The Princeton Mathematics Community in the 1930s
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ALBERT TUCKER

CAREER, PART 2

This is a continuation of the account of the career of Albert Tucker that was begun in the interview conducted by Terry Speed in September 1975. This recording was made in March 1977 by Evar Nering at his apartment in Scottsdale, Arizona.

Tucker: I have recently received the tapes that Speed made and find that these tapes carried my history up to approximately 1938. So the plan is to continue the history.

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 Cal Tech. 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 Cal Tech 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 in rather an intuitive way to deal with questions of field theory and especially of fluid flow. I read for the first time then 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] Duff 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.

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 the national defense. This became known as the Princeton Fire Control Research Project, where fire control refers to gun fire 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 had charge of the mathematics portion of the Army Specialized Training Program, and although I did no teaching 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 all 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 suddently 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 in 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.

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 '30s. 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 came 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 disseratation 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.

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I want to retrogress. 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 lithoprint 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 the commercial publishers—I'm talking about the late '30s—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 very well 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 letter press, the *Annals Studies* were lithoprinted from typescript. But it was this use of type-script 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 the all the material for the cover, decided on the price that should be charged in order that we would recover the typing, planographing, 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, letter-press 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 away from his—and asked me how I would feel about undertaking to edit, 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 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 discussed it also 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, upperlevel 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's 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 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 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.

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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, asking 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 I 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 transporation 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 nonlinear. 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 the linear programming.

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 people now distinguished 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 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 queer combination of organizations trying to find a 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.

In 1954 I was appointed to the Albert Baldwin Dod Professorship of Mathematics. This chair was set up 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 Euoropean organization that was an outgrowth of the Marshall Plan. I gave lectures on the mathematics of operations research in Norway and Sweden and 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 as a leave of absence, but there were unexpected adminstrative problems at Princeton so I had to go back to help Jack Milnor with the administration of the department at Princeton. But from 1963 on, I had the opportunity to devote myself in a whole-hearted way to teaching the things that I was interested in teaching. During the period that I had had heavy administrative responsibilities I had taught calculus to set an example, so to speak. Indeed, I had usually had 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 an occasional 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 an opportunity to teach a graduate course that 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. 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 got a great deal of pleasure from the course.

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 sides of high-school mathematics, or what could be high school mathematics, brought into this all-too-ambitious program. I also participated in the 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, to strengthen, to 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.