylor: Clark University is now a slight embarrassment to the American Association of Universities. You know it is still a member.

Tucker: Yes.

Givens: Could we come back to Wedderburn just for a moment?

Tucker: Yes.

Givens: Wedderburn did some important theorems in the early history of linear algebra. I attended his lectures. Last night and this morning I looked at the mimeographed notes which I've retained over these fifty years. In my days at Princeton in the '30s he was, I think, not very prominent. He was not really very active I think. On the other hand he signed my dissertation, and he read it to the extent that he saw that there was a theorem in it which contradicted a theorem in the literature. Now, Veblen was not a person who read much of mathematics. Did he ever tell you to go read a paper? He just didn't do that. People came and told him about mathematics, and I'm sure that at some time in his career he had read more than he was doing when we were with him.

So Wedderburn found these two theorems contradicting one another. Well, by a great good luck it was a British author of some distinction who had published an incorrect theorem. Mine was right, and I've never felt so pleased with good luck in my life. Anyhow, Wedderburn's notation in linear algebra, in working with matrix algebra, I think was a great deal better than was appreciated. The main notation was that in the van der Waerden, volume 1.

This is a chance to say that in my days at Princeton the graduate students were expected to know the contents of the first volume of van der Waerden; the second volume was for such geniuses as Jacobson. I think that's a little different now. (I remember a story. It was told that Hermann Weyl once said that he couldn't read Jacobson's writing after 10:00 in the morning. I don't know whether Jacobson would have told you that or not.) So Wedderburn's notation was not accepted, and that I think led to his lack of influence in linear algebra in his later years. My memory is that he died probably in '36 or so. Is that about the time?

Tucker: Oh no. '48.

Givens: '48? Then my whole memory of that is totally wrong.

Taylor: When were his colloquium lectures? They were in the '30s, weren't they?

Tucker: Wedderburn died after Lefschetz became chairman of the department, and Lefschetz became chairman of the department in '45. I'm pretty sure it was '48, because he just died, dropped dead, in his home, and it wasn't discovered for a day or two. And he had no relatives at all in the United States.

Tucker: He was never married.

Givens: I once heard that when he died he had a great number of detective stories on the shelves of his bedroom.

Tucker: And I was sent over by Lefschetz. I had to go to the Princeton Bank, the executors, to get the key. I was sent over because he had willed his papers and books to the mathematics department. I had to go over there and spend days going through his belongings. I remember very well that it was Lefschetz who sent me over there to do it. If Eisenhart had still been chairman of the department, it would have been done otherwise.

Givens: I just misremembered the date after so many years. There was certainly a little tragedy about Wedderburn's later years wasn't there?

Tucker: Well, he had some sort of a breakdown, a nervous breakdown, that occurred in the late '20s.

Givens: I didn't know that.

Tucker: I wasn't aware of this until long afterwards, but there was a student [Karen Parshall. A.T.] of Herstein who got interested in Wedderburn as a person and wrote to Princeton, and any letter now that comes in of that type is automatically turned over to me. So I did a little background research and found that in the Royal Society volumes of obituaries there is one of Wedderburn written by Sir Hugh Taylor, and he really did a dandy job on writing that obituary of Wedderburn. If you can get a hold of that Royal Society obituary of Wedderburn you'll be interested very much in reading it.

Givens: While my association with Wedderburn was limited, I certainly appreciate this type of information. As far as evaluating the development of mathematics, you made a remark to me when I was a student, which I suppose you've forgotten, that one should not underestimate the importance of notation. Now as a brash graduate student I thought I knew that. I duly appreciated it and have remembered your remark. Perhaps we're not careful enough with notation. The notation of the mathematical corpus is not always as wise or as effective as it might be. We ought to be a little freer about notation in some ways. Now let me be a little more concrete about that. I've known a great many linear algebraists in my day, some of them very, very good. I think a lot of them would have benefitted if they had known the Einstein notation for tensor analysis in dealing with tensors with two indices. I don't think they knew the fundamentals sometimes of the various kinds of mapping between the spaces.

Tucker: I felt that the algebraist at that time who had the best feeling for Wedderburn was MacDuffee.

Givens: That's interesting because there is a footnote in MacDuffee's *Theory of Matrices*—that summary, you know the thing—which was critical to my own work on the eigenvalue problems for real symmetric matrices. He said something could be done for an arbitrary ring, rather than sticking to a field. And I said if it could be done that easily maybe it can be done on a computer. This is also to give you a little bit of the flavor of the conflicts in the mathematical sciences which have developed over these 50 years. I'm pretty sure I'm correct that Dieudonne has written someplace that MacDuffee's *Algebra*, not the *Theory of Matrices*, was the worst mathematics, worst volume of mathematics, or some such phrase as that. I must say I resented that very much because I think MacDuffee did a lot. This is to point out the kind of conflict which exists when you have people saying that applied mathematics is bad mathematics, which a very distinguished mathematician recently said, and Dieudonne saying this about MacDuffee's volume on algebra.

Now I have a reason for commenting on Dieudonne because Dieudonne left Northwestern and went back to France and made available a professorship that I then got. But we differ in some matters. I understand that Dieudonne has done marvelous work. Anyone who has any sense at all would be jealous of his great distinction as a mathematician.

Tucker: Is there anything more you have on your list?

Aspray: I'd like to see whether you have any more to say about Robertson, any of the three of you? We haven't heard much about Robertson.

Taylor: Robertson seemed to go back and forth between Princeton and Cal Tech a great deal.

Taub: He got his degree at Cal Tech.

Taylor: I think he liked to be in California.

Taub: I think actually that he always hoped to have a position at Cal Tech.

Taylor: Really? He came down from the state of Washington following E.T. Bell. I believe that's the origin of it I think.

Taub: Yes, and he was very disappointed after he had gotten his degree at Cal Tech and had gone to Goettingen for a year or two that he didn't get an appointment at Cal Tech. I never got straight whether Bell introduced some difficulty in connection with such an appointment or whether Millikan just didn't want him.

Tucker: I think it would be the latter.

Taylor: Well, Millikan and Robertson corresponded about various things. That is, Millikan asked Robertson's advice about whom to appoint to Cal Tech, I know that, to some extent.

Taub: At any rate, he had a strong attachment to it. He visited there, and then when the offer for a position came there, there wasn't any argument. That was it. He took it. He didn't even consult anybody at Princeton. Angela was not too happy about packing up and going, but they did as soon as the offer came through. And he was a very interesting person in many respects. He was very interested in people, and he went out of his way to help people in all sorts of things. He didn't have too many students there. I think I was one and I think Foster was another.

Aspray: No, Foster was a Church student.

Tucker: Foster's thesis was formally with Veblen, but it was really done with Church. He told us that.

Taub: Well, his own field was mainly relativity, although in those days anybody in mathematical physics did both relativity and quantum mechanics. He came under the influence of Weyl insofar as he wrote a translation of Weyl's *Group Theory and Quantum Mechanics*.

Tucker: Right.

Taub: In connection with that he became very interested in group theory, both in continuous groups and in then their relation to physics. This led to his interest in the application of group theory to the cosmological problem. He was one of the few people who started the business of characterizing spaces for cosmologies in terms of groups. As you probably know there's a whole class of them called Robertson-Walker Spaces. In fact sometimes Friedmann's name is put in, and they are called Friedmann-Robertson-Walker, or FRW Spaces.

Taylor: Did Tommy Tompkins ever have a position at Princeton?

Tucker: He was an instructor.

Taylor: I think he was there, but I'm not sure in what situation, the year I was there.

Tucker: He came first of all as a National Research Council Fellow and then stayed on as an instructor.

Taylor: He might have been an instructor there the year I was there. I'm not sure.

Tucker: He was instructor until he was called to do cryptographic work for the Navy.

Taylor: He came to UCLA, of course, a good deal later, and I knew Tommy pretty well at UCLA for a good many years. Polly [Tommy's wife] is still alive by the way. Tommy and I had rather big arguments sometimes about the situation at UCLA because he had very definite opinions ...

Taub: He probably wanted you to get people who were applied mathematicians. To come back to Robertson, he interacted very much with a lot of people in Princeton in those days. When Leopold Infeld came to Princeton, Robertson and Infeld became very close. It's hard to pinpoint his work during his Princeton period, but one contribution he made was that he straightened out how the relations between the so-called Einstein-Infeld-Hoffman approach to the equations of motion and some earlier work done by Levi-Civita in the 2-body problem. There's a paper in the *Annals* that Robertson wrote straightening all this out.

As soon as the war came he just got completely involved in the war. In fact I went back to Princeton during the war, because he said come back. Then he went off to England, and I stayed in Princeton to do the things that he had been working on. Then after the war he decided to go to Cal Tech because he wanted to. I can tell you one thing, that when he left Cal Tech to go back to Washington and I said, "For god's sake, Bob, what in the world are you doing out this way?" And he said I expected Dubridge to help me stay out of this and he wouldn't.

Tucker: He was a scientific advisor to SHAEF [Supreme Headquarters Allied Expeditionary Force].

Taub: He was scientific advisor to SHAEF, and then he was also involved in the weapons evaluation group in Washington. My own guess is that he was also very much up to his neck involved in this AISOSS group that went over to Europe as soon as the war in Europe was over and tried to comb through and find out what was going on, what the Germans were up to, and what had happened. Well, maybe you can say he was a weak character and he just didn't stand up for what he wanted to do. He claimed he wanted to go back and do work, but he just got dragged into all this other stuff afterward. He never did really go back. And then he died very suddenly from an embolism after an automobile accident.

Tucker: Yes.

Givens: I don't think we've talked about Eisenhart, Dean Eisenhart. He was a very busy man in my days at Princeton. He was Dean and professor of mathematics, and he was writing his books on differential geometry. He brought the proof sheets of his book to class, and we studied from them. He was also a man who made the effort to do something for graduate students. I remember his entertaining at his residence next to the Graduate College once. I went there for some evening discussion with some philosopher. [Paul Elmer Moore, if my memory serves after 50 years. I do remember a discussion of the *New York Times*, which was lavishly praised but then dismissed: "Of course they don't *really* care about the T-R-U-T-H." J.W.G.] I remember that as a contribution to my education. He was that kind of a person in discussions.

He conducted classes, however, rather casually. He was too busy I think to make much preparation. Typically he would come in and ask if there were questions. Now Rosser was a very able graduate student and prepared questions. So he would ask the questions about the problems, and Eisenhart would proceed to work them. Incidentally, I was impressed to read quite recently, in the last few months, that the Mathematical Society is going to reprint Eisenhart's *Differential Geometry*. I think that's a tribute to the fact—which should be noted from time to time—that people did do things in those days which lasted.

Fred Ficken was a very intense and eager graduate student, and he worked on his dissertation under Eisenhart's direction. He did not have the kind of supervision that which one might have hoped for and which I'm sure Eisenhart would have wished to provide. I hope I'm not saying anything in any way derogatory about Eisenhart, but Fred had to leave the university and work for a year. He wrote his dissertation and sent it back.. I think it is very much to his credit that he did so. He was a good friend of mine and is dead now.

There are one or two others that I'd like to mention. Alonzo Church ought to be mentioned someplace in this discussion. He was a very distinguished logician. You may remember that I took an interest in him some years later. I was able to offer him a position at Wayne State University, in which I'm sure he had no interest, but ... anyhow. Alonzo Church was a thorough mathematician. You ought to realize how thorough. He really wanted to work at the foundations, and he did. He was a logician and one of the major mathematical logicians of his generation, cetainly in the United States. Isn't that true?

Tucker: Oh yes.

Givens: Well, he was somehow reduced to giving a course in real variables, a semester course on real variables. This is the only time to my memory when a teacher was replaced by another teacher during a course in quite this way. Along about Christmas Hille took over the course in real variables. Now Hille was a major worker in this field, and it was perfectly reasonable and all. But I've always wondered if one of the reasons for this was because Church, in establishing firmly the foundations of real variables, had reached the number one at Christmas! I am very fond of Church. I think he was a good teacher, I learned some things from him, but the picture of uniform excellence throughout, and in every detail, was not realistic at all.

Another I'd like to mention is Henry Wallman, a student who had come from a totally different educational and cultural background. He had grown up I think in Brooklyn.

Tucker: That's right.

Givens: I believe he went to Sweden in McCarthy days.

Tucker: That's right and I have visited him at Chalmers Institute of Technology. Well, he must be retired now I guess, but last I heard his title was professor of electrotecnics.

Givens: Well, I have a high regard for him. Did then and do now. I remember a conversation having to do with the general nature of ethics. He grew up in a culture where, if you played basketball, you cheated if you could because the referee was there for the sake of policing that. That was the way it was. I had, on the other hand, gone to the University of Virginia, and while I was not on the honor committee I was pleased to be suggested for it the year I left, for the following year. The attitude there was the concept of self-policing, that one did not lie or cheat on exams and that the one exception was to protect the honor of a woman. This was regarded as the kind of cultural background which existed at the University of Virginia in those days, quite different from Brooklyn.

Taub: I must say that your example points out the difference greatly.

Givens: Points out what?

Taub: The difference between Virginia and Brooklyn.

Givens: That's what I was trying to do. That's the whole purpose of this. Well, may I give a couple of criticisms of the general social structure in Princeton. Not that I knew all that much about it. Not at all.

I once was pressed into service to be the escort for the young woman who was the social editor for the local newspaper. She thought it was some distinction that when her mother married her father her mother's parents sought recommendations, and her father was able to get one from Woodrow Wilson. She was the social editor of the local newspaper. Anyhow, she took me to an event which I would not have known had even existed in Princeton: a horse show. There was a social group in Princeton in those days which celebrated the major event of a horse show in the spring. There were also two organizations which sponsored dances, a series of dances during the academic year. There weren't too many of the graduate students in mathematics who went to those. I think I went to all of them, at least as many as I could. These were black tie events. One British mathematician I remember had a very handsome cape, red-lined evening cape which he sported.

Tucker: This was the Little Club.

Givens: I think that was the name of one of them. I've forgotten the other. The von Neumanns on one occasion arrived there, and my date (who was later my first wife) asked Mrs. von Neumann about her new baby. And she said she's a cute little thing. Anyhow, that cute baby is now the vice-president of General Motors. There was a social life that was not participated in by most of the graduate students. I happened to have had several years of graduate work before I came there, and I had a car, and I went to these things. Enough.

Tucker: Well, there was quite a social life with the Robertsons

Taub: And the von Neumanns ...

Tucker: ... and the Alexanders.

Givens: And the parties at the von Neumanns were quite liquid affairs, weren't they?

Tucker: Oh yes.

Taylor: There was also a very formal social structure, different from this I suppose. I remember that I had been married only two years, and we were here in Princeton, and my wife said evidently there is something that we ought to know about calling and being called on.

Tucker: The cards.

Taylor: The cards and all of that stuff.

Tucker: Yes, that was typical of the Veblens and the Eisenharts. The first year I was there I called Sunday afternoon for tea at the Eisenharts and the Veblens, and then I made the mistake of doing this with the Alexanders. And Mrs. Alexander couldn't understand what I was there for. Jimmy wasn't around, so she had to contend with me. She wanted to give me something to drink, something alcoholic rather than tea, and she was quite put out with me because I was still a tee-totaller at that time.

Givens: This reminds me of one other conversation I had with a man named Cooper. The first Mrs. von Neumann's second husband was a graduate student in physics at this time and had been there for many many years, I think both as an undergraduate and as a graduate student. He remarked to me that he thought he had, in all these years, never spent a weekend in Princeton. I don't know the facts, but I interpreted this to mean that he went to Long Island for a house party on weekends and that Princeton wasn't the place to stay.