DEANE MONTGOMERY

(with ALBERT TUCKER)

This is an interview of Deane Montgomery at the Institute for Advanced Study on 13 March 1985. The interviewers are Albert Tucker and Frederik Nebeker.

Tucker: Perhaps I might start things out by asking you about your first coming to Princeton, which I think was in 1933-34.

Montgomery: 1934.

Tucker: Oh yes, 1933-34 you were at Harvard. You had taken your Ph.D. at Iowa with E.W. Chittenden in 1933, then National Research Council Fellow at Harvard, and then the following year at Princeton.

Montgomery: Yes, so I came here in September 1934, and I found the atmosphere extremely pleasant. The Institute had started the year before, and all the mathematics of the Institute was combined with that of the University in Fine Hall. It was a little crowded [laughter], but I found the whole thing an extremely pleasant experience and I've been sentimental about it ever since.

Nebeker: Now you were a National Research Council Fellow at Harvard. How is that you came to Princeton after that year?

Montgomery: Well, I decided to make a change. I'd heard fine things about Princeton. I think that at Princeton there were more things, probably, that I was interested in. At that time my main interest was in a form of set theory. I was especially interested in Borel sets, analytic sets, and projective sets, and I knew quite a lot about those. Not a whole lot about other things [laughter]. In fact, there weren't

many people in the country who knew about those things. One man who did was von Neumann, and I think that played some part in my decision, although I don't think he was very interested in those things at that time, and I soon lost my own interest.

Nebeker: But your Ph.D. was in topology.

Montgomery: Yes, though partly in real analysis in a way. The year before I'd also been studying topology at Harvard. We had a private study group consisting of Norman Steenrod, myself, Garrett Birkhoff, and M.R. Hestenes. Just about that group. [Solomon] Lefschetz' book was in existence, and we thought of reading that, but we found it was too hard for us. So we began with [Oswald] Veblen's *Analysis Situs.* We read all of that, and we read some other things too. That's how we got started. So I was interested in those things, and I'd become interested in groups of transformations, partly because Birkhoff told me that dynamics was a great field—you know, they're one-parameter groups. So I read some about dynamics, which I thought, and still think, is a great field, but I never did much with it. Of course it's connected with topology.

Nebeker: Was your Ph.D. more in what's now called point-set topology?

Montgomery: Yes, I guess you'd say that, although it was not the R.L. Moore kind of topology. It was the kind that was very popular in *Fundamenta mathematica* at that time.

Tucker: The Polish topology.

Montgomery: Yes, about half the articles in *Fundamenta* were about that sort of thing, and about half were about the theory of curves—R.L. Moore type. I never read any of the R.L. Moore type.

Nebeker: But then you got into algebraic topology?

Montgomery: Yes, algebraic, or at least geometric topology. To some extent algebraic, including groups of transformations.

Nebeker: Was that because of the group at Harvard you worked with?

Montgomery: Partly, but I was always rather independent. You know, I tended to do what I pleased. In many ways it was a mistake, but in some ways it was in my nature. I was influenced by that group, sure. I was influenced by Birkhoff, and I was influenced by the people I met here. It's hard to know where all you influences come from.

Tucker: Did you have much to do with Marston Morse at Harvard?

Montgomery: No, I didn't. I knew Morse then, and I knew [Hassler] Whitney then. I didn't have much to do with either one. I went to Whitney's course for a while, and then I dropped out, because he was talking about metric spaces, which was one thing I knew as much about

as Whitney [laughter]. Not about much else, maybe, but I certainly knew about that. I went to M.H. Stone's course in real variables. This was the first year the theory of Lebesgue intergral had been given at Harvard, so they were going into that in great detail. And although I was trained at Iowa, that was one of the other things I knew completely thoroughly. So I dropped out of that.

Tucker: Well, Chittenden was very well informed. You know, he came to Princeton in the '30s for a year and had a room in the same place I was staying, so I got to know him quite well.

Montgomery: That was probably the year after I was here, probably '35-'36.

Tucker: I really had an opportunity to learn firsthand what a wealth of mathematics he knew.

Montgomery: Yes, just died two or three years ago.

Tucker: Well, then you were at Smith until you came back. You first came, I think, on a Guggenheim.

Montgomery: That's right. I came on a Guggenheim in '41-'42, and I was technically at the Institute, or both the Institute and the University. I forgotten, but I think there was still a policy of being at both if you were at one.

Tucker: Yes.

Montgomery: So I was at both really, that year. Then I went back to Smith, and then I was invited to come and teach Army students at Princeton for a couple of years.

Tucker: '43-'45.

Montgomery: Yes, and then I worked for a year with von Neumann. He was just beginning to have a program in numerical analysis. I never worked very hard on that; by that I mean that I tried to do it in a conscientious way, but in my spare time I always thought about something else. In general, my experience with war work was like that: I didn't really want to think about it much, but I tried to be conscientious about it even while I tried to keep on with what I was really interested in. In other words, I was not of much use.

Nebeker: Was that the period you wrote the paper with Bargmann and von Neumann.

Montgomery: Yes. Von Neumann suggested these things about how in view of the large number of computations you could do per second round-off errors might become a serious problem. He wanted some investigation of that sort of thing.

Tucker: It still is a serious problem.

Montgomery: Of course. This led me think that numerical analysis was quite a good field.

Tucker: You know I worked briefly in this numerical analysis that von Neumann was starting out. This was because the work I was doing at the University, the defense-related work, stopped rather suddenly. It was between terms; I couldn't start in teaching at the University immediately. So for two or three months—I've forgotten just how long it was—von Neumann had me on his project. Because he thought of me as a combinatorial topologist, he had me working on a generalization of finite differences to higher dimensions, where there was the question of what sort of a subdivision you would use—rectangles, triangles, or possibly even hexagons.

Montgomery: I didn't know you did that, Al, but it sounds like a good question.

Tucker: I was rather sorry to stop it and go back to my regular job at the University. I hadn't worked on it long enough to get anywhere, because of the many possibilities of dividing the plane or three-space. Describing it as finite differences is quite inadequate.

Did you have much contact with Veblen in the first year that you were here?

Montgomery: No, practically none. I probably met him. Most likely I was invited to his house. At that time life in Princeton was more formal than it is now. I seem to remember being invited there to a black-tie dinner with a number of other people. I didn't know Veblen well at all; I just barely knew him at that time.

Tucker: I guess Alexander would have been the one you had most contact with.

Montgomery: Well, yes, I had some contact with him, but not a lot really. It was hard to have contact with Alexander. I had more in later years, but I was never his intimate buddy. I tended to see him because he was a friend of Veblen's; when I would see Veblen, I would sometimes see Alexander.

Nebeker: Did you get to know Veblen better when you were here during the war?

Montgomery: I got to know him a little more when I was in Princeton during this period we spoke of, and I got to know him quite well after I came here permanently, in 1948. First I was a permanent member, then in '51 I became a professor. I knew Veblen quite well in that time, probably as well as anybody during that time.

Tucker: Well, you had a certain common background.

Montgomery: Yes, that didn't play any part in that. You know there's a woman who wrote a history of the Institute for Advanced Study from

the beginning till 1950. It's under lock and and key somewhere, once the faculty was allowed to read it. I thought I was a friend of this woman, but reading it over I found that essentially it's a hymn of praise to Oppenheimer and Leidesdorf [chairman of the IAS Board of Trustees], whom she was writing it for. Of course, Oppenheimer was at war with Veblen, so I come out pretty badly. I come out as somebody that Veblen brought here as his assistant hatchetman, which is the way Oppenheimer might have viewed me [laughter]. I didn't think of myself that way, and I don't think Veblen did. I don't think the fact that we both had some roots in Iowa or Minnesota had anything to do with it.

Tucker: Did you ever talk very much hard mathematics with Veblen?

Montgomery: Not a lot, a little bit. Of course at the time I knew him best he was from 68 to 80 years old. His interest then was in differential geometry, which I didn't really know anything about. I regret that.

Tucker: Spinors and such.

Montgomery: Lately I've been trying to learn some, but at that time I didn't make much effort in that direction. I should have.

Nebeker: You were at Yale also, after the war.

Montgomery: Yes, I was at Yale for two years. That was a pleasant life.

Nebeker: I wondered if you could compare the mathematical communities at those places you were—Harvard, Princeton, and Yale—in the '30s and '40s.

Montgomery: I'm no doubt a Princeton partisan. I always found it a more open and friendly place than either of these two others. At the time I was at Harvard, of course, I'd arrived practically as a farm boy from Iowa. I found Harvard rather stiff and conventional, and there was really no way at that time to meet people very well. There was no common room, say. If I wanted to see Birkhoff or Stone, which I did occasionally, I would have to make an appointment or maybe lay in wait after they had a course just to ask them a question. That's one of the things I liked about Princeton: everybody was in the same building and you met frequently.

Nebeker: What about number of talks and visitors and such?

Montgomery: Well, there were, I think, at that time more at Princeton than there were at Harvard, and I'm sure there were more here than at Yale. When I was at Yale it was emerging from, well, quite an old period. There were some good mathematicians there, certainly, but there wasn't an overwhelming number, I don't think. There was a great feud going on at Yale, which had gone way back in history before many of the participants then were there even. I was the only

one on speaking terms with everyone in the department when I was there.

Tucker: It was mainly between [Oystein] Ore and [Einar] Hille, wasn't it?

Montgomery: It came to be that, but it didn't originate that way. Hille was married to Ore's sister.   While I was there Ore was very seriously sick in the hospital for about six weeks, hovering between life and death.   From time to time I'd call up his wife to ask how he was, and the way his sister would find out how he was was by calling me and asking me what I'd heard.   That's a rather intense feud, I would say.   I don't mean to say that in Princeton everybody loves everybody else.   Certainly not, that would be a very unnatural situation.   But things were more open.   You knew people, and they were much easier to talk to, at least I found it to be that way.   And I liked that.

Tucker: I certainly agree with you, because I was at Harvard as a National Research Council Fellow the year before you were.   I was there '32-'33.   I was actually offered the Peirce Instructorship, because Morse very much wanted to keep me around because he was learning the singular homology theory and that sort of thing from me, which he wanted for his calculus of variations in the large.   I turned the offer down to come back to Princeton at a lesser salary, because I found the atmosphere so much more congenial here than at Harvard.

Montgomery: I had a somewhat similar experience when I was at Smith. When I had been at Smith about three years I was offerred an assistant professorship at Harvard, which I declined.   You know, it may have been a mistake, but it reflected my feeling I suppose.   I'm not boasting, but it's some reflection of how Harvard had affected me.   I don't think it would now affect me in the same way.

Nebeker: Do you think the more congenial atmosphere and the greater contact among mathematicians results in more research?

Montgomery: I think it probably does.   It brings a lot more points of view to bear on whatever you're doing.   I think Princeton is a hell of a place if you consider yourself the best mathematician in the world, because it's hard to be sure, and even if you feel sure a lot of other people don't feel sure, and it's rubbed into you all the time.   If you're not quite so ambitious in your thoughts about yourself, it's very pleasant.   It's a little depressing to realize that whatever you do somebody else could probably do better.   You have to be sort of tough, maybe, to take it, I mean on a permanent basis.   Not everyone can, by the way.   That's why some people have resigned from our faculty, I think.

Nebeker: I noticed that you have written quite a few joint papers and even joint books.   It seems to me that with many of the mathematicians the primary benefit of the atmosphere was social rather than leading to collaborations in research.

Montgomery: Whether that's true in general, I don't know, I haven't thought about it. But mathematics is a highly individual thing. In the end you have to sit in a room by yourself a lot of the time, even if you're being a collaborator, and think about things and try to understand them. It isn't like chemistry or physics where you may build some big pile of machinery, and you have six or eight people take part and all put their names on the paper—and so does the head of the laboratory whether he knows what's happening or not. That doesn't go on in our subject.

So I think Princeton was a pleasant place, but besides that I think it was a great place at that time—and that it still is. The two institutions here complement each other, and the fact that they're entirely separate administratively is very good. I think if they tried to be closer than they are, it would probably break down. It's good to have them apart and to have informal contacts I think. But I think the mathematicians in Princeton at that time, and still for that matter, make one of the best mathematical centers in the world. I'll put in a plug for Princeton.

Nebeker: How would you describe the change in the relationship between the Institute's School of Mathematics and the University's Department of Mathematics when the Institute got it's own buildings? Of course in the early years they were together physically. Did the move make a great difference in the amount of contact between the two groups of mathematicians?

Montgomery: Well, it certainly made some. They weren't in such close physical contact.

Tucker: With the war coming along it's difficult to say. That overshadowed other things.

Montgomery: Yes, but I don't think it made a big difference really. Many people go back and forth still.

Nebeker: To talks?

Montgomery: Yes.

Tucker: I think the young people who came to the Institute liked very much to come over to Fine Hall, because there seemed to be somewhat more going on there, such as graduate courses and a great variety of seminars. There was much more of this going on at Fine Hall than at Fuld Hall. I don't mean that there wasn't a great deal of mathematics going on at Fuld Hall, but it was mainly in terms of individuals or informal contacts. Things like seminars and courses seemed to go with graduate study, whereas the whole point of the Institute was to deal only with people who were no longer students.

Montgomery: I think that's true. Also the Institute for a time did not have very many seminars. In recent years it's had more, and it's possible that at times it's had too many. I think you can have too

much structure; it may have a tendency to make everybody feel that that's what they have to do. I think one should avoid that. Nobody short of God knows exactly what everybody should do. I know there are some people who feel that they know, but I never believed that even when I was a young man. Certainly I now know that I don't know enough to push people towards some one thing.

Tucker: At some point I would very much like to have Deane talk about Oswald Veblen. Right now we're in what used to be Veblen's office.

Montgomery: Sure. There's his picture.

Tucker: I think that Deane is the person who knew Veblen best.

Montgomery: In his later years, that may be true.

Tucker: But I'm sure you heard him talk a great deal about his earlier years.

Montgomery: Well, I did, yes. Veblen was a generous man in his comments about other people. He and [Solomon] Lefschetz were not friends. Lefschetz had a great hatred for Veblen; I don't think Veblen had that for Lefschetz. I don't think I ever heard him say a harsh word about Lefschetz. The nearest he came to it was one time I made some remark about this feud, and he said, "It's true that he seems to want to consign his old friends to oblivion." [Laughter.] And then he changed the subject. Whereas I've heard Lefschetz talk a great deal and very vociferously about Veblen. This was mostly before I knew Veblen.

Tucker: Well, despite my close association with Lefschetz, I always had good relations with Veblen. I think there were times when Veblen tried, through me, to influence Lefschetz.

Montgomery: Well, that may be. I don't know about that at all. I think Veblen was underneath an extremely forceful man. He was a little deceptive in this way. He had a rather hesitant way of speaking, very tentative and diffident, but he really was an extremely forceful man. I think he played a great part in building up the Department at Princeton. He's not the only one who helped, but I think he was one of the strongest forces in that. Of course he and Alexander were probably responsible for bringing Lefschetz here. Maybe this is one thing Lefschetz had against him.

Tucker: Well, he wanted to have it credited to Alexander. He always thought the best of Alexander.

Montgomery: He was very attached to Alexander. And von Neumann, too, although he disapproved of some of von Neumann's later activities. He strongly advised him not to go to the Atomic Energy Commission, which made von Neumann rather angry.

Nebeker: Why did he advise against that?

Montgomery: I would have done the same thing, had he asked me.

Tucker: I would, too.

Montgomery: I don't think a mathematician should be involved in politics. I think it's a sign of weakness if a mathematician—or a physicist, for that matter—gets involved in politics. Physicists are much more tempted in this direction than mathematicians. That's one of the advantages of mathematics, I think.

Tucker: To get back to Veblen and the building up of the math department. I've been reading about the background. One wonderful source of information is what R.C. Archibald wrote at the time of the semi-centennial of the American Mathematical Society.

Montgomery: Yes, he has sketches of the former presidents.

Tucker: All of the former presidents up to 1938, with three or four pages concerning each one.

Tucker: We were talking about Veblen's tremendous influence, which, as you have pointed out, was almost imperceptible at the time it was happening. It is only in retrospect that you are able to appreciate the leadership that he was giving, because, as you say, he was so diffident.

Montgomery: Well, that continued in his time here at the Institute, I think, and even into his retirement. He described Princeton one time to me, Princeton when he first came here—I don't know, 1910, let's say—as a rather pleasant place which certainly as a whole did not want to have a good mathematics department. It just wasn't on their mind. But somehow or other over the years they got one without quite realizing it [laughter].

Tucker: Well, I think H.B. Fine had a great deal of hope for the Department.

Montgomery: Oh, he gave great credit to Fine, certainly. No, he didn't say that he was alone in his desire, but he meant the place in general.

Tucker: Fine, you know, is the one who got the University research fund, say about along about 1926, and the Fine Professorship which Veblen enjoyed for many years was essentially the accomplishment of H.B. Fine.

Montgomery: Oh, certainly.

Tucker: And of course the building, which may have had a great deal to do with it, it's hard to say. When you first came to Princeton was it the building that created the atmosphere?

Montgomery: The building contributed a lot, I think.

Tucker: Yes, that common room.

Montgomery: Oh, certainly. One story that Veblen told me about that.
There were funds for this building, and Wedderburn was assigned first
to draw up plans for it when Veblen was in England. When Veblen
came back he said he looked at these plans and thought they might be
suitable for a small outhouse on a farm [laughter]. So I guess he
helped change these plans considerably.

Tucker: Oh, I'm sure he did. Of course there were actual architects,
but I think that the architects took ideas that Veblen had presented to
them.

Montgomery: You know, some years ago they were building one of
these little buildings here at the Institute. It happened that the
foreman on that construction was the foreman of the crew that built
Fine Hall. He and Veblen were out there one day talking, and this
fellow said, "My God, what a difference between this building and Fine
Hall."

Nebeker: What was your own experience with the common room in Fine
Hall in '34 and '35?

Montgomery: Well, I found it a very pleasant place. You met people
there. Now, I've never met Leopold Infeld, but I read his book. He
said the common room was a place of great jealousies and intrigue, and
of competing for jobs. There was, of course, competition for jobs. I
got some kind of a job and he did not, and maybe that makes the
difference.

Nebeker: Did you go every day to the common room?

Montgomery: Oh, I'm sure I did, for a while at least. I may not have
stayed there a long time, but I'd be there a few minutes almost every
day, or maybe an hour or two for that matter, if I happened to fall to
talking with somebody.

Tucker: Seminars were usually arranged either to follow tea or precede
tea. Going to tea was just something that everybody did. They might
only be there for a few minutes. If you wanted to arrange to see
somebody on an informal basis, you simply said, "See you at tea." The
afternoon tea was Veblen's invention.

Montgomery: It comes from his English influence, I guess.

Tucker: Well, before Fine Hall was built, Veblen had an office in
Palmer Hall, up on the floor with the 300 numbers. (I think it was the
second floor.) Of course it was intended as a lab office for a
physicist, so there were bunsen burners there. Veblen organized an
informal tea club there, mainly with the help of the Englishmen who
were around, like Henry Whitehead. I didn't participate in this.

I was here for two years before Fine Hall was put into use, and at the beginning of that third year when we started using Fine Hall, Veblen asked me to come and see him. He said, "You're the chairman of the tea club." I said, "Well, I don't care for tea." "Oh," he said, "have coffee or cocoa, whatever you like, but as Procter Fellow, you are automatically the chairman of the tea club." During that first year, it was all done by graduate students, and everyone who held a fellowship had to take a turn. We worked out a rotation: certain people would do it on Monday, certain people would do it on Tuesday, and so on through to Friday. But Veblen appointed me to be the chairman [laughter] as he called it, and I had quite a time getting some of the fellowship holders to do their share. A chap by the name of [J.L.] Barnes, who later became an electrical engineer...

Montgomery: The husband of Mabel Schmeizer, I suppose.

Tucker: Yes, that's right. He was the treasurer of the tea club. We levied dues, and all this sort of thing. I'm very thankful to Veblen for having forced me into that, because, from that time on, the idea of the common room and tea was very congenial to me. I suppose I would have come to it in time, but he just said, "Do it!"

Nebeker: What about when the Institute moved here—was there a similar afternoon tea?

Montgomery: Yes, there still is.

Nebeker: Was that Veblen's influence?

Montgomery: I assume so.

Tucker: Oh, of course.

Montgomery: He never talked about it to me, but it seemed like a continuation of it.

Nebeker: Did he himself always go to afternoon tea?

Montgomery: Oh, no, not always in his later years, but I suppose he did when he was younger. In his later years sometimes he and I would go out in this woods that he owned, and walk around there in the late afternoon and have tea at his house.

Tucker: He was a great outdoorsman.

Montgomery: Yes, he was. He seemed to be fond of the outdoors all his life, and liked woods.

Nebeker: Did he often take walks in the Institute woods?

Montgomery: Oh, yes. I never took part in this, but he organized what was called a wood-chopping group. They used to go out and clear some of the paths that nobody had cleared at that time. You see

the fact that the Institute got these grounds was probably influenced very heavily by Veblen. He lived on Battle Road.

Tucker: I pointed the house out to you as we were coming over.

Nebeker: Yes.

Montgomery: He knew this area, and I guess he knew that this farm was for sale and persuaded the Institute to buy it. I think also he had a great influence—this is more important perhaps—in the early history of the Institute. I think he probably had something to do with the fact that it was located in Princeton. At least he told the story as though he had some influence on it. You know, I've seen the Japanese movie, *Roshomon*, where the same incident is recalled in four different ways by four different people, each one making himself considerably more prominent. Maybe the history of anything is like that.

The Bambergers wanted to do something. They got in touch with Flexner, perhaps thinking that they might do something in medicine, but in the meantime Flexner had changed his interest toward something like this. The Bambergers had made all their money in Newark, so they wanted to have it in the vicinity of Newark. I think Veblen told me once that he had pointed out that Princeton is in the vicinity of Newark, and that came to be accepted. He probably thought it was a good idea to have something like the Institute, but he thought it was very important to have it near a university, so that the two could interact. He always felt that that was extremely important. For him the main purpose of the Institute—I say this in my obituary of Veblen—was for the impact it had on the academic scene, especially on the American academic scene, and in particular its influence on young mathematicians.

He said once that there were probably two different conceptions of how the Institute should function. One, which was adopted more by the historical school, I would say, is that it's a group of great scholars who occasionally communicate with the public and who have great thoughts. Veblen said he and Einstein and Weyl didn't feel up to that; they thought it was much more important to do this post-doctoral thing, to have what influence they could on post-doctoral fellows and let them have influence on each other. He thought that was the central role. The School of Mathematics here has largely followed that tradition. I think many of the physicists have had that tradition, too. Historians, not quite so much; though in recent years, a little more. They tended more to think of it as a lifetime fellowship for themselves. Somehow they didn't feel the same loyalty to the place and the same responsibility for pulling it up that the mathematicians did.

Nebeker: Are you talking about permanent members of the Institute? That Veblen would select people who would work with younger mathematicians?

Montgomery: I don't know that that was topmost in his mind. I think that he felt that you had to try to get one of the best people available

from a very small group of the best people, and that if he failed it was not for lack of intention. But I certainly don't think he failed in his original faculty. I don't know that everyone here quite understood the terrible drive that he had toward excellence. There was one conversation I had with Oppenheimer, I'd say about three years before he died. We'd always had a lot of wars with him, the mathematicians had in particular. But I thought this silly, and I thought we should have some talks.

We had three or four heart-to-heart talks for an hour or two. Each time he received me with great charm. None of this ever did any good [laughter], but in the course of it I said to him something like what I've been saying. I said, "You know, it may very well be that I'm not good enough for the job, but that's not my fault. And since I'm here I, want to do the best we can." And I said, "That's the attitude of the Math Department. We want to have, you know, the absolutely best people available, and don't let personalities or anything enter in, just try to get the best people you can." And I said, "I don't think everybody on that faculty understands that." Oppenheimer said to me, "Deane, you're right. Not everybody does understand that. I understand it." I said, "Well, I think you do." He said, "But I never believed in it." [Laughter.] You know, if I were to tell this story to the public, nobody would believe it. But it's an absolutely true story, and I knew it was true, but I just didn't expect to hear it from him [laughter]. He said, for example, "When we brought Mr. X here"—this Mr. X is now gone—"we weren't trying to get the best guy we could. We wanted a fairly good guy who would be a pleasant associate." I knew that [laughter]. He had these moments of truth.

But Veblen was really not that way. I mean he had a sort of a rule. One time he said, "Well, if they're two fellows that seem about equal on ability, always choose the greater son-of-a-bitch to be sure you're not showing bias for your likes. I don't know if he was completely serious, but I thought he was serious. But I don't know much about what happened in the formation of the Princeton department. I simply wasn't here. I know what Veblen told me, which was not a whole lot. He talked more about the Institute to me, and so on.

Tucker: Well, I think it's very hard to tell exactly what part Veblen had in the Institute, for the reason that he influenced people without their being aware of it. I've read the autobiography of Abraham Flexner; you probably have, too. I've read again and again the part that he tells about the forming of the Institute for Advanced Study. And it's clear to me that he takes much greater credit than he should.

Montgomery: It's the *Roshomon* story.

Tucker: He says that he wanted to have Einstein from the very beginning, that he essentially courted Einstein, and that it was when Einstein was deprived of his citizenship that he became willing to come. Then his second person was Weyl. He says that he was told by Hadamard that Weyl was the person to get, that Weyl was the successor of Hilbert. Then he doesn't say anything more about actual people.

(PMC25) 13

Montgomery: Yes. I don't think it was like that. Veblen made the remark to me once—see, he was appointed, I believe, as the first professor.

Tucker: That's right. He started in 1932, and the rest didn't start until '33.

Montgomery: What Veblen said to me—he wasn't bragging, he just said it—was "I got my slate for mathematics." You know after Mrs. Veblen died the family gave me a few pictures they didn't want. Among those is one that Miss Dukas had never seen. She wished that she had, because she would then have put it in this book that she and somebody wrote. It was a picture of Einstein and Veblen marching in the academic procession in Princeton in 1920. Einstein is in long tails, formal clothes, the way I suppose German professors used to be. They were just walking along. I guess they were going to give Einstein an honorary degree. He gave a few lectures here sometimes.

Tucker: Yes, he gave lectures that went into the book published by the Princeton University Press called *Meaning and Relativity.*

Montgomery: Yes. I wasn't able to identify the year of the picture, but Miss Dukas could. She said it was 1920, I think. So I gave her a copy. No, I don't think Einstein would come here unless Veblen had been associated with it. I don't think if just Flexner had been starting an institute that Einstein would have joined.

Tucker: No, Einstein was very well aware of the fact that Eisenhart and Veblen had worked in differential geometry. Indeed, I've seen the copy of a letter that Eisenhart wrote. The Eisenharts were going to Europe for the summer. This would have been 1919 or 1920, something like that. Eisenhart was asked by the President of Princeton, who was Hibben, to call on Einstein and make him an offer for Princeton University, to come either permanently, or to come as a visitor. This ended up with Einstein agreeing to come to give some lectures. He went to Cal Tech at the same time, to give some lectures.

Montgomery: I can't be sure, I'm just telling you what Miss Dukas told me.

Tucker: There's no doubt at all that it was this background that made Einstein willing to come.

Montgomery: Oh, sure.

Nebeker: What was the relationship between Veblen and Einstein? Did they collaborate in any way?

Montgomery: No, they never collaborated. They knew each other very well. For instance, at the time Mrs. Veblen died I was the executor of the estate. I didn't go to the auction of the effects, but one of them was some old second-hand trunk with the name Einstein on it that he'd left in their place in storage. Somebody probably paid quite a bit of money for it, I guess, I don't know [laughter].

But Einstein and Veblen were good friends. See, Einstein was not so actively involved in the Institute. He didn't have the same loyalty to it, I think, that Veblen did. He probably thought Veblen was right, and let Veblen do it. Einstein became active in some of the early battles of the Institute, and so did the others. But I don't think most of them would have unless Veblen had been talking to them about it. For instance, Flexner wanted to bring in some economists, and he brought them into the Department of Economics. You see, he was motivated partly because it was the time of the Depression. He wanted to do something for the country—this appeals to the Trustees, too. I guess he sort of argued, I don't know if he put it quite this way, that these fellows will solve the Depression and, you know, eliminate war, maybe, for all time. That sort of thing.

Veblen went over to talk to the economists at the University. I think one he talked to was [D.L.] Kemmerer, who was a well known economist at Princeton at that time. Kemmerer said, "These fellows are about at a level of the average assistant or possibly associate professor we have in our department." Well, on the basis of that sort of talk, this thing was violently opposed by quite a lot of people at the Institute, but Flexner got his way. But it led to a sort of revolution in which Einstein took part. I don't think he would have if Veblen hadn't been agitating about this in a quiet way. Neither do I think von Neumann would have, or Weyl, for that matter. Weyl, though he may have been a greater mathematician, I don't think had the ability to build up the place that Veblen had. I mean they had certain complementary talents.

Nebeker: Was that principally a matter of interest, having this goal and working for it, or a matter of personal qualities of some kind?

Montgomery: Well, I don't know. I wouldn't be able to explain it. Of course for one thing, Veblen was a native American. I know he says in his obituary of Birkhoff, which I quote in my obituary of Veblen [laughter], that it's difficult to imagine the feelings of those early days when there was really very little mathematics in the United States. You were just beginning not to have to go abroad to study. It's difficult to imagine the sort of a religious fervor that some of us had toward establishing the subject in the United States. I think that Birkhoff had that, and I think Veblen had it. I don't think many of these other people felt that same urge. They came from different backgrounds. Of course, even if you come from the United States, you don't necessarily have that feeling. I don't think Alexander did, though he was happy to go along and support the effort.

Tucker: Yes. I don't think Eisenhart had it.

Montgomery: He had more of it.

Tucker: I think Eisenhart and Veblen teamed up extremely well.

Montgomery: Oh, yes, Veblen always gave Eisenhart and Fine credit.

Tucker: One little point I picked up in the interview that was done with Churchill Eisenhart: Churchill Eisenhart's mother was the first Mrs. Eisenhart, not the one that you would have known.

Montgomery: Oh, I didn't realize that.

Tucker: She died when Churchill was five years old—no, even before that.   When Eisenhart remarried—this was when Churchill was five years old—they went to Europe or somewhere on their honeymoon, and the Veblens took care of Churchill Eisenhart.   That's just a nice illustration of the generosity that Mr. and Mrs. Veblen had.

Montgomery: Well, maybe the Veblens wanted children of their own and couldn't have them.   I never knew what that situation was.   Well, this has led me to think that, in general, people on faculties may have some kind of ideals toward making a place good, but they don't want to do much about it.   So they're very glad if some fellow like Veblen comes along and is willing to make a tough effort.   They're willing to support it in a kind of a quiet way.   And I think any place, probably, where there's a good department, it is pretty much like that.   It's centered around one or two or three or four people who somehow get the rest to go along—with what they believe to be right, but just never expect to carry out themselves.   I'm inclined to think that's the way things are built up and that they're very fragile.   It's easy to take them down, even when you have them going.