Raymond L. Wilder (1896–1982) *received a master's degree in actuarial mathematics from Brown University in* 1921, *then went to the University of Texas intending to complete his actuarial training. Instead he became a student of R. L. Moore and received a Ph.D. in mathematics in* 1923. *He served on the faculty of the University of Michigan from* 1926 *until his retirement in* 1967. *He was President of both the AMS and the MAA, an AMS Colloquium Lecturer and Gibbs Lecturer, a recipient of the MAA Distinguished Service Award, and a member of the National Academy of Sciences. Among his books are* Topology of Manifolds *and* Introduction to the Foundations of Mathematics. *His reminiscences, recorded in* 1976, *are published here for the first time.*

Reminiscences of Mathematics at Michigan

RAYMOND L. WILDER

This is Raymond L. Wilder, Professor Emeritus of Mathematics, speaking on July 24, 1976. At the request of Professor Phillip S. Jones and also of Professor Allen Shields, Chairman of the Department of Mathematics at the University of Michigan, I am making an informal recording of my impressions of my years of active teaching here at the University. I came here in the fall of 1926. That spring, I believe Professor James W. Glover became chairman of the department and (according to the information which John W. Bradshaw gives in his history of the department) "immediately set himself to a task of revivifying the department". The curriculum at that time was of a fairly classical type. It gave a set of courses through the advanced calculus, and I believe some Fourier series. These courses in advanced analysis were given by Professor W. B. Ford. Applied courses in geometry, projective geometry and synthetic geometry I believe were given by Bradshaw. The history of mathematics was represented by Louis Karpinski. All in all it was a very good curriculum, representative of the time. However, it did need modernization and this is one of the first things that both G. Y. Rainich and I set out to undertake when we came here.

It might be interesting to point out Professor Glover's method of going about getting new members of the department. He evidently made dittoed copies of a flyer that he sent around to those he considered promising young mathematicians, inviting them to respond if they felt they might be interested in a position at Michigan. In my own case this flyer came to me while I was at Ohio State University in Columbus, Ohio. Apparently it was thrown on the porch by the mailman and picked up by my oldest daughter who was around two years of age at the time, and she tore it up into small pieces. Later on my wife came out on the porch, found the pieces and put them together again and when I discovered what it was, I did write to Professor Glover. This is how close I came to never coming to the University of Michigan! I am sure that if I had not responded, Professor Glover would not have taken any further action in my case. The result of his research was to bring here Professors G. Y. Rainich, whom we informally called Yuri, and James Nyswander, as well as myself.

I should have mentioned that two prominent people who were at the University at that time, namely T. H. Hildebrandt and Professor Alexander Ziwet, were on the engineering side. At that time, the mathematics department in the engineering college was separate from the L.S.A. department. Hildebrandt was a student of E. H. Moore and showed great promise in real analysis. Alexander Ziwet was not a research man as I understand it, but he was active in the affairs of the American Mathematical Society and saw to it that the library received the foundation of a good collection of mathematical journals and treatises. I also should have mentioned that the department, under the stimulation of Professor Glover and his assistant Harry C. Carver, built up an actuarial program as well as a statistical program. In 1926–1927 I believe Professor Wicksell from Sweden was a visiting professor here in statistics.

After consulting the early catalogs, I find that Professor Rainich introduced courses in differential geometry and relativity in 1926 and I myself introduced a course in analysis situs (the term, originally introduced by Gauss, by which one indicated the subject of topology). I notice in the 1927–1928 catalog that Rainich gave a course in quadratic forms and quadratic numbers. Nyswander gave a course in algebraic theory, Hopkins was giving a course in celestial mechanics of the classical type, and Karpinski a course in the theory of numbers. In the 1928–1929 catalog I notice that I introduced two courses in the foundations of mathematics and Rainich was giving a course in continuous groups. I apparently was also running a seminar in analysis situs, having at that time acquired enough students to justify holding such a seminar.

So far as I can determine it had not been the policy of the department to hold seminars in addition to the regular courses, with exception that Professor Wicksell evidently gave a seminar in statistics during the year that he spent here. In the year 1928–1929, in addition to the seminar which I was giving, there was a seminar on functions of a complex variable given by Rainich, and one in differential equations presumably given by Nyswander; also a seminar the second semester in differential geometry given by Rainich. From then on, as I recall it, the custom of giving seminars became quite common. I might note that although these first seminars apparently received credit and were obviously, or presumably, counted in a man's teaching load, as time went on the number of seminars increased, no credit was given, and also the time given to such seminars was not normally included in a man's teaching load. The teaching load in those days, I think, was around twelve hours. It might vary from eleven to twelve, depending on the number of hours of credit given to a course.

In 1929, two new research men were brought in, namely, Arthur H. Copeland, a Harvard Ph.D., and William L. Ayres, a Pennsylvania Ph.D. Ayres was a topologist and Copeland had apparently specialized in Boolean algebras and foundations of probability. At the end of his history of the department, Professor Bradshaw notes that the number of doctorates which had been given up to 1922 was only eleven, but that in the following eighteen years there were seventy-four doctorates given. That brings us up to 1940. He makes a statement, "increased interest and activity in mathematical research on the part of members of the staff have naturally accompanied this growth", referring to the growth that had occurred during that period to 1940. I don't want to leave the impression, however, that the interests of the department became solely devoted to research. I think it fair to say that all three of us who came in 1926, as well as the later additions in 1929, were generally good teachers, and Rainich in particular was very much interested in the development of the students here at Michigan and gave an unusually large amount of his time to conferring with students. However, we realized that mathematics was not a static thing; it was a growing thing, and in order for the department to take its place among the foremost departments of the country, it was necessary to build up the number of courses in modern mathematics, as well as to keep up interest in what was going on in the journals and in mathematical research generally.

One thing that I must speak of which is not recorded anywhere (certainly in the department records) and which I think had a great deal to do with building up mathematics here at Michigan, was the formation in 1927, a year after we came here, of a Research Club by Rainich and myself. We felt that the Department Club which met monthly in the evening was not accomplishing very much in the development of interests in research. This small club that we founded came to be called "The Small C" as distinguished from the large club, the one that met monthly. However, because we wanted to include only people who were active in research, we did keep it secret and this was perhaps not a good feature of it. It was our practice to meet at one of the members' homes every Tuesday evening. We had a portable blackboard which was taken care of by Professor Ben Dushnik. We had an hour's scientific paper, normally on research being done by a member of the club, sometimes on a mathematical result of great importance which we felt that the members should know about. I don't recall the exact composition of the Small C when it started; I know Rainich and I and also, I believe, Professors Denton (whom Professor Bradshaw mentions in his writing), Dushnik, Donat Kazarinoff, and Shohat were members. In all there were, I believe, eight members of the mathematics department, and one member of the philosophy department, namely, Professor Harold Langford whose specialty was mathematical logic, and three members of the physics department, Professors Otto Laporte, George Uhlenbeck, and Samuel Goudsmit. Professor Rainich took the responsibility of sending out notices of where the meetings were to be held on Tuesday evening. I have endeavored to find any records which he may have left of these meetings, but so far as I can tell, they were all destroyed.

It was our custom whenever a visiting mathematician or physicist of note came to the University to give a talk, to invite him to talk to the Small C, and he was unofficially made a member at that time. I recall now two doctoral students who were in the Small C in that early period, namely L. W. Cohen, who later became head of the Mathematics Panel of the National Science Foundation, and also Edwin Miller, who was very active in mathematical research until his untimely death during the war period. I recall also that Professor T. H. Hildebrandt was made a member a few years after the formation of the club.

In 1934 Professor Hildebrandt was made chairman of the department. This perhaps created a situation which ultimately we felt was not too healthy for the status of the Small C. Since he was a member of it, and since the existence of the Small C inevitably became known to members of the department who were not engaged in research, this led to a general feeling on the part of the latter that the Small C was a political organization and that department affairs were being settled unofficially in its meetings. Now, it is true that during the refreshment period which followed the paper at a meeting, there was some discussion of possible new members of the department, as well as of things that were going on in the department; but so far as settling anything in regard to the department was concerned, the Small C certainly did not do this. By the time Hildebrandt became a member, the Executive Committee system had been introduced in the department. The Executive Committee was composed of five members, in addition to the chairman, consisting of representatives of the graduate division of the department, the Literary College, the engineering side of the department (which had now been combined with the L.S.A. department), and a member-at-large who had a one-year appointment. It was in the Executive Committee that new appointments were made and policies discussed. The only influence that the Small C could have had on this was that inevitably, in addition to the chairman, there would be members of the Executive Committee in the Small C, and anything that was discussed in the Small C might presumably influence the opinions expressed in official meetings of the Executive Committee. However, I believe it was not until 1947 that we agreed that the Small C should be disbanded. We had become well aware of criticisms being made by non-members, but more important, the department by this time had acquired enough new research people that it was impossible to get them all into the Small C and continue our informal way of meeting at one another's houses. So we felt that we should disband and promote as well as we could the introduction of a weekly colloquium to be held in the afternoon by the mathematics department, it being understood that this colloquium should be devoted to research papers of a current nature. The only one who objected to disbanding the Small C that I remember was one of the founders, namely, Professor Rainich. But even he could understand the impracticality of continuing the activities of the group.

I should say something about the effect of World War II on the mathematics department. Of course there was a greatly increased demand for courses during the war, particularly because of the participation by the University in the meteorological program of the Air Force. I recall that we used to have large mathematics classes in the Law Building, and these big classes were cut up into sections later to be handled by instructors and teaching fellows. Periodically the Air Force sent examinations to be held and these were conducted in the large auditorium in the Rackham Building. The problem of increased staff was met by bringing in people from other departments who were mathematically competent, and in some cases using people such as faculty wives who had received master's degrees in mathematics before they were married. I remember that Professor Langford, whom I mentioned in connection with the Small C, was one of those who taught courses in the department. (I suppose that there wasn't much demand for philosophy courses during that time, so that it was easy to secure his services.) At any rate, the department did manage to go through the war years without too much great suffering on the part of the staff, although the increased teaching load was undoubtedly a factor holding back research to some extent.

However, the effects of the war and its aftermath were not confined to these matters. There was, perhaps, a much greater impact made by the introduction very soon after the war, in the later 1940s, of the system of grants for research by the various government agencies. I believe the Office for Naval Research was one of the first of these, and of course later, in addition to the Army, Navy, and Air Force, the National Science Foundation was formed and a system of grants instituted by this agency. I can recall that on the Executive Committee there was considerable discussion about the effect that these grants were going to have. We were particularly worried that recipients of grants would be taken from their teaching, since faculty members, in addition to sabbatical leaves, would be able to take extra leaves because of their grants. It is not easy to oversee the research of a student who is in one place and whose thesis adviser is somewhere else. However, as the years went by I think it was generally conceded that the system of grants was beneficial, especially as student grants ultimately became available. It took a good deal of adjusting and as of now, 1976, government grants seem to be a fixed feature of the university scene. Basing my philosophy on the old "if you can't lick 'em, join 'em", I myself have had grants and certainly these have sometimes made possible things which I couldn't otherwise have done. In particular, I had a grant early in the era of grant disposals, in the year 1949–1950, when I went out to California Tech and wrote the first version of my book, *Introduction to the Foundations of Mathematics*, as well as doing research in topology. So I am not of the opinion that the grant system was an entirely bad influence on university research and development.

There were also new areas of mathematics which owed their stimulation, possibly their existence, to the effects of the war. I remember that both Professors Thrall and Copeland were interested in the new mathematics that was being created in the theory of games and mathematics for the social sciences and, of course, the introduction of the electronic computers was greatly accelerated by the war. If I had the time to do so I could probably take the catalogs and note the evolution in new courses and so on that went on. In my own field of topology there occurred the introduction of courses in algebraic topology and later in differential topology.

Another factor which I believe had a very beneficial influence on the evolution of the department was the Ziwet lectures. These were founded as a result of a bequest to the college by Professor Ziwet in 1929. The first Ziwet lectures were given in 1936 by Professor Edouard Cech. Professor Cech was a Czechoslovakian topologist who was responsible for the so-called Cech homology theory and was also known for other works in the field. He lectured for a two-week period, setting the pattern for later Ziwet lecturers. The later Ziwet lectures were given by such prominent mathematicians as Professor John von Neumann, Saunders Mac Lane, Claude Chevalley, Henry Whitehead, and others whose names I don't recall at this particular time. I think we had one or two lecturers a year until the war started; and afterwards, at intervals of four or five years. I think these lecturers had a very beneficial influence on the department because the lecturers would mingle both professionally and socially with members of the department during their visits, so that they really had quite an influence over the long range. I might also say something about the emergence of the *Michigan Mathematical Journal*, which is now one of the best mathematical journals publishing research articles. During the late 1920s, a committee was appointed by Professor Glover, consisting of Rainich as chairman, and Harry Carver and myself, to look into the possibility of establishing such a journal. We turned in a report to the chairman, and I believe that the idea of financing such a journal was put in the alumni magazine, along with some other worthwhile projects, as something that might attract some alumnus or other. However, nothing came of this, and I believe that after the war when the journal was really established, we looked for this report that we had gotten out earlier and couldn't find it. (As a matter of fact, at that time we were unable to find any of the department records that accumulated during Glover's administration.) There is no question, however, that the establishing of the journal has enhanced the reputation of the Michigan mathematics department and that it has justified whatever it has cost to run such a journal.

I think I should say a few words about the policy concerning the way in which courses were assigned instructors. When I came here in 1926, I recall that, as I think I mentioned before, Professor Bradshaw was teaching the geometry courses and that Professor Ford was teaching the courses in the classical analysis. The policy seemed to be that whoever represented a field was to teach the courses in his field. Now before I came here I had taught courses in such subjects as Fourier series. I had gone to considerable trouble to set up courses of this type at Ohio State, and I remember that I was rather taken aback when I found that I could not teach such courses here at Michigan. As a matter of fact, I found myself teaching courses in mathematics of finance (because of my previous training in actuarial mathematics), some courses in elementary algebra and trigonometry, and graduate courses and seminars. This went on, as I recall it, for quite a few years. This pattern may have been a hangover from the olden days; I don't know how widespread it was in American universities. Staffs were not large and presumably there might not be more than one man in a given field. I recall that at the University of Texas, R. L. Moore made it a policy not to let anyone teach the courses in his field of point set topology. As a matter of fact, if a student who had earned his degree under Moore didn't go on to another institution he just stayed at Texas and had to teach other kinds of courses. That was a policy that Moore had established for himself there. So the pattern may have been quite general. However, at the University of Michigan there has been clearly a gradual weaning away from this idea, particularly taking advantage of the fact that the staff increased so much in size over the years. It was no longer considered, after a number of years, that a man who belonged to a field which was already represented here could not be hired. For instance, I had been here only three years when W. L. Ayres was given an assistant professorship in 1929. This was at my request. However, it was ten years later, I believe, before I brought in another topologist, namely, Sammy Eilenberg, who came over from Poland just as Hitler was about to strike that country. This was partly a result of wanting to save a life of a person, and at the same time to build up the department here. Eilenberg came here as my student and the Graduate School accepted him on that basis, although there was some opposition from Professor Peter Field who was at the time on the graduate board and felt maybe I was bringing in Eilenberg as a new member of the faculty, which I did not have in mind at that time. However, since the war affected the United States soon after, and, as I've already mentioned before, teachers were in demand, it was only natural that Eilenberg (who knew English very well) was given courses to teach, and then he ultimately became a regular member of the staff. We also brought in a former student of mine, not one of my doctorates, but a man who had done his first research under my direction here at Michigan, namely, Norman Steenrod. I don't recall the year he came, probably around 1947. For a little while, then, we had four topologists in the department; viz., Ayres, Eilenberg, Steenrod, and me. Ayres left in 1941 to accept the mathematics chairmanship at Purdue, leaving three of us. However, there was no question about the teaching of courses. The courses in topology were passed around one to another, according to each individual's desires and what he felt he was competent to teach. Later on we brought in Hans Samelson. Now I am beginning to forget the order of appearance; I think perhaps Moise and Young came next, and then Raoul Bott. The field of topology has been gradually built up here by this policy of bringing in new material in the field and making sure that all aspects of this rapidly growing field (topology had perhaps its greatest growth during this period) were represented, and different individuals had chances to teach the aspects of the subject in which they were most interested. I don't know whether this influenced the department in any way to do this in other fields, although it may have.

I believe that if I were asked to describe the evolution of the mathematics department at Michigan, I would divide it into three periods: in the first period I would place all of the development up to 1926 when Professor Glover became chairman. I think that at that time the bringing in of new material, particularly of the calibre of Rainich, was greatly responsible for the rapid development from that point on. Then the next period, I think I would designate as from 1926 up to and including World War II. I think in the third period I would place everything from the end of World War II up to the present, calling this perhaps the modern period. This way the department would have its early period, a second period of rapid development, and then a modern period. Certainly in the modern period the rapid development has continued; during this period the department has had the benefit of grants from the federal government and other sources, and this has been an accelerating factor. Of course, all designation of *periods* in the development of an institution is bound to be somewhat arbitrary. I have not wanted to imply that during the first period up to Glover's succession of the chairmanship there wasn't any research done. For instance, I do feel, however, that the curriculum at that time was representative chiefly of the mathematics of the nineteenth century. However, I do not know well what the contents of all the courses were then. For example, I should imagine that whenever Hildebrandt taught the course in real functions, he certainly must have taken into account such subjects as Lebesgue integration, since he, being a product of the E. H. Moore School at Chicago, certainly was up-to-date in these subjects. Possibly in statistics many twentieth century ideas were brought in. However, I cannot speak with knowledge of that period, of course, since I didn't come in until 1926 myself. I do think that the curriculum at that time was a good curriculum, a strong curriculum. I have no idea what the standing of the department was; i.e., how it rated nationally. As Bradshaw pointed out in his history, there were doctorates given earlier. I don't know who gave these, but I would guess offhand that they were probably done by such staff members as W. B. Ford, perhaps Louis Hopkins in celestial mechanics, and possibly Hildebrandt.

I am going to look now at the items or questions which were raised in a letter to me under date of February 4, 1976, by Professor Phillip Jones. I think I've already touched upon some of these. In his "Section I", he asks, "What was the status of the department when you arrived? Item a, adequacy and modernity of the course offerings and of the staff." I think I have touched upon this fairly well, certainly as far as I could. I failed to mention Karpinski, who was strong in the history of mathematics, no doubt had a good national standing at the time, and probably had been responsible for some of the doctorates which Bradshaw mentioned. Referring again to Professor Jones' letter, major item 2 asks, "What were major changes over the years and the causes? Item a, hiring and promotion policies." I think I have already touched upon this topic. The policy has always been, as I recall it, to hire people who were both good teachers and capable of advancing the frontiers in their own field by their research. There has been a very liberal policy all along, in my opinion, regarding the fields represented by the new appointments. I haven't said anything about applied mathematics. The development of mathematics generally, in this country, during what I call the first period, was gradually from what was considered "applied" (a practical mathematics) to "pure" mathematics. So that during the second period, the University of Michigan, as in most mathematics departments, established itself in what we call research in pure mathematics. About the time of the war, I believe, there was some agitation for getting in more people in applied mathematics. Applied mathematics up to the time of the war seemed generally oriented towards the needs of the engineers and was not, as I recall it, a very strong representative of what we were coming to think of as applied mathematics in the modern sense. I recall distinctly one instance that might throw light on this, and this concerns Professor Friedrichs, who was a Ziwet lecturer in 1946. In inviting Professor Friedrichs here at the time, we felt that since he was one of the most outstanding and most promising people available in modern applied mathematics we should invite him and consider the possibility of offering him a position here. Now I know that there was a considerable discussion of this on the Executive Committee in the department, but it was finally turned down, and I have felt that this was perhaps a mistake. It is well known that Professor Friedrichs went to New York University and became one of the leading lights in the Courant Institute, and I think that the University of Michigan missed out at that time on a good chance to enhance its reputation in the field of applied mathematics.

Professor Jones' second item, 2b, concerns the development of seminars, who stimulated them and when. I think I've already touched upon this and indicated that Rainich was particularly active in this regard. Educated in both Russia and Germany, he had a very broad knowledge of mathematics and undoubtedly enjoyed more the development of students via seminars, than doing his own research. The department probably went somewhat "overboard" by the time the third period developed, in that we had about twenty seminars going at one time, and I began to feel that maybe the students were spending too much time in seminars and not enough time on their own mathematical research. It was not unusual, I think, for a student to spend more time in seminars and reading in the library than doing his own thesis work. In regard to Professor Jones' third item, 2c, "changes in funding", I believe I already touched on this in my remarks regarding government grants. The funding here was, of course, that of what I've called period three, i.e., postwar period, and is now a permanent, or semipermanent, feature of the mathematical scene.

The next item, 2d, "changes in teaching load, hours, levels". When I came here in 1926, I believe the teaching load was from twelve to sixteen hours per week. Instructors were given sixteen hours, I believe. Possibly those of professorship rank had twelve-hour loads. I recall distinctly what happened in 1932 when I was asked to give the Symposium Lecture at the Chicago Section of the American Mathematical Society. I felt that in order to do an adequate job I ought to have a little more time at my disposal to work in the General Library. These Symposium Lectures are no longer given, but they were a feature of the spring meeting in Chicago of the Midwestern Section of the American Mathematical Society. There were two hour-lectures; they were given in the afternoon, one lecture for an hour, then an intermission, and then one lecture for another hour. One didn't accept the responsibility of giving one of these lectures very lightly. Unfortunately that year was during the period 1930–1932 when Professor Field was acting chairman of the department, during Glover's absence. I asked Field if I could have my teaching load reduced to eight hours while preparing my Symposium Lectures and he said no, it was impossible to give time off for the writing of advanced papers; these, as I recall, were his exact words. I presume this was a general attitude at that time. What one did in his research was something *extra*, something outside the regular academic program. That naturally has changed.

Today I think most of the larger universities have teaching loads of six hours per week and this is general for the whole staff, not just for the professors.

Passing to Professor Jones' fourth item, labeled "miscellaneous, item a, how did we happen to build strength in topology?", I think I have covered that. I was the first topologist here so that I feel as though the topological program here was sort of my baby. Item 2b, "was it true that some of the courses assigned to some topologists turned out to be topology?" Now this is a very interesting question and would not have occurred to me, but it should have occurred to me perhaps, for I recall when I started my foundations course, I found that despite the description of the course in the catalog, many of my colleagues thought that I was giving a course in topology. As a matter of fact, I remember that during Professor Field's incumbency of the chairmanship (this was about three or four years after I started the course), he suggested at one time when we were discussing courses for the following year that I give the foundations course to Professor Ayres to teach. Well it was immediately apparent he thought the foundations course was a topology course, and I explained that it wasn't, and I believed that anyone who taught the course should have had some interest in, or some grounding in mathematical logic, the theory of the infinite, etc. Though this is just a sample, it may be that in the later periods there was some feeling of this type. Particularly, perhaps, when a topologist taught a course in real analysis, he might bring in more topology than would normally be brought in, wherever it was applicable. However, I didn't know of any cases where the course turned out to be topology; I think that would be an exaggeration.

Coming now to Professor Jones' third item, 3c, "when, why, how did a conscious effort to bring in foreigners develop?" He gave examples, Eilenberg, Rothe, Brauer, and so forth. Well, I suppose that when Glover brought Rainich here there was no thinking on his part that he was bringing in a foreigner. This is my firm impression. Certainly when I induced the administration to bring in Eilenberg, I wasn't thinking of him as a foreigner; I was thinking of him as a *mathematician*. I think in general there has been no discrimination in this regard, but possibly I am wrong. I believe that we have been quite fortunate at Michigan in the foreigners that we have brought in, and that they did not feel that it was their sole function to do research and a small amount of lecturing. They generally participated very little, however, in such things as committee work (Rainich was an exception), which is one area certainly where I think I've heard the criticism made that foreigners would not in general be doing their part. It was not so much that they would be unwilling to do so, in most cases, but simply that they were not familiar with our ways in general and they couldn't be expected to serve efficiently on committees. I believe that there may have been a feeling around the country that the University was taking in foreigners in order to make positions available to people who otherwise could not get positions. In other words, it was deemed a sort of charitable gesture. I don't recall ever having this feeling in the case of the University of Michigan. I remember one case where a Japanese mathematician in this country had no university position in prospect; his name was Kodama. Realizing that he was a good mathematician who was in somewhat desperate straits, I spoke to the chairman about getting him here. However, I don't think I did this because he was a foreigner, so much as because I thought he was a good mathematician who was available. Incidentally, we did not keep him as a permanent member of the staff; he may have been here around two years. I recall also that I was involved in one other case, namely, Rubens Lintz, a Brazilian who seemed by his publications to have had considerable ability and who I felt would profit greatly by coming to this country. We brought him here on my contract; I don't recall whether it was an NSF or Air Force contract. Later, however, I believe we did give him some teaching. Again, we did not keep him. Afterwards he went to Canadian universities. Accordingly, my judgment is that generally we did not bring a man in because he was a foreigner.

Coming to Jones' next item, item d, "who stimulated and supported Michigan conferences in topology, complex variables, etc. The University, NSF, donors, University Press?" Well, here I can only speak for the conferences in topology of which I recall two. One of these was the topology conference of 1940, for which I recall talking to Graduate Dean Yoakum and asking for help to bring outstanding topologists here. I remember that he gave me a budget of $1,000. The war made it impossible for foreign topologists to come, although some did send abstracts of papers. We did have a good representation of topologists from the United States, and I remember I turned back around $35 of the $1,000. I don't recall that we gave anyone an honorarium, although we did help with travel expenses. I believe among the present members of our staff who first came to Michigan at the time of this conference were Professor Wilfred Kaplan and Professor Erich Rothe, who later became permanent members of our staff. The University Press later published a volume called *Lectures in Topology* which contained most of the papers, in complete or abstract form, which were given at this conference. Not a large edition was published. I don't know how large it was, maybe 300 or possibly 600 copies. They were all sold out shortly, and later the press felt that perhaps the demand would warrant publishing a new edition, or new printing. The department chairman, whose advice was sought, felt that this was perhaps not warranted, that there would not be enough demand. However, I can recall getting requests in recent years for copies of this volume which, of course, was no longer available. I don't know who financed the printing; I don't think it came out of my $1,000, but probably it was financed by the University Press itself. Then there was a topology conference in 1967 which was conceived of as being in my honor at the time of my retirement, and which I believe was funded by the National Science Foundation. Professor Frank Raymond can tell more about this so far as its funding, etc. was concerned. I don't know about the conference in complex variables, and I presume that Wilfred Kaplan could give information in this regard. Neither do I know about possible other conferences.

Going on to Professor Jones' item e, "how was the *Michigan Mathematical Journal* formed?"; I believe I have really covered that. Item 4, also labeled, "miscellaneous", asks, "what do you regard as interesting and/or significant about the history of the University of Michigan's Mathematics Department?" This is a question that requires some reflection and possibly I haven't given it enough. I have, in thinking of this question, set down what I considered reasons for the growth and reputation of the Michigan mathematics department: First, the policy of hiring people who were good in both teaching and research. I know of several cases where people did not gain tenure because of the fact that their teaching did not measure up to our standards, and, of course, I also know of cases where people were let go that we had considered to be promising in research, but who later did not live up to their promise. Secondly, I have put the building up of a good library. This is something that Michigan is noted for amongst mathematicians the world over, I think. We have here at the University of Michigan a collection of books going way back in history, and which ordinarily could not be found anywhere except in places like the John Crerar Library, Library of Congress, and Harvard University Library, and possibly the Brown University Library, to name some that come to me offhand. I don't think this is due to any one person, but certainly Alexander Ziwet is to be credited very largely for this. Pick at random any book which was published during the first part of the century or prior thereto, and you are likely to find Alexander Ziwet's signature in it, as having donated it to the library, and there is no question that the support of the University in giving funds for the library is to be credited in good part for the library here. Karpinski used to make periodic visits to Europe to buy books, both for himself and for the library. Thirdly, I think that the influence of the Small C, which I have already mentioned, had considerable to do with the building up of the department. I think it was a healthy influence and until the beginning of what I call period three, I think it contributed indirectly to the bringing to the University of outstanding people. Fourthly, I want to mention the policy of inviting eminent visitors. The using of the Ziwet bequest for bringing outstanding lecturers who could spend a period of around two weeks here has certainly had a great influence, and in addition to that, of course, there has been the bringing in of lecturers who have given one or two lectures, possibly paid for by somebody's grant. This kind of thing is stimulating to the department and it enhances the reputation of the University. Fifthly, the expansion in fields such as algebra, analysis, statistics, topology, foundations of mathematics, and so on, contributed much to the department's reputation. I have not gone much into statistics because it is not my field of interest, and I think that it will be found later that Professor Harry Carver would be willing to contribute something in this regard.

Finally, I again want to credit my colleague, Professor Rainich, who gave so much of himself to stimulating the interests of students, suggesting innovations, and giving advice to the chairman. Generally, I think the chairmen, and I think this is particularly true during Professor Hildebrandt's chairmanship, have been anxious to have good advice. I won't say that the chairman always acted on it, but this is not to say that he didn't accept advice generally and his decisions were usually in the best interest of the department.

I think that I have now covered most of the items that I had in mind when I started this oral history, if one can call it that. I realize that I may have made some mistakes here and there. Generally, however, I think what I have said fairly represents my memory and opinions, and if there are any points at which amplification is needed and I am able to do so, I would be very glad to cooperate.