The Princeton Mathematics Community in the 1930s
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(with ALBERT TUCKER)

This is an interview of Leon Warren Cohen at Princeton University on 13 April 1984. The interviewers are William Aspray and Albert Tucker.

Aspray: Will you tell us something about your backgrount prior to coming to Princeton?

Cohen: I came to Princeton after writing my dissertation at the University of Michigan in Ann Arbor. My thesis advisor was Raymond L. Wilder. A year or two before I left, in Wilder's seminar we looked at James W. Alexander's great paper on a proof and extension of the Jordan-Brouwer separation theorem. That was particularly interesting because it brought together two types of topology, which in the United States were separate,—there was even hostility between the practitioners of the two types of topology. 'Hostility' may be too strong a word, but that is the impression I got. If I'm not mistaken, the leaders of the two schools, R.L. Moore and Oswald Veblen, had earlier been at Chicago at the same time.

Tucker: As a matter of fact, Veblen supervised R.L. Moore's thesis, just a year after E.H. Moore had supervised Veblen's thesis.

Cohen: That makes another connection, because the reason I went to Michigan was that I had published a note on Lusin's Theorem which attracted the attention of T.H. Hildebrandt, who was a student of E.H. Moore *and* the guy who offered me a quarter-time teaching-assistantship at Ann Arbor.

Aspray: Can you tell me a little about these two opposing schools?

Cohen: There were two approaches to topology. One had to do largely with local properties; it was called point-set topology. The great leader of that school in the United States was R.L. Moore. He set up his school at the University of Texas, which had an enormous output both in topological research and in topologists. The other school was established and led by Oswald Veblen. It had its roots, I suspect, in the work of Poincare. It had more to do with the algebraic invariance of topological properties, with topological properties in the large rather than in the small. The two schools had, of course, the same concept of an invariant, that is, a property that is unchanged when homeomorphic transformations are applied to the space.

Wilder was a pupil of R.L. Moore. When I got to Michigan I took a course with Wilder as well as one with Hildebrandt. There were even connections between these two, but I won't go into them. Wilder was very attractive. He followed Moore's method of teaching mathematics, which was to tell the students nothing but the conjectured theorems and tell them to go and prove the damn things. It was treason to look things up in the literature.

Tucker: Moore would remove from the library all the books the students could conceivably use.

Cohen: It had curious consequences. It developed great initiative in the search for original proofs, but it left the students relatively ignorant of mathematics because they were not in the habit of reading the literature.

The paper by Alexander brought these two schools together. The main result of this paper was a duality theorem, which was similar to the classical algebraic duality theory of Poincare. While Alexander's work was algebraically quite similar to Poincare's, geometrically it was quite different. Wilder and the few of us in his seminar spent practically a whole semester analyzing the proof by Alexander. It was a beautiful thing, and it was full of implications for us. It later led Wilder to develop what was called unified topology, in which he cast the results of the point-set people in an algebraic form, so that the local results could be expressed algebraically. A remarkable thing.

That was my introduction to the two schools of topology, and I wrote my dissertation on the point-set end of the subject. I became interested in dimension theory, and I spent a year in Ann Arbor after completing my dissertation. During that year there was some discussion of this unfortunate separation between the two schools of topology. Wilder came to me and said, "I think you ought to apply for a National Research Council Fellowship, go to Princeton and find out what Veblen and his school *is* doing, and come back and tell us." Apparently no one had done that, from either school to the other. Well, I was a simple-minded, innocent guy. You want me to get a fellowship and go to Princeton? Okay, I'll go to Princeton. And so it happened that I came to Princeton as a sort of spy. That was about 1929, one year after my dissertation.

Being in Princeton was a revelation to me. In Ann Arbor I thought I was quite some "punkins". In Princeton there were world-class people, and I must say they scared the life out of me. I didn't like algebra, I was prejudiced against the whole blooming business. But I had this responsibility, so I tried to find out what was going on. I received enormous help and encouragement. Oswald Veblen had many wonderful characteristics. He was a kind man. To put it bluntly, it was because of Veblen's kindness that I was able to face the situation here.

Actually when I wrote a paper here, which I did in the first year, it didn't have anything to do with topology. It developed some ideas I had gotten in a course with Hildebrandt, some ideas concerning linear equations with infinitely many unknowns, the work of F. Riesz. That was something I was then studying.

There was some big project going on, led by Lefschetz, a second edition, if I'm not mistaken, of his colloquium lectures.

Tucker: No, it was the colloquium lectures themselves. They were published in 1930

Cohen: You will forgive me. My amnesia is pretty general.

Tucker: And in the year '29-30 Lefschetz lectured on the subject of his book, so that there was a course in analysis situs both terms.

Cohen: But he wanted to add an appendix on infinite complexes. Infinite complexes forced further connections between the two schools, because the question of convergence arose naturally. It arose in a particularly nasty way, which didn't become clear, at least to me, for some time. The groups which were involved gave rise to a non-archimedean order, and not much was known about this.

I met with Professor Lefschetz at his invitation in the afternoons once or twice a week. We discussed the material he was involved in. Lefschetz had a profound geometrical intuition, in the great tradition of the Italian school. Lefschetz could smell a theorem where most mathematicians would not suspect it at all. He stated the theorem he smelled, and the statement was usually correct, though maybe it needed to be modified. But when it came to working out the details of the proof, there were things Lefschetz found too boring to be bothered with. I, on the other hand, had been forced, in my modest education, to look at every inequality and to prove everything. The result was that there were frequent misunderstandings between Lefschetz and me. I would say, "Professor Lefschetz, it's wonderful, but I don't quite understand it." He would look at me intensely, as if to say, "You idiot, how is it possible?". But he didn't—you know he was polite. He'd say, "Please, do something about it." Usually I wasn't able to. I don't know to what extent I was any help to Lefschetz. He was kind enough to mention my work in a preface or someplace. We did spend a good deal of time together that spring, and it was breaking new ground."

Aspray: I've heard of this quality of Lefschetz's. Did he get assistance from colleagues, assistants, or graduate students in finishing off proofs?

Cohen: I suspect he got a great deal of support from Al Tucker.

Tucker: Well, also from Bill Flexner, who was a couple of years ahead of me. In that first year—it was also your first year—I was so naive that I didn't realize all that was going on. But looking back, I certainly agree with you that he got a great deal of help from students. I don't think, though, it was from colleagues.

Cohen: I am inclined to agree with that. As a matter of fact, there was a certain rivalry in the department. It was based on a mathematical problem that arose from the Alexander duality theorem and the earlier Poincare Theorem. The algebraic analogies were so striking that it was conjectured—I don't think it was more than conjectured at that time—that there was a single theorem encompassing both of these. This, of course, was in the minds of both Alexander and Lefschetz, and each of them would have loved to have found it. The finding of it, it was clear, would be a great contribution to mathematics.

Aspray: What was the result?

Cohen: The general theorem was found, a truly algebraic theorem, by Pontryagin, a Russian.

Aspray: At what time?

Cohen: I don't know, 1935 or several years later. But the approaches to that involve the next chapters in this story. In a conversation with Alexander, he asked me, "What do you propose to do?" I told him that I had found necessary and sufficient conditions for a metric space to be imbeddable in the 1-dimensional continuum of real analysis, and that my program was to prove a similar theorem for all dimensions. Alexander looked at me, smiled and said, "That's a bully problem," and walked away. It was wonderful. I never got it completed, nor has anyone since. At any rate, that shows you what a simple-minded duck I was when I got here and how nice Alexander was about it.

Aspray: Did you have further dealings with Alexander?

Cohen: Yes. We had a couple of social contacts, and I listened to his lectures. He was a beautiful lecturer. He had a wonderful mathematical style I still battle to find. The question is, How can you be precise and at the same time comfortable in your expression? It's difficult, but he had this capacity.

Aspray: More so than other people?

Cohen: Oh yes. You see, you run across, on the one hand, these people who imitate as closely as possible the language of mathematical logic. It avoids errors, but it becomes difficult to read. It's long, it seems to contain a lot of irrelevant stuff, and it's unpleasant. Then, on the other hand, these people who have the gift of intuitive exposition. They make things intuitively clear, but when you come to make it precise for yourself you find you're left with the problem of deriving all the details, which can be enormously difficult. Now to strike the right balance between there two is a gift of mathematical expression that Alexander had.

Aspray: Can you place some of the other people who were at Princeton at that time on that continuum?

Cohen: Yes. Bohnenblust and Hille. I followed their lectures because my original love was real analysis, not topology at all. Both of these people were, in their fields, eloquent lecturers, who simultaneously gave the understanding of the general scheme of the proof *and* provided the details. Their writings reproduced this on paper.

There was another man visiting here who was very good at that: Harald Bohr, the brother of the physicist. I still have a lively picture of Bohr's lectures. Bohr lectured with three objects in his left hand: a piece of white chalk, a piece of red chalk, and a cigar. I think I wasn't the only one who waited for him to put the chalk in his mouth and write on the blackboard with the cigar. It never happened. He was an excellent expositor.

Tucker: He was also a famous soccer player; he played for the Danish national team.

Cohen: Anyway, it was a great time for me.

Aspray: You were telling us about Alexander.

Cohen: Yes. Toward the end of the spring we met. He said, "Look, in the summer I go to Chamonix, and I do a little mathematics too. Would you like to come? We can work together." I agreed, and that summer was spent in Chamonix. We would work during the week. He lived in a little hut in a place called Les Houches, and my wife and I lived in the attic of a guide's hut in the mountains three or four miles away. We would do mathematics, and once a week we would go into the mountains and climb. And just as Alexander was elegant and precise in his topology, he was elegant and precise on the mountains.

Aspray: Had you been a climber before?

Cohen: No, that was absolutely the first time. He was patient. He led us both on conditioning climbs, which were simple, and then on more demanding climbs, until I got to feel at home on the rocks. Even my dear wife came to feel at home on the rocks. It was a wonderful time. A lot happened that summer.

The summer was devoted largely to a serious investigation of infinite complexes and the abelian groups which they generated. The question of non-archimedean convergence came up, it was dealt with, and a joint paper came out of that. It was not a complete solution of the problem—it was ultimately solved—and, if I'm not mistaken, our work even had a flaw in it which Alexander corrected in a subsequent paper. But it broke a path, and it opened up the whole question of non-archimedean ordered abelian-groups for me. Some years later, with Casper Goffman, of Purdue, I wrote a series of five or six papers on nonarchimedean analysis. The fact that that was possible was due to my being with Alexander that summer.

Aspray: That was after your first year? Was that the summer of 1930?

Cohen: Yes. My fellowship was renewed, and I had another year there. The dates must recall to us that the economic bottom had dropped out of the world. That's another story; I might get to it later. But there was an incident, I believe in my second year, of a mathematical nature that indicated the kind of stimulation and excitement there was at Princeton in those days. Karl Menger came. Menger, as you know, was one of the two people who developed dimension theory, the other being Urysohn, a Russian.

Tucker: Alexandroff lectured on dimension theory in Princeton during the spring of '31.

Cohen: It happened that at an informal colloquium in Ann Arbor several years earlier, I had the temerity to announce that I had proved the equivalence of the Menger dimension and the Urysohn dimension in Hausdorff space. So my teachers asked me to tell about it at this informal colloquium. The day before I was to speak I prepared the lecture and discovered that my proof was no good. This promised disaster. So I worked most of the night, and I discovered something even more interesting—an example to show that the two theories are not equivalent. I reported on that, and it subsequently appeared in *Comptes rendus.* I'd forgotten it, and Menger came to give a talk on dimension theory. At the end of the talk I said, "Excuse me, Professor Menger, it's true the two theories are equivalent in metric space, but not in Hausdorff space." "Show me," he said, and pointed to the blackboard. I went to the blackboard, drew a great big picture, and began describing the picture. He interrupted me and pointed to something in the picture. When I saw what the point was, I pointed to a second place in the picture. He nodded and pointed to a third place. I pointed to another place in the picture. I think there were three sets of exchanges. Not a word said. At the end of this unspoken discussion he said, "Why don't you send me a reprint?", which was sort of amusing. It turns out that in the generality of Hausdorff space the Urysohn theory of dimension is more intuitively acceptable.

Tucker: As I remember, Menger came in the fall of that year, and then Alexandroff came in the spring. He gave a full course, which [Nathan] Jacobson, [Robert] Walker, and I attended. We used to hold two or three sessions before each lecture to make sure that we understood everything that Alexandroff had said.

Cohen: That's interesting. Some of these things are purely personal.

Aspray: No, they're quite useful.

Cohen: Okay. I reported on my dissertation at a meeting of the American Mathematical Society, in New York. Alexandroff was in the audience, and when I finished my little talk, he got up and said, "Are you aware that Urysohn and I published this result several years ago?" Fortunately, I was aware. I rose and said, "Professor Alexandroff, did you assume that your space was compact?" "Oh, yes," he said. I said, "Well I didn't." We both sat down.

In the summer of 1929, before coming here, I went to Europe for the summer and spent about ten days in Goettingen. Emmy Noether was there, so I went to her seminar. It consisted of six people; Alexandroff was one of them, and van Kampen, a very distinguished topologist, who unfortunately died very young, was another. Gottfried Koethe was a student of Emmy Noether's. He later visited the University of Maryland, where I had become chairman of the department. We found that we had been sitting in Emmy Noether's seminar for ten days together without knowing it. The end of this story came years later through the good offices of the great J.W. Alexander. He finally found me a place to go when my fellowship ran out. I went to the University of Kentucky, and it took World War II to get me out of it.

Tucker: Incidentally, wasn't the first place Richard Brauer went, when he came to the United States, the University of Kentucky?

Cohen: Let me tell you something about that, because it involves Princeton also. I went to Lexington in the fall of '31. Shortly after getting there, I got a letter from Oswald Veblen containing a list of young mathematicians in Germany who were in trouble because of the political situation. Veblen asked if it might be possible to find a place for one or more of these in Lexington. Well, I had a colleague there who was a number theorist, a student of Dickson in Chicago. I took the letter to him, and we looked at the list of people. One of them was Richard Brauer, who was in Koenigsberg at the time, who had been a student of I. Schur in Berlin, and whose bibliography interested us. There was no algebraist at Kentucky, so we decided we would like to have Brauer come. Of course one difficulty was that the state of Kentucky was broke and the University of Kentucky was almost broke.

Aspray: Because of the Depression?

Cohen: Yes, this was 1931. I went to another colleague of mine, a bacteriologist, who had been living in Lexington a long time. I said, "How do we get some money?" He said, "I'll tell you what. You come with me. We'll walk up and down Main Street, and we'll simply hit every merchant on the street and demand money." Which we did. We raised a sum, not very much, I must say, but I was able to write to Veblen and say, "We have this amount of money. There is a place here. We would like to have Brauer." The result was that Brauer came. He spent a year in Lexington, and then he went to Princeton as Weyl's assistant at the Institute. There in Lexington we had an eventual winner of the National Medal of Science.

He was not the only National Academy of Science man who started in Lexington. Courant, who was not one to let an item like this go unnoticed, got hold of me after Brauer left and said, "I have a young student who needs a job, Fritz John." We were able to arrange a job. By that time the national situation had improved somewhat, and Fritz John came to Lexington. So the influence of Princeton, and Veblen in particular, had widespread effect. I mean that the department of mathematics at Lexington is today pretty respectable; at that time it was, I must say, rather sad.

Tucker: I have a vivid memory of Jimmy McShane during the period he was at Princeton, which was after Fine Hall was built. He was here for only a year or two, until about 1935 when he went to Virginia. He and I shared an office. I remember him standing in the common room and looking out the window. There was Emmy Noether walking up. She'd come from Bryn Mawr and was walking up from the train station. He said, sort of thoughtfully, "You know how you can tell a penguin from Emmy Noether?" Then he answered his own question. "A penguin doesn't have a briefcase." Noether always wore the same outfit; I think she had only this one. And she was almost as big around as she was high, so it was an apt description. She came to Bryn Mawr sometime around '33.

Cohen: Let me make an observation on Emmy Noether's appearance. As I say, I sat in on her seminar for about a week. At first glance she looked like the cleaning woman who had come to erase the blackboard. As a matter of fact, she would stand in front of the class with a sponge, which she used to wipe the blackboard. She was a large woman in a shapeless gown, but her eyes, behind absolutely clear glasses, had an intensity that was in stark contrast with the rest of her slack appearance. Now, I don't know what a penguin's eyes look like.

Aspray: Can you say any more about Veblen's role in placing refugee mathematicians?

Cohen: It was a large role. I have some recollections which I think might even be correct about a difference of opinion between Oswald Veblen and another influential American mathematician, G.D. Birkhoff. It was the Depression. Young American mathematicians were finding it hard to get appointments, and the question of whether to bring in foreign mathematicians to occupy positions which would then not be available to American mathematicians was debated. Veblen took what I would call the broader view. I hesitate to attribute views to Veblen, but the considerations that seem to have actuated him were two: a concern for the welfare of mathematics itself, and a humane concern for certain individuals who had talent. Veblen was a grand man, and the people for whom he made it possible to come to the United States made a great contribution to mathematics. G.D. Birkhoff opposed him on this.

Aspray: Can you give me Birkhoff's position?

Cohen: Well, one of the quotes that circulated which I seem to recall is "If these distinguished people come and take the positions, the young American mathematicians will become hewers of wood and drawers of water."

Tucker: It was also argued whether the refugee mathematicians should participate in the teaching, which was the bread and butter of mathematics departments.

Cohen: Right.

Tucker: In this matter Veblen acquired an unfavorable reputation with the administration of Princeton University, because he more or less said teaching has low priority.

Cohen: Which is interesting, because one of Veblen's principal contributions—in his books and in the people with whom he was associated—was teaching, and he was effective as a teacher.

Tucker: It was to any young research-mathematician that he said, "Don't waste your time doing any more teaching than you have to."

Cohen: Yes, I remember appreciating that very much, as one of the young people.

Aspray: Did he take his own advice?

Tucker: Of course from 1926 on he was research professor at the University, and after that at the Institute. In neither position did he have any teaching responsibilities. I suppose at some point early on he must have been a regular teacher.

Aspray: Another question raised by your stories concerns the placement of postdocs. A large number of people came through Princeton in this period, and you pointed out that it was Princeton people who helped you obtain the position at Kentucky. Do you think Princeton people felt a responsibility for placing postdocs? What was the attitude?

Cohen: I had a feeling they felt some responsibility. I was earlier at another university—I don't think I should name it—where one of the big subjects of discussion in the bull sessions among us graduate students was the apparent lack of interest on the part of the faculty in the life of the students. They seemed not to care one way or the other. I didn't have that impression about Princeton. For instance, Leo Zippin was kept on, I guess at the Institute, for four or five years, because there wasn't a suitable position for him. My impression was that young mathematicians of some talent were regarded as resources to be saved.

Tucker: This was the attitude of Abraham Flexner also. Partly to carry out some educational ideas of his own and partly to help people who had become stranded, so to speak, at the Institute, he arranged to place several of these, three or four anyway, in the New York City high schools. It didn't work out well because it was already a blackboard jungle there. I remember one of these people, George Garrison, who was put to teaching at a high school close to Times Square. It was the Harlem Boys Annex. He told me that there the teachers had to go in pairs to leave the school. When he wanted to have something put on the board, he would stand at the back of the room and have a student put in on the board, because the first time he put something on the board, a ripe tomato smashed on the board beside his head. He then managed to get into the City College system, and he eventually became chairman at CCNY. I mention this as an example of the concern there was, even on the part of the director of the Institute for Advanced Study, for placing people who were stranded.

Aspray: What was behind my question was the question whether Princeton felt an obligation to look after its own graduate students first, whether there was a distinction between their graduate students and the people who came to Princeton as postdocs.

Cohen: In connection with the remark earlier on your part, about subsequent influences, let me recall World War II. The Office of Scientific Research and Development—I think it was called OSRD—set up a number of applied mathematics groups at various universities to deal with problems arising from the armed forces in connection with the war. One of the oddest collections of mathematicians to form one of these applied mathematics groups was the one at Columbia under the direction of Saunders MacLane, which contained such distinguished mathematicians as Adrian Albert, George Mackey, Daniel Zelinsky, and a number of others, no one of whom had any interest in applied mathematics. I think Hassler Whitney was one of them. This was housed with Sam Wilks's statistics groups, but there at least there was some reason to believe that they could be useful.

Tucker: There was the STRG-C statistical research group at Columbia, and there was the STRG-P at Princeton. But there was a branch of the Princeton group that was housed at the same place as the Columbia group, and this was the one that contained Mosteller and John Williams.

Cohen: I would see them around. I never knew what outfit they were connected with, but they were around then. Anyway, Saunders asked me to join his group, and I did. One of the general principles of life that I had learned at Princeton was that all this crying down of pure mathematics because it was too far removed from the real world was nonsense, and that the proper basis for the application of mathematics was the development of good mathematics. I was thoroughly indoctrinated with this. Well, it had a certain small result in this AMG-C.

The problem that we were presented with as a group was the problem of plotting the course of an attacking fighter against a bomber. The problem seemed to be this. The bombers flew straight and level at constant speed, and they had guns with handle bars that could be aimed. The fighters flew any old way with the guns hitched in their wings; in order to aim the guns you had to aim the airplanes. To tell the gunners on the fighters where to point their guns you had to know where to expect this crazy airplane to be. The existing doctrine was quite wrong, and as a result we lost a number of planes, I think in the African campaign. So it was a matter of some importance to get what was called an aerodynamic attack course, so you could figure out where the planes were.

We all worked on that problem. I had to work on it. There had been some results offered. They weren't very good because they weren't very computable—for some reason the computations were unstable. I didn't know anything about computation. I didn't know anything about aerodynamics. And I made, without realizing it, a very unconventional choice of coordinate system for the attacking airplane. Everybody in the world, but I, knew that an airplane did not fly in the direction that its nose pointed. Instead of choosing the x-axis in the direction of flight of the plane, I took it to be the longitudinal axis of the airplane. The result of this seemed to be just enough difference in the crazy equations to make the computation of results stable. The only reason I got this was that I didn't know a thing in the world about aerodynamics. The result that I got checked out. I considered this a justification for the Princeton view of mathematics.

Tucker: Tukey and I were involved in a fire-control research-project.

Cohen: I always thought that the fire control had to do with the fire department. I didn't know what fire they were talking about.

Tucker: There was something discovered in our group, by I think George Brown. It concerned the Navy doctrine on gun duels at great distance, say 10 or 12 miles, where you could pinpoint your target horizontally but had to lob your shell, so the question was to get the correct angle of elevation. The Navy doctrine was to try to establish a bracket with your first two or three shots, and then proceed to bisect the bracket.

Cohen: They called that the pinching process when I was a student.

Tucker: We were able to prove statistically that the thing to do is always to shoot so that you think you have a 50-50 chance of establishing a bracket. With this new doctrine after four or five rounds you would be 1 round better than someone following the old doctrine. This was something that was done by statistical simulation.

Cohen: You mentioned Tukey, which reminds me of his dissertation. In his dissertation he noticed something about some work of mine that I hadn't noticed. Shortly after I got to Lexington I began to fiddle with the notion of uniform convergence in Hausdorff space. The trouble with that is that you have no control over the size of the neighborhoods. Then it occurred to me to take Hausdorff's first denumerability axiom—you recall, all the neighborhoods with index 1 were, so to speak, the biggest—and uniformize with respect to the index of the neighborhood. I published a paper on uniform spaces. Andre Weil was, at the same time, publishing this in Paris.

Anyway, I wrote several papers about this and forgot it, and then Tukey's dissertation came out and he kindly sent me a copy. In it he pointed out that in my later papers on uniform spaces I had not demanded uniformity over the whole space, but only what could be called local uniformity. My results were therefore more general. This had not occurred to me. Tukey mentioned that in his dissertation; I thought it was very pleasant.

Aspray: Earlier you contrasted Princeton with another university you had been at, saying that Princeton seemed to take care of its students. This was with reference to finding them jobs. What about looking out for their well-being while they were here—social environment and such?

Cohen: That was the difference I had in mind. Let me say I hate to make this contrast, because I have very fond memories of Columbia, where I was an undergraduate and where I got my master's degree. There we had little contact with our professors outside of the lectures. At Princeton there was the tea every afternoon, where you had a chance to talk to professors. You saw them as people, not just as mathematicians. That was in contrast to the situation at Columbia. In the year, or, I don't know, year and a half, I spent as a graduate student of mathematics at Columbia, the graduate students would get together over a beer or something and say, "What are our professors like? Do you know anything about them?" The answer was always No.

The mathematician whose name I couldn't think of, who was in Noether's seminar, was Gottfried Koethe. He later became professor, and I think rector, of the university in Frankfurt. I met him again for the first time when I was on the staff of the National Science Foundation.

In a way that's connected with Princeton, although perhaps not so closely with the University as with the Institute. The situation was this. After being on leave from Kentucky for a number of years and wandering around in the war business, I found it necessary to go to New York on family business. Leo Zippin, whom I had met in Princeton, helped to get me a job at Queens College. I stayed there a few years, and then I came to the Institute as a fellow of the Ford Foundation to do a book which I then completed ten years later. But I didn't want to return to Queens college. The National Science Foundation had gotten its first budget, and the first program-director for mathematical sciences got tired of it after about six months and wanted to leave.

It was at that point that I asked Mina Rees, "Is there anything possible in Washington?", because I was sort of desperate to remain away from Queens. She said that the National Science Foundation needed a program director for mathematics, and she talked to Marston Morse about it. Marston Morse, who was at the Institute, was very kind and recommended me. That's how I went to NSF. While I was there, Koethe came as an official representative of the German Ministry of Education, I believe it was, to visit the National Science Foundation. We met and discussed problems of the support of mathematics. Later, when I got tired of the National Science Foundation, I went to the University of Maryland. We invited Koethe to come. He came, several times, for periods of a year. I got to know Koethe very well.

Aspray: Al, you started to make a remark about NSF.

Tucker: This follows up Leon's contrast, in contact between students and faculty, of Columbia and Princeton. In the summer of 1963 I was asked by the National Science Foundation to visit a number of institutes that were going on that summer. As a sort of inspector, I was to spend a day or so at each. I remember emphasizing in the report I made to the Foundation, which it was my obligation to make, that the principal difference I noticed among these institutes was that with some of them there was communal life. For example, the single members of one institute were housed in a dormitory and took their meals together in a dining hall associated with that dormitory, and the married ones were living nearby and were required to come at least to lunch—they could take other meals in the dining hall too, of course. Also the lecturers had to take lunch in the dining hall. There was, adjoining the lecture halls, a departmental library where the members were encouraged to use books and study. There was sort of a Fine-Hall atmosphere.

At the other extreme was an institute at Penn State University, where there was a summer school going on anyway. The participants were housed wherever there were vacant rooms, and there was no plan for them to eat together. There was no contact between the students and the lecturers except in the classroom. I made the point in my report that it seemed to me that the most important thing in planning an institute was to provide as many contacts as possible outside the classroom. This is exactly the point that Leon was making.

Cohen: Talking about my post-Princeton history, I want to mention another thing that happened which I'm very proud of. I had the opportunity once to urge the University of Maryland to violate the state law. The situation was the following. We came into contact with a boy about 10 or 11 years old, in junior high school, who exhibited considerable mathematical talent. At that time I was chairman of the Department, and he and his mother came to see me. I had already heard about him from some of my colleagues. He behaved extremely well. I took the example of the City of New York, which allowed released time for religious education. The kids were allowed to leave school, go to parochial schools to get some religious education, and return. So I said, "If that can be done, why can't we allow for released time for mathematical education." We arranged with the junior high school for him to come to the University; his mother brought him and took him back.

After a year or so it became clear that he should not stay in the junior high school at all. He came full-time to the University, with no official status. It became clear that this couldn't go on. We had to admit him as a student. So I went to the Dean of the Faculty and explained the situation. He said, "I'm sorry, it's impossible. There are only two ways in which it is lawful for a student to enter the University: either he has a high-school diploma or he passes an equivalency examination." This kid was still in junior high school, so he clearly had no high-school diploma. A necessary condition for taking the equivalency examination was that you be 21 years old, and he was not quite 11. Clearly there was no legal solution, so I said, "How about doing it anyway?" Of course the objection was that if this became known, every mother in the state of Maryland with a son would come storming onto campus and demand admission. I said, "Suppose nobody knows. That might be possible." So he said, "Look, don't try to exploit this thing for publicity. We simply keep our mouths shut." The upshot of it was that the Dean of the Faculty was able to persuade the regents, who permitted the kid to be admitted.

He graduated with honors, and we wanted to put him into graduate school. I wrote to a number of places, many of which had had bad experiences with infant prodigies. But among the people I wrote to was Al Tucker at Princeton. I told him that this boy showed none of the stigmata of genius; he was just a nice kid. I asked if he would consider it. Al came through. Princeton admitted him, and he took his doctorate. As you probably know, he now has a Fields Medal and is a professor at Princeton.

Aspray: You should mention his name for the tape.

Cohen: Charles Fefferman, of course. There's an outcome of my couple of years at Princeton.

Aspray: You mentioned that tea here is part of the social environment. I understand that it went beyond tea. There were a few faculty members who were great entertainers at their homes and would generously invite graduate students and postdocs. Did you participate in this?

Cohen: Yes. Alexander had tango parties; we went to them. My wife was treated warmly by Mrs. Eisenhart and other ladies in the department and by Mrs. Trowbridge, wife of the Dean of the Graduate College. We were made to feel quite at home. I must say that for me there was an obstacle to overcome with this friendly atmosphere: because of my background I was more than diffident about these social contacts. I simply was not prepared for so formal an academic society, but they were gracious, even to the point of not insisting, of not taking offense when I didn't encourage personal contact,—which I think was very important.

Aspray: That brings to mind a question I haven't thought about before. There are a large number of mathematicians who don't seem to have very well developed social graces. I could see these things as being awkward for a number of mathematicians. Did you see that that was a problem?

Tucker: It certainly was for me, because I came from a family that was very puritanical. My father was a Methodist minister, and I was the only child. I didn't have much in the way of social contacts, and that's probably what made me a mathematician. When I came to Princeton I hadn't been away from home before; I lived at home when I went to the University of Toronto. The way I broke social ice was simply to do what I was told was customary here, which was to go on a Sunday afternoon and call on senior members of the faculty. I found this went happily, except when I tried to call on the Alexanders. They weren't accustomed to having tea at that time of day—it would be liquor of some sort. I was a tee-totaler, and Mrs. Alexander—there was no one else there but Mrs. Alexander when I arrived,—tried hard to get me to take something. Finally she finessed me by offering me some Irish coffee, and I didn't know what Irish coffee was.

The answer to your question is that there were many graduate students who were unaccustomed to any social things of this sort. I remember also envying Bill Flexner, who was completely accustomed to this life.

Cohen: The non-mathematical side of it was interesting, because, you see, the Depression hit in the fall that I came here. A number of friends of mine in New York, where I grew up, were much interested in leftist movements, so I knew about these things although I never formally associated myself with them. Prior to my going to Kentucky, I made the acquaintance by chance of some representatives of John L. Lewis's United Mine Workers of Kentucky who had come east. I engaged to raise some money for them, which I did not do too successfully; around Princeton, I collected some money. Lefschetz thought this was very foolish on my part, and he was right, but I was a bullheaded guy.

Tucker: Alexander would have thought differently.

Cohen: That's right. As a matter of fact, I suspected that one reason Alexander became so much interested in me at the end of that year was that he heard about this. As I recall, he was a socialist.

Years later Lefschetz and I were on an easier footing. After he retired from Princeton, he organized the Research Institute for Advanced Studies as part of the Glen L. Martin Corporation. There was a young topologist, a student, I think, of Lefschetz's, who was at a meeting of the American Mathematical Society, held at Princeton, to present his dissertation. I came to Princeton to recruit him for the University of Maryland. In the audience was Lefschetz. When this kid finished his talk, which was the first on the program, I left the room to look for him. I didn't know that Lefschetz too had left, and we bumped in to each other in the corridor. He was surprised and said, "What are you doing?" I said, "We're looking for the same man." We agreed on what we would do; we would offer him a joint appointment, which we did.

That started a collaboration between Maryland and RIAS. At one time, when Lefschetz was interested, I urged the University of Maryland to take over the whole outfit, lock stock and barrel. The

President very foolishly didn't do it, and RIAS went to Brown University. We could have had it, if it hadn't been for this ex-quarterback who was President. But that's a different story. The Lefschetz connection ended up cordial. I think it was cordial on his part all the time, it was simply that in the beginning I was scared.

Aspray: You were at a number of different universities during the '30s. You later on had positions with the government which allowed you to see what was going on around the country. Could you make some comments about Princeton as compared to other research centers in the '30s, also considering the kinds of students produced for the next generation of mathematicians?

Cohen: It probably is not fruitful to make comparisons. Each of the great places developed a style and an approach to mathematics and to mathematics education, which was *sui generis*. One of the reasons they are distinctive as contributors to the mathematical culture is that they have this personality.

Tucker: One specific point we were talking about earlier. I was describing how all the time that I was at Princeton as a graduate student I never took a written examination, nor did I at any time receive a grade. The only fixed requirements I had were to pass my two languages, which was done orally with Lefschetz, to pass the general examination, which was a 3-hour oral examination, and to defend the thesis in an oral examination. This freedom, coming as I did from the University of Toronto, where I was examination bound, impressed me greatly. I think that that reaction probably was usual. I wonder about your experience at Columbia and Michigan.

Cohen: Let me compare Columbia and Michigan and Princeton, because these are the places where I studied. I had very good teachers at Columbia. There was G.A. Pfeiffer, who I think was a Princeton Ph.D., although not famous as a research mathematician. There was J.F. Ritt, a very distinguished analyst. And, as a young man, Marshall Stone came and spent a year.

Tucker: Smith?

Cohen: No, Paul was still in Kansas with Lefschetz. I met Paul later when we became neighbors in Vermont. As an example of the apparent lack of concern for students, I might mention that while I was a student I wrote a letter to Sierpinski in Warsaw. A 1-page letter suggesting that he might be interested in a proof that I found of Lusin's theorem. To my great surprise, he wrote back immediately and said that it would appear in *Fundamenta mathematica*. When I told my professors about this, they said, "You have some nerve doing that."

Tucker: Did you have examinations to pass?

Cohen: Yes. They had a wonderful grade at Columbia during my graduate period, called H, which meant that you had attended the course but not taken the examination. It was not a failing grade. It testified to your persistence, that's all.

Tucker: At Michigan?

Cohen: Yes. Life was personally very pleasant there. I was well treated. One of the nice things about Michigan was that they had very few graduate students when I was there. They hadn't had a Ph.D. candidate for some years. There was the regular Math Club and the colloquium. And Rainich, a Russian immigrant from the first world-war, bought a rubber blackboard which rolled up. He suggested to Wilder and one or two others in the department that we meet every week in his house; somebody might make some mathematical remarks, and we could have tea. This was known as "the small c" to distinguish it from "the capital c", which was the normal math club. This thing lasted for 20 years, with only the statement after each meeting, "Well, come to my house next week." They invited a couple of graduate students to attend this thing, and so I had my first introduction into human mathematical exchange. That was very good.

Tucker: I remember Tommy Tompkins telling me about that. He was a student of Rainich, who came to Princeton as an NRF and stayed on as an instructor.

Cohen: It was a great institution.

Tucker: Were there course examinations there?

Cohen: I don't remember. I don't know that I enrolled in any courses for credit. I don't think I took any examinations. You see, I had my master's degree. They treated me like a kid who was growing up in the family, which was fine with me.

When I got to Maryland in 1958 it was already a large department. I scarcely got to know all my colleagues. I don't know how many thousands of students there were. We managed to set up an informal thing, so that whenever any student turned up who looked promising, he was babied and taken into the family. We started an honors program at Maryland. My daughter, who is at Rutgers, told me a story related to that. She said the department secretary recently spoke to her and said, "We have a strange situation. There's a student who's a senior, and he says he's a math major. We have only one grade for him, an A, but that seems to be the only math course he's taken." She said, "I'll look into it." It turned out that this kid had come from Maryland; he had taken all the undergraduate courses while he was still in high school. That sort of thing was possible at Maryland, but it reached only a small fraction of the students; most weren't particularly interested.

The summer of '29, if I remember correctly—it was before I came to Princeton—after spending a couple of weeks in Goettingen, we went to Munich. I visited the university and was introduced to this young man, Salomon Bochner. I had another interesting meeting there. Caratheodory had lectured in the United States and come to Ann Arbor while I was there. A very impressive fellow. One of the few books I ever bought was his *Vorlesungen ueber Reelle Funktionen.* So in

Munich I reminded him that we had met at Ann Arbor. He insisted that I come to visit him at his home, and for the first time I saw what a well-endowed chair could do for you. He lived in great comfort. He was a very pleasant host; he put me at my ease, and we had a pleasant discussion for about half an hour.

Tucker: I met him at the international congress that met in Zurich in 1932. On a Wednesday afternoon there were about a dozen excursions from which to choose. It happened that he chose the same excursion I did. It involved a cogwheel trip to the top of some mountain, Rigi I think.

I was just starting my year as a National Research Council Fellow. I purposely started at Cambridge, England, so that I could go to the international congress and then immediately start the fellowship. Maybe I took the stipulations much more stringently than I was supposed to, but in the six weeks off I counted on going home to Toronto to visit my parents for Christmas, and this wouldn't leave much of the six weeks. Well, this was part of my upbringing. Of course, when you went with Alexander to Europe that was, I think, sufficient explanation if you had to give any. This was a freedom that never occurred to me at the time.

Cohen: I mentioned this little hut that Alexander had. I guess it was about a year ago that I noticed tacked onto the door of one of our offices the announcement of a great scientific conference scheduled at Les Houches. "My God," I said, "they have a few guides' huts and that's all." I did not know that this tiny village had become an important scientific center. In 1930 the scientific center consisted of Alexander's hut.

Tucker: I'm glad to hear this story, because my next-door neighbor was co-director of a conference that was held in Les Houches just last fall.

Cohen: Yes, it was a tiny village. It was a stop of a narrow-gauge railroad that ran from Chamonix to Grenoble. The world changes.

I went to Brown on a sabbatical leave because J. D. Tamarkin was there. He had made a kind remark to me years earlier. Tamarkin and Hille, whom I met in Princeton, were collaborators and published jointly. I began to work on a little problem in functional analysis, and when I mentioned it to Tamarkin a week later he said, "Hille has a student at Yale who is working on the same thing." His name was Nelson Dunford, and a joint paper came out of it.