

***From the collections of the Seeley G. Mudd Manuscript Library,
Princeton, NJ***

These documents can only be used for educational and research purposes (“Fair use”) as per U.S. Copyright law (text below). By accessing this file, all users agree that their use falls within fair use as defined by the copyright law. They further agree to request permission of the Princeton University Library (and pay any fees, if applicable) if they plan to publish, broadcast, or otherwise disseminate this material. This includes all forms of electronic distribution.

Inquiries about this material can be directed to:

Seeley G. Mudd Manuscript Library
65 Olden Street
Princeton, NJ 08540
609-258-6345
609-258-3385 (fax)
mudd@princeton.edu

U.S. Copyright law test

The copyright law of the United States (Title 17, United States Code) governs the making of photocopies or other reproductions of copyrighted material. Under certain conditions specified in the law, libraries and archives are authorized to furnish a photocopy or other reproduction. One of these specified conditions is that the photocopy or other reproduction is not to be “used for any purpose other than private study, scholarship or research.” If a user makes a request for, or later uses, a photocopy or other reproduction for purposes in excess of “fair use,” that user may be liable for copyright infringement.

The Princeton Mathematics Community in the 1930s
Transcript Number 32 (PMC32)
© The Trustees of Princeton University, 1985

ALBERT TUCKER

MATHEMATICAL JOURNALS AND COMMUNICATION

This is the fourth in a series of interviews with Albert Tucker in his office in Princeton University. The interviewer is William Aspray. The date is the 13 April 1984.

Aspray: I'll let you begin where you'd like.

Tucker: The outstanding Princeton publication as far as mathematics is concerned is the *Annals of Mathematics*. Certainly one of the leading journals in mathematics for the whole world. Some people will even say *the* leading journal.

Aspray: Are you speaking of today or in the 1930s?

Tucker: I'm speaking of today. We must point out that this year, 1984, is the hundredth anniversary of the *Annals of Mathematics*. It was begun in 1884 by a professor at the University of Virginia, Ormond Stone, who paid the expenses of the *Annals of Mathematics* for the first ten years of its life.

Aspray: Was this one of the very first math journals in the United States?

Tucker: The first mathematics journal in the United States was the *American Journal of Mathematics* which is located at Johns Hopkins.

Aspray: And always has been.

Tucker: Yes, so certainly in terms of seniority, there is no doubt that the *American Journal of Mathematics* comes first in American.

mathematics. *Annals of Mathematics* was the second. As I say, it was founded by Ormond Stone at the University of Virginia in 1884. In 1899, the direction of the journal was taken over by Harvard University. Various of the mathematicians at Harvard, such as Maxime Bocher and W.F. Osgood, served as editors. There were usually two or three named at a time, and there was a certain rotation. But in 1911, for some reason that I don't know, the journal was transferred from Harvard to Princeton and has been published at Princeton by the Princeton University Press since 1911. In 1933 when the Institute for Advanced Study was getting started, the School of Mathematics of the Institute took joint responsibility with the University's department of mathematics in editing the *Annals*.

Aspray: Has that continued?

Tucker: That has continued ever since. In the early days here at Princeton I think it was [J.H.M.] Wedderburn who did most of the editing, and then in the late 1920s [Einar] Hille and [Solomon] Lefschetz took over the editing. Hille left in 1933, and von Neumann then replaced Hille in team with Lefschetz. Then late in the 1930s, about 1938 I believe, H.F. Bohnenblust, joined Lefschetz and von Neumann as the designated editors of the *Annals*. As long as I've been around it's been understood that the faculty in mathematics at the University, and then later also the mathematicians at the Institute for Advanced Study, were all assistant editors for the *Annals*.

In his period, Lefschetz was the dominant editor. Von Neumann was quite happy to do only what he was asked to do. Bohnenblust served as informal editor for several years before he was actually named as editor. The *Annals* at first had four issues a year. I believe that the annual subscription to the *Annals*, at the time that it was taken over by Princeton University in 1911, was \$2 a year.

Aspray: Can you tell me something about the circulation numbers?

Tucker: No, I can't, but I think that at that time when it was \$2 a year all of the established mathematicians of the country would have automatically subscribed to the *Annals* and also to the *American Journal*.

Aspray: I see.

Tucker: It was quite customary in those days for a mathematician to have his own private library. Nowadays it has become rather too expensive to do this. So now it's the library subscriptions that are the mainstay of a journal such as the *Annals*. Until along about 1930 the *Annals* was mainly an American publication, that is, the papers that came to it were mainly by American authors. But starting in the Thirties, and especially later when it became a joint undertaking by the University and the Institute, it received its papers from all over the world.

Aspray: Was this a recognition, do you feel, by non-American mathematicians, that the American mathematical community was becoming worthy of their publishing in American journals?

Tucker: Oh, yes. Also at that time between the two world wars, the European journals were having a hard time of it. By that I mean they weren't able to publish many papers, and at some time in the Thirties the *Annals* changed over from four issues a year to six issues a year. There got to be two volumes a year, and one volume of three issues was as large or larger than the previous annual volume of four. So when there came the papers from all over the world this wasn't to exclude American papers, because the *Annals* simply expanded its size to include the papers from elsewhere while handling also the American papers.

Aspray: In the Thirties, how did the *Annals* compare with other journals, both American and foreign? What were the major journals of the Thirties for mathematicians?

Tucker: Well, of course in Germany *Mathematischen Annalen* and *Zeitschrift*, particularly the *Annalen*, still played an important role. You would find a large number of von Neumann's early papers published in the *Annalen*. And then there were various journals, the *Proceedings of the London Mathematical Society*, the *Proceedings of the Cambridge Philosophical Society*, and French journals. Of course the *Comptes Rendus* was always the journal in which to try to get quick publication of a new result. Also the *Proceedings of the National Academy of Sciences* in this country served a similar purpose. At that time, of course, there were really no outstanding journals in other countries, such as the USSR or Japan. There was in Sweden the *Acta Mathematica* that had been founded by Mittag-Leffler. Of course, I was very close to things with the *Annals*, indeed I served as an unnamed assistant-editor working with Lefschetz in the 1930s, but it's my objective feeling that the *Annals* represented an exciting new feeling and that with the development of topology and various new fields the older journals, particularly the *Acta*, tended to be pretty much in the traditional fields.

Aspray: I see. That was true of *Mathematischen Annalen* also.

Tucker: Yes, although I think that it was less staid than the *Acta*.

Aspray: What about the various publications of the American Mathematical Society? How did they fit into this scheme?

Tucker: The *Transactions of the American Mathematical Society* was a journal at the same level as *Annals*. But it's my opinion that the *Transactions* has always suffered from the fact that it was a committee job.

Aspray: I see.

Tucker: That the refereeing was heavy handed and publication was slow and *Transactions* was fussy. They wouldn't take a short paper; anything under ten pages had to go to the *Bulletin* and also they didn't want very long papers. The *Annals* would publish a paper of two pages or a paper of one hundred pages. There was a much freer attitude. Also in the time that Lefschetz was editor—this extended from the late Twenties on into the Fifties, I think he was editor for something like 28 years—he really ran the thing and would often invite a paper. He would hear of some new result, and he would write and solicit the paper and promise publication without refereeing.

Now the *Transactions* was run democratically, no favors, everybody treated the same. The *Annals* was run with a great deal of favoritism, and this would have been tragic if it hadn't been for the tremendous perception of Lefschetz who seemed to sense when something was a very good paper, scarcely without reading it. Some intuition told him, I guess. He must have made some mistakes, though I can't cite any. Of course, this also made a lot of enemies, because two people, say, would be competing to get first publication in some new thing and the person who published in the regular American Mathematical Society channels through the *Transactions* got nipped by the guy who got it in with Lefschetz.

There are many things that you can say against this autocratic system of Lefschetz. I used to fight against it when I was an assistant editor. I said that having a paper refereed was a service to the author as well as to the readers and that there were often things that a referee could catch that the author would very much appreciate having the opportunity to change before the thing was published. But Lefschetz just brushed this aside.

Aspray: You told me off tape several days ago that most of the refereeing was done in house.

Tucker: That's right.

Aspray: When would a paper be sent out?

Tucker: Only when we couldn't get anybody to do it in house.

Aspray: So that meant that you could get rather rapid turnaround on papers.

Tucker: Yes. And I would say that 95% of what was published did not have any outside referees. And even outside referees were usually friends, so to speak.

Aspray: I see.

Tucker: People that we could count on to act just as though they were here.

Aspray: Speaking of rapid turnaround on referring of papers, you've told me a story off tape of von Neumann's reviewing technique, about his turning the pages. Could you?

Tucker: It was Herman Goldstine, I think, who told you that. I didn't ever have direct information about that, but I can believe that it was very true. I think also Wigner mentioned that von Neumann would apparently read a paper that he wanted to know about just by turning the pages. His brain worked with what seemed to be ten times the rapidity of any other mathematician.

Aspray: Was most of the research of the Princeton community published in the *Annals*?

Tucker: Yes, that's where my thesis was published and an earlier paper before that I had written under the impetus of Eisenhart. Indeed in that case what I thought I was doing was trying to convince Eisenhart of a certain criticism that I had about a chapter in his book on Riemannian geometry.

This was when I was taking the course from him in my first year here in Princeton. I saw what I thought was a much better and more satisfying way of doing something with regard to the Riemannian geometry of subspaces. So I first of all suggested to him this, and then he asked me to write it out for him. Then this was repeated two or three times. He would read each draft that I gave him. Finally one day towards the end of my first year here at Princeton, he said, "Well, Mr. Tucker, I would like to submit this for publication in the *Annals of Mathematics*. And until he said that, I had no idea that I was writing a paper. I was just trying to make a point with him about material in the course that he had been teaching. So he automatically thought something that could be published should be published in the *Annals*. You sent it elsewhere only if there was some reason for not sending it to *Annals*."

At times *Annals* got quite a backlog, so there were times when you had a chance of faster publication elsewhere. But there just is no doubt that *Annals of Mathematics* has been part of the Princeton mathematical picture ever since it came here in 1911.

Aspray: At about what date did it come into this fairly preeminent position?

Tucker: I would say about 1930. By say 1934, which is just fifty years ago, halfway, it was established as a leading, if not the leading, mathematics journal in the world. Of course, applied mathematicians would say that *Annals of Mathematics* was a journal of pure mathematics and that is probably correct. So *Annals* can be attacked on that score.

Aspray: I assume it wasn't one of the leading journals for the publication of statistics either.

Tucker: No, and now that brings up another point. That there have been two other journals edited at Princeton but never thought of as Princeton journals. *Annals*, that's Princeton. One of these journals is the *Journal of Symbolic Logic* which was edited for a long time by Alonzo Church.

Aspray: Started in what year?

Tucker: I can't tell you when the *Journal of Symbolic Logic* started.

Aspray: I believe it was '34, but I'm not certain.

Tucker: I see. I would have thought it was somewhat earlier than that. I don't know that Alonzo Church was ever named as the managing editor of the journal, but for most purposes he was the managing editor. This continued, as a matter of fact, for the *Journal of Symbolic Logic* until Church retired and then had a career since then at UCLA.

Aspray: So it followed him to UCLA.

Tucker: He followed it. At that particular time I remember my sorrow that the University refused to continue any longer the free space that it was giving for the editing of the *Journal of Symbolic Logic*. They wanted to charge rent for the space, not for Church's office, but for the couple of assistants who were working on the journal. They were not from Princeton University, and the University was expecting the Association for Symbolic Logic to pay rent, so to speak, for that. It was then that a group at UCLA thought that they would like to take on the journal. They did this and provided a very good home free of charge to the journal and the Association.

They also offered Church a visiting professorship until he would reach the retirement age at the University of California. Even after that he was to have a position with stipend as long as he wanted it. I believe he still has that, although he had his eightieth birthday last summer.

Church was, I feel, never properly appreciated by Princeton University. This was very strange because Church was a Princeton undergraduate, so he counted as a Princeton alumnus in the full sense. All his degrees were from Princeton University, bachelor's degree, master's degree, doctor's degree. But he was so aloof from everything except the *Journal of Symbolic Logic*. The Dean of the Faculty told me on one occasion that he often met Church crossing the campus, and he would speak to Church and Church would not speak in reply. Things like this, very trivial, nevertheless mitigated against his being given the credit which he merited on scientific grounds.

Aspray: The other journal?

Tucker: The other journal was *Annals of Mathematical Statistics* which was started in the late Thirties, very close to 1940. Sam Wilks

undertook the editorship and I think continued that editorship until the late Forties.

Aspray: Was there some effort to publish statistics prior to this time in *Annals of Mathematics*? Was there a reluctance on the part of the staff to do so?

Tucker: Well, *Annals of Mathematical Statistics* was established as the journal of the Institute of Mathematical Statistics.

Aspray: I see.

Tucker: It was started because it seemed that the mathematical qualities of the American Statistical Association, which had existed for some time and which published a journal, had become rather low. So some mathematicians such as [H.L.] Rietz, who was the professor at the University of Iowa with whom Sam did his Ph.D.; and [H.C.] Carver, a mathematical statistician at the University of Michigan, and [Harold] Hotelling, decided to form an organization of their own with the emphasis on mathematical statistics rather than run-of-the-mill statistics. And having set up this Institute of Mathematical Statistics they very naturally decided that they wanted to have a journal, which they called *Annals of Mathematical Statistics*. The fact that it was called "Annals" had nothing to do, as far as I know, with *Annals of Mathematics* which was just a name which was often applied to a journal. Wilks did a dandy job of editing that journal and making it a very worthwhile journal.

There were some other publications that arose in the 1930s. In 1933 when the School of Mathematics of the Institute for Advanced Study came to Fine Hall the professors, Weyl, von Neumann, Marston Morse, gave courses of lectures. They were in the habit of doing this in their previous positions, and although there was no requirement that a professor at the Institute for Advanced Study give courses, they did. The lectures were usually there in Fine Hall, in the central lecture room, attended by people who were at the Institute, the members, many of the them post-doctoral people, and by graduate students of the department of mathematics in the University. Indeed often there would be people that came in from neighboring places to follow a course of lectures.

Notes were taken, often by an assistant to the professor. He was given the job of taking notes of the lectures, writing them up, and having them edited by the professor. Then these would be mimeographed, and the people in the course could for, say, a dollar or two subscribe to the notes, which would be turned out in batches a week or two at a time. At the end, if someone wanted to, the accumulated notes could be brought in, and we could send them somewhere to have them bound. I think there was a charge of 25 cents for binding. I have a whole drawer full of these notes. This is the way in which they were bound, and these were lectures by Alonzo Church. Now these notes [showing a set of notes] were taken cooperatively by, these are all graduate students.

Aspray: So there are six or seven people ...

Tucker: Yes, and perhaps two would take notes at the same time and put their notes together, Then at another time there would be another pair, and the duty was shared, because Church didn't have an assistant to do this.

Aspray: I see.

Tucker: So it was either done by an assistant, or it was done by volunteers from the class. I happen to have been put in charge of the mimeographing machine by Dean Eisenhart when I began as an instructor in 1933. The mimeographing machine was down in the basement of Fine Hall. Up until that point it had been used without any supervision. The result of this was that various things had gone wrong with the machine, and it was decided that it had to be supervised. Somehow this supervision of the machine turned into it being my job to coordinate all this note business.

For the running of the machine we used student labor. At that time there was a federal program to aid students under the Works Progress Administration, an F.D.R. program. So students could be paid for operating the mimeographing machine, a dollar and a half an hour or something like that. The other problem was collating. This was usually done in the common room around the large table that was in the center of the common room. That table was large enough, more than large enough, to serve as a ping-pong table. And I've known occasions when it was used as a ping-pong table. The piles of page 1, page 2, page 3 would be put around that table. Then we would collect all of the graduate students or others around at that particular time and have a "sorting bee". A person would simply take up a copy of page 1, move on take a copy of page 2, and so we would circulate around the table. Each time you went around the table you had a full copy. So the only cost for these notes was the paper and ink. We charged enough, a dollar a copy, sometimes fifty cents a copy if the set of notes was not very great, to cover these incidental expenses.

But these notes began to be known around the world, and we would get orders for the notes. We would often have to take the stencils and rerun them two or three times, until the stencils were too worn to make additional copies. We sold the copies outside Princeton for the cost in Princeton plus postage. And because of this business, we actually developed an order form that people could use to order copies. Before we realized it we were getting into business.

The first change that was made was to change from mimeographing to lithoprinting. This was done along about 1937 or '38. There was an outfit at that time in Ann Arbor, Michigan by the name of Edwards Brothers that did lithoprinting of course-notes and that sort of thing for the whole country. They had a very efficient operation going there. So we had the course notes lithoprinted instead of mimeographed. We had to charge more for them, but we didn't have all the nuisance of doing it ourselves. Also there was the problem of

storage, so we got the Princeton University Press to agree to store them for us. We gradually got them into the business of filling orders from the outside for a 25 % commission.

But it still seemed too haphazard, so in 1940 a new publication was started called *Annals of Mathematics Studies*. Here is one of these early *Studies*. This is Study Number Six, "The Calculi of Lambda Conversion", by Alonzo Church, 76 pages, \$1.25. Done from pretty straight typing, nothing fancy, and we shipped off a typed master copy to Edwards Brothers. In due time they shipped back the number of copies that we had ordered. The minimum order that we used in those days was 200 copies, but it very often rose above that. They were priced so that if we sold the 200 copies we broke even.

Aspray: I see.

Tucker: But we would very often have to do reruns which cost very little, and we made a profit on the reruns. The Princeton University Press accepted responsibility, and you'll see here that this was copyrighted by the Press in 1942. But the Press did not take full responsibility until several years later; it wasn't regarded as a publication of the Press. They stored them, they filled orders, collected 25% handling charge, and the rest of what came in was put in a bank account. I was the only one who could draw from that account, and I used that to pay the bills at Edwards Brothers and any other incidental expenses. We would have to pay for the typing of the manuscript. The present chief secretary here, Genny Dzurkoc, did some of this typing early on with an IBM electric typewriter with a carbon ribbon in order to make a clear sharp impression for the lithoprinting. The first one of these *Annals Studies* was "The Algebraic Theory of Numbers" by Hermann Weyl, 227 pages, \$2.35. The second was one we heard about from John Tukey, "Convergence and Uniformity in Topology" by John W. Tukey, 95 pages, \$1.50.

Aspray: But they were all still written by people from the Princeton community.

Tucker: Some of them were written by people who were visitors here. Number Five here is "The Two-Valued Iterative Systems of Mathematical Logic" by Emil L. Post. Now he was not at Princeton. I think he was at Columbia or N.Y.U., but he frequently came to Princeton and did editorial work with Church for the *Journal of Symbolic Logic*.

There were two reasons for the title *Annals of Mathematics Studies*. One reason was that the editors of the *Annals of Mathematics Studies* were nominally the editors of *Annals*. The other reason was that Lefschetz was anxious to have an escape hatch for long papers that would be sent to *Annals*. So if they would get a long paper—long by Lefschetz's definition was roughly 100 pages—and felt that it would take up too much of the available space in *Annals* and delay the publication of other papers, the author would be told that he had two options: to have it published as an *Annals of Mathematics Study* or to withdraw the paper and try somewhere else. I don't think actually that there were

very many cases where the author chose to put it in *Annals of Mathematics Studies* at that time, because *Annals* had a regular printed format whereas the *Studies* had a typewritten format. And at that time *Annals of Mathematics Studies* seemed second rate in terms of appearance. But as you can tell from the fact that the authors were Hermann Weyl, Goedel ("Consistency of the Continuum Hypothesis" was Number 3 on the list), and F.J. Murray ("An Introduction to Linear Transformation in Hilbert Space").

These were very fundamental monographs that couldn't have been handled at that particular time by a commercial publisher. No commercial publisher would have touched something where he didn't have, say, an expectation of selling 2000 copies. Whereas we were perfectly willing to go ahead with the idea of selling only 200 copies. Nowadays *Annals of Mathematical Studies* is regarded as a fully reputable form of publication. Just recently we had Volume 100; I have it at home. This is Volume 101. So at about the same time that *Annals of Mathematics* has its 100th Anniversary *Annals Studies* has passed its hundredth volume because of course two or three or four may have come out in a year. Now very often the *Annals Studies* is used to present the papers from some conference. It is most often used I think for papers by a group of authors who would like to have their papers appear together, rather than here and there in the journals. In that way it really now has the status of journal publication.

Aspray: I see.

Tucker: Also because it has established itself now, most libraries that want to have a good mathematics library place standing orders for *Annals Studies*, and when it comes out, it is automatically sent to these libraries. And many libraries shelve *Annals Studies* all together rather than author by author.

Aspray: I see.

Tucker: Now at the time that *Annals Studies* was started there was nothing in the way of inexpensive paperback publication of higher mathematics in the United States. In England there was something called the Cambridge Tracts. Of course nowadays there are commercial publishers, Springer and so on, that are putting these things out all the time. So that perhaps at the present time, no one would start *Annals Studies*, but because it's going, and has almost an established position it keeps on going.

But at the time it was started it filled the void. And I must say that through all the various things that I've had a hand in here at Princeton, there is nothing that gives me greater pride than *Annals of Mathematics Studies*. My name has never appeared as an editor. I have been responsible for some of the individual studies. There are five studies on contributions to the theory of games that are edited by me. Each one is edited by me and someone else. The first two were edited by me and H.W. Kuhn, but as for the series itself, even though I regard it as my series in the same way that Lefschetz regarded *Annals of Mathematics* as his baby, my name never appears.

Aspray: I see.

Tucker: I hope that on my tombstone they will put 'Founder of *Annals of Mathematics Studies*'. Another thing that was started in the Thirties was the Princeton Mathematical Series. This is of regular hardback books. Here is one of the earlier ones by Claude Chevalley. It is Number Eight. One strange thing is in these first eight volumes, three of them have to do with continuous groups. I remember the Princeton University Press at the time having great misgivings that there should be such a redundancy, as they thought. One of these, which is the first volume in the series, *The Classical Groups* by Hermann Weyl. Number Two, *Topological Groups* by L. Pontryagin. Number Eight, *The Theory of Lie Groups* by Claude Chevalley. But oddly enough, these books helped sell one another, so this turned out to be an advantage.

The way this series got going is that there was a mathematical physicist in the department at that particular time, about 1937-38, E.U. Condon. He was at that time the editor of a series of monographs in physics for the publisher Prentice-Hall. He suggested to Prentice-Hall that perhaps Prentice-Hall ought to start a similar series in mathematics. So he came to me one day and said that he would like to take me in to meet some of the people at Prentice-Hall in New York. I went in with him, and there was some general talk, and we had a very nice lunch somewhere and came back to the office of the president of Prentice-Hall. He pulled out a contract and was ready to sign me then and there to edit a series of books for Prentice-Hall. The terms were perhaps fairly generous, that I was to get a two-percent overriding royalty on all books published in this series. But I had to do the reviewing, the refereeing myself, or pay out of my own pocket to have it done.

Aspray: I see.

Tucker: I, being cautious, said that I would like to think about it. I came back to Princeton and went to see Dean Eisenhart. I asked him his advice on it, and I also talked about it with Professor Lefschetz. Eisenhart said quite firmly that he felt that I shouldn't do it. As he said, "If you're going to edit a series of books, I think you should do it for the Princeton University Press." Well, long afterwards I learned that he had been trying for years to get the Princeton University Press to start a series of mathematical books. The Press had always declined to do this, feeling that it would lose money to do a series of mathematics books. But Dean Eisenhart said, "Let me talk to the Press." And he talked to the Press.

This all happened very quickly. His position and the fact that I had this offer from Prentice-Hall ... he argued that since Prentice-Hall was willing to do it, and they weren't willing to take a loss, that the Princeton University Press had nothing to worry about. So a contract was written for a series of mathematical books, and the contract was written for a preliminary five years. The Press was not expected to take more than two books a year and also had the right to submit the

manuscripts to their regular editorial committee. And subject to all these conditions the book would be printed with the title the "Princeton Mathematical Series". No royalties to the author or to the editors. But I wasn't at all mercenary, so feeling that I would be doing something that Dean Eisenhart and Professor Lefschetz wanted to have done and that this would presumably be to the credit of Princeton mathematics, I willingly consented to this and gave Prentice-Hall my refusal.

However, because I was at that point still not a tenured member in the department—I became tenured later that year—it was decided that there should be two others. These were Marston Morse, representing the Institute for Advanced Study, and H.P. Robertson, who was professor of mathematical physics at the University. But throughout the first years of the Princeton Mathematical Series they were more or less figureheads. The series did very well. The five year period ran out while the war was on and nothing was said about it. It wasn't until about eight years that it was finally realized that things were going on without any extension of the contract, so there was an automatic extension to a ten-year contract. At the end of the ten-year contract the Press just took it over completely. In other words, they felt they no longer needed any protection to publish the series, and at that point it was decided to give the authors a regular royalty, the same as would be given by a commercial publisher, ten percent. But no editorial royalty.

I was told many years afterward that a professor at Brown, A.A. Bennett, who took on the editorship for Prentice-Hall, by about 1960 was getting in the neighborhood of \$50,000 a year. It was only perhaps at that point that I had any regrets. But Bennett did all the refereeing himself. And he did a very fine job of it, because I remember when John Kemeny and company were having their *Introduction to Finite Mathematics* published by Prentice-Hall, Bennett went through their manuscript and suggested many changes to them that they adopted and felt had helped a great deal in the production of that book.