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The Princeton Mathematics Community in the 1930s
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ALBERT TUCKER

THE INSTITUTE FOR ADVANCED STUDY IN THE 1930s

This is an interview on 12 July 1984 of Albert Tucker in his office at Princeton University. The interviewer is William Aspray.

Aspray: In this session we will talk about the personalities and research interests of the permanent members of the Institute for Advanced Study in the 1930s. Professor Tucker.

Tucker: We have already talked at considerable length about Oswald Veblen, who was the leading figure in the School of Mathematics of the Institute for Advanced Study. He had been before that a leading figure in the Department of Mathematics at Princeton University. We have also spoken of James Alexander, who moved from being professor at the University, at least on a part-time basis, to being a professor at the Institute. John von Neumann had been associated with Princeton University from about 1930 on. He and Eugene Wigner had held jointly a professorship in mathematical physics. One of them would be at Princeton while the other one was in Berlin, then they would change. So von Neumann was already well acquainted with Princeton when the Institute began in 1933. But his activities seemed to widen from mathematical physics, such as quantum mechanics, to a variety of fields. Although von Neumann did not work particularly with pre-doctoral students—one exception to this is Israel Halperin—he always had several people at the Institute to exchange ideas with. At that time, I was not particularly interested in the things that von Neumann was occupied with, which seemed mainly to be operators in Hilbert space and things of that sort. He was interested also in the lattice theory that Garret Birkhoff was developing at Harvard. Garrett Birkhoff, son of G.D. Birkhoff, was a frequent visitor to von Neumann at Princeton. It was out of the interest in lattice theory that von

Neumann developed his so-called continuous geometry, which is really a lattice whose finite aspects have been replaced by a continuum in which dimension—a real number between 0 and 1—corresponds to elements of the lattice that are on the same level. One of the people that von Neumann worked with especially was F.J. Murray. They wrote papers together on linear operators and continued this collaboration on into the following decade. I am sure there were other things von Neumann was doing that I wasn't aware of.

Aspray: Let me ask about three areas then. Logic.

Tucker: He did nothing more in logic after Goedel's great accomplishment that ruined the Hilbert program.

Aspray: Okay. Game theory.

Tucker: I was not aware that he was doing anything on game theory, but it does seem that at the very end of the decade he was again occupied with game theory. He had written his basic paper on the subject in 1928, which got published in *Mathematische Annalen.* Sometime in the late '30s something reawakened his interest. Until hearing Halperin talk about his spending much of the summer of 1940 with von Neumann, I had not realized that von Neumann had turned back to game theory as early as that. I had thought that it was during the war period. The book by von Neumann and Morgenstern was published in 1944. I am sure that it took quite a while to write, also to print. So it is likely that von Neumann towards the end of the decade was again looking at games.

Aspray: Hydrodynamics. Fluid flow properties.

Tucker: I think Bargmann has told us that von Neumann was working in that area also in the late '30s. Bargmann came in about 1938. While Bargmann was here mainly as an assistant to Einstein, he worked also with von Neumann in fluid flow problems.

Aspray: Is it possible that research on those problems was, as early as '37 or '38, spurred by involvement in government work?

Tucker: I don't know, but I doubt it. Of course, von Neumann was a close associate and friend of Theodor von Karman at Cal Tech. And hydrodynamics and aerodynamics were the specialty of von Karman. Von Neumann, of course, was quite peripatetic, so he could perfectly well have spent some weeks at Cal Tech with von Karman and during that period worked on fluid dynamics. Since I had supervision of the Princeton Mathematical Notes, as they were called, I was aware of the subjects von Neumann lectured on enough to have notes made, and there was nothing in game theory or in fluid dynamics. (These lecture notes or seminar notes were up until about 1938 mimeographed. After 1938 they were planographed by Edwards Brothers in Ann Arbor, Michigan. They were sold informally.)

Aspray: Shall we turn to Hermann Weyl?

Tucker: Weyl seemed to me to be a master of all forms of mathematics. But after coming to Princeton he seemed interested mainly in algebraic things. The assistant that he would have each year was an algebraist. This assistant was someone that he selected. One year it was Richard Brauer, and another year it was Al Clifford, who was an algebraist who had been trained at Cal Tech, in the school of algebra headed by E.T. Bell.

Weyl gave every year a course of lectures. He was accustomed to doing that and continued doing it even though there was no requirement that he as a professor at the Institute do so. Notes were taken, and these notes of Weyl's lectures were much sought after. He was interested in algebra and especially groups. After some lecture notes were done, he turned his work in this area of classical groups into the first volume of the Princeton Mathematical Series, which came out in 1938.

Earlier on he had done quite a bit in mathematical physics. One of his books was translated into English by Howard P. Robertson. I have forgotten whether it was the book on groups and quantum mechanics. But I do not remember him lecturing on mathematical physics after he came to the Institute. Perhaps this was because he felt he should defer to Einstein in this regard.

Aspray: Why do you think Weyl came to Princeton? Why was he attracted to the Institute?

Tucker: The reason was Hitler. Of course Weyl was quite unsympathetic to the Nazis. Earlier on he had been at Zurich, but he had left Zurich to go to Goettingen. I think he was appointed to the chair that was vacated by David Hilbert. There had been some unhappy feelings at the ETH in Zurich about his leaving to go to Goettingen. Perhaps it could have been arranged for him to return to Zurich, but just at that time the offer came to him from the Institute for Advanced Study. I think it seemed a heaven-sent opportunity to escape from Nazi Germany and to be involved in what seemed to be a very promising undertaking. Also, Weyl had been at Princeton in 1927-28 as the first Jones Professor of Mathematical Physics.

Aspray: Was the presence of von Neumann a favorable factor?

Tucker: I think so, yes. Because von Neumann had been at Zurich when Weyl was there. So the two men knew each other before they were together at the Institute for Advanced Study. Weyl's lecturing style was rather formal and dignified. In his interview Bob Walker mentioned 'Holy Hermann', which was a nickname Weyl was given because of his rather ponderous way of lecturing. As a person he wasn't at all stuffy, but his lecture style was ponderous. He still talked as in German sentences, long sentences with long words and with the verb as near the end as possible.

Aspray: How would you compare his teaching style with that of von Neumann?

Tucker: Very different. Von Neumann was so terribly quick in lecturing that people had to slow him up by asking questions. It was understood in his classes that people would ask questions to slow him up. I think he was quite aware of that and was grateful for this help from the audience. Von Neumann had a way of taking an idea that he had and explaining it very quickly and very clearly. The trouble was that you would still be thinking about that idea when he was presenting the next one. Weyl, on the other hand, spoke rather slowly in well-rounded sentences. There was a certain jerkiness about von Neumann's lecturing and manner, whereas there was easy dignity in Weyl's style. Weyl's interest was in overall views of things. Von Neumann was very much problem oriented. Problem isn't quite the right word; von Neumann would deal with the point that came up as a thing by itself.

Neither von Neumann nor Weyl seemed, at that time, to have much interest in things geometric or topological. Weyl had in the Twenties done some interesting things in combinatorial analysis situs. He had given some lectures in Spain, and his wife, who was Spanish speaking, had translated them into Spanish, and they were published in a Spanish journal. This article, which related Kirchhoff's laws and electrical networks to topological networks, was in the field of my interest. I asked him if he had a copy of the paper in German, because I found the Spanish a bit difficult since I didn't know Spanish. He said, "I can do better than that. I can provide you with an English translation." Someone had made a translation into English and had sent a copy to him.

But Weyl, in his lecturing at Princeton, was not going into geometry or topology. The same thing was true of von Neumann. Indeed I don't think von Neumann ever did anything that could be described as topological. I mention that because von Neumann did so many things that many people feel he was a master of everything. I feel that it was Weyl who was the master of everything. Von Neumann was quite eclectic in the things he worked on. Indeed I remember one occasion in the common room when there was some discussion going on of a topological theorem. Von Neumann immediately started out to construct his own proof of this. His was a sound proof and quite ingenious, but I regarded it as the sort of proof that an analyst would come up with rather than the sort that a combinatorial topologist would come up with.

On the other hand, it is true that later on in some work that he did on the so-called optimal assignment problem that he got interested in combinatorial mathematics. But here again the way he was proposing to go about proving things was what I would describe as an analytic way. It was Harold Kuhn who took the problem and obtained a nice algorithm for its solution, the so-called Hungarian method. The name 'Hungarian method' was partly in honor of von Neumann, who had come up with the problem of optimal assignment and had related it to to a certain 2-person zero-sum game. But also Kuhn, in trying to trace back and get ideas for an algorithmic solution of the problem, found in the work of two Hungarians back in the early '30s—Denes Koenig and E. Egervary, a student of Koenig—the ideas which he then turned into his ingenious combinatorial algorithm.

Aspray: Would it be fair to say that, since the thrust of the work of Weyl and von Neumann was away from geometry and topology, it helped to complement the research at Princeton and to allow many different things to go on here, rather than just geometry and topology which were the strong point?

Tucker: Of course. This was amplified by all the people who were coming to the Institute for Advanced Study as visitors. During that period, not quite in 1933, but starting about 1935, there were probably 50 or 60 people, post-doctoral people, from all parts of the world here in Fine Hall, listening to lectures, attending seminars, exchanging ideas at tea, and so on.

I don't feel that I can say very much about Einstein. Others, such as Bargmann and Hoffmann, I hope will speak to that. Let's turn to Marston Morse.

He was not at the Institute the first year that it opened. He came, I think, in the fall of 1934, from Harvard. I had been associated with Marston Morse in the spring of 1933 when I was at Harvard as post-doctoral National Research Fellow. He was my supervisor. As I think was typical of him, he involved me in what he was doing at that particular time. He was writing his well known book, *The Calculus of Variations in the Large.* For that, he needed certain information from topology, something that's called singular homology theory, which Lefschetz had been working on but was still not fully worked out. So Marston had me helping him with that. During the months that I was there I got very little done on what I was trying to do myself. I must say, however, that when the book was published the preface had a handsome acknowledgment of the assistance that I had provided during the period that I was at Harvard.

So the book was finished and published by the time that Morse came to the Institute for Advanced Study. What he was working on then he called analysis in the large; he had moved from calculus-of-variations in the large to analysis in the large. This was a study, by analytical methods, of properties, such as dynamical properties, that were not local properties but were global properties. Of course you can't deal with matters in the large without bringing in some topology. If you were thinking of topology as being useful to other parts of mathematics, then it is in this way of studying problems in the large, as opposed to the typical procedure of classical differential geometry, which is to study them in the small.

Morse seemed automatically to gather a group of people around him. Now this was not true, I felt, with any of the other professors at the Institute, other than Einstein. While there were certain people who gravitated to von Neumann and Weyl to consult them and discuss problems and such, there was not some large program that von Neumann or Weyl had in which they were trying to enlist workers, whereas Marston Morse did this with his analysis in the large. Some of the people who came and worked with him were people who had previously done Ph.D.s with him at Harvard. Everett Pitcher was one

of these; Stewart Cairns was another. It was a somewhat small group, but it was a very energetic group. There were also people from Europe who participated to some extent in that effort. Walther Mayer, for example, who came in the first place as assistant to Einstein and who then became interested in the Morse's work, and I think Herbert Busemann. Morse gave lectures just as he had at Harvard, although he wasn't required to do that.

Aspray: How was he as a lecturer?

Tucker: He was a good lecturer, a bit on the formal side, but clear and certainly well prepared. I think that is what I can say about Marston Morse. And that's the list of the people who were called professors. There were others who came to the Institute who were called permanent members. Indeed, Alexander fairly soon after the Institute got started changed his status from professor to permanent member.

Aspray: What was supposed to be the difference between these two categories?

Tucker: As I understand it, the professors were all paid the same salary and had many privileges and responsibilities. Each one was entitled to an assistant, and it was the professors who met and made decisions for the School of Mathematics. There was no chairman or head. Veblen was regarded as the doyen of faculty meetings and was the instigater of most things, but it was the professors meeting together who decided on the people who were to be brought to the Institute and the stipends they were to be paid. They were the Board of Directors. The permanent members were regarded as having a sort of tenure, but they did not participate in decision making. Their lot was as the Board of Directors made it.

Einstein had insisted that Walther Mayer be given a place at the Institute or he wouldn't have come. But after a year or two Walther Mayer did not any longer serve as assistant to Einstein. This was done by some younger person such as Banesh Hoffmann, Valentine Bargmann, Peter Bergmann, or Leopold Infeld. Walther Mayer continued as a professor at the Institute in all but name. He would occasionally give a course of lectures, and he was fully active. Goedel was another person who was a permanent member of the Institute. Incidentally, the permanent members, if they wished, could go off somewhere for a year and then come back and resume their permanent membership. Goedel exercised this privilege. About one year in three he would spend back in Vienna until 1939. Walther Mayer, by the way, was also a geometer. I don't think that at that time there were any other permanent members.

Now there were many distinguished visitors who came to the Institute for a year or for a term. We heard earlier today about P.A.M. Dirac spending time at the Institute. I think he was around for about two years; I am not sure whether these were consecutive or not. He was not called professor as a professor at the Institute; he was

called professor because he was a professor at Cambridge. So it was actually very confusing in Fine Hall where there were University professors of various ranks, Institute professors, permanent members of the Institute, and visitors of various ranks. Mayer was never called professor; he was called doctor. It was same with Goedel; he was called doctor. There wasn't any particular importance attached to these titles. One thing I noticed with amusement as I was looking through the membership lists was that a distinguished mathematician who was visiting at the Institute was listed simply as Mr., Mr. Maxwell H.A. Newman, because he hadn't yet reached the stage in the British heirarchy where he was at the professor level. He was a fellow of St. John's College, Cambridge, and he was a lecturer at Cambridge University. He didn't have a doctoral degree because that wasn't customary in the British system at that time. So he was here simply as Mr. Maxwell Newman. On the other hand when G.H. Hardy was at the Institute he was called Professor Hardy because he held a professorship at Cambridge.

Aspray: If we look at the people that were chosen as permanent faculty members in the School of Mathematics, what sort of group do they make? How rational were the choices of people? Who might have been considered at the time and wasn't? Who expected to be considered at the time and wasn't?

Tucker: Let me answer this by saying that Abraham Flexner relied very much on the advice of Veblen in the appointments that were made to the School of Mathematics at the Institute. But earlier Flexner had gone around the world, visiting universities and asking people in various fields, "Who are the world leaders in your field?" One of the reasons that mathematics was chosen to start the Institute was that Flexner found much greater unaninimity in the ranking of the great mathematicians than in any other field. I'm not aware what other offers may have been made.

Now I have heard a rumor that G.D. Birkhoff was offered a professorship at the Institute and that it was only after he declined that an offer was made to Marston Morse. But I don't know whether this is true or not. It's really just gossip. It may very well have arisen because of the feeling that if they were picking the leading mathematician at Harvard it would have had to be Birkhoff rather than Morse. But Veblen may have felt that Morse would fit in better than Birkhoff because in a way Veblen and Birkhoff were rivals. Along with [G.A.] Bliss at Chicago they would, say in 1930, certainly have been on any list of the five leading mathematicians in the United States.

So there I don't know. I think the choice of von Neumann is clear by the criteria of getting the best mathematical talent in the world. Weyl was chosen, I think, because he had in some way inherited the mantle of Hilbert. Of course Einstein certainly was the greatest name in mathematical physics. The one question, I would think, is Alexander's appointment. There again I would say that this was very much the Veblen influence. Veblen felt that Alexander was one of the greatest mathematicians of that time. And I think that Veblen was

right except for the personality matter, that Alexander was not competitive. He just wanted to toy with his mathematics. So he didn't have the drive that von Neumann or Veblen or Morse or even Weyl had. I say *even* Weyl, because Weyl seemed like some sort of god. He just seemed to have such a tremendous grasp of everything, so one didn't feel that he was competitive because who was there to compete with. The same thing was true of von Neumann, that there was no one that he had to compete with. But he nevertheless was a terrifically competitive person, and he would often treat a young mathematician very roughly if it seemed like that young mathematician was ...

Aspray: A rival?

Tucker: Yes. Nobody else saw this person as a rival, but von Neumann did. But Alexander had none whatsoever of this competitive spirit, and that's the reason I think he was misplaced at the Institute. He realized it, and that is why later he asked to be made a permanent member instead of a professor.

Aspray: Were there other people on the Princeton University faculty who thought they should have appointments at the Institute? Lefschetz, for example?

Tucker: I think that of course Lefschetz compared himself with Alexander. Lefschetz had the qualities of competitiveness which Alexander did not. And I think that from the point of view of Flexner and Veblen it was a mistake to choose Alexander over Lefschetz. But I think Veblen and Lefschetz didn't hit it off at all. There was nothing particularly deliberate about it, but their personalities were totally different. Lefschetz was very down-to-earth, even rather foul-mouthed at times. Very rough-and-ready. Whereas Veblen was a patrician. He was always dignified. There was such a disparity in their temperaments that conceivably one of the reasons for choosing Alexander was as a means of passing over Lefschetz. Certainly Lefschetz had mixed feelings about the School of Mathematics at the Institute. I think he was very glad when the Institute got its own building and moved away in 1939.

When Fine Hall was built, a special office was built for Lefschetz, intended for him. It had certain features so that he could do things with his artificial hands. He had special filing arrangements: not a filing cabinet but a special set of drawers that he could use. His door, instead of a knob, had a hook so that he could open the door with the lower part of him arm. So Lefschetz felt that a great deal had been done for him at Princeton and that he really shouldn't complain. Although I suspect that he felt that he had been passed over, his attitude was to make the best of it and not show any bitterness, and indeed to do everything within his power to keep up the research strength of the mathematics department at the University. He took over the leadership, as far as the research side of things was concerned, the leadership that Veblen had previously exercised in the mathematics department.

Aspray: Was there any effort, as far as you could tell, to choose people for the Institute so as to provide a wide coverage of mathematics?

Tucker: Well, the people chosen certainly did provide a wide coverage. I guess that it may seem that the classical fields were neglected, but by this time Veblen had more or less concluded that geometry per se was a dead end in research, that the greatness of geometry was in the past, and that his interests and the interests of Alexander in topology would cover that area. But there really was little in the direction of classical, hard analysis or in the direction of number theory. But more recent appointments at the Institute have certainly covered those areas. I think the feeling was that with the six professors it was not going to be possible to cover all fields.