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The Princeton Mathematics Community in the 1930s
Transcript Number 20 (PMC20)

BANESH HOFFMANN

(with ALBERT TUCKER)

This is an interview of Banesh Hoffmann by telephone. He is in New York. The interviewers, in Princeton, New Jersey, are Albert Tucker and William Aspray. The date is 13 October 1984.

Aspray: Why don't we begin the interview by asking you what the events were that led to your coming to Princeton in 1929?

Hoffmann: I was at Oxford University. I was sort of a strange case. I had first of all taught myself Pitman's shorthand. Secondly I had fallen in love with relativity, and I had taught myself relativity because there was no one at Oxford who was giving any lectures on it. I was neglecting my regular course work, and I would have been in quite a sad situation if it hadn't been for the fact that in my last year there was an exchange of professors: G.H. Hardy, who was then at Oxford, went to Princeton for a year, and Oswald Veblen, who was at Princeton, came to Oxford for a year. And it just so happened that Veblen was interested in what he called projective relativity.

Aspray: What was projective relativity?

Hoffmann: Well, in general relativity you have a theory of gravitation. Now there was an attempt to get a larger geometrical structure that would let you handle in a unified way gravitation and electromagnetism. One of those attempts was by [Theodor] Kaluza and elaborated by [Oskar] Klein. The idea was to add a fifth dimension in an undeveloped form, sort of an embryonic thing. I can go into details if you want.

Aspray: I don't think that's necessary.

Hoffmann: Now Veblen, who was a geometer, realized that this fifth dimension wasn't really needed, that you could make a four-dimensional theory in which you had as the basis projective geometry and used homogeneous coordinates, that you'd have five homogeneous coordinates for four-dimensional space and time.

Tucker: We understand.

Hoffmann: Now, Veblen came to Oxford and gave lectures. I attended the lectures because they were about relativity. I'd never heard about the Kaluża-Klein theory or the projective theory. Veblen explained it all. I asked a lot of questions. It ended up by his inviting me to come to Princeton and be his research assistant. My duties would be not only to work with Veblen, but also to take shorthand notes of his lectures and then write them out so that they could be distributed. And after being in Princeton a while, I was doing a fair amount of this for various visiting people.

Tucker: Tell us about when you arrived in Princeton, the things that struck you?

Hoffmann: Well, let me tell it from the point of view of Oxford versus Princeton.

Tucker: Yes.

Hoffmann: When Veblen came to Oxford he was an absolute revelation to me. His whole attitude towards mathematics was different from the attitude that I had absorbed in the English schools and then at Oxford. I think that the essential difference was that the Oxford professors and lecturers presented series of lectures that were beautiful—complete and with no open questions. This was the general impression that I had. But Veblen was completely different. The Oxford lecturers discouraged any interruptions; they were presenting a beautiful structure, and we shouldn't ask questions, we should just take it down.

Veblen was, I don't know how to describe it, cautionary. He gave the impression that mathematics was an open subject that was still developing and that you couldn't give honestly a closed series of lectures. He not only allowed interruptions and questions, he rather enjoyed them. There was, however, one person, who shall be nameless, who asked too many questions, very long questions that turned out to be dogmatic statements rather than questions. That was the one instance in which I thought that Veblen was showing impatience. But on the whole his attitude was quite different from what I had been brought up to think of as the essence of mathematics.

Tucker: I can make a comparison for you. At Princeton, I felt that Wedderburn taught in the style that you noticed at Oxford. Whereas the other professors that I had courses with at the same time had this open attitude.

Hoffmann: Yes, you're quite right about Wedderburn. I'd forgotten. As a matter of fact, wasn't he educated in England?

Tucker: He was educated in Scotland, Edinburgh.

Hoffmann: Scotland. I see.

Tucker: But that's not too different. Who were some of the professors that you took courses from as a graduate student?

Hoffmann: The chief one, apart from Veblen of course, was H.P. Robertson. He was in relativity, so naturally I would gravitate towards him. He gave a series of lectures, and I think also he and Ed Condon gave a weekly seminar.

Tucker: Yes, that's right.

Hoffmann: They were trying to keep us current with all the frustrating quantum things. You probably know that Ed Condon happened to be in Germany when the quantum revolution was really fomenting. He was so depressed. It was so hard to keep up with what was going on that he decided that he was going to give up his attempt to be a professor. But somehow or other he got to Princeton, and I think that it was because people persuaded him that, though he may not have thought he knew it, he knew it better than anyone else in America at the time. I was closer to Robertson than to Ed Condon, but both were very important for me, along with, of course, Veblen.

I have a little story about Ed Condon. His marriage was, I think, not completely happy, I'm not sure. Anyway, he was discussing a paper that someone had done. I've forgotten what it was. But to make it general the writer of the paper said, "Let there be n sexes." And Ed Condon, in writing a review of this article, wrote, "Breathes there a man with hide so tough, he thinks two sexes aren't enough."

Aspray: Can you compare the styles of Robertson and Veblen?

Hoffmann: Veblen always seemed to be more tentative. If he said something, he was not willing to sharpen it too much, I think. Robertson and Condon and others didn't have so much of that tentativeness. I'm not making any sense.

Tucker: No, you are. The word tentative in reference to Veblen is right. I have the same feeling.

Hoffmann: I'm glad to know that. When I met Veblen I had never heard of Veblen, but Eisenhart's book on Riemannian geometry had made me realize there was an Eisenhart and that he was at Princeton. I therefore came to Princeton with the strong feeling that the really important person was Eisenhart. That was not the case. It was very clear that the prime mover in everything was Veblen, assisted by Eisenhart, but Eisenhart was not the prime mover. I thought also that Veblen's mathematics was more profound than that of Eisenhart.

Tucker: Yes, and much broader.

Aspray: Did you study at all with T.Y. Thomas?

Hoffmann: I knew T.Y. Thomas, and I chatted with him a lot. I don't know, I may have taken a course with him, that would be on tensor analysis. Probably I took that course. My feeling is that I knew a fair amount of the subject before I took the course. It wasn't so much of a revelation to me.

Aspray: What about von Neumann?

Hoffmann: I was very often asked, I told you, to take notes in shorthand and write them up, then take them to the professor and make corrections, and finally have them distributed. You know English wasn't von Neumann's native language, but he spoke faster than anybody I've ever heard speak. It was really quite a job taking that down even in shorthand. So my one overwhelming feeling about von Neumann is he spoke so fast.

Tucker: He also thought very fast.

Hoffmann: He thought very fast, yes, and he was extraordinarily subtle. He was most impressive. You've heard the story of Robertson driving von Neumann to somewhere. Von Neumann asked him what he was working on, and Robertson said such and such an equation. By the time they got to the end of the ride von Neumann had solved the equation in his head. Had you heard that?

Tucker: No, but it's typical.

Hoffmann: Yes, he was incredible.

Aspray: You mentioned earlier that you had come to Princeton as Veblen's assistant. Had you intended to get a doctoral degree at Princeton?

Hoffmann: I don't know when it was I decided to work for a Ph.D. It must have been in Oxford that Veblen persuaded me to think of a doctoral degree.

Aspray: How is it that you came to write your dissertation most closely with Veblen, rather than with Robertson or one of the others?

Hoffmann: The situation was not as simple as your question suggests. You see, I learned about projective relativity from Veblen in his lectures at Oxford, and when I came to America Veblen wanted me to work with him. The problem was that Veblen was an outstanding geometer but didn't have much feel for the physics of relativity. I was not a geometer at all, but I did have some feel for the physics of relativity. So in a sense I was complementing Veblen. We published the paper "Projective Relativity" jointly, and then it was decided that I should do something on my own for a thesis. That paper didn't count as my thesis.

There was a theoretical physics colloquium going on, and Wigner and Robertson asked me to look into some basic questions in relativity that hadn't been explored, one of them being how do you make measurements, experiments, to tell you what the components of the metric tensor are. You would have to shoot particles and find their trajectories in your coordinate system and so on. It was a complicated matter.

Well, there were three or four such things, and I reported on them at the seminar. Wigner and Robertson were so pleased that they wrote to the board of *Reviews of Modern Physics* and asked them to publish my paper, which they did. Then that wasn't considered enough for the Ph.D. thesis, so I asked Robertson what was a good topic that I could do just to make it look like a thesis. He suggested that I try gravitational waves and electromagnetic waves. So I worked on it, worked on it, worked on it.

After several months I was utterly disgusted, because I had shown that you couldn't have such waves if you wanted them to be spherically symmetric. For a week I was biting my nails wondering what on earth I should do. Suddenly it occurred to me, "My goodness, you've got a much better result than the waves. You've got a theorem that they can't exist." I am really amazed that for a week I had been utterly despondent. Well, that paper was accepted as a thesis. It was so short that I decided to publish it in a journal with small page length, and I think it had about 13 pages.

Tucker: That was "On the Spherically Symmetric Field in Relativity"?

Hoffmann: That's right.

Aspray: I know that Veblen was quite enamoured with the intellectual and social environment at Oxford. Could you make some comparisons of Oxford and Princeton?

Hoffmann: My impression was in a way the opposite. I remember Veblen saying to me, with sort of scorn in his voice for the Oxford system, words to this effect: "Look, you have mathematicians, each mathematician assigned to a college. There they are, off on their own, and they meet only once a month when there is a meeting of the Mathematics Club of Oxford or something. That's not the way to do mathematics. You should all be in the same building, and you should all be available and chatting." He was describing the sort of atmosphere there was at Princeton, which I found very comfortable. You could meet the professors on a sort of equal footing at teas, and ask questions and not be bawled out for doing so. I didn't get the impression that Veblen was impressed by the social arrangements at Oxford, but that may have been social arrangements not with regard to mathematics. I really don't know.

Tucker: But he did like the architectural style. As you know, when Fine Hall was built it had many features that were, we thought, copied by Veblen and Wedderburn from the Oxbridge.

Hoffmann: Absolutely. When I came to Princeton it seemed to me that someone had a sort of salt shaker with Oxford-looking buildings in it and just shook it all over the campus. It was very amusing.

Tucker: Yes.

Hoffmann: You know the Magdeline Tower in Oxford. There was a building with a Magdeline Tower in Princeton except that it was not very high. I've forgotten the building, it's on Nassau Street.

Tucker: Yes, it's Holder Tower. And the Graduate College was very English-looking.

Hoffmann: Indeed, yes.

Tucker: And the gowns that you wore to dinner.

Hoffmann: Oh yes.

Tucker: That was not at all American.

Hoffmann: No. It was interesting to see that when Veblen lectured in Oxford he didn't wear a gown. Other lecturers all wore gowns. The only exceptions were for experimental physics and experimental chemistry, in that the gowns might be dangerous. So they had a special dispensation that allowed them to lecture without wearing a gown. Of course when I came to America, nobody wore a gown for lectures at all. As you remark, at the Graduate College they did ask us to wear gowns for meals.

Tucker: At the University of Toronto where I did my undergraduate work, many of the professors, even those teaching mathematics, wore gowns to their lectures. So that when I came to Princeton there was no sharp change from my previous experience.

Hoffmann: Oh, so you didn't notice it.

Tucker: I mean that the atmosphere of the Graduate College seemed much at home for me coming from Toronto.

Aspray: Did Veblen make his views about the relative advantages of the Princeton situation versus the Oxford situation widely known while he was in Oxford?

Hoffmann: That I don't know. I remember the scorn in his voice as he told me, words to this effect: "All these mathematicians. They meet once a month, and then each goes to his own little cubby-hole and develops psychotic symptoms almost and has no contact for a whole month with fellow mathematicians." Obviously Veblen felt quite strongly about it, but I don't think he would have expressed himself to the authorities with quite the vehemence that he did to me.

Tucker: Did you know Henry Whitehead?

Hoffmann: Oh yes, of course.

Tucker: What were your contacts with him?

Hoffmann: Well, he was in pure mathematics, and I must say that I'm not really a mathematician at all. So while I knew him and chatted with him, I didn't really follow the work that he was doing. He was doing it alone and with Veblen as you know.

Tucker: Yes.

Hoffmann: He wrote that book.

Tucker: It was a Cambridge tract.

Hoffmann: Was it really? *The Foundations of Differential Geometry.* I'd forgotten it was a Cambridge tract. Veblen had previously done the quadratic differential-forms book as a Cambridge tract.

Tucker: That's right. They were both Cambridge tracts.

Hoffmann: I see. In that *Differential Geometry,* do you recall the marvelous definition of a geometry?

Tucker: You mean, "It is whatever people of sufficient taste say it is."

Hoffmann: You've got the essence of the phrase. I thought that was wonderful.

Tucker: Veblen over the years had been in almost every form of geometry, and I feel that he was trying at the time we were graduate students to develop a definition of geometry that would encompass everything the way the definition of Felix Klein had in the 19th century.

Hoffmann: Yes. You mean the Erlanger Program? One never heard of that at Oxford until Veblen came.

Tucker: Yes.

Aspray: Since you were in Princeton in 1930-31, you are one of the few people we can ask about the founding of the Institute. Do you have any memories of its first getting started and the talk about it?

Hoffmann: I was completely on the outside, except that every so often I learned some things that were happening. Veblen didn't discuss it with me. I have the impression that Veblen was really the prime mover, even though the money came not from Veblen.

Tucker: I agree.

Hoffmann: He was tremendously enthusiastic about the possibility. I feel strongly that if it hadn't been for Veblen the Institute might not have settled in Princeton. Veblen arranged for the Institute to use the Princeton University buildings to some extent before they built their own.

Aspray: Do you have some memory of how much of Veblen's time was spent during that period in working on founding the Institute, as opposed to his own research and work with colleagues and students?

Hoffmann: That's very hard for me to say. My strong impression is that it was a considerable amount of his time that was being used on behalf of the Institute.

Tucker: However he did not become an actual professor of the Institute until 1932. The year '32-'33 was, I think, the critical year. The decision had already been made to have the Institute at Princeton, but the assembling of the faculty and so on was done by Veblen in the year '32-'33, when he was already designated as a professor at the Institute and paid by them starting in '32. The other professors in mathematics did not start until the fall of '33.

Hoffmann: Yes. I'm looking at my notes here. Veblen at Oxford was a preacher almost. He wanted to convert people from the English type of mathematics into the more venturesome things like topology and group theory and so on. I think that there was a man, M.H.A. Newman, who was studying topology, and that Veblen sort of boosted him.

Tucker: Yes, Max Newman spent a year at Princeton in '27-'28, and he developed ideas in topology, or analysis situs as it was called at that time, in conjunction with Alexander and Lefschetz.

Hoffmann: I see.

Tucker: I got to know Max Newman very well, because I spent a term at Cambridge right after I got my Ph.D. and he was the supervisor of my National Research Fellowship. Perhaps you know that he died earlier this year.

Hoffmann: Oh, no. I didn't know that.

Tucker: There was a very good obituary of him in one of the recent notices of the American Mathematical Society, quoting from the obituary in the London *Times*. And Peter Hilton, a topologist at SUNY Binghamton, had the duty of writing the official obituary for the London Mathematical Society. I heard from him just recently asking for anything I could contribute from when Newman was here.

Hoffmann: Well, my feeling was that Veblen had exerted pressure so that Newman could get the position at Cambridge. Certainly Veblen was enthusiastic about that.

Tucker: Well, he had a fellowship at Johns, but did not ever get a professorship at Cambridge. His professorship was at Manchester. He became the head of a very strong school of mathematics in the '50s and '60s at Manchester.

Hoffmann: I see. Veblen talked about him quite often and was pleased that he was a topologist.

Tucker: That was because Max Newman was at Princeton the year before Veblen was at Oxford.

Aspray: Were there other people that you think Veblen had influence on at Oxford, as far as their research interests went?

Hoffmann: I really don't know what the ultimate effect of Veblen's visit was. You would not see any two mathematicians as different as Veblen and Hardy.

Tucker: That's right.

Hoffmann: I remember my friends said, "Oh Hardy, marvelous person." So I went to one of his lectures, and I couldn't understand a single word of it. So I decided, "Well, Hardy is not for me." Quite possibly some of my friends who could understand Hardy couldn't understand Veblen.

Tucker: Are there any of your fellow graduate students that you remember particularly?

Hoffmann: Well, there's Ed McMillan who won the Nobel Prize.

Tucker: So you were closer to the physics group than to the mathematics group?

Hoffmann: Yes.

Tucker: Did you know H.F. Bohnenblust?

Hoffmann: Oh, Bohnenblust, yes. He was in pure mathematics, wasn't he?

Tucker: Yes. And did you know [A.H.] Taub?

Hoffmann: Oh, Abe Taub, yes. He and I were much closer together in the work that we were interested in. There was George Shortley, who did that book with Ed Condon, the complete book, as it were, of atomic spectra. And Howard Robertson, who translated Weyl's book into English.

Tucker: That's right.

Hoffmann: George Shortley and I had sessions with Robertson going through the English translation of the book, with Robertson explaining

things to us. Then Shortley and I would make lists of misprints—there were so many misprints all through the book. It was quite embarrassing, but Robertson was very happy about it and urged us to continue. There was Leon Cohen.

Tucker: Yes.

Hoffmann: Leo Zippin and [Edward] Linfoot.

Tucker: Yes, I remember Linfoot. Because of the fellowships that were available, such as the Commonwealth Fellowship and the Princeton Procter Fellowships, there were a number of British students in mathematics all the way through that period.

Hoffmann: That reminds me that H.P. Robertson had open house once a week at his apartment with his wife. I used to go there regularly as did various others; there might be half a dozen or more. It seemed as if many of them found this a home away from home because they were not indigenous Americans.

Tucker: Yes.

Hoffmann: That I remember fondly. It was really a marvelous experience.

Tucker: Then you were, I think, three years at Rochester before you returned to Princeton.

Hoffmann: That's right. I returned for two years in Princeton and then went to Queen's College.

Tucker: Would you like to speak a bit about those two years that you were back at Princeton?

Aspray: Before you do, could I ask you a couple of questions about your Rochester period? This was a period in which the Depression was in full swing.

Hoffmann: Indeed, yes.

Aspray: How was it that you found a job at Rochester? Was it difficult?

Hoffmann: When I got my Ph.D. it was '32, and there were no jobs anywhere that anyone could see. I was trying this place, that place, the other place, and nothing happened. Then I saw in the papers that George Eastman had died and had left several million to the University of Rochester. So I went to see Eisenhart, who was handling this sort of thing, and I said, "Maybe there's an opening there." He suggested, "Why don't you write?" I think he wrote also. Then I was called for an interview, and there was a man there, Charles Watkeys.

Tucker: Yes, I remember him.

Hoffmann: He asked me to his house, and he began playing some music. One was a composition of his own. I didn't know it was his own, and I said, "You know that sounds very Elizabethan." He said, "Oh, you noticed." He said, "That's my composition. I did it in the form of an Elizabethan madrigal." He was a cellist, and I played the piano. I think that to some extent on that basis he urged that I should be given the job. And I got the job just like that. Pure accident.

Aspray: What were the conditions under which you came back to the Institute?

Hoffmann: At Rochester in the math department there were three people who were permanent. They would take on an instructor for three years, and no matter what they wouldn't extend it beyond three years. While I was there in 1932, one of the three permanent people became ill, and so they asked me to take over his work, his lectures and all, which I did. The man recovered, and at the end of three years, although they said they were very pleased with me, they simply wouldn't give me any extension. I was really in quite a pickle because of that. I came back to the Institute, and Veblen was so sweet. He called me into his office, and he gave me a pep talk, and he arranged for a stipend from the Institute. That was renewed for a second year. You know it's interesting that my being fired from Rochester led to my meeting Einstein and working with him.

Aspray: What were your duties at the Institute?

Hoffmann: No duties. I was given a stipend. It was $1,000 or so, and I could do whatever I wanted. In the second year of my research at the Institute, Leopold Infeld came, and it turned out that Infeld had been working on what was called the Born-Infeld electromagnetic theory. I had worked on it independently. So when Infeld came I got in touch with him, and we hit it off nicely. We made a joint paper incorporating his ideas and my ideas and extending them.

When that was done, Infeld said, "Why don't we go to Einstein and see if we can work with him?" Infeld had met Einstein in Berlin. It would never have occurred to me to dare ask to work with Einstein, but we went and Einstein said, yes, he would be happy to have us work with him. He said he could offer two different problems. One was to find nonsingular solutions of the gravitational field equations under certain conditions. The other was to apply what is now called the E-I-H method for finding the motions not just of test particles but of large bodies in mutual gravitation and showing that the motion came as a result of the field equations alone and not as the result of an extra postulate like the geodesic postulate. Luckily Infeld and I chose that one, because no one has ever solved the other problem. There was Peter Bergmann there, and he was working with Einstein on that other problem among other things.

Tucker: We have located the reference you gave me of the symposium that was held to celebrate the hundredth anniversary of Einstein's

birth. It has the strange title, if I may make a pun, *Some Strangeness in the Proportion* and was edited by Harry Woolf.

Hoffmann: Right.

Tucker: There's a chapter there "Working with Einstein" [by Hoffman], so we will make that chapter a reference for this interview.

Hoffmann: There's also another one. The one that we were just talking about had to do with a meeting in Princeton.

Tucker: Yes, and there's a companion volume for the meeting in Jerusalem.

Hoffmann: Precisely. It is called *Albert Einstein: Historical and Cultural Perspectives* and was edited by G. Holton and Y. Elkana

Tucker: But I have looked at the one from Jerusalem, and there was not the same opportunity there for reminiscing.

Hoffmann: Well, there was. I gave two papers. I gave one which was not reminiscing, but then on pages 401 to 404 were reminiscences by me. There I told of a dinner party—Einstein invited me to his house—and of a sort of intellectual game that we played. Did you read that?

Tucker: Is all of the Jerusalem symposium in one volume?

Hoffmann: No, it's in two volumes.

Tucker: Oh, that's the trouble. I was looking only at the first volume.

Hoffmann: That volume was about quantum unified theories, the gauge geometry and so on. It is not the volume in which we had reminiscences.

Tucker: Yes, I will get the second volume.

Hoffmann: You might also be interested in my main paper in the second volume, which was on pages 91-105, but that's not reminiscences, really.

I have one or two little anecdotes that you might find amusing.

Tucker: Please.

Hoffmann: I told you that I was making notes, and that there were colloquia and so on. I didn't mention that one of the extraordinary things about Princeton was the caliber of the people that you would come to know. Here is a short list: Einstein, von Neumann, Wigner, Pauli, Dirac—a lovely person, Dirac—Bohr, Weyl, and also the Princeton faculty, Robertson, Condon, etc. Now you probably know

that Wigner was a very careful person, not liking to hurt anyone's feelings. Do you know the story about Wigner taking his car to be repaired? It was repaired but broke down almost immediately. He took it in again and again. At last in real exasperation he said to the mechanic, "Go to hell, please."

Tucker: Yes, I learned that story from Joe Hirschfelder.

Hoffmann: Oh yes, I remember Joe. Well, that's an introduction to the story that I want to tell you. Pauli was giving a seminar, and Pauli was an absolutely overwhelming person. He finished up something or other, and then he said, "Any questions?" Wigner, putting his hand to his lips, you know, as if he shouldn't really be saying this, said "Well, if such and such would such and such, etc." And Pauli interrupted him and said, "No, no, no. You're absolutely wrong. What was the question?"

Tucker: I know that gesture of Wigner's. I have seen it on various occasions. He put his hand to his mouth as if to say he should keep quiet, but then he spoke.

Aspray: What was your relationship with Wigner? Had you taken courses with him when you first were at Princeton?

Hoffmann: I think I took one course with him in fact I remember that I did. Wigner was presenting all sorts of things, one after the other. I must say that on one occasion I got utterly lost. It is possible that on this occasion pretty well everyone got lost, but Ed McMillan put up his hand and said, "Does this mean that this whole diagram then moves steadily to the right?" And Wigner said, "Ah, yes, exactly so." So Ed McMillan was the only person, I think, who knew what was going on there. It was not easy to follow, but that's only because I didn't have the background. But I did have the background for Robertson.

Aspray: Did you have association with Wigner when you came back to the Institute?

Hoffmann: I don't know what you mean by 'association'. We were friendly, but he was not the key person—the key person for me apart from Veblen and Einstein was Robertson.

Tucker: Yes. Well, we will thank you very much for this.

Hoffmann: I've really enjoyed it, especially because you are able to corroborate some of the things that I felt.