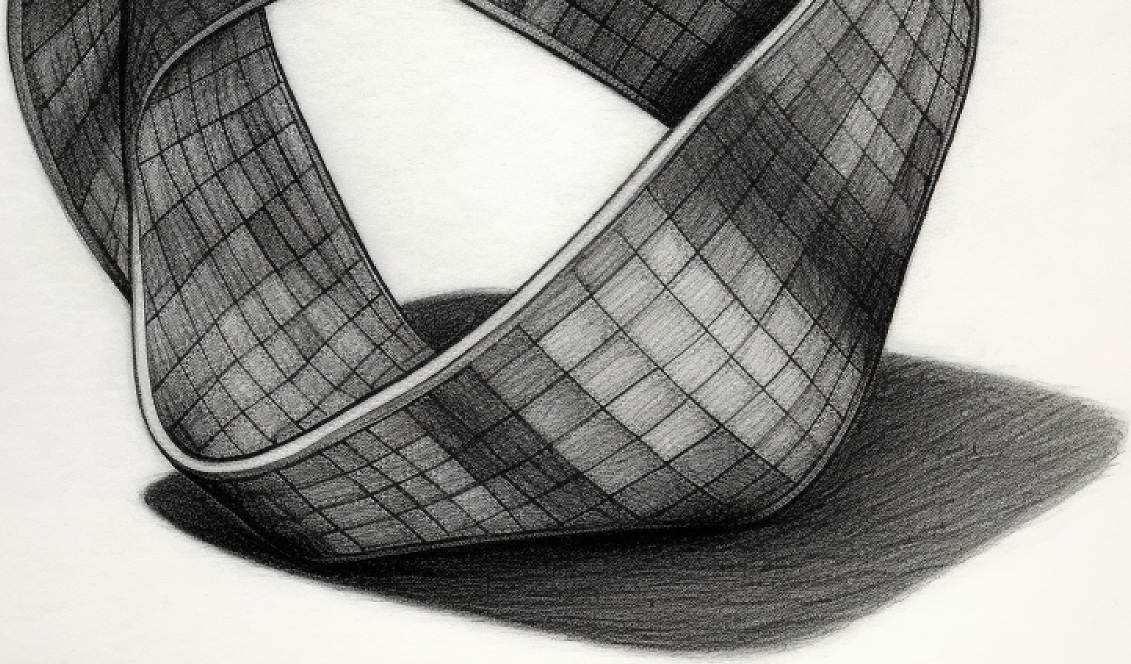

The Career of Albert Tucker: An Oral History

Albert William Tucker (1905 – 1995) was a distinguished mathematician and educator whose work significantly shaped postwar economics and business. He joined the faculty of Princeton University's mathematics department during a transformative period in the 1930s, marked by the influence of luminaries such as John von Neumann and Albert Einstein, and presided over the department during the 1950s and 1960s.

Tucker's best known contribution to the field of mathematics was the establishment of the mathematical foundations of linear programming. He was also a gifted communicator who introduced the concept of game theory to a broader audience, crafting the renowned example known as the Prisoners' Dilemma. His mentorship cultivated a remarkable generation of mathematicians and the first generation of game theorists, including John Nash.

Tucker's generosity extended to his scholarly work, as he often chose not to publish his own papers to make way for emerging voices in the field. His lasting impact on mathematics and his role as a mentor and educator continue to be celebrated in the academic community.

About This Oral History

The 1930s saw the flowering of a unique mathematical community at Princeton University with the construction of a luxurious new building Fine Hall (now Jones Hall) dedicated to the mathematician and Dean Harry Fine and designed to facilitate a real community of mathematicians engaged in research and closely linked with mathematical physicists in the attached Palmer physics laboratory to which it was connected and shared a joint math-physics library.

This community was unlike any other in America before that time and perhaps afterwards, and had important consequences for American mathematics. With the planning and founding of the Institute for Advanced Study at the beginning of the decade, originally having only a mathematics department, which then shared Fine Hall with the university mathematics department as a single institute during the period 1933 to 1939, starting with three of the university's leading mathematicians joined by Einstein and Gödel and attracting many visitors, a very exciting environment developed which many students and faculty were loath to leave.

Half a century later, in 1984, one of the original participants, Albert Tucker, himself a former mathematics department chair at Princeton, was motivated by Princetonian historian of science Charles Gillispie to capture some of the personal reminiscences of the period on tape with the help of William Aspray, which were then transcribed and organized into a body of written transcripts by then graduate student Rik Nebeker.

***

Almost another half century later, in 2022/23 Carl Cervone, the grandson of Albert Tucker and son of Barbara Tucker Cervone, created a digital version of the original audio transcript, with the assistance of three college students, Arman Singh, Ethan Wei, and Shreya Devaran, who were inspired by Tucker's work and teachings. This digital transcript only includes the portions of the Oral History Project titled:

- Albert Tucker: Career Part I
- Albert Tucker: Career Part II

The original recordings of these conversations are housed at Princeton's Seeley G. Mudd Manuscript Library, and scanned transcripts of all of the Oral History Project conservations are accessible via the Princeton Department of Mathematics website.

Contents

Part I

Childhood & Early Schooling: 1905 - 1923

Speed: Would you begin with your early schooling and with what led you to take up mathematics as a career?

Tucker: Well, I was born in Canada, in Ontario, not too far east of the city of Toronto. My father was a Methodist minister, and I was the only child of my parents. We lived in several small towns as I was growing up because Methodist ministers, then anyway, moved about a great deal. We were never longer than three years in any one place.

I was late in starting school, almost eight years of age when I first went to a school. I had learned to read and to do many things already at home. My parents were both very much interested in my education, and at a very early period my father bought a children's encyclopedia called The Book of Knowledge, which came from England. It was in that that I did my first reading. At a later time, but still when I was a school boy, he bought the eleventh edition of the Encyclopaedia Britannica. So I had pretty good facilities at home, and because I moved around from place to place I had a rather lonely childhood, and didn't have much in the way of friends. I suppose that this had a certain influence in turning me at an earlier age than most children to things of the mind.

My mother tells me that when I was four or five years of age the thing I used to ask for was, "Draw me a map." There was always a globe in the home, and that was practically a play thing of mine. When I started school I moved ahead very rapidly. I think I went through what would have been three years, the first three years, in one year. I felt that I had very good teaching and did well in all my work. I don't know that I had any particular liking for mathematics over other things. The one thing I noticed about mathematics was that I could do it more easily than the other things. Latin I did well in, but with a great deal of hard work. Mathematics I did equally well but with no work at all.

The point where it became clear to my father at least that I had some mathematical gifts was when I was in my second year of high school. I was in high school for five years, the last year corresponding to grade 13. In other words, I did an extra year in high school in order to prepare for an honors course at the university, rather than the so-called general or pass course. In my second year of high school I had geometry. The mathematics was I think almost entirely geometry. And that was my first formal acquaintance with geometry. It was a small school, about 100 students. There were just three teachers, and the principal of the school, a man, was also the one who taught mathematics and science. He had observed that most students he was teaching could not do much with the original problems. So he had them learn the propositions by rote, if that was the only way the students could learn them, because we had to take examinations that were the same all over the province. He knew that if students could handle all the book propositions that were on the paper, then they would get a passing grade, not an honors grade but a passing grade. So he concentrated on teaching the propositions. There were these problems there in the book but nothing was said about them, so I was just concentrating on the propositions.

On the other hand I didn't particularly want to commit them to memory, so what I was in a way doing was really understanding the propositions. Well, about the middle of the year he gave us an examination. He just used an old provincial examination, and he forgot all about the fact that there were some originals on the examination. Well, I did the propositions and I did the originals. He came that night to see my father and said, "You have been coaching your son," because my father had actually planned to become a teacher of mathematics before he decided to go into the ministry and he had been through about two years at the university. He had taken calculus, and then he had switched to philosophy as a more appropriate background for a career as minister. So the high-school teacher, who knew something about my father's background, thought that my father had been coaching me at home and had me do the originals.

My father said no, that he had not done this at all. The high-school principal said, "Well, it's just fantastic that he has done these originals although he has never been given that sort of thing to do in class. He has just done them." This so shamed him that for the rest of the year he really taught a bang up course. No more just teaching the propositions, he really threw himself into it and then was a very good teacher of geometry. He had a very good grasp of it, it was just that as principal of the school he had so many other things to do that he was just following the path of least resistance. He was so pleased with me that he told my father that he thought that I should aim to become an actuary. He felt that becoming an actuary was, so to speak, the top thing that one could do with mathematical skills. Anyway this then made my father very interested in my mathematical ability, and of course this rubbed off on me so that I was aware that I was pretty good at mathematics, And this I think stimulated me to take a greater interest in mathematics and to delve more deeply. Before that, because I could do it so easily and had no problem on examinations, I was inclined to spend my time mainly on other things that did require my effort. But this served to put back more of my interest in mathematics.

In my fifth year in high school I had an introduction to analytic geometry, a very serious course in trigonometry, both plane and spherical, as well as a good deal of algebra and even a little bit of solid geometry. So I had a great deal more than students seem to get nowadays, even in Canada.

University Years: 1923 - 1928

When I entered the university I enrolled in the honors course in mathematics and physics. This was a sort of basic program that branched in various ways later on, so there were about five or six different end possibilities. In my first year at the university I had about three courses in mathematics that went throughout the year, as well as courses in physics and chemistry. One of the mathematics courses was a course in conic sections which I had from someone who is quite well known in the mathematics world, John L. Synge of Dublin. He gave a very rigorous course following an old British classic book, C. Smith, *Conic Sections*. This was not just a book in analytic geometry, but also an introductory book to invariance and things that are now a part of...

Speed: Algebraic geometry.

Tucker: Yes. We studied very thoroughly all the transformations that dealt with a general equation in the second degree, as well as even some of the projective properties. So that was a very demanding course, not at all modern, but one learned a tremendous amount in such a course.

Then I had a course which was called Introduction to Analysis which was entirely on limits. It was quite serious, you know. Besides doing series and that sort of thing we did the problem of how to define the area of a plane figure. In other words this was an introduction to calculus without ever any mention of derivative or integral. It was all set up in terms of limits.

Speed: Was that based on Hardy's book by any chance?

Tucker: There was no book.

But it was very much like Hardy's *Pure Mathematics*, which I got to know later. I didn't know about it at that time. And then there was another course that was called Higher Algebra and it was based on another old British book, Hall and Knight.

This was the complete Hall and Knight, so that it got into some elementary number theory. It was again a course that involved many of the ideas that are now regarded as part of abstract algebra.

Speed: Theory of equations.

Tucker: Yes, theory of equations and so on. There was no calculus in the first year except a course that the professor in physics gave us one hour a week, based on the book of Sylvanus Thompson, Calculus Made Easy. This was so that we could use the calculus in the physics. At the end of that year Synge left to go back to Trinity College, Dublin. He left a note with the head of the department saying there's a student in the first year by the name of Tucker who should be watched. I didn't know about this until considerably afterwards. But the head of the department from that point on took a very fatherly interest in me. He was an old Irishman, a bachelor, very much a man of the world. But he had a very sharp knowledge of mathematics, not in the sense that he had produced mathematics, but he was really a scholar in the best sense of the word. In my second year I had simultaneous courses in differential calculus and integral calculus. The professors giving these, one was fresh from Cambridge where he had stayed on as a fellow for a few years. Then he came to Canada. He was English. His name was W.J. Webber. The other, a man by the name of Samuel Beatty, was a Canadian who had grown up in the University of Toronto.

They conspired very beautifully to teach their courses, so that while Beatty in the differential calculus was teaching the usual thing about derivatives and such, Webber in the integral calculus was teaching essentially the Riemann integral with no mention at all of derivatives. Then suddenly after about six weeks the two courses came together in the so-called fundamental theorem of calculus. This was very spectacular.

They also did an unusual thing that year. It didn't pan out for most of the students, but it was great for me. They used as a textbook in both of these courses the *Cours d'Analyse* by de la Vallee Poussin, in French of course. This really introduced me to rigorous presentation of mathematics.

I continued in the second year to follow physics as well as mathematics. Indeed, physics attracted me more than mathematics because physics has, so to speak, more glamor to it. I happened to have that year a course which was an introduction to the history of physics given by the chairman of the physics department. He had attended, during the summer of 1925, a conference in Italy in which there was discussion of the new results that were beginning to appear in quantum theory. So he said to the students in the course, "I don't know anything about this quantum theory. It's too mathematical for me. Who's the best mathematician in the course?" There were about 50 students, and they looked over towards me. "Well," he said, "I want you to make a report on quantum theory." You know, I had no background. There was nothing at that time in the way of a textbook. There were just a few papers by Dirac and Heisenberg, and I couldn't make very much of them.

Speed: Did you know German?

Tucker: I had taken German in high school. This was very difficult. I was not able to get German until about my last year of high school because it was so soon after World War I. In Canada they had dropped the teaching of German in World War I, and it was only just starting to come back. But I was lucky that there was a man teaching German in the school that I attended in my fourth year of high school.

I actually attended four different schools in my five years of high school. In my fourth year I attended this school where there was this man by the name of Schultz, who was born in Canada but of German parentage, and who spoke German and really had a very good feeling for the language. He wasn't able to get very many students so he lavished a lot of attention on the students he did get. So I couldn't make my way easily with these papers on quantum mechanics. Anyhow, I gave this report, and I have no recollection of what I was able to say. But the chairman of the physics department was terribly impressed, so he right then and there said that if I would choose to major in physics that he would make sure that I had an opportunity to go on to post-graduate work. This was the first time that there had been any mention of post-graduate work. I had been thinking of getting my bachelor's degree and then probably teaching mathematics and physics in a high school, or perhaps becoming an actuary. That was also a possibility. But this raised the question then of post graduate work.

Well, I spent several weeks talking to all the people I could in mathematics and all the People I could in physics. I finally decided that Physics was not for me, and that mathematics was. In the meantime the head of the mathematics department, when I had told him what Sir John McClennan, the head of the physics department, had said, "We can match it." But I really felt that physics was more glamorous. There's no doubt about that. It fascinated me more than mathematics, but when I talked to the physicists and the mathematicians I found somehow that when I talked to the mathematicians that we seemed to be talking the same language.

Most of the physicists there were experimental, and, you know, they would talk about magnetism in terms of a lot of little magnets that were in the middle of a big magnet. It was a very mechanistic view that they still had towards physics. And somehow or other this didn't satisfy me. I felt that they were not coming to grips with their subject. It was a descriptive rather than an analytical approach. Of course I was right, but I was very naive at the time. So I told people at the time that the reason I chose mathematics over physics was simply that I liked the mathematicians better than the physicists. So in my third and fourth years I dropped physics and concentrated on mathematics.

In my third year I had a very good course in differential geometry out of Eisenhart's book and a course in projective geometry and a course in differential equations. I had a very eminent man teaching me differential equations, J. C. Fields, after whom the Fields Medals were named when they were established by the International Mathematical Congress. But he was ill at that time and died shortly afterwards. He wasn't feeling well enough to do anything except go through the motions of teaching the differential equations course. Then in my fourth year I had, for some reason I don't know, two courses in complex variables. One entirely from the Weierstrass point of view, the power-series point of view, and the other pretty much from the Riemann mapping point of view.

Graduate Studies: 1928 - 1929

Tucker: I stayed on for an additional year to get my master's degree. This was largely because I had not been able to decide where I wanted to go for post-graduate work. The head of the department wanted me to go to Paris. He thought that was the place to go, but I was worried about having to study mathematics in a foreign language. And after Paris he suggested, because I said I was interested in geometry, Rome and Bologna because the Italians then were very active in geometry. He also mentioned Göttingen. He said, "All you've got to do is decide where you want to go, and I'll get you the money." He knew various wealthy people who would simply set up a fellowship if he went to them and asked them to do it. Of course my father as a minister didn't have the resources to pay for my graduate education.

I knew so little about things that I didn't know that in many places you could apply to that place for admission and that perhaps they would furnish the financial support. I couldn't make up my mind, so I stayed on for a fifth year at Toronto working for my master's degree. Now I was a teaching fellow in that fifth year, and this was a wonderful experience for me. There were actually three teaching fellows, but two of them dropped out very shortly after the year began. A very simple expedient was used: as they dropped out, their duties were given to me. So I ended up teaching three courses, two of which were entirely my responsibility. One was a course in advanced calculus for aeronautical engineers. That was the first year at Toronto that there had been aeronautical engineers. They were a select group. Several were mechanical engineers at the end of their second year. This picked group started the program in aeronautical engineering, and it was decided that they ought to have more calculus than the civilians and mechanicals got. So I was just assigned the job of teaching these 10 or 12 students the advanced calculus that would be appropriate to the needs of aeronautical engineering. And I did it. I don't know that it was as well done as it might have been, but I was asked to do it, so I did it.

Another was a course in mathematics for economists. There were only about six students. They were all students who had started out in mathematics and then switched to economics. I had not at that point had a course in economics in my life. It was suggested to me that if I took a book by Alfred Marshall, Principles of Economics, I would find an appendix in which mathematical things such as marginal utility and marginal revenue and elasticity of demand are explained and the appropriate calculus notation introduced. There was nothing like R. G. D. Allen's book in mathematical economics available at that time. It came shortly after. I really had to find my own way and even improvise things to teach. I think I ended up most of the second half of the year in teaching statistics, which of course matters for econometrics.

Well, the third course was to run a problems session for a course in actuarial science. I had been through that course, so this was a perfectly straightforward job. But for both the other ones I had a great deal of investigating to do. Also in that fifth year I had the privilege of going into the stacks in the library. Before that I simply had to send in a card for any book that I wanted. So with that privilege I spent hours in the stacks just looking. Until that time I had no idea of the availability of mathematical books even at the textbook level, and I found out about journals for the first time that year. Another thing that I got out of being a teaching fellow is that I was given an office. It was a tiny little room up in the attic. The first time I entered this room there was a table and a chair and a naked lightbulb that hung down from the ceiling. That's all there was in the room, except on the table there was a Princeton catalog. So, of course, I studied that Princeton catalog.

As I said earlier, geometry was my favorite part of mathematics, and when I saw the courses that were described in this Princeton catalog it seemed as though about half of them were in geometry with the name of Eisenhart attached to some, and the names of Veblen, Alexander, and Lefschetz attached to others. Of course, later on I found that these courses were not given every year. Indeed it was sort of luck of the draw what courses would be given in any one year, but there was nothing said about this in the catalog. They were all there, and I just assumed that they were all available. So I decided almost instantly that Princeton was the place where I wanted to go. So I went in and told the head of the department that I wanted to go to Princeton. "Oh," he said, "I don't think you want to go to the United States." He said, "You should go to. " He went through his list again, and I admitted that I was timid about going to a foreign country. "Well," he said, "then you should go to Cambridge." So I wrote to Cambridge to get information about the courses at Cambridge. And the information came back and I compared it with the Princeton information, and it seemed very poor indeed.

At that time there was a 19th-century-geometer by the name of H. F. Baker. He had written a many-volume book on geometry, which was a summary of things as they were at the turn of the century. I was very naive, but this didn't attract me in the same way that this variety of courses, analysis situs, Riemannian geometry, projective geometry, and so on did. And I knew that I had studied from Eisenhart's book on differential geometry. I also knew the book of Veblen on projective geometry. So I went back and said, "No, I want to go to Princeton." "Well," he said, "I don't know what we're going to do with you Mr. Tucker. You don't seem to be able to take advice." He said, "If you insist on going to the United States, there are only two places that are worth it, and these are Ha rvard and Chicago." So I wrote off to Harvard and Chicago and got catalogs back from there and made the comparison of the courses there with the Princeton courses. They were even worse than the Cambridge courses.

So I went back again. I at least was stubborn. I said, "Princeton is the place I want to go." And he really got angry with me. It was the only time in all the many years of excellent relations we had that he got angry, and I had to leave feeling this was an impasse. But there was a Frenchman by the name of [Jacques] Chapelon teaching then at Toronto. He had been a student of [Jacques] Hadamard in Paris, and he regularly returned to Paris because the Toronto academic year would finish about the first of May and didn't resume again until the beginning of October. In Canada the academic year was very short so that the students could go and work on the farms during the long summer. So he could actually spend five months a year in Paris and still have his job at the University of Toronto.

Well, he saw me and apparently perceived that I was depressed, and so he said, "What's the matter†" And I unburdened myself of the story. "Well," he said, "let me see these catalogs that you have." So I gave them to him. Well, he had gone to the same lycee in Paris that Lefschetz had gone to. They had known one another as schoolboys, and he had tremendous respect for Lefschetz. He felt that anywhere that Lefschetz was must be a good place. So he went to the head of the department and took up my cause. He said that he had looked into it and felt that I was right. The best place for me to go was Princeton. Well, the head of the department did a complete about face and said he would arrange for me to go to Princeton.

He wrote to Princeton to a man that he knew there, who was the chairman of the mathematics department, a man by the name of H. B. Fine, after whom Fine Hall is named. But Fine was already dead. Fine died in December, and the letter didn't arrive there until January. So the letter wasn't answered until about April.

I thought that there was no hope, and I had just about reconciled myself to the idea of staying on at Toronto and getting a Ph. D. there when a letter came from Eisenhart saying that Dean Fine had died and the letter had found its way to him. He wrote that it was now too late for Mr. Tucker to apply for admission in normal course as a graduate student, but that they were short one person to serve as a half-time instructor, and that if Mr. Tucker had teaching experience and could be recommended highly in this respect; they would consider him for this part-time instructorship. The appointment as part-time instructor automatically carried admission as a graduate student. Well, thanks to all this teaching that I was doing, there was no problem at all in recommending me as a teacher. So I was admitted to Princeton and went there in September of 1929 to start graduate work. And except for leaves-of-absence and such I've been at Princeton ever since.

PhD Studies: 1929 - 1933

Speed: You arrived in Princeton in September of 1929?

Tucker: Yes, in September 1929, and I taught six hours a week, two sections of analytic geometry during the fall term, and differential calculus during the spring term. I took three courses, one from Eisenhart. He had written about three years before his book on Riemannian geometry, and in this course, which went throughout the year, we went through most of that book. It was his system that when he had taught a course once or twice he would then turn that into a book, and once he had the book available he let the book teach the course for him.

At the end of a class he would say, "Now for next time read such and such in the book and try such and such problems." And when he would come to class next time he would sit down and ask if there were any questions about the material in the book. Once those questions were disposed of, he would then ask members of the class to go to the board and do problems. He was very mild and jovial, but if he asked you to do a certain problem and you declined his disappointment was very hard to take. He didn't make any sarcastic remarks, but it was very clear that he was sadly disappointed. I don't think it would be good if all courses were like that, but an occasional course of that sort is, I think, a pleasant change.

I also had a course from Lefschetz. He was just finishing his first book on topology, and he was lecturing on the basis of his writing of that book. When I arrived at Princeton in 1929 'analysis situs' was the term used. But Veblen had written a book with the title Analysis Situs which had been published as one of the colloquium volumes of the American Mathematical Society. Well, Lefschetz wrote his first book on topology to be published in the colloquium series also, so he couldn't use the term analysis situs. He wanted, as he would say, a short snappy title. He didn't want any long-winded title.

So to get a title that would be different from 'analysis situs' he decided to use the word topology which had not been used prior to that in English. It had been used in German. Indeed, there was a book written about 1850 by a student of Gauss by the name [J. B.] Listing that had 'Topologie' in its title. But there was no precedent in English, the word 'topology' did not exist. Lefschetz invented it as the title of the book that he was writing. Well, once he decided to use that word, he mounted a campaign to get everybody to adopt it. He was very successful, mainly I think because the word topology lends itself to all sorts of derivative words, whereas analysis situs does not. So in one year ‘analysis situs’ was dropped and topology was adopted.

The course that I had with Lefschetz based on his Topology was a rather poorly organized course because Lefschetz was always too restless to do things in a systematic fashion. When he would give a proof he would start at the beginning, get impatient and jump to the end, and come back from the end, and still usually leave a great big gap in the middle. He was notorious for his sloppiness in mathematical rigor. On the other hand he had just remarkable intuition. I don't think he ever published a result that was wrong. His proof may have been quite incomplete, but his results were always very sound.

I was more attracted to the Riemannian geometry of Eisenhart because it was more orderly than the disorderly topology of Lefschetz. Indeed, in the course of my first year at Princeton, I wrote my first paper. I didn't know I was doing it. I went to Eisenhart about the middle of the year to say that I felt there was something that he had developed in his book that could be done in a much simpler and neater way. Well, he was quite patient in listening to me, and he suggested that I write this down so that he could look it over. I did this. And he made certain criticisms, and I rewrote it two or three times. Finally, he bowled me over by saying, "Now, Mr. Tucker, I would like to submit this for publication in the Annals of Mathematics." Up to that point the idea of me writing a paper had not occurred to my mind. I was just doing this to make my point with Dean Eisenhart.

At this time he was Dean of the Faculty. He held the second most important position in the University administration, next to the president. So he was very heavily involved in administration. But he did take the time to spend with me about these ideas, which led to me writing my first paper without knowing I was writing a paper. I did submit it to the Annals of Mathematics, and it was published about a year later, published while I was still a graduate student.

I continued to be interested in both differential geometry and in topology. I also took a course in my first year from Hille, which was a course in complex variables. I had already had a considerable amount of complex variables in Canada, and this gave me a considerable advantage, with the result that I made a very good impression on Hille. So in my first year there I won three very important friends, Eisenhart and Lefschetz and Hille. In the spring term in my second year, Lefschetz exchanged with Alexandroff, so Lefschetz was in Moscow and Alexandroff was in Princeton. I took the course that Alexandroff gave, which was a course leading up to dimension theory in the Alex and roll form rather than in the Menger-Vietoris form.

I had some topology also from J. W. Alexander. It was very difficult for him to give a course, because he was always wanting to work with the ideas that were then in his mind, but at the same time he was a perfectionist, so until he had these ideas in good form he didn't feel able to talk about them. The things I had from him were rather in bits and pieces. He would do something in, so to speak, seminar form that he had nicely worked out, and then he would get to a point where things didn't satisfy him and he would just call it off.

I had some influence from Veblen also. Veblen at that time held the research chair, so he did not give courses. He held only a seminar. He was working at that time on the foundations of differential geometry, something which he subsequently published as a Cambridge Tract with [J. H. C.] Whitehead. Whitehead was there as a graduate fellow; he took a Princeton Ph.D. along about 1931, about a year before I got my Ph. D.

Speed: He'd come from Cambridge?

Tucker: Yes. He was working very closely with Veblen. Veblen had spent a year, around 1927-28, in England at Oxford. He had traded positions with Hardy. This was when Hardy was still at Oxford. Veblen was very much impressed by Oxford and Cambridge, and he really tried when he came back to Princeton to copy in many ways what he had seen at Oxford. Particularly the idea of having afternoon tea at which everybody gets together. This was quite new in America, but succeeded so well at Fine Hall, which was built just shortly after that and had a room in it just for this purpose, that it has been copied very widely in other mathematics buildings throughout the United States. It was started by Veblen.

We had a little verse that we used to jokingly say about Veblen:

> "Here's to Cousin Oswald V.,
> Lover of England and her tea,
> He is that mathematician of note,
> Who uses four buttons to fasten his coat."

He was very tall, particularly tall from the shoulders to the thighs, and he always wore a coat that had four buttons down the front, not just three, and these were always fully buttoned.

Veblen was trying very hard to come up with something which as of 1930 would do for geometry what the Erlangen Programme of Felix Klein had done around 1870. He was trying to define geometry in such a way that you would have differential geometry and topology included in a meaningful way. Projective geometry, non-Euclidean geometry, and so on are very well characterized by the group point of view. But to define topology as the study of the group of all homeomorphisms of a space onto itself is completely unsatisfactory. Nor is the group point of view satisfactory for differential geometry. Well, after putting a great deal of effort into this, Veblen finally came to the conclusion that any definition of geometry that would include all of the things that he wanted to have included would also include all of mathematics. I was right there to hear this conclusion when Veblen reached it and heard him tell the various things that he had tried and why they had failed. This has actually had a profound influence on my attitude towards mathematical education.

Now there are some parts of mathematics, algebra for example, that you can define in terms of the subject matter, the content. But geometry I feel you cannot. Geometry is a point of view. Even a somewhat emotional point of view. I can tell people how intensely I love geometry, but I can't tell them what geometry is in any way except simply by samples. Indeed Veblen somewhat jokingly proposed the following as a definition of geometry: something should be regarded as a part of geometry if at a given time there were people of taste who said that it was part of geometry. Well, I didn't ever work with Veblen in the way I did with Eisenhart and later with Lefschetz, but I was certainly very much influenced by him.

During the summer between my first and second years at Princeton, this would have been the summer of 1930, I was given by Lefschetz to take home to Canada some chapters of the manuscript of his book. I was to criticize these and find any slips that there were. But he didn't really define what he meant by criticism, so I really let myself go. In the fall I came back with a proposal for him to rewrite the book. I felt that he was changing his standpoint between the combinatorial view, where he was thinking of a manifold as made up out of cells, and the point-set view, where he was regarding a manifold as defined by point-set properties. He was using whichever of these points of view suited him at a particular point in the book. Well, my orderly attitude was that he should carry the purely combinatorial as far as he could and only bring in the point-set, which he needed for the final nature of a manifold, when he had done everything that he could do with the combinatorial.

Well, at first he just laughed, scoffed at my suggestion. But I persisted in arguing this, so he proposed that I should give a seminar in which I would present the way I thought the book should be written. He attended this seminar. Two of my close friends were R.J. Walker, later at Cornell and who wrote a book on algebraic geometry, and Nathan Jacobson, the algebraist who ended up at Yale University. These and some others as well as Lefschetz attended this seminar that I gave. At the start I had the plan to go as far as possible with the combinatorial. I had to implement that plan as I went along. Lefschetz was a critic. He never had the least bit of hesitation, right in the midst of anybody speaking, right in the midst of a sentence, of breaking in and, as we called it, heckling. So between Lefschetz heckling and my fellow students' criticism, I gon a good working over, which helped a great deal to sharpen things and push me on.

Well, that turned out to be my thesis. It was published in the
in 1933 under the title "An Abstract Approach to Manifolds." And in the end Lefschetz became very enthusiastic about it. Years afterwards he had a standard answer to people who would come to him with some combinatorial idea. He would say, "Have you looked in Tucker's thesis? It's probably in his thesis." So I got my Ph. D. in 1932.

Speed: So you were working on preliminary work over the summer?

Tucker: The summer of ’30 and then throughout the next two academic years.

Speed: So two years on it.

Tucker: Yes.

Speed: And how did you find that period? Was it all straightforward, plain sailing?

Tucker: It was all plain sailing, but of course I didn't realize that this was going to be my thesis. It was like the thing with Eisenhart. I was doing it mainly to make my point with Lefschetz that one could do the combinatorial first and only then bring in the point-set. So in a sense I was doing the thing not so much as research as exposition. It was research only in the sense that it was something that had never been done before. But as far as I was concerned the efforts were largely expository efforts of putting things in the proper order and in making sure that there were no gaps in it. But during that period I was never held up by anything. It just moved right forward.

Speed: Has this been a common characteristic of your research?

Tucker: It has. Indeed, in retrospect I would say that most of the things of mine that have been published that would be called research have been done with the aim of trying to simplify and unify. There has been almost nothing of my work where a certain problem was presented and I set out to find a way of solving that problem. Instead in most cases it was creating a certain structure, and the structure just created itself if you pushed ahead and used good sense and judgment.

Speed: Yes, I noticed the similarity between what you were saying about this work and the story about your involvement in linear programming.

Tucker: Yes.

Speed: So when you actually are confronted with a clear-cut problem, say that somebody just presented to you, what do you think about this? Are you attracted to that sort of thing?

Tucker: No. The one exception that I can think of is my combinatorial lemma for the n-cube. When I was a graduate student everybody was talking about [E.] Sperner's lemma for the n-simplex. This had been published in 1928, and I began my graduate work in 1929, so this was very fresh and very exciting. Well, in 1933 [Karol] Borsuk published a paper in English translation, "Three Theorems About the Sphere". These were antipodal-point theorems.

Well, shortly after this paper was published Lefschetz drew it to my attention and remarked that it should be possible to prove these theorems of Borsuk by Sperner's lemma. He remarked, "You're good at combinatorial things, Tucker. Why don't you try to get an elementary proof of the antipodal-point theorems?" Well, this attracted me. I worked at this on and off for quite a few years. I couldn't get anywhere with it, so I would drop it and then a few months later come back to it.

During World War II, I had occasion to do a fair amount of traveling related to the war research in which I was engaged, and so I was often many hours in an airport between planes. This was all in the United States. I was not involved overseas in any way. And I would busy myself during these long waits by thinking about mathematics. And I thought about this Sperner's lemma and antipodal-point business, and in a flash the idea occurred to me that should have occurred to me years before. There is no chance of using Sperner's lemma because the n-simplex doesn't have antipodal symmetry. You do not have a vertex opposite a vertex, you have a face opposite a vertex. So if you are going to use simplicial methods to do antipodal-point theorems you need to work with something like the octahedron which has antipodal symmetry.

Well, once this very simple idea occurred to me it didn't take me long to devise a lemma like the Sperner lemma for the octahedron that would prove the Borsu k theorems. This work did not get published in the ordinary way, but is published in Lefschetz' Introduction to Topology. There is a certain part of that book, about 10 pages, devoted to antipodal point theorems, and he says right at the beginning that this material was communicated to him by A.W. Tucker. The earlier part of the chapter deals with Sperner's lemma and things like the Brouwer fixed-point theorem that you can prove by means of Sperner's lemma. The end of the chapter deals with my octahedral lemma and proving the antipodal point theorem.

Incidentally I was supposed to have been his co-author in that book, but when we tried to work together, we just couldn't. I wanted to do everything in an orderly fashion, and he just didn't have the patience to do this. So we agreed to part company. But he did include this stuff of mine.

Speed: Is this his Princeton volume, or is it the colloquium volume?

Tucker: This is his Princeton volume called *Introduction to Topology*.

I mention this as one instance of where I had a specific goal and didn't know how to accomplish it and spent many years of fruitless effort before I finally saw the clue to the thing. A very, very simple clue. Once I thought of that then everything just went quickly. Later in a paper published in the Proceedings of the First Canadian Mathematical Congress I developed another lemma, for the square in this case, or in general for the n-cube, which I will be talking about because I feel that it is much more promising for computational purposes than the octahedron, because a cube is so much more regular thing to apply to analytical matters than the octahedron.

Academic Life at Princeton in the 1930s

Speed: Well, we've got roughly speaking to the end of your PhD, in the early ’30s, but we haven't touched a topic of some interest to me, namely Princeton in the ’30s.

Tucker: Well, in the fall of 1931 we moved into Fine Hall. It was built as a memorial to Henry Burchard Fine, who was the chairman of the mathematics department more or less from around 1900 until he died in 1928. Veblen was the one who took the lead in planning Fine Hall and in getting some wealthy alumni by the name of Jones, who had been students at Princeton at about the same time that Fine had been a student, to contribute. They had known Fine over the years, and they were persuaded to put up half a million dollars to build and endow Fine Hall.

It was built on a very lavish scale, at least for the United States. The principal offices, or studies as they were called, were paneled with oak that was brought from England, and they had fireplaces in them. This showed the effect of the year that Veblen had spent at Oxford in exchange with Hardy.

Hardy came to Princeton for a year, and Veblen went to Oxford. Indeed Veblen was away at Oxford at the time that Dean Fine died. As soon as he returned to Princeton he set about planning a memorial for Fine in the nature of a mathematics building and finding the funds for this. It was built to be about 50% bigger than seemed to be needed at the time. We moved in in the fall of 1931; this was the start of my final year as a graduate student.

I had the fellowship that year which was regarded as the plum, the Procter Fellowship. I was called on by Veblen to organize the tea club. The building had a very nice lounge, or common room as we called it, and the plan was that that would be the focal point in the building. It proved to be exactly that. There was no morning tea (as is the Australian practice), but there was afternoon tea and coffee, usually around 4:00. The staff and graduate students gathered there for tea and to chat. Strangely enough, there we usually stood around as we were drinking our tea, not sitting as you do here. There is a certain advantage in sitting, but at the same time when you're standing you can move around. You’re not anchored to one place as much. So if you hear snatches of a conversation over here that you want to participate in, you go over and join that group.

There were only about 25 graduate students in mathematics at the time that I went there. There would be about eight or ten new students coming in each year. Princeton had not yet, at the time I went there, started to produce PhDs. There had been some, but these had been in a sort of hit and miss variety. But from the time that we went into the new Fine Hall there was a steady production of PhDs. The year I got my PhD there were, I think, three PhDs awarded, one of whom was Banesh Hoffmann, the mathematical physicist.

That extra space that had been planned for in Fine Hall was immediately put to use, because in 1933-34 the Institute for Advanced Study began. It had no buildings, so the Institute simply rented space in Fine Hall for the School of Mathematics. So Fine Hall from ’33 to ’39 was the place where not only the University mathematicians were, but also those of the Institute for Advanced Study. There was no attempt to separate these groups. Indeed, no one paid very much attention to the question of "Am I paid by the University or am I paid by the Institute?" It was all one mathematical group. It is because of this that it became customary in Princeton, and also in other places, to refer to the "Fine Hall group" rather than Princeton University or the Institute for Advanced Study. So Fine Hall became a name for that group working there, and in the Institute this included Veblen and Alexander and von Neumann, who all had been previously at Princeton University. Indeed they continued to occupy the same offices that they had occupied, but they were paid by the Institute rather than by Princeton.

Speed: Could you speak a little about how the Institute came into being? The reasons behind that.

Tucker: The Institute was the conception of its first director, who was Abraham Flexner. Abraham Flexner and his brother Simon Flexner had been very prominent in criticizing the higher education in the United States for being too vocationally minded. With funds provided by the Rockefeller Foundation and by the Carnegie Foundation, both of them, I think, participated in making a study of medical education. They rated the various medical schools in terms of the criteria which they felt should be used. They concluded that there were only about five good medical schools in North America, and two of them were in Canada, McGill and the University of Toronto. In the United States they chose only Hopkins and Harvard Medical School and maybe that was all. I thought there were more.

Following this study on medical schools they did a study on graduate education, PhD education, and they were very harsh in their criticisms. Well, some wealthy people in Newark, New Jersey, by the name of Bamberger, who had a big department store there, were friendly with the Flexners, and when one of the Bambergers died he left a large sum of money to establish a school of advanced study. His deed of gift essentially said that Abraham Flexner was to be the director of this school, and that the school was to be set up as Flexner wanted to design it. It was to have its own board of trustees and be completely independent from any other educational institution.

As to the location of this, the Bamberger will said that it was to be located in New Jersey or in an adjacent area, which could have allowed it to be in New York City or in Philadelphia because these are both across rivers from New Jersey. But Flexner decided to locate it in Princeton because he wanted a small town atmosphere rather than a big city atmosphere. It was also necessary to locate it somewhere where there would be a good library already in existence. Over the years since then, the Institute has built up a library of its own, but it still depends on Princeton University library for general library purposes. It has its specialized library, probably as far as mathematics is concerned it's got all it needs. But if somebody wants to look up something in, say, geology, then they have to go over to the University libraries. So it was important to set it up somewhere where there was already a good library. Princeton was the natural choice for this.

Flexner's idea was that you should have an institution where there would be no degrees given, no examinations, that the people who came there to study were people who had already passed the student stage.

So anyone that comes there is supposed to have a PhD or the equivalent of it. Also there were to be professors, but the professors had no duties except to be scholars and to be residents and be available for properly qualified people to consult them.

The decision was made to start with a school of mathematics. I've heard the story—and it seems quite plausible—that, in the year before they were going to try to begin, Flexner traveled around the world, and everywhere he went he asked people in various fields "Who are the leading people in the world in your field?" He found that he got the best general agreement in mathematics. There were other fields where there was almost complete disagreement, but in mathematics he got pretty good agreement. Also, of course, mathematics doesn't require very much investment in the way of laboratory and so on.

So he decided to start with mathematics. He knew Veblen, and undoubtedly Veblen had had some influence on his deciding to start with mathematics. He appointed Veblen as the first professor of mathematics and the first professor in the Institute for Advanced Study. He then asked Veblen to put together a group, of course consulting with Flexner all of the time. Veblen started with von Neumann, who was an obvious choice I think. He chose Alexander, who was not an obvious choice, but Alexander had been a protege of Veblen. Alexander is a topologist, who worked in knot theory and such. A very able person, but at the same time such a perfectionist that he published very little, and students found it very hard to consult with him because of his perfectionism. Then Hermann Weyl, Albert Einstein,...

Speed: Gödel.

Tucker: Gödel, and then Morse. Marston Morse came somewhat later, and Hassles Whitney still later. The original ones were Veblen, Alexander, von Neumann, Weyl, Einstein, and Gödel. Gödel did not hold a professorship until much later. He held just a so-called permanent membership.

Then people came for a year or two; sometimes the Institute provided the funds, sometimes they came on fellowships of various sorts from all over the world. Particularly in the very early days, due to the situation in Germany, many of the people at the Institute for Advanced Study, in mathematics at least, were refugees. And each one of the professors had an assistant of his choosing, and this assistant was reasonably well paid. So Hermann Weyl, for example, took Richard Brauer to be his assistant as a way of giving him a position while it would be possible to look around and find a position for him. After being Weyl's assistant for about two years, he went to the University of Toronto. Indeed it happened that I served as the negotiator with the University of Toronto in bringing this about. Because Hermann Weyl and von Neumann were accustomed to giving lectures, they continued to give lectures, even though that was not part of their duties. Several of these courses of lectures at that time led to books of one sort or another.
Fine Hall was just a remarkable place to be. The various seminars that were going on, the courses, the informal atmosphere. It was really just a sort of a mathematical club. Because this was the Depression, the young people were usually trying to live quite frugally and would live in just a rented room in town and eat meals at an inexpensive restaurant. They were mainly single, so spent most of their time, except for eating and sleeping, in Fine Hall. There was the library on the third floor, the top floor, that was never locked. If you could get into the building, you could get into the library, and it was a very pleasant place to read and

study. Of course there were lots of offices, studies, in the building, and there was the common room.

You could go into the common room anytime between 9:00 in the morning until 3:00 the next morning, and there'd probably be somebody there, playing chess. Later on, Go took over, but in the beginning it was chess, particularly a form of chess called Kriegspiel. Or there would be bridge played there. I was a bachelor until 1938, and so during those years I spent most of my time at Fine Hall.

Speed: Did you find yourself picking up large amounts of mathematics in many areas? Was it difficult to remain interested or specialize in your own area? How did it affect the young person?

Tucker: Well, I felt that a large part of the learning that I did was from my fellow students. We would get together and discuss things. These things were referred to as "baby seminars", where there would be no member of the staff present. We were encouraged to have these. And I felt that a large part of what I learned was from these. The courses, particularly at the beginning, were important. My first year at Princeton I had courses from Eisenhart in Riemannian geometry and from Hille in analysis and from Lefschetz in topology, or analysis situs as it was still called at that time.

Speed: What about logic? Would you take the course by von Neumann on functional operators?

Tucker: No. I could have, but it was very clear that if you went to everything, you were going to have no time at all to think. If you went to everything, all you could do was be a piece of blotting paper. So at the beginning of the year I would shop around. This was the customary thing to do. Then after a week or two, I would single out three or four and concentrate on them. But of course some seminars I would attend also.

Veblen had a weekly seminar that was mainly in geometry, but it was a seminar that he could make whatever he wanted to. Then there was a topology seminar that was run jointly by Lefschetz and Alexander. My interest was in geometry; that's why I had chosen to go to Princeton. I was interested both in the differential geometry sort of thing with Eisenhart, and in the topological sort of thing with Lefschetz and Alexander. I had quite a bit of difficulty making up my mind, when it came time to write a thesis, as to whether I would do it in differential geometry or whether I would do it in topology. My first published paper, which I did with Eisenhart during the first year that I was at Princeton, largely by accident, was in differential geometry. My thesis was with Lefschetz in topology.

I took only enough of other things to pass the oral examination, which included real variable, complex variable, modern algebra, and two topics of the student's own choice, which for me were topology and Riemannian geometry. One thing that I do very much regret is that I never took Wedderburn's course, because he was in a sense the last of the classical algebraists. And yet it was things that he did that provided the groundwork for things in modern algebra. But I was repelled by his style of lecturing. He wrote everything out before he lectured and more or less memorized it, and then wrote it on the board. No questions allowed.

Speed: Did he have graduate students at that time?

Tucker: Oh yes.

Speed: So he was still an active researcher?

Tucker: Yes. A graduate student of his at that time and a very good friend of mine was Nathan Jacobson. Another friend of mine who did his PhD with Wedderburn is Merrill Flood. He hasn't gone on in algebra, though, the way Jacobson did.

Joining the Princeton Faculty in 1933

Speed: What happened to you after you finished your PhD? Was it soon afterwards that you were taken onto the faculty of Princeton University?

Tucker: I had gone to Princeton with the expectation that if I succeeded in getting a PhD there, I would return to the University of Toronto. When I saw that my PhD was certain, I wrote to the University of Toronto and was told, "There is a moratorium on any appointments, and there is a 10% cut in salary for the regular staff. Can't you find something to do for a year, or two?" So I applied and was awarded a postdoctoral fellowship of the National Research Council of the United States. I was away from Princeton, then, in the year '32- '33.

I spent the fall term at Cambridge, England. I wanted to attend the International Congress that was held at Zurich in August, 1932, so I combined going to the Congress with starting my fellowship at Cambridge, where my supervisor was M. H. A. Newman, then a fellow of John's College. Then I returned to America in December, to Harvard, where my supervisor was Marston Morse. I was at Harvard from December until June. Then because the fellowship was a 12-month fellowship I went to the" University of Chicago for the summer quarter. The University of Chicago at that time was the only place where there was mathematical work going on officially during the summer period.

At Harvard I helped Morse with certain topological tools that he needed for the book he was writing. He was writing a book called Calculus of Variations in the Large and so he really picked my brains, because he needed the sort of topology that was done at Princeton for the purposes of his book. Also at that time I wrote a paper in the calculus of variations. Initially I wrote it for him, to explain certain things that I thought would be useful to him. Then he proposed that I write it as a paper. Of course when I got to thé University of Chicago I was also there somewhat involved in the calculus of variations, because that was the big thing with [G. A.] Bliss and [L. M.] Graves and the people at the University of Chicago. So although my objective had been for that year to do certain things in topology, the only place that I had been able to do any of this was in working with Newman at Cambridge. Marston Morse was such a strong personality he immediately annexed me. I don't know that he ever asked me what I was interested in or wanted to do. He just essentially took me over.

Well, in the spring of 1933 I was offered an instructorship at Haryard which would have been for two years beginning in the fall of 1933. This so-called Peirce instructorship was arranged by Morse because he was anxious to keep me around there as a helper. So I wrote to Toronto again and said, "I have this offer at Harvard, but my aim is really to go back to Toronto. Do you want me?" And the word came back, "No, we have no opening for you, and Harvard's a pretty good place. You'll get some more seasoning." But I then consulted Lefschetz and Eisenhart at Princeton. And they said, "Well, why don't you come back to Princeton?" So I said fine, and I went back. They sort of matched the Harvard offer of an instructorship at Princeton, and I much preferred Princeton to Harvard because Harvard had nothing like Fine Hall.

At Harvard there was no place where mathematicians got together, except in formal courses and colloquium meetings and so on. There was no informal life there, and the year I was away from Princeton I missed the informal life at Fine Hall. So when I had a chance to go back there, I jumped at it. The following year, after I had returned to Princeton, I got an offer from Yale. This was arranged by Hille, who had moved from Princeton to Yale. He felt that Yale ought to have something in the way of topology, so he persuaded them to make me an offer. I went there and was interviewed by Oystein Ore and gave a talk and so on, and they offered me an assistant professorship. I went back to Princeton, and Princeton met the offer. So I stayed only one year as an instructor, and then I moved up to assistant professor.

What had made positions possible at Princeton was the Institute for Advanced Study. Veblen, Alexander, and von Neumann were all taken off the Princeton payroll, and they were getting top salaries. The two principal people left in the department, Eisenhart and Lefschetz, decided that the thing to do was to bring in young mathematicians. So over a period of about a year there were five people who were appointed there as assistant professors in mathematics: Bohnenblust, Bochner, McShane, Wilks, and myself.

It was a very difficult time to get a job, and I had been very lucky to get these offers from Harvard and from Yale. This was largely because I was in the field of topology, which was beginning to be recognized. Harvard, because of Morse, and Yale, because of Hille, recognized this; they arranged these, but they were only temporary positions, no guarantee of them continuing beyond the initial appointment. But because of the Fine Hall atmosphere I chose the Princeton opportunity over both Harvard and Yale, even though people pointed out to me that I wouldn't have so much competition in these other places as I would at Princeton. Nevertheless, I didn't think of it from that point of view, because I still thought that I was just putting in time until I could go back to Canada. So the question of whether I had a better long-range future at Harvard or Yale or Princeton just wasn't an issue with me.

It was 1938 when I had my first real offer from Canada. Oh, at the University of Toronto by this time they had sort of said, "Well, now if you insist on us finding you a position, we will." But they didn't make it sound as though they really wanted to have me. It was sort of, "Well, if you've got nothing else, we'll find something for you." The first really enthusiastic offer I got was from the University of British Columbia in 1938. And I went there, I flew there—it was the first time I ever flew in my life—and spent a couple of days seeing the place and being interviewed. I was very favorably attracted to it and came back to Princeton intending to accept the appointment. But then Eisenhart and Lefschetz went to work. They said, "You have your roots here now, and you've become a key member of the department. We want you to stay, and we'll be very hurt if you leave." So I stayed.

At that point I was given a so-called tenured appointment as associate professor. I realized then that I was not going back to Canada. But until that point I had always thought that ultimately I was going back to Canada. So at that point I took out my first citizenship papers and set about becoming an American citizen. That was the story.

Part II

The Late 1930s and Early 1940s

In the late ’30s I was working in combinatorial topology with not a great deal of results to show. I guess I was really more interested in my teaching. I had an opportunity to teach an undergraduate course in topology, combinatorial topology, that is, classification of 2-dimensional surfaces and that sort of thing. I also had an opportunity to develop a course in transformational geometry. At the graduate level, I was attempting from time to time a course on n-dimensional manifolds.

In one graduate course that I gave around 1938, I had a very sharp critic in the audience, John W. Tukey. Every time I came up with a definition of a combinatorial manifold, Tukey would come up with a counterexample. The course ended in a draw. He was a graduate student at that time.

In 1940-41 I had my first sabbatical leave of absence. This was spent during the fall at Northwestern University and in the spring at CalTech. I was trying to write a book on combinatorial topology to go with the undergraduate course that I had been teaching at Princeton. But I felt that I didn't know enough about the beginnings of topology, and I did want to try to make this projected book take account of the early history of the subject. At CalTech, I had the good fortune to have much contact with Eric Temple Bell. He knew a great deal about the history of mathematics, though more from the algebraic side because that was his particular interest. Indeed he told me quite early in our discussions that he really had no competence in topology. Nevertheless, he was able to suggest source references for me to look at that I had not encountered.

So instead of writing the book that I had planned, I became a student of the history of topology. In the course of this, I discovered that mathematical physicists in the second half of the 19th century had used topology as rather an intuitive way to deal with questions of field theory and especially of fluid flow. I read for the first time the first few chapters of Maxwell's Electricity and Magnetism and found that a large part of the first chapter of Maxwell is topological, dealing with questions of circulation, vortices, and such.

This led me to realize that some of the mathematical physics that made use of topology had a bearing on some recent work of W. V. D. Hodge on harmonic integrals. And I followed this up and wrote a paper on, I've forgotten the exact title, but it had to do with boundary-value problems for general manifolds. I sent this paper off to Lefschetz asking him to submit it to the Proceedings of the National Academy of Sciences. I assumed that that had been done, but when I returned to Princeton in September 1941 from the year's leave of absence I discovered that Lefschetz had not submitted the paper to the Proceedings of the National Academy of Sciences because he was having a fight at that time with the editor of the Proceedings. Instead, he had submitted the paper to the Annals of Mathematics of which he was editor. I was very upset because the paper did not have complete details in it. It was merely an outline, a projection of what I intended to do, and it seemed to me that that was not an appropriate paper to be published in the Annals of Mathematics. So I withdrew the paper, and the paper has never been published.

Much later on I showed it to Don [D. C.] Spencer, and he and a student of his by the name of [George] Dull made use of my results in a much better form that was then available. So my leave of absence in ’40-'41 taught me a great deal about the history of topology and also might have led me into profitable research on harmonic functions on manifolds.

National Defense Work: 1941 - 1944

I had been back in Princeton only a week or two when my old friend Merrill Flood came to see me and asked me to join him in a project concerned with national defense. This became known as the Princeton Fire Control Research Project, where fire control refers to gunfire controlled by range or height finders and later on, not in our work, by radar. I agreed to do this. I had some personal feelings at the time as an ex-Canadian, because Canada had been in the war since September 1939. I even wondered whether I should go to Canada and present myself as someone who had passed through the officer training program at the University of Toronto and actually had nominally a reserve commission in the Canadian army. The opportunity to, in some sense, become involved in what was to be the war as far as the United States was concerned was something that I welcomed.

So from September 1941 until about 1944 I worked for the Princeton Fire Control Research Project, for which I was the so-called Associate Director. Merrill Flood was the Director. I did this in addition to carrying the normal teaching load at the University. So there wasn't much opportunity to continue the work that I had started on harmonic integrals. During the war my teaching soon became involved with the Army and the Navy.

In 1943 the Army Specialized Training Program started at Princeton, and somewhat later the Naval College Training Program. I was in charge of the mathematics portion of the Army Specialized Training Program, and although I did not teach in the Naval Program, I had some administrative responsibility for that. The one somewhat unusual piece of teaching that I did was a mathematics refresher for Naval officers who were being trained in radar.

An amusing incident with this was that it was my job in this mathematics refresher course to explain the use of the log log deci -trig slide rule. One of the trainees to whom I was explaining the log log deci-trig slide rule was one of the three authors of the Keuffel and Esser manual on the slide rule, a man from the Naval Academy by the name of Bland. But I was able to show him a procedure for spherical triangles that he did not know!

In the Fire Control Research Project my duties were mainly administrative and editorial. The products of the project were reports, usually written to meet some need that had been put to us by the military. It was my job to edit these reports and make them readable for military officers. They often came to me in rather abstract technical and mathematical form, and it was my job to get these changed into a more readable form. But I did participate to some extent in the research and did quite a bit of traveling, because we had to keep in touch with work that was going on at Fort Monroe, Virginia, and later at Albuquerque, New Mexico, and at Colorado Springs.

I even made one or two inventions, very simple-minded ones. One of the objects, one of the instruments that we dealt with, was a photo-theodolite. This was used to check on the performance of height finders and range finders. The photo-theodolite for army purposes had its angles graduated in mils (800 mils 45 degrees). There was no problem at at I about elevation, but azimuth was a problem because in the heat of following an aerial target it often occurred that the photo-theodolite would be rotated more than one complete revolution about its horizontal axis and the counter that recorded the azimuth read up to 10,000 mils and then returned to zero.

Well, this caused a great deal of mathematical confusion when the data were analyzed. So I made the suggestion that the two lowest counters be decimal and the two higher counters be octal. This had the effect of counting up to 6400 mils and then returning to zero. But 6400 mils is one complete revolution. This had an unexpected bonus that it really was counting by octants of the circle, and for trigonometric purposes all you need to know are the trigonometric functions for one octant and then everything else is a simple transformation. Therefore it turned out that on this counter the first digit at the left told you what octant you were in and the remaining three digits told you the reading in that octant. This very simple idea probably cut the computational work almost in half. I had occasion in 1956, when I was in Australia, to visit the data center for the Woomera rocket range and discovered that they were using photo-theodolites with the counters that I had devised. When I started asking some questions about these counters, they suddenly realized that I knew much more than a casual visitor, and because I had not established clearance for this visit I had to leave very quickly.

Another anecdote from my Fire Control Research days involved some work that had been done by George W. Brown, one of the young statisticians working on the project. The military doctrine, it was actually Naval doctrine, was that in long-range artillery fire the aim of the first two rounds fired was to establish a bracket. You were usually able to determine very accurately the direction in which the gun was to be fired, but the elevation of the gun which was related to the distance to the target was much more a matter of guesswork. So the doctrine was to shoot in such a way as to create a bracket on the first two rounds and then to proceed by repeated bisection of the bracket that was obtained on the first two rounds. By an empirical trial-and-error study George Brown was able to show that the optimal doctrine is to fire in such a way that you have a 50% chance of a bracket on the first two rounds. Then you continue with that until you have established a bracket. Then you do the bisection process. His calculation showed that in a naval engagement the new procedure would save about one shot after five rounds, that with five rounds you would be as close to your target as with six rounds by the old doctrine.

Well, the chief of Bu Ord (Bureau of Ordinance) in Washington saw a copy of Brown's report, and he immediately summoned someone from the project to go to Washington. We got the word, and we were to be in Washington the next day. Flood and I went. The admiral and his aides cross questioned us on the report, and we explained it. But then the chief of Bu Ord said "But have you tried this out in the field?" We explained that we didn't have the facilities at our disposal for trying this out in the field! Apparently though, it was tried out because the doctrine was changed to the 50% chance of establishing a bracket on the first two rounds.

The Fire Control Research Project ended before the end of the war largely because radar had come in and displaced optical range finders. I then served as an assistant to S. S. Wilks in the statistical projects that he was supervising, partly at Princeton and partly at Columbia. In particular, I served as his deputy in dealing with a very small project at Columbia that included just two people, John Williams and Frederick Mosteller. I also served for a few months as a member of the von Neumann project at the Institute for Advanced Study, which was concerned with methods that might be useful for the high-speed computer that von Neumann was starting to develop. In this project I was working with Valentine Bargmann and Deane Montgomery.

The Post-War Years: 1946 - 1949

In 1946, when regular university work was again going full steam, I returned to doing only my Princeton University work. I was having some difficulty resuming the topological investigations which I had followed before the war. So in 1948, when I had the opportunity to become involved in some other research that seemed interesting, I took it. This occurred in a rather fortuitous way. George Dantzig, who then was working for the Air Force at the Pentagon as a statistician, came to Princeton to see John von Neumann. He had actually visited John von Neumann in November 1947 to tell von Neumann about the simplex method and what it was good for. On that occasion von Neumann had foreseen the duality that is now such a familiar feature of linear programming. With von Neumann's encouragement, Dantzig had made arrangements to get the Air Force to fund a university-based project to deal with the mathematics of linear programming and related topics.

Dantzig came again in the spring of 1948 to see von Neumann, and on that occasion I met him just by accident. He told me why he had come to Princeton, that he was seeking from von Neumann suggestions as to at what university such a project could be set up, who would direct it, and how the task of that project should be stated. He got general encouragement on all of these points, but no specific suggestions. So I asked what linear programming was, and he gave me a five-minute introduction to linear programming in terms of the transportation problem. Well, the network aspect of the transportation problem caught my interest because it seemed to have some connections with the combinatorial topology of one-dimensional complexes, electrical networks, Kirchhoff's laws, and things like this that I had played around with in the late 1930s. I said that it seemed to me that there would be connections with some combinatorial topology of graphs.

Well, it was this rash remark of mine that led to a project being set up at Princeton University with me as the director. Work started in the summer of 1948. Oddly enough, the project got set up under the Office of Naval Research, partly because the Air Force at that time had no research office and also because the Office of Naval Research already had a project at Princeton under the direction of Solomon Lefschetz. It seemed the easiest way to get started quickly to add this project that I directed as a sub-project to the one that Lefschetz already had with the Office of Naval Research.

I got two graduate students to work with me in the summer of 1948. They had just completed one year of graduate study at Princeton. One was David Gale and the other Harold Kuhn. We were trying to find initially as precise a relation as we could between a matrix game and linear programs. To put it another way, we were trying to see what the connection was between linear programming and matrix games. Von Neumann had seen almost immediately when Dantzig told him about linear programming in November 1947 that a linear program resembled the problem faced by one of the two players in a matrix game. It was because of that that von Neumann foretold the duality of linear programming. By the end of the summer we had established pretty sharp connections between linear programs and matrix games and had spelled out the duality, that linear programs come in pairs, with each maximization program there was a companion minimization program.

From that time on my own mathematical work has been largely in linear programming and related matters. David Gale did his dissertation with me in 1948-49 in linear programming and game theory. Others who were working with me as graduate students at that time were [Lloyd] Shapley, [John] Nash, [Donald] Gillies, and [Jim] Mayberry. In 1949 there was a conference at the University of Chicago arranged by Tjalling Koopmans. This is now regarded as the Zeroth International Symposium on Mathematical Programming. There was a very good attendance at that conference.

Digression: Editing The Annals of Mathematics

I want to regress. I want to go back to the 1930s when I became involved in mathematical publication. I served as an assistant to Solomon Lefschetz in the editing of the Annals of Mathematics. My job was to get manuscripts refereed. My colleague, Bohnenblust, had the job of taking manuscripts that were accepted for publication and seeing them through the printing process. I did this for several years, but at the same time I was put in charge of the mimeographing of mathematical notes.

This was in the period when at Fine Hall we had both the University's department of mathematics and the School of Mathematics of the Institute for Advanced Study. The early professors at the Institute for Advanced Study gave lectures even though there was no requirement in their positions that they give lectures. But von Neumann and Weyl and Morse and the others had been accustomed to giving courses of lectures, and they continued to do so. It was during the Depression, and funds became available through a section of the WPA to pay for odd jobs. One of these was the production of mimeographed material generated by the courses given by the professors at the Institute and at the University.

It was my job to supervise this, and it unexpectedly became a thriving business. People elsewhere heard about the lecture notes and wrote in and asked to get copies. We often had to rerun lecture notes several times. We saved the stencils, so rerunning them was a fairly inexpensive business. But we finally reached the stage that it was too much to do in the amateur way that we were doing it. The mimeographing machine was run by students hired by WPA funds, and the collating was usually done by graduate students for free. Then the notes had to be bound, and they had to be sent to the people who ordered them. It reached a stage where one of the secretaries was spending most of her time taking care of the Princeton Mathematical Notes.

So I sought another means of production. I found that Edwards Brothers in Ann Arbor, Michigan were lithoprinting such material. So we arranged with Edwards Brothers to get the notes lithoprinted. They were typed in Princeton in more or less the same fashion, except they weren't typed on mimeograph stencils. They were typed on master paper and then sent to Ann Arbor, and the finished copies were returned to us. Well, the company Edwards Brothers was actually willing to do the distribution for a 25% commission, but it seemed to me that it would be better if the Princeton University Press would do the distribution. So I approached the Princeton University Press and got the commitment from the Press that anything that Edwards Brothers could do, the Princeton University Press could do. The lithoprinting was still done by Edwards Brothers in Ann Arbor, because there were very few lithoprinting companies in those days, but when the copies were printed they were shipped to the Princeton University Press, which took care of the mail order of copies and the filling of those orders. Well, this was the beginning of the very successful enterprise The Annals of Mathematics Studies.

The first of the Annals Studies was one by Hermann Weyl on the algebraic theory of numbers. The Annals Studies was started in a rather strange way. At that time the Annals of Mathematics had a surplus of papers, and the editors felt that they were plagued especially by long papers, papers of a hundred pages or so. At that time the Annals had a total page count for the year of perhaps 700 or 800 pages, and so two or three 100-page papers took up almost half of a year's production. So it was decided, largely by Lefschetz, that the formalizing of the Princeton Mathematical Notes could be combined with a means of publishing long papers or perhaps monographs consisting of several papers on a single topic. And this was the reason for the name Annals of Mathematics Studies, to enable the editors of the Annals of Mathematics to transfer long papers or groups of papers to the Studies. That's the reason for the title.

Although it would have been most natural for me to have been named the editor of the Studies, Lefschetz felt that I was too young and not sufficiently well known to have the clout that was necessary to be the editor, so the idea was that the editors of the Annals of Mathematics were also the editors of the Annals of Mathematics Studies. At that time the editors of the Annals were Lefschetz, von Neumann, and Bohnenblust. Thus Annals of Mathematics Studies was started.

This was 1940, if I remember correctly, and this was the first series of mathematical publications in the United States that could publish some esoteric work that no commercial publisher would touch. In those days, commercial publishers published practically nothing in the United States of an advanced nature in mathematics. There were some publications, such as the Colloquium Series of the American Mathematical Society and some other volumes that were subsidized by the National Research Council. I knew the Cambridge Tracts, and in my own mind I thought of the Annals Studies as an analog, an American analog of the Cambridge Tracts. Of course the Cambridge Tracts were printed in letterpress, the Annals Studies were lithoprinted from typescript. But it was this use of typescript composition that made the Annals Studies economically possible.

It was touch and go at the beginning. We had a kitty of about $1000 from the surplus from the mimeographed notes, and with that $1000 the Annals Studies was launched. I did the work of getting manuscripts. First of all, of seeing to the decision of which manuscripts would be accepted. At the beginning, most of them came from the Princeton area. Then of getting them typed on the master copy paper and sent off to Edwards Brothers. I prepared all the material for the cover, decided on the price that should be charged in order that we would recover the typing, pagination, and other costs, and even handled the advertising of the Studies.

The whole thing involved a great deal of detailed work, such as experimenting with the best typewriter to use. We tried with one of the early Studies doing the thing by an old variable typewriter called the Varityper. This was the Study written by Tukey. That turned out badly because the Varityper was so slow; it took a great deal of typing time to accomplish the result. We ended up using an IBM electric typewriter and putting in the special symbols, Greek letters and so on, by hand. We developed some templates that could be used for this purpose. The first Study that we felt was completely satisfactory was the one of [Paul] Halmos on finite-dimensional vector spaces. In that one we got very good cooperation from the author in the form in which the manuscript was submitted, and the results were very satisfactory, almost elegant, in appearance, yet there was a minimum of work beyond the typed composition.

In 1938 another book series began: the Princeton Mathematical Series, letterpress books. The way in which this series arose was that a colleague, E. U. Condon in mathematical physics at that time at Princeton, was the editor for Prentice Hall of an international series in physics. He came to me one day—my office was only about two doors away from his—and asked me how I would feel about undertaking, for Prentice Hall, a companion series to his in mathematics. I was taken completely by surprise, but I agreed to go with him to New York and meet the president of Prentice Hall to discuss this. When I got there I was lunched and everything was very fine, but there was a contract for me to sign. I said that I wanted to think that over and consult with my senior colleagues at Princeton. I came back and went to see the chairman of the department, Eisenhart. I also discussed it with Lefschetz. Eisenhart told me that he felt that if I was going to edit a series and Prentice Hall claimed that it was going to be advanced books, upper level undergraduate and graduate level—that really was the level of Condon's series—that I should edit such a series for the Princeton University Press instead. There were further discussions, and it was decided to have a series of advanced mathematical books published by the Princeton University Press.

Many years afterwards I learned that this had been a long-standing idea of Dean Eisenhart and that he took the opportunity of my invitation from Prentice Hall to try to bring matters to a head with the Princeton University Press, which had turned down the idea previously. With Prentice Hall as a competitor, the Princeton press agreed to the idea. There were all sorts of side conditions. It was a very complicated contract that was entered into between the Press and the editors of the series. The editors of the series were Marston Morse, H. P. Robertson, and A. W. Tucker. Again it was felt, especially by Lefschetz, that there needed to be senior people and better-known names involved in the editorial work. But as often happens, the editor junior in age does the work. The Princeton Mathematical Series started also with the first volume by Hermann Weyl on the classical groups. Both series, the Annals Studies and the Princeton Mathematical Series, did very well.

The timing was fortuitous. We got ourselves going a little bit before World War II, and we kept going during World War II, so that after the war when there was a general educational expansion after the hiatus, the Annals Studies and the Princeton Mathematical Series were there for the whole world to use. The Princeton University Press took complete responsibility, except for editorial details, for the Princeton Mathematical Series, but with the Annals Studies the Press regarded itself merely as a distributor. Finally, about 1947 I tried to force a showdown with the Princeton University Press by refusing to do anything more myself with the Annals of Mathematics Studies. This caused some hardship for authors who had been hoping to have manuscripts published by the Annals of Mathematics Studies.

Indeed, one of them, Aurel Wintner of Johns Hopkins University, threatened to sue me and the Princeton University Press for not going ahead with the publication of a manuscript of his. In the end the Princeton University Press capitulated and agreed to take the same full responsibility for the Annals of Mathematics Studies that the Press took for the Princeton Mathematical Series. I feel a very strong interest in both of these series, but I must say that my favorite of the two is the Annals Studies: because it, at the time it was started, was quite unique. It was really the only means in the United States for the publication of long manuscripts which did not have a sufficient audience to justify commercial publication. In more recent years the commercial publishers have fallen over one another to publish such books, but at the time the Annals Studies was started there were no takers.

Sabbatical at Stanford: 1949 - 1950

Let me return to the story of my own research. I had broken this story off at the time in 1948 that, with Kuhn and Gale, I had started on linear programming and related topics. In 1949-50 I had my second sabbatical leave, which I spent at Stanford University. It was there that the paper on nonlinear programming, jointly with Kuhn, was initiated. It was also during that year at Stanford that I invented the "prisoner’s dilemma" as a cover story for a two-person non-zero-sum game in which the dichotomy between cooperative games and noncooperative games was made simply and sharply. And during that year I became involved as a consultant to the Rand Corporation.

This involvement was an accident in a way. Merrill Flood, who had become a project officer at the Rand Corporation, decided to have a workshop on linear programming, more specifically the transportation Problem. He wrote to agencies in Washington, including the Office of Naval Research, as king that representatives be sent to this workshop.

I was being partly supported at Stanford by the Office of Naval Research, so one day I received a telephone call from Washington asking me to attend this workshop at the Rand Corporation. I got the phone call one day, and took the train the next day to go to Los Angeles. In all my long dealings with the Office of Naval Research that was the only occasion when I was asked to do something specifically for the Office of Naval Research, otherwise I was left completely to my own devices. The Air Force also sent a representative, Robert Dorfman (now an economist at Harvard). The two of us were the only participants in the workshop who were not Rand people. I’ve forgotten now how many weeks it lasted. I would go home weekends to Palo Alto, but it must have gone on four or five weeks. And this was very interesting in many respects because it was my first contact with applied linear programming.

The problem that Flood had decided to have the workshop study was the routing of the empty tankers of the U.S. Navy. This was a transportation problem somewhat like that studied by Tjalling Koopmans when he was with the War Shipping Board during World War II. Of course with the tankers the Navy had very complete information, so we could study the way in which the tankers had been routed in the last few years. Using linear programming we were able to come up with a considerable improvement. The optimum that we were able to suggest was something like 10% better than the empirical optimum that had been worked out by the Navy.

However, when we presented our optimum schedule to the Navy, it was rejected for a very good reason. The Navy tankers had home ports. And it was important to the morale of the crews that these ports should be visited at reasonable intervals. Families were at these ports. Now the Navy schedulers were well aware of this side condition, but the information that had been furnished to our workshop did not include it. This was my first experience with the failure of a mathematical model to take account of conditions that were very important, but which no one had expressed and put into the mathematical model.

In 1949-50 at Stanford University I had a very good opportunity there to think about linear programming and games in which I had become involved in 1948. I did teach two courses at

Stanford to fill a gap caused by the move of Donald Spencer from Stanford to Princeton. I taught a graduate course in topology and a graduate course in game theory.

Through the accident of having an office in the basement of the building occupied by the psychology department, I had an encounter with the chairman of the psychology department, Professor [Ernest] Hilgard, that led to me giving an elementary presentation of game theory to graduate students in psychology. I presented in this talk some simple examples of matrix games, but I didn't want to leave the impression that two-person zero-sum games were all there was to game theory. So I devised an example of a two-person non-zero-sum game for the purposes of this talk. To give this some psychological color I concocted the example that is now very well known as the "prisoner's dilemma".

It was just an incident in my stay at Stanford, but it probably is the thing that has aroused the greatest interest, except possibly for a paper on nonlinear programming which Kuhn and I presented at the Second Berkeley Symposium on Probability and Statistics organized by Jerzy Neyman in June 1950.

This paper on nonlinear programming came about because at Stanford, where I had some leisure to think about things, I asked myself why, when I first was introduced to linear programming by George Dantzig in 1948 by means of the transportation problem, did I say that I felt that there were connections between linear programming and electrical networks. When I looked into the literature, especially the work of Maxwell, I discovered that the electrical network problem, developed first by Kirchhoff and about 20 years later by Maxwell, could be regarded as minimizing a positive-definite quadratic function, the so-called heat loss, subject to the linear equations of conservation of flow. When you considered this quadratic problem of constrained optimization, you got as the necessary and sufficient conditions for the solution the two laws of Kirchhoff. This is what nowadays would be called a linear complementarity problem. So it wasn't linear programming that I was thinking about when I said there was a connection between the transportation problem and Kirchhoff's laws, it was quadratic programming.

So I started to write a paper on quadratic programming, but I remembered that in the summer of 1948 when Gale and Kuhn had first been working with me we had realized that a maximization problem of linear programming, if attempted by the traditional methods of Legrange multipliers, showed that the Lagrange multipliers were the dual variables. So I felt that I should get in touch with Gale and Kuhn and ask them if they wanted to participate in the writing of this quadratic programming paper. Gale declined. He said he'd had enough of that sort of stuff. (Of course he came back later to the programming field.) Kuhn accepted.

So by correspondence between Stanford and Princeton, where Kuhn was finishing up his Ph.D. in group theory with Ralph Fox, we wrote this paper. It started out in quadratic programming, but then we realized that in the minimization of a positive-definite quadratic form the important thing was the convexity of the function. So one thing led to another, and the paper when it finally was completed was called "Nonlinear Programming."

Perhaps it might more properly have been called "Convex Programming", but we just picked the name non linear. It was in this way that what is now referred to as Kuhn-Tucker theory came about. Of course, we now know that it should be called Karush-Kuhn-Tucker theory because Bill Karush had anticipated what we did in 1950 in his master's thesis at Chicago about 1940. But his work was done in the context of the calculus of variations where it didn't attract attention, and our work was done in the context of mathematical programming where it was viewed as the first breakthrough from linear programming.

Returning to Princeton: 1950 - 1963

When I returned to Princeton from my leave of absence in 1950, there was great interest in linear programming and the theory of games, and the project supported by the Office of Naval Research under my direction had a great deal of activity. Many graduate students were participating in the weekly seminar we had, there were visitors, conferences were arranged from time to time, and there was a series of Annals Studies called "Contributions to the Theory of Games". The first of these, I think, was published in 1951, and this proved so successful that a second one appeared, I think, about 1954. The first two contributions to the "Contributions to the Theory of Games" were edited jointly by Harold Kuhn and myself. In 1956, Kuhn and I edited a volume on linear inequalities and related systems, which had sufficient impact in the world that [L. V.] Kantorovich had that volume translated into Russian. This work on linear programming, linear inequalities, and game theory continued very actively at Princeton and still does. The Office of Naval Research stopped supporting the project about 1970, but after that the National Science Foundation picked up the project, and it is now directed by my colleague Harold Kuhn.

It is impossible to give, except in some written form, the list of all the distinguished people who participated in that project. In 1953, Lefschetz retired, and I was made chairman of the mathematics department. From 1953 until 1963, I had what seemed to me the very heavy administrative duties of chairman of the department. I continued with ordinary teaching, and with the work of the logistics project, as it was called, supported by the Office of Naval Research.

Summers from about 1954 on, I participated in the summer institutes that were started at about that time, supported by the National Science Foundation. These were institutes both for college teachers and secondary-school teachers of mathematics. Institutes in which I had a hand were at the University of North Carolina at Chapel Hill, the University of Oregon at Eugene, the University of Washington at Seattle, the Oklahoma State University at Stillwater, and the list goes on and on. In more recent years, the summer institutes in which I participated were mainly at Bowdoin College in Brunswick, Maine. There were even three summers when we had summer institutes at Princeton; the first one was organized by Sam Wilks, and the other two were organized by me.

Also, in 1953, I became chairman of the Commission on Mathematics set up by the College Entrance Examination Board to examine the mathematics curriculum in secondary schools on which the mathematics examinations of the College Board were based. The work of the Commission on Mathematics went on from 1953 until about 1959 when our report was finally published. In many respects, the Commission on Mathematics began the movement to what is called, I think unfortunately, the "new math". In 1958, the School Mathematics Study Group was set up under the directorship of E. G. Begle, and that much more extensive effort continued the work of the Commission on Mathematics of the College Entrance Examination Board.

The work of the logistics project went on all of this time. We had conferences from time to time, and publications were produced, mainly volumes in the Annals of Mathematics Studies. I was nominally in charge of these things, but the work was really done by some very able people who were working with me—such people as Jim Griesmer, Harlan Mills, Philip Wolfe, and others.

In 1960, I was asked by the nominating committee of the Mathematical Association of America to be the president of the Mathematical Association from 1961 until 1963. I was not particularly anxious to take on the additional administrative work, but I always had the feeling that one shouldn't duck a job and expect somebody else to do it, so I accepted. It turned out that this involved not only the presidency of the Mathematical Association of America but also involved me in an even more onerous responsibility, serving as chairman of the Conference Board of the Mathematical Sciences. The Conference Board had been started in the late '50s, and the time had come that the president of the Mathematical Association was asked to take a turn at being the chairman of the Conference Board. There was a crisis, and it even seemed as though the Conference Board was going to break up. It just seemed to have organizations involved in it that had such different mathematical aims. Of course, the American Mathematical Society felt that it was the mathematical organization, but against this, there were the claims of the Society for Industrial and Applied Mathematics, the Mathematical Association of America, the National Council of Teachers of Mathematics, the Association for Symbolic Logic, the Institute of Mathematical Statistics, not to mention the Operations Research Society, the Association for Computing Machinery, and the Econometric Society. You can see that it was a peculiar combination of organizations trying to find common ground and to find some way in which these organizations could support one another.

So, the two years from '61 to '63 were very difficult years for me, but not so much because I was president of the Mathematical Association of America. There, the very able work of the Secretary of the Association, Henry Alder, made things fairly straightforward. Also, the Mathematical Association hired a part-time secretary to help me take care of the correspondence and the files. I had no such assistance from the Conference Board, which had a very restricted budget. We finally did get Baley Price to act as the executive officer in Washington for the Conference Board, but throughout the two years, it was a constant struggle to hold things together and try to accomplish something.

In 1963, I was freed from the presidency of the Association—I continued for about six years as a member of the Board of Governors—and from the Conference Board. At the same time, I was freed of the chairmanship at Princeton. Not completely, though, because the new chairman in 1963 was Jack Milnor, and it didn't seem right to have such a brilliant research mathematician burdened with the day-to-day operations of the department. So, I continued as a co-chairman of the department with Milnor, and indeed later with Gilbert Hunt, the chairman who succeeded Jack Milnor.

Further Travels and Appointments

In 1954, I was appointed to the Albert Baldwin Dod Professorship of Mathematics. This chair was established in, I think, 1869, one of the oldest endowed chairs at Princeton, to honor a man who had been a professor of mathematics in the College of New Jersey, as Princeton University was then known. After Eisenhart (Dod Professor 1924-45) retired, perhaps a year later, Emil Artin was appointed the Dod Professor of Mathematics. But in 1953 when Lefschetz retired as the Fine Professor, the research professorship in mathematics, Artin was made the Fine Professor and the vacant Dod Professorship was given to me. My title now is the Albert Baldwin Dod Professor of Mathematics Emeritus.

I forgot to mention that I had my third leave of absence in 1958-59. This was spent mainly in Europe where I served as a visiting lecturer for a branch of the Organization for European Economic Cooperation, the European organization that was an outgrowth of the Marshall Plan. I gave lectures on the mathematics of operations research in Norway, Sweden, Denmark, and Belgium. This was a very pleasant experience, because I had an opportunity to meet some of the leading people in mathematical economics in these countries. One of these that I had considerable contact with in Oslo was Ragnar Frisch, one of the first winners of the Nobel Prize in Economics.

In the summer of 1956—that is the American summer—I was a Fulbright lecturer in Australia. This was arranged by my good friend Larry Blakers at the University of Western Australia, who had taken his Ph.D. at Princeton in the '40s with Lefschetz. He arranged things with the man in charge of the Fulbright program in Australia, who had been a classmate of his at the University of Western Australia. While it is not possible for the host country to specify the exact person that is to be awarded a Fulbright lectureship, it is possible to specify the set of individuals that are desired. And it is a mathematical trick that you can specify a single individual by specifying a set that consists of a single individual. They so spelled out the qualifications of the person that was desired, that he should be a topologist, that he should be interested in the theory of games and linear programming, that he should be active in the reform of the secondary-school curriculum, and so on, that there was really no one else eligible to apply for this lectureship. Of course, Blakers had found out in advance that I was willing to apply. I did apply and spent from May until September down under.

I managed to visit New Zealand for a couple of weeks on my way to Australia. In Australia, I lectured for three weeks at the University of Sydney, three weeks at the University of Tasmania, three weeks at the University of Western Australia, and the final period at the University of Melbourne, which just happened to be celebrating its 100th anniversary. I participated in that celebration as the representative of Princeton University, and at the same time, the inaugural meeting of the Australian Mathematical Society was held at Melbourne. So although it was just a three-month visit, I really became very well acquainted with the Australian mathematicians. Indeed, at the time that I left, it was remarked that I probably knew more Australian mathematicians than any Australian mathematician knew.

I was pretty much freed of administrative duties in 1963. I guess I should mention that in the fall of 1963, I was a visiting professor at Dartmouth College and had a very good time there. I had expected to spend the whole year on leave of absence, but there were unexpected administrative problems at Princeton so I had to go back to help Jack Milnor with the administration of the department at Princeton. But from 1963 onwards, I had the opportunity to devote myself wholeheartedly to teaching the things that I was interested in teaching. During the period that I had heavy administrative responsibilities, I had taught calculus to set an example, so to speak. Indeed, I had usually been in charge of the large freshman course in calculus, but I didn't really enjoy calculus. I was teaching it out of a sense of responsibility. But from 1963 on, I had an opportunity to teach mathematical programming, game theory, graph theory, and occasionally, a graduate course. I didn't teach a graduate course very often because I felt that there were so many members of the department who should have had an opportunity to teach a graduate course, and so I did this only occasionally.

I continued to teach the sophomore course in geometric concepts which I had started back around 1947 and had taught almost every year from then on. This was a general education course, or as it is called at Princeton, a distribution course, a course to satisfy distribution requirements. There were no prerequisites other than the mathematics required of all students entering Princeton. It was a course in which historical and philosophical aspects were emphasized. I developed the course and derived a great deal of pleasure from it.

In the early '60s, I became a consultant to a secondary-school education project at Columbia University directed by Howard Fehr. This went on for several summers, and I tried to exert a moderating influence, perhaps with not too great success. I did get some of the more concrete and combinatorial aspects of high-school mathematics, or what could be high school mathematics, brought into this overly ambitious program. I also participated in framing some of the geometry that went into it, a greater variety than would otherwise have been there.

In 1961, I was honored by Dartmouth College with an honorary Doctor of Science degree. This was in gratitude for the counsel that I had given to the administration at Dartmouth in trying to update, strengthen, and reinvigorate the department of mathematics. I was the one who brought Dartmouth in contact with John Kemeny. Another honor that I received in 1968 was the Distinguished Service Award of the Mathematical Association of America. While mentioning these things, I perhaps should also say that I was a member of the initial committee for the Sloan Fellowships. This was a committee of five scientists, two physicists, two chemists, and one mathematician, which set up the Sloan Fellowship Program in direct touch with Mr. Sloan. We selected the Sloan Fellows for the first three or four years of the program, and then rotation set in. Also, I was an initial member of the committee to select recipients, or at least to advise the president on the awards of the National Medal of Science. This was a presidential appointment by John F. Kennedy, and I served for about four years on this committee through the first term of President Lyndon Johnson.