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An Interview With Albert W. Tucker

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A. W. Tucker

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An Interview with Albert W. Tucker

Stephen B. Maurer

The mathematical career of Albert W. Tucker, Professor Emeritus at Princeton University, spans more than 50 years. Best known today for his work in mathematical programming and the theory of games (e.g., the Kuhn–Tucker theorem, Tucker tableaux, and the Prisoner’s Dilemma), he was also in his earlier years prominent in topology. Outstanding teacher, administrator and leader, he has been President of the MAA, Chairman of the Princeton Mathematics Department, and course instructor, thesis advisor or general mentor to scores of active mathematicians. He is also known for his views on mathematics education and the proper interplay between teaching and research. Tucker took an active interest in this interview, helping with both the planning and the editing. The interviewer, Professor Maurer, received his Ph.D. under Tucker in 1972 and teaches at Swarthmore College.

A Career as an Actuary?

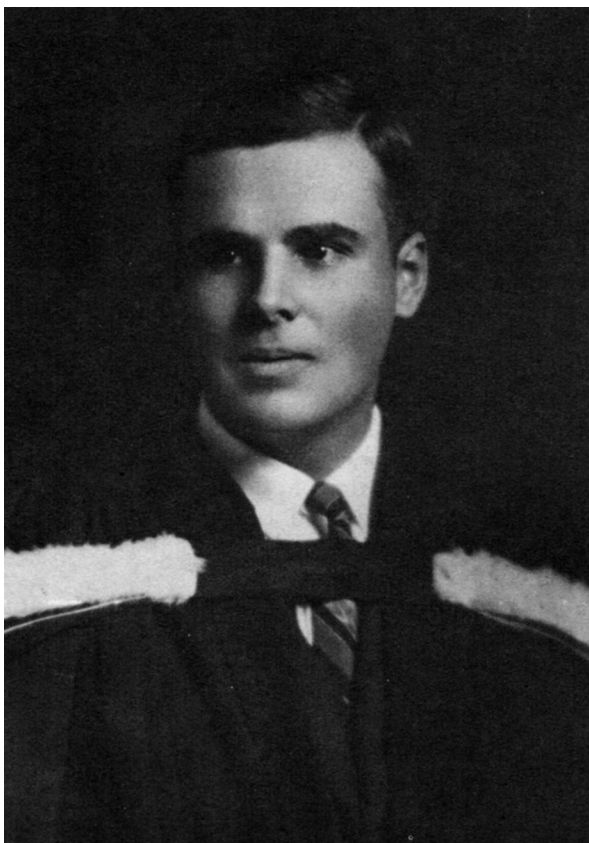
TYCMJ: *Al, let’s start at the beginning: tell us where you grew up and how you got interested in mathematics.*

Tucker: I grew up in Ontario, Canada, where I was born in 1905. My father was a Methodist minister. We lived in several small towns on the north shore of Lake Ontario. It was while I was going to high school that it first seemed I had talents in mathematics. It was a 3-teacher school, with the principal teaching science and mathematics. A few weeks into my Euclidean geometry course, the principal decided to give us a test. For his own convenience, he used part of a provincial examination. This contained both questions of knowledge and “originals”. He had not previously given us any originals and didn’t expect us to answer them. Well, I didn’t know this, so I answered the originals. That night the principal came to see my father and wanted to know if my father had been coaching me, because he knew my father had taught mathematics for a year or two. My father said no, he had not. Then the principal said: “I think your son must be a mathematical genius. I think he can have a very promising career as an actuary!”

From that time on my parents thought of me, their only child, as a budding mathematician. For myself, what I realized was that although I did well in all my subjects, I did well in mathematics without trying.

Mathematics or Physics?

TYCMJ: *You attended the University of Toronto. Tell us about that.*



A. W. Tucker in 1928 as a University of Toronto Graduate.

Tucker: I entered the University of Toronto in 1924. In those days, there was a Pass Course, which took 3 years, and Honors Courses, which took 4. I enrolled in the Honors Course in Mathematics and Physics. There were about 75 enrolled in this Honors Course in my year. Almost all our courses were in mathematics and physics. Other than that I had courses in chemistry and astronomy and 4 or 5 elective courses, including one in so-called "Religious Knowledge."

My own idea at that time, as far as I had a goal, was to become a high-school teacher of mathematics and physics. I knew very little about what an actuary was, but on the other hand I had had high school teachers I thought very highly of.

At the end of the first year I was first in my class. (I had also been first on the provincial scholarships examinations before entering, in mathematics and in Latin.) I didn't know it at the time, but a professor who was leaving to go back to his native Ireland, J. L. Synge, who had taught me a course in conic sections, left a note to the chairman of the department that there was a young man in the First Year by the name of Tucker who bore watching.

The chairman of physics was also watching. In my Second Year he taught me History of Physics. During the summer he had attended a conference in Italy where for the first time he heard about quantum mechanics. He had the fashion, a very good one which I followed later on, of having students report on various topics. He assigned me to report on quantum mechanics. At that time nothing was available except a few published papers. I read these. I don't think I understood any of them, but I put together some sort of report which greatly impressed him. He called me into his office and urged me to switch from the straddle between mathematics and physics to pure physics.

He and the chairman of the mathematics department communicated about me, though they were not on good speaking terms, and agreed that I should go on in one or the other but not in both.

I took their advice, but it was a very hard decision for me. I was more attracted to physics. It seemed to have much more glamor. These were the early days of relativity and quantum mechanics. But I found I was able to talk more satisfactorily with the professors of mathematics. I felt when I talked to them that I knew what we were talking about, whereas the physicists were always talking in terms of analogies. This was before physics had become mathematicized in the modern sense. If there had been real mathematical physics at Toronto in the modern sense, I probably would have opted for that.

So I chose mathematics. When I did this, I realized I was going somewhere other than into high-school teaching. There were jobs in the schools only for those with a joint specialty in mathematics and physics.

“Princeton Was the Place I Wanted to Go”

TYCMJ: *How did you decide on Princeton for graduate school?*

Tucker: Early in my Fourth Year, the mathematics chairman, Dean DeLury, called me in and told me I should be thinking about graduate study and that I ought to go abroad. He felt Oxford, which was preferred by my father, was not a good place for mathematics. Cambridge was good, but best of all was Paris, he thought. When I didn't take to the idea of Paris, he suggested Göttingen or Bologna—he knew I was very interested in geometry. But I was really frightened of studying in a foreign language. So I wrote to Cambridge and got information about courses there. These were mainly 19th century style courses. Of course, I didn't know anything about the quality of these courses, but somehow they didn't impress me.

So in order to postpone making a decision, I stayed on at Toronto for a fifth year as a Teaching Fellow to get a Master's degree. I had a very good year of teaching. Originally I was given one course, but soon the other two teaching fellows dropped out and I was teaching three! I taught Advanced Calculus to the first small group of aeronautical engineers and Interest and Bond Values in a laboratory session for Pass Course students. Also, I taught Mathematics for Economists. This was to Third Year students, mainly students who had started in mathematics and physics. The trouble was there was no adequate textbook at that time. Now mark you, I had not ever studied economics. Of course, the students hadn't very much knowledge of economics either! So we learned together. But this was a year course, and after two

or three months I had done all the mathematical economics I could lay hands on. So I finished the year teaching pretty much straight statistics, which I felt these students ought to know. There again I had really not had much statistics myself.

As a Teaching Fellow I got a tiny, bare office. The first time I entered that office, the one piece of reading material in the room was a Princeton graduate catalog! I looked at that catalog and saw the courses that were listed—a course by Veblen on projective geometry, a course by Lefschetz on algebraic geometry, a course by Alexander on combinatorial analysis situs, a course by Eisenhart on differential geometry and another course by Eisenhart on Riemannian geometry. Instantly, I decided that Princeton was the place I wanted to go.

Well, I went to Dean DeLury and told him this. He said, “Oh, I don’t think that’s a very good idea,” and started in again on going abroad. “But if you insist on going to the United States,” he concluded, “there are only two places, Harvard and Chicago.”

I wrote to Harvard and Chicago for catalogs and compared offerings, and I decided the geometry courses there were not nearly as attractive as at Princeton. So I went back to Dean DeLury and told him what I had done and that I felt Princeton was the place for me. He said—this was the only time I really saw him angry—“Mr. Tucker, somehow you don’t seem able to take advice!”

I left his office thinking there goes any further mathematics study. It never occurred to me that I could just apply. I was so naive that I thought I could only go to one of these places if in some sense Toronto sent me there.

It happened before I left for the day that I saw one of my favorite teachers, Professor Chapelon. He sensed something was wrong and got me to explain. He said, “Well, let me see these catalogs.” It turned out he had gone to the same lycee in Paris as Lefschetz and thought Lefschetz was a terrific mathematician. He also thought highly of others at Princeton. So he went to Dean DeLury, and said, “I think that you should not discourage Mr. Tucker from going to Princeton,” and explained.

To Dean DeLury’s great credit, he immediately reversed himself and told me he would write at once to his good friend H. B. Fine. He did, but unknown to us, the letter arrived shortly after Dean Fine’s death. Weeks went by without an answer, and again I became very discouraged. Finally, a letter came to DeLury from Eisenhart, explaining that Mrs. Fine had recently turned over the letter to him. It was now too late for me to apply in the regular way. But, they needed a part-time instructor in mathematics, and if DeLury could recommend me in that capacity, I could come and also start graduate study.

Thanks to all the teaching I was doing, Dean DeLury had no problem recommending me. So I was appointed a part-time instructor at Princeton for the 1929–30 school year, with a salary of \$1000 and free tuition.

TYCMJ: *I take it you enjoyed your first teaching at Toronto. Did you have any other early indications of your interest in teaching?*

Tucker: Pretty early on at University, I spent hours writing up my course notes. In many of the courses there were no textbooks. I wrote up my lecture notes as though next year, if necessary, I could teach the course! That was somehow my aim in learning: to be able to explain the material or teach it. I suppose this was early evidence of my very strong pedagogical impulses.

How to Write a Thesis

TYCMJ: *At Princeton you got your Ph.D. under Lefschetz. How did this come about?*

Tucker: Both my first two pieces of research, the first, a paper written under Eisenhart, and the second, my thesis with Lefschetz, came about through trying to improve their books! With Eisenhart, during my first year at Princeton, I took his course in Riemannian geometry. Along about the middle of the year, in the chapter on Riemannian subspaces, I saw what I regarded as a flaw in the presentation of covariant differentiation. I went to see him after class and made this criticism to him. He said very courteously, why don't you write this out. The next week I gave him 3 or 4 pages. He made certain criticisms and suggested I rewrite it. This was repeated several times. Finally, one day when I had given him about the fourth rewriting, he said, "Mr. Tucker, I would like to submit this for publication in the *Annals of Mathematics*." Well, you could have knocked me over with a feather! I had had no thought at all that I was writing a paper. I was just trying to make my point with him.



Young Mathematicians of Graduate College, Princeton University, 1932. *Front row:* J. L. Barnes, N. Jacobson, J. B. Rosser, G. Bol, C. B. Morrey, B. Hoffman. *Second row:* T. Graham, R. J. Walker, W. C. Randels, A. W. Tucker. *Third row:* E. W. Titt, Garrison, E. F. Beckenbach, M. M. Flood. *Top row:* D. Marfield.

Lefschetz, at the end of my first year, gave me some chapters of the manuscript of his first topology book to look over for errors during the summer in Toronto. Well, when I came back in the fall, I gave him comments not just on typographic errors and other small things which were not right, but I proposed that he *rewrite* these chapters along different lines! Oh, he was very scornful of all this, but I persisted about it. He went off for the second term (he and Alexandroff in Moscow changed places) but while he was gone I kept on working up my ideas of how his book should be written. At the end of the second summer I presented him with a lengthy screed on it. By then his book had been published, but again I was just trying to win my point. So Lefschetz said, "You better write this up and get done with it, because until you do that I see you're not going to go on and do anything else!" He actually set up a weekly seminar for me to present this material. As this went on, he got more and more enthusiastic about it. Finally he said I ought to make it my thesis, which I did. But before he said that, I had not been thinking of it as research.

Often graduate students have asked me "How do you get started writing a thesis?" I would say, there are lots of ways, but here is one way I have had good experience with myself. Take something you are interested in, mull it over, and make it your own. There's a good chance that in doing this you will find new ways of looking at the material, and this will turn into something that's publishable.

Incidentally, Lefschetz was the one who introduced the word topology, for the title of this first book of his, published in 1930 in the Colloquium Series of the AMS. There was an earlier volume in that series, written by Veblen, called *Analysis Situs*. Lefschetz wanted a distinctive title and also, as he would say, a snappy title, so he decided to borrow the word *Topologie* from German. This was odd for Lefschetz since he was French trained and *analysis situs* was Poincaré's term; but once he decided on it, he conducted a campaign to get everyone to use it. His campaign succeeded very quickly, mainly I think because of the derivative words: topologist, topologize, topological. That doesn't go so well with analysis situs!

Also, Lefschetz was the one who invented the term algebraic topology, for his second Colloquium volume. The subject had been called combinatorial analysis situs or, later, combinatorial topology.

TYCMJ: *Let's talk now about the changes in your own work. Your work in mathematical programming and games does not appear to be closely related to your work in topology. Is it? If not, how did you get involved in these new areas?*

Tucker: Looking back now, I feel I have always been interested in combinatorial mathematics. When I was called a topologist, it was the combinatorial cell structure that interested me. Someone who studied combinatorial topology was called a topologist; I should have been called a combinatorialist, but the term just didn't exist then. So in 1948, when I had the opportunity to move into other parts of mathematics, I probably didn't consciously recognize them as combinatorial, but I think intuitively I was attracted to them for this reason.

The story of how I became involved in games and programs has been told by Harold Kuhn in that very fine survey article he has done on nonlinear programming [3]. Briefly, the Pentagon was very impressed with George Dantzig's 1947 invention of the simplex method and wanted to set up a university-based project to study linear programming further. In May 1948, Dantzig came up to Princeton

from Washington to consult with von Neumann about such a project. At the end of the day, George needed a ride to the train station at Princeton Junction. I just happened to be introduced to him then and offered him a ride, during which he gave me a five-minute introduction to linear programming, using as an example the transportation problem. What caught my attention was the network nature of the example, and to be encouraging, I remarked that there might be some connection with Kirchhoff's Laws for electrical networks, which I had been interested in from the point of view of combinatorial topology. Because of this five-minute conversation, several days later I was asked if I would undertake a trial project that summer, and I agreed. The two graduate students I got to work with me were Harold Kuhn and David Gale. Thus began an Office of Naval Research project that continued over two decades.

Many people think there was a sudden change in direction for me in 1948, but it was really things I had been interested in before that led into the things we did in this seemingly new direction.

Combinatorial Mathematics

TYCMJ: *Let me pursue the nature of the "change" a little further. You've pointed out that you were always combinatorial in your interests. Nowadays you are also very algorithmic in your approach. There is a quote you very much like by Hermann Weyl, and which you taught me: "Whenever you can settle a question by explicit construction, be not satisfied with purely existential arguments." Did you have this constructive, algorithmic attitude early on as well?*

Tucker: This attitude has been a gradual thing with me. From 1948 to about 1957 I was really interested in existential results, so my approach to programming was in terms of "convex geometry," and I had never bothered to examine the simplex algorithm carefully! There was somehow in my mind—something very common with mathematicians—a compartmentalization between numerical results and theoretical results.

In 1957 I was a consultant to a project at Dartmouth of the MAA Committee on the Undergraduate Program to write an experimental text for a second year course for students in the biological and social sciences. My job was to help write a chapter on linear programming. Well, up to that point, in any talks I gave on linear programming (I had not yet given any linear programming courses), if I wanted to start off gently, I began with an example that could be solved graphically in 2 dimensions. But when it came to describing the subject in a book, I felt that one had to present a solution method that would generalize to higher dimensions. The first thing to look at, naturally, was the simplex method. And the more I looked at the simplex method, the more I became fascinated by it. I began to see that it had very interesting structure. (One can prove from this structure, by purely combinatorial and algebraic means, that the algorithm must terminate. But by definition, the algorithm terminates only if it reaches a tableau of one or another specified form, forms from which the existence or nonexistence of certain feasible or optimal solutions is obvious. Thus the proof of termination proves the fundamental "alternative theorems" of the subject, while simultaneously showing how to compute the correct alternative in any given case—something the original proofs did not do.)

“Unify and Simplify”

For me it was a revelation to see an algorithm which, if you let it, would develop the theory for you. Ever since that time, it has been my aim to make theory and the numerical methods of solving problems as unified as possible. I guess you can say in some sense it has always been my aim to unify and simplify. I believe that the simplification very often occurs through obtaining meaningful examples, examples which, if you understand them, don't need a lot of theory—the examples carry the story.

I believe I began teaching a linear programming course in the late 1950's. It was by dint of teaching this to undergraduates that the algorithmic side continued to develop for me. But it was only after teaching the course several years that I got it organized into what I think now is a very nice form [4]. I feel that all along things which I have done that might be called research have been intertwined with my teaching, and I don't know where to draw the line between one and the other.

“Develop Courses for Students”

TYCMJ: *In addition to a linear programming course, you have developed several other courses during your career. Tell us something about them.*

Tucker: When I started as an instructor at Princeton in 1933, I had the opportunity to develop two new courses. One of these was a junior course in elementary combinatorial topology. I taught that almost every year until World War II, and several times since then. That particular course has been turned into a textbook by Donald W. Blackett [2]. Various students took the course and later became topologists.

The other course was one in “college geometry”—now rather out of fashion. I taught various geometric transformations in the plane, for instance, the 17 infinite Euclidean patterns. It was a low-brow survey of geometries in the sense of Felix Klein: projective transformations, affine transformations, inversions, that sort of thing.

After I became involved in games and programs, I did most of my course development in that direction. In addition to the linear programming course, I developed an undergraduate course in the theory of games and one on combinatorial mathematics, mainly graph theory. In the more recent years before I retired, I taught the linear programming course every fall semester and alternated in the spring semester between the other two.

The most unusual course I developed was a course in geometric concepts. Soon after World War II, Princeton, like many universities, introduced general-education or distribution requirements. The natural sciences requirement could only be met by laboratory courses, so the one place where a mathematics course could fit was the catch-all Area IV called “History, Philosophy, Religion.” Well, I had been a member of the faculty committee which drew up the plan, and which urged every department to develop distribution courses, so I took it on myself to try to design a mathematics course to fit in Area IV. The course I worked out was called “Evolution of Geometric Concepts”. In this course I tried to trace geometric ideas

from conics before Euclid down to present-day topology. The course had no prerequisites, and I concentrated on material that could be treated verbally and pictorially. I talked about things such as the Pascal configuration and the Lorentz transformation, but would depend upon plausibility arguments rather than proofs. For instance, the Pascal result clearly holds for a hexagon inscribed in a circle so that opposite sides are parallel. If you make an oblique projection of this, you get a general configuration. I don't think there is any other course that I have taught as often and for which I have the same fondness.

I feel that the chance to develop a course is a tremendous opportunity. There is a lot of work involved, but it's very rewarding. Students feel that a course that is being developed for *them* is much more meaningful than a course that is just being taught from some textbook. Also, if the instructor handles the responsibility of developing a course in an intelligent and sincere fashion, he will learn a great deal and it will make him much more interested in the job of teaching.

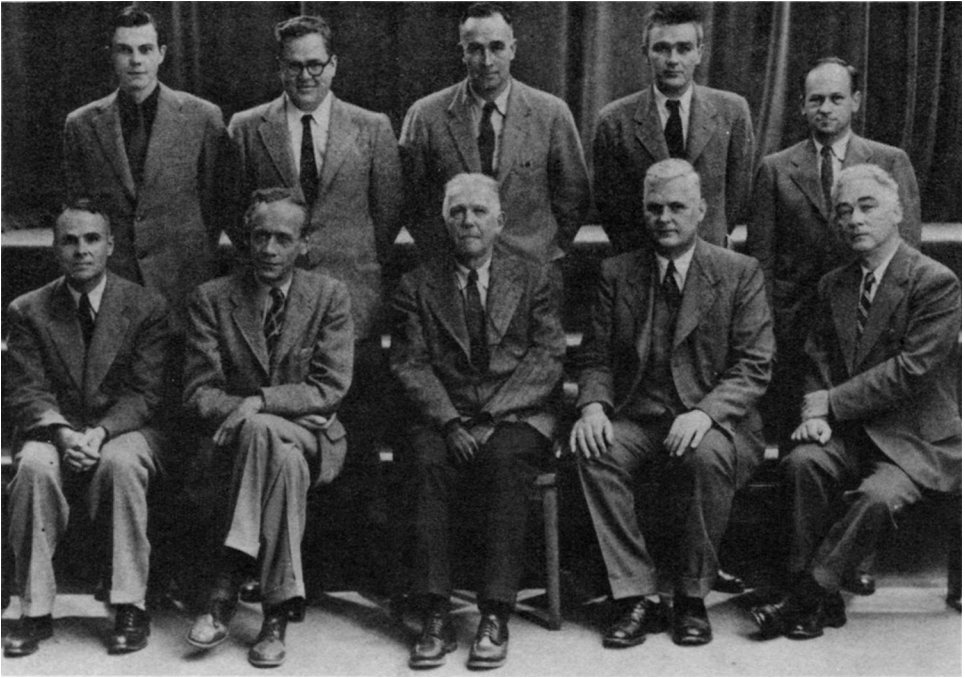
The Purpose of a Ph.D.

TYCMJ: *What about your philosophy of teaching on the graduate level? What do you see as the purpose of a Ph.D.?*

Tucker (laughing): We could spend all night talking about that! I was one of the people who took an interest a number of years ago, when there was a great shortage of college teachers, in the idea of having a Doctor of Arts degree. This would not require an original contribution to knowledge but could be attained by satisfactory work over a reasonable period, like a Master's degree. I felt very strongly that if someone did a publishable piece of research, the *publication* was the acknowledgment, the credit, the reward, and that a degree to bless that was not necessary. So I was quite happy to have a doctorate degree given to anyone who reached a certain level of mathematical maturity, and I really didn't care what sort of doctorate it was called. If it was done by a research thesis, fine, but if it was done by a so-called scholarly thesis, fine also.

As a thesis adviser, I felt that my principal role was one of encouragement. Almost all my Ph.D. students seemed quite self-reliant. Very often I really did nothing for them mathematically; I simply was the straight-man against whom they could bounce their ideas.

I sometimes would suggest a general area for a student, and I had some fortunate successes. I had one student, E. F. Whittlesey, who had had to drop out because of family financial problems and came back many years later to retake his General Examination, which he had failed the first time. He passed nicely the second time and then came to me and said "I want to do a thesis with you in topology." Well, I was no longer working in topology. I felt that I was obliged, however, to meet his request. He had been very much interested in the undergraduate topology course I mentioned earlier, and he said he would like to do something with the sort of cutting and pasting I had used to classify 2-dimensional manifolds. So I said, "Why don't you try to do a classification of 2-dimensional *complexes*." Well, it had been proved by Reidemeister that you could find a finite 2-dimensional complex that would have any given finitely generated group as its fundamental group, so this topological problem looked as if it might be as difficult as the problem of classifying finitely generated groups.



Princeton University Department of Mathematics, 1951. *Front row:* A. W. Tucker, E. Artin, S. Lefschetz, A. Church, W. Feller. *Back row:* J. T. Tate, J. W. Tukey, D. C. Spencer, R. C. Lyndon, V. Bargmann. *Absent:* S. Bochner, R. H. Fox, N. E. Steenrod, E. Wigner, S. S. Wilkes.

Of course, I didn't really think Whittlesey would solve this problem. I regarded it as unsolvable. I thought he might find a subclass that he could classify, and that then this would become an important subclass, just as earlier lens spaces had become an important subclass of 3-dimensional manifolds. But he went off to where he was teaching, and I neglected to tell *him* this! I don't know if this was carelessness or what, but it was apparently very fortunate that I didn't tell him. About three months later he called me and said he had solved it. Well, of course I didn't believe him, but I asked him to come to Princeton at the first opportunity. The next weekend he came, we worked on it until noon Sunday, and by then I was convinced that he *had* solved the problem. I told my colleague Ralph Fox about this, and he didn't believe it either. But when the thesis was turned in, he was the second reader and approved the thesis. His remark to me then was, "What devil got into you, to set him that problem?" and I had to explain. Anyway, that, of all my experiences with thesis supervision, was the most fortuitous.

Another unusual case was Marvin Minsky. He was a graduate student at Princeton in the early 50's. He was given support through a research assistantship with the Office of Naval Research project I directed. I very quickly discovered that he was very talented and had all sorts of original ideas.

One day towards the end of his first year at Princeton I asked him if he had any plans for his thesis. He said no, but he supposed he would do a thesis in topology because that's what so many of the other students were doing. "But what would you really like to do?" I asked. He indicated he would like to develop some of the ideas he had been interested in as an undergraduate concerning the relation between computers and the brain. "Well," I said, "why don't you!" He replied, "The Department would never accept a far-out thesis like that!" "No, as far as I'm concerned," I said, "the only requirement for a Ph.D. thesis is that it should be an original contribution to human knowledge; there's no limitation about far-out!" "But who would supervise it?" Minsky asked. "I'm willing to supervise it. I can't help you with the material," I explained, "but I will serve the formal purposes." And so we agreed that he would try to develop his ideas and put them in thesis form.

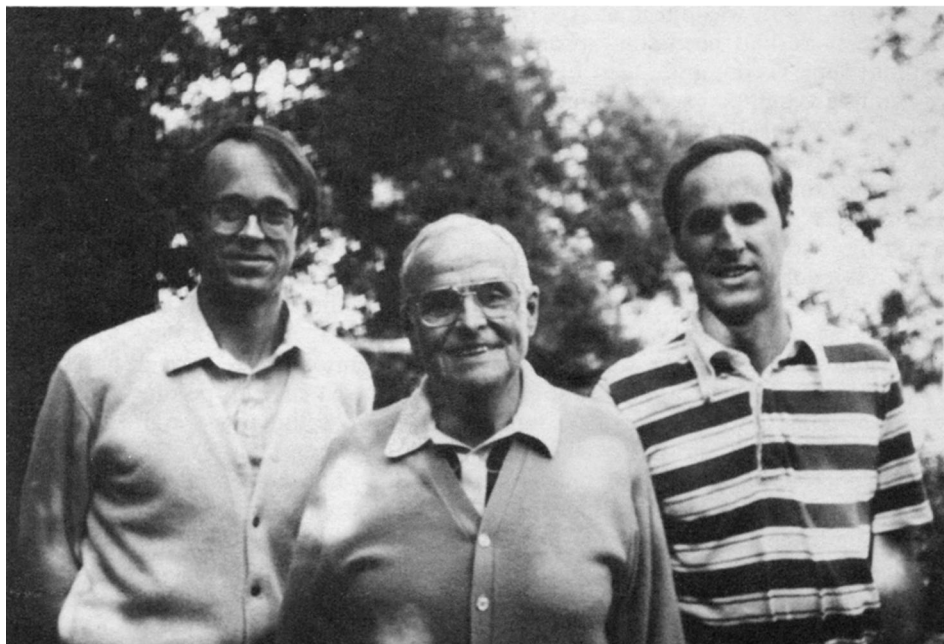
I did have some qualms about this, because there would have to be a second reader of the thesis, and the report of the readers would have to be accepted by the Department. But I really felt strongly that Minsky should develop his ideas on "artificial intelligence," as it is now called.

Well, he did. In the end his thesis was about 300 pages. As I recall, the title was "Neural Networks and the Brain Problem". He did it all on his own. When I saw him I would ask how things were going, but this was really just general encouragement. I was the first reader and John Tukey was the second. We also had an independent reading done by the chairman of the biology department. The point of this was to have him assess whether or not the physiological assumptions that Minsky made were reasonable ones. He said they were, and put it in writing. So the report was made to the Department and there were no objections.

There is a profile of Minsky published a year ago in the *New Yorker* [1], in which mention is made of his exceptional thesis and of the informal club-like atmosphere of Fine Hall that he shared with creative contemporaries, such as John McCarthy and Lloyd Shapley.

TYCMJ: *Al, let me ask one more question about teaching, of a more personal nature. Both your sons, Alan and Tom, are active mathematicians. In bringing them up, did you stack the deck? [N.B.: Alan is also called Al by some, but must be called Alan here, for obvious reasons.]*

Tucker: I'm very happy that my sons seem to share my mathematical tastes, but this has not come about through any pressure from me. It's quite clear that through some osmosis they acquired values that inclined them towards mathematics and its teaching. The thing I can't really understand is why they both have combinatorial interests akin to my own. They did not attend Princeton and I at no time tutored them. Of course, I would chat with them about the mathematics they were studying. At one time Alan was having difficulty, he thought, in finding a thesis adviser. But I knew that he was enjoying the work that he did summers at the Rand Corporation, where he was associated with Ray Fulkerson. So I suggested that perhaps he could start a thesis with Fulkerson and complete it at Stanford, where he was a student. This indeed happened, thanks especially to George Dantzig.



Father and Sons:
Alan, Albert, and Tom Tucker
—All Mathematicians.

Founding of the Annals of Mathematical Studies

TYCMJ: *I understand you've been involved in a number of editorial activities over the years which are not well known but which you feel have been important. Tell us something about them.*

Tucker: Well, in 1933, when I was appointed to the mathematics faculty at Princeton, I was assigned the job of handling the manuscripts which came in to the Annals of Mathematics, until they were either accepted or rejected. Lefschetz and von Neumann were the editors, but Lefschetz made most of the decisions. At that time the Annals had no paid secretary. So when a manuscript came in, Lefschetz would take a quick look at it. In cases where he knew the author or the subject matter, he would make a snap decision whether to accept it or reject it. The other papers were turned over to me, and I had to find referees for them. Then I rode herd over the referees. I have no good substitute to suggest for the refereeing system, but it's a pain in the neck for just about everybody concerned. Anyway, if there was correspondence, it was always signed by Lefschetz; I never formally appeared as having anything to do with it. But it was all dictated and handled by me.

Also in 1933 I was put in charge of the department mimeograph machine, merely because there had been some problem about people using the machine carelessly without supervision, and Dean Eisenhart, the chairman of the department, believed in running a tight ship. The mimeograph was used to run off course notes, and not much else. But there were a large number of course notes. The Institute for Advanced Study had just recently been established and shared old Fine Hall with the Princeton mathematics department. Institute professors such as Oswald Veblen, Hermann Weyl, John von Neumann and Marston Morse had been accustomed to lecturing, and even though they were not required to lecture at the Institute, they just did it anyway out of habit. Also, the Institute professors had distinguished assistants, and these assistants would take notes of the lectures. Some of these sets of lecture notes were on research; for example, the first publication of von Neumann on the work he was doing on linear operators came in such notes.

People would hear about these course notes, even in Europe, and they would write for copies. So this became a business. We priced the notes originally to cover just the cost of ink and paper; the work of mimeographing and collating was done by students. It was a very amateur enterprise. But when it became clear that one of the two mathematics secretaries was spending a sizeable fraction of her time on correspondence about these notes, something had to be done. Several remedies were tried starting in 1937, but the final successful change was made with the creation in 1940 of the *Annals of Mathematics Studies*, which were published by the Princeton University Press as photo-offsets. The course notes became volumes in the *Studies*. At the same time the *Annals of Mathematics* had been having difficulty with what to do with long papers. Such papers were transferred to the *Studies* as monographs, and this is why the series was called the *Annals of Mathematics Studies*.

So, just because of jobs I was assigned as low man on the totem pole, I had a major role in the creation of this distinguished series.

My other experience at that time in editing—it's another long story with chance elements—was in helping to establish and run the Princeton Mathematical Series (an equally distinguished series of hardback full-length advanced books, also published by the Princeton University Press). Because it was felt that I was too young, inexperienced and unknown to be sole editor of this series (this was 1938 and I had just been given tenure), two other editors were also designated, H. B. Robertson and Marston Morse.

There are several interesting stories about how books got into the Princeton Series, especially books by European authors published during World War II. On the other hand, I feel that the *Annals Studies*, which have now passed 100 volumes, is a greater contribution than the Series. The Series was not too different from what other publishers might do, but at the time the *Annals Studies* were started there was *nothing* in the United States in the way of low-cost paperback editions of serious mathematics. Gödel's consistency of the continuum hypothesis might never have been published if there had not been the *Annals Studies* in which it could be done at a low cost.

"Mathematics Must Become More Algorithmic"

TYCMJ: *We have before us a picture of a combinatorial problems seminar at the IBM Research Center. I understand this is an unusual picture.*

Hold Seminars in Combinatorial Problems



COMBINATORIAL PROBLEMS INSTITUTE participants convene for planning session at the Lamb Estate. Left to right are Professor Merrill M. Flood, University of Michigan; Dr. John L. Selfridge, department 494, Lamb Estate; Professor Herbert J. Ryser, The Ohio State University; Dr. Ralph E. Gomory, manager, Business Research; Professor Albert W. Tucker, Princeton University, chairman of the Institute; Dr. Edward F. Moore, Bell Telephone Laboratories; Dr. George B. Dantzig, the RAND Corporation; and Dr. James H. Griesmer.

Tucker: Well, the picture is not unusual, but the workshop, which was held in the summer of 1959, was the first as far as I know in the area of combinatorial mathematics. The participants—there were about 15 of us—spent a considerable amount of our social time in trying to define what we meant by combinatorial mathematics. We were all agreed that networks or graphs fell in combinatorial mathematics, and also that many aspects of matrices were combinatorial. And of course we agreed that the traditional combinatorics arising out of permutations and combinations was part of the subject. But outside that we all had our own conceptions of combinatorial mathematics. It seems strange to me now that it was such a short time ago that this area of mathematics was being defined and recognized.

TYCMJ: *Al, what do you see as the greatest challenge facing mathematics in the coming years?*

Tucker: The computer revolution. Mathematicians have set great stock in abstract mathematics in which concepts and rigor have been the dominant things. But now algorithms are really important. There have been algorithms around from Euclid's algorithm on, but they have been regarded as rather unusual. I think that mathematics will have to become more and more algorithmic if it is going to be active and vital in the creative life. This means it is necessary to rethink what we teach, in school, in college, and in graduate school. In our emphasis on deductive reasoning and rigor we have been following the Greek tradition, but there are other traditions—Babylonian, Hindu, Chinese, Mayan—and these have all followed a more algorithmic, more numerical procedure. After all, the word algorithm, like the word algebra, comes from Arabic. And the numerals we use come from Hindu mathematics via the Arabs. We can't regard Greek mathematics as the only source of great mathematics, and yet somehow in the last half century there has been such emphasis on the greatness of "pure" mathematics that the other possible forms of mathematics have been put down. I don't mean that it is necessary to put down the rigorous Greek style mathematics, but it *is* necessary to raise up the status of the numerical, the algorithmic, the discrete mathematics.

TYCMJ: *We've talked very little about you personally. What are some of your hobbies?*

Tucker: I certainly do like to travel, and fortunately I've had many professional opportunities for this. Even during vacation trips I like to visit with mathematicians and give talks. My favorite place for travel has been Australia. I've been there four times as a visiting lecturer. The city of Perth in Western Australia is my favorite city. That's where I would live if it weren't so far away from everything else that I'm tied to.

The other hobby I might mention is that I like detective stories. It isn't that I read them so much to try to guess the end; I really read them for just relaxation. I have quite a collection of paperbacks. I like best the classical British detective stories, which I started reading when I was a student in Toronto.

Early on I had liked chess, but I swore off chess when I discovered that after playing a keen chess game I had difficulty sleeping at night. I was continuing to concentrate on the game. So I switched to reading, and found that somehow detective stories provided me with the sort of relaxation I liked.

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