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The School of Mathematics at the Institute for Advanced Study

ARMAND BOREL

In the late twenties, Abraham Flexner, a prominent figure in higher education, had made an extensive study of universities in the U.S. and Europe and was extremely critical of many features of American universities. In particular, he deplored the lack of favorable conditions for carrying out research. In January 1930, while preparing for publication an expanded version of three lectures he had given in 1928 at Oxford on universities, he saw in the New York Times an article on a meeting of the American Mathematical Society (AMS), in which Oswald Veblen, professor at Princeton University, was quoted as having stated that America still lacks a genuine seat of learning and that American academic work is inferior in quality to the best abroad. He immediately wrote to Veblen, saying there was not the slightest doubt in his mind that both statements were true and hoping that Veblen had been correctly quoted. In his answer, Veblen confirmed these views, described the context of his remarks and wrote in conclusion:

> Here in Princeton the scientific fund which we owe largely to you and your colleagues on the General Education Board, is having an influence in the right direction, and I think our new mathematical building which is going to be devoted entirely to research and advanced instruction will also help considerably. I think my mathematical institute which has not yet found favor may turn out to be one of the next steps. Anyhow it seems to me to fit in with the concept of a seat of learning.

The first Faculty of the School of Mathematics (minus J. von Neumann) with the second Director. From left to right: J. Alexander, M. Morse, A. Einstein, F. Aydelotte, Director, H. Weyl and O. Veblen.

(Photograph courtesy of the Institute for Advanced Study.)

Here Veblen was alluding first to the efforts, initiated by Fine and pursued with the help of Eisenhart and Veblen, to improve research conditions in his department and to the construction of what became Fine Hall; second to a plan for an "Institute for Mathematical Research" he had outlined and presented (without success) around 1925 to the National Research Council and to the General Education Board of the Rockefeller Foundation. It was to consist of four or five senior mathematicians who would devote themselves entirely to research, their own and that of some younger men, and of some younger mathematicians. Members would be free to give occasional courses for advanced students. It could operate within a university or be entirely independent of any institution.[1]

Shortly before, Flexner had been approached by two gentlemen who were surveying medical education on behalf of two persons who wanted to use part of their fortune to establish and endow a medical college in Newark. Since Flexner was an authority on medical education in the U.S., it was only natural to seek his counsel. He advised against it, explaining why in his opinion there was no real need for a new institution of the type they had in mind. Instead, he showed them the proofs of his book on universities and outlined his plan for an institution of higher learning, where scholars would pursue their researches and interests freely and independently. They were so fascinated by it that they swayed the potential donors, namely Louis Bamberger and his sister, Mrs. Felix Fuld, born Caroline Bamberger, convinced them to look into this possibility and soon introduced them to Flexner. This initiated a series of discussions and a correspondence extending over several months, at the end of which the Bambergers agreed enthusiastically to back up Flexner's plan, on condition that he would be the first director. A certificate of incorporation for a corporation to be known by law as the "Institute for Advanced Study - Louis Bamberger and Mrs. Felix Fuld Foundation" was filed with the state of New Jersey in May 1930 and the New York Times announced in June the creation of an Institute for Advanced Study, to be located in or near Newark, on a gift of $5 million from Louis Bamberger and his sister, Mrs. Felix Fuld. Veblen learned about it for the first time through that press release, although there had been a little further correspondence between the two about the idea of an Institute, but carried out *in abstracto*, at any rate on Veblen's side. He wrote immediately to Flexner that he was greatly pleased and he expressed the wish that this Institute would be located in the Borough or Township of Princeton "*so that you could use some of the facilities of the University and we could have the benefit of your presence.*" This heralded an increasing involvement of Veblen with this project, first as a consultant, then

[1] For this and the development of mathematics in Princeton until WW II, see William Aspray's article in *A Century of Mathematics in America, Part II* (editor, P. Duren, with assistance of R. A. Askey and U. C. Merzbach), Amer. Math. Soc., Providence, R.I., 1989, pp. 195–215.

as a professor having the primary responsibility for the building up of the School of Mathematics.

The Institute was eventually to consist of a few schools, but Flexner decided early on to start first with one in mathematics, because "mathematics is fundamental, requires the least investment in plant or books and he could secure greater agreement upon personnel than in any other field".[2] He began to make extensive inquiries in the U.S. and in Europe as to who would be the best choices for a faculty in mathematics. Among American mathematicians, the two most prominent names were those of George D. Birkhoff and Veblen. Flexner started with the former, on the theory that Veblen was already in Princeton anyhow. An offer was made, at an extremely high salary and accepted in March 1932, but Birkhoff asked to be released eight days later. After further inquiries, Flexner came to the conclusion that: "*If the Princeton authorities agreed willingly and unreservedly, we could not do better than to select Veblen.*" They did so quickly, and Eisenhart telegraphed to Veblen in June:

> Have talked with those concerned and they approve. Congratulate you heartily. Look forward to big things.

1932 was marked by extensive travelling, wide ranging consultations, and discussions, correspondence and negotiations with Veblen, Einstein and Weyl. (Of course, no outside advice was needed in the case of Einstein, and Flexner forged ahead as soon as he understood that he might be interested.) In October two faculty nominations were announced, that of Veblen, already effective October 1st, 1932 and that of Einstein, effective October 1st, 1933 (as well as the nomination of Walther M. Mayer, the then collaborator of Einstein, as an "associate"). It was also announced that the new Institute would be located in or near Princeton (a shift formally proposed in April 1932) and would be housed temporarily at Fine Hall. The school would officially begin its activities in Fall 1933, but in fact, during the academic year 1932–1933, Veblen already conducted a seminar in "Modern Differential Geometry."

It is well-known that Einstein was enthusiastic from the beginning ("Ich bin Feuer und Flamme dafür," he had stated to Flexner) and excessively modest in his financial requirements, but the negotiations were not all that smooth. In 1933 Flexner learned that Einstein had also accepted a professorship in Madrid and one at the Collège de France. Since their residence requirements were minimal (in the former case, nonexistent in the latter), while those of the Institute were for him only from October to April 15, Einstein did not see any incompatibility; on the other hand, if Flexner felt otherwise, he would agrée to terminate the arrangement with the Institute.... The Madrid offer also included the right to name a professor and Einstein tried to use it as

[2]A. Flexner, *I remember*, Simon and Schuster, New York, 1940, pp. 359–360.

a leverage to secure a professorship at the Institute for W. Mayer (without success). In summer of 1933, Flexner had asked whether Einstein could arrive soon enough to participate in a general organizational meeting of the members of the school on October 2nd. Einstein felt he could not because this would entail spending one month away from W. Mayer, which would be too detrimental to his work. He arrived on October 17. He was reminded of that when he complained later that he had not been consulted about invitations and stipends. The collaboration with Mayer was over within a few months.

In Europe, the two names of mathematicians mentioned to Flexner above all others were those of G. H. Hardy and H. Weyl. While in Cambridge, Flexner got readily convinced that there was no way to lure Hardy away from Cambridge and he turned his attention to H. Weyl. (Hardy and Einstein, as well as J. Hadamard, had singled out Weyl as the most important appointment to be made from Europe.) Both he and Veblen, who had received an offer in June and was in Europe at the time, began discussing the matter with Weyl. He was interested from the start, in spite of strong misgivings about leaving Germany, and immediately expressed some desiderata about the school. First he thought it was absolutely necessary to add to Einstein, Veblen and himself a younger mathematician, preferably an algebraist. Weyl commented (in a letter to A. Flexner, dated July 30, 1932):

> The reason lies with the plans for filling the three main positions. By his personality, Veblen is certainly the most qualified American one can wish as the guiding spirit in an institution such as the one you have founded. But he is not a mathematician of as much depth and strength as say Hardy. The participation of Einstein is of course invaluable. But he pursues long-range speculative ideas, the success of which no one can vouch for. He comes less under consideration as a guide for young people to problems which have necessarily to be of shorter range. I am of a similar nature, at any rate I am also one who prefers to think by himself rather than with a group and who communicates with others only for general ideas or for a final well-rounded presentation. Therefore I put so much value on having a man of the type of Artin or v. Neumann.[3]

[3]Der Grund liegt mit in der Art der in Aussicht genommenen Besetzung der drei Hauptstellen. Veblen ist zufolge seiner menschlichen Qualitäten sicher der geeignetste Amerikaner, den man sich als führenden Geist in einer solchen Institution wie der von Ihnen gegründeten wünschen kann. Aber er ist doch nicht ein Mathematiker von ähnlicher Tiefe und Stärke wie etwa Hardy. Einsteins Mitwirkung ist natürlich unbezahlbar. Aber er verfolgt spekulative Ideen auf lange Sicht, deren Erfolg niemand verbürgen kann. Als Führer junger Leute zu eigenen, notwendig auf näher gesteckte Ziele gerichteten Problemen kommt er weniger in Betracht. Ich bin von ähnlicher Natur, jedenfalls auch Einer, der lieber einsam als mit einer Gruppe gemeinsam denkt und mitteilsam nur in bezug auf die allgemeinen Ideen oder in der fertigen gerundeten Darstellung. Mit darum lege ich so viel Wert auf einen Mann vom Typus Artin oder v. Neumann.

In fact, this was important enough to Weyl that Flexner included in his official proposal to him: "*the understanding that when the right person has been found, an algebraist of high promise and capacity will be appointed*". Later Weyl also pointed out the necessity for him to be allowed to give now and then regular courses. He was of course assured he would be welcome to do so, and he accepted in principle the offer in December 1932. But then, in three successive telegrams on January 3, 4, and 12, 1933 he withdrew, then accepted "irrevocably" ("unwiderruflich") and withdrew again. Later on he apologized profusely, explaining he had not realized he was suffering from nervous exhaustion. In his last telegram, he had given as his reason that he felt his effectiveness was tied to the possibility of operating in his mother tongue (a worry still faintly echoed in the foreword to his *Classical Groups*). But the deterioration of the conditions in Germany, in particular the passing of laws not only against Jews, but also against Aryans married to Jews (his case) made his leaving Germany all but unavoidable and in the course of the year he accepted a renewed Institute offer and began his activities at the Institute in January 1934.

The year 1933 also saw the addition to the school faculty of James Alexander and John von Neumann. It had been agreed between Eisenhart, Flexner, and Veblen that an offer would be made to either Lefschetz or Alexander, who both wanted the appointment. The choice fell on the latter, for reasons I have not seen stated anywhere. I have heard indirectly that Eisenhart had said he could more easily spare Alexander than Lefschetz. In view of the much greater involvement of the latter in all the activities of the department, this seems rather plausible. It is also well-known that later Lefschetz was not stingy with critical remarks about Veblen or the Institute. (In 1931, Flexner had asked his views first on the desirability, nature and location of an Institute and second on whom he would choose in mathematics, were he asked to do so. His answer to the second question was Veblen, Alexander and himself from Princeton, Morse and Birkhoff from Harvard; from Europe, he would add above all Weyl, but, since he was holding the most prestigious chair in mathematics in the world, there was no chance to attract him.) J. von Neumann had been half-time professor at the University for some time and the University was trying to make other arrangements. Veblen had suggested to offer him a position at the Institute but at first Flexner was reluctant to take a third mathematician from Fine Hall. However, after Weyl redeclined and after a further conference between von Neumann, Eisenhart, Veblen, and Flexner, an offer was made and quickly accepted. It was also agreed that the two institutions would, henceforth, jointly publish (and share the financial responsibility for) the *Annals of Mathematics*, with managing editors Lefschetz (who had been one since 1928) and von Neumann.

The appointment of Marston Morse in 1934, effective January 1st, 1935, brought to six the school faculty, which was to remain unchanged for the next ten years. To have assembled within three years such an outstanding faculty was an extraordinary success by any standard. In a report to the trustees of the Institute in January 1938, Flexner credited for this achievement Veblen and the help received from the University, in particular from L. P. Eisenhart, then dean of the faculty.

It was, of course, a tremendous boost for the development of the school that it could function in the framework of an outstanding department, strongly committed to research, and make full use of its facilities, vastly superior to those of any other mathematics department in the country. President Hibben and Eisenhart felt that the development of the Institute would be mutually beneficial, although the Institute was offering unique conditions for work, superior salaries, and therefore might again be successful in attracting faculty members besides Veblen. But others in the university community apparently had different opinions, so that, after the third appointment from the university faculty, Flexner and some trustees, in particular L. Bamberger, felt they had to assure the university authorities they would not in the future offer positions to Princeton University professors. As far as I can gather from the record available to me, they did so early in 1933 in one conversation with Acting President Duffield, (Hibben was retired by then). Whether this was meant for a limited time or forever, I do not know. I also have no knowledge of an official written statement by the Institute to that effect, nor of one by the University taking cognizance of such a commitment. On the contrary, the only university document of an official character on this matter I know of (prior to 1963, see below) takes a completely different position. To be more precise, L. P. Eisenhart had written to A. Flexner on November 26, 1932:

> I agree with you that the relationship of the Institute and our Department of Mathematics must be thought of as a matter of policy extending over the years. Accordingly I am of the opinion that any of its members should be considered for appointment to the Institute on his merits alone and not with reference to whether for the time being his possible withdrawal from the Department would give the impression that such withdrawal would weaken the Department. For, if this were not the policy, we should be at a disadvantage in recruiting our personnel from time to time. If our Trustees and alumni were disturbed by such a withdrawal, as you suggest, they should meet it by giving us at least as full opportunity to make replacements intended to maintain our distinction. The only disadvantage to us of such withdrawals would arise, if we were hampered in any way in continuing the policy which has brought us to the position which we now occupy. This policy has been to watch the field carefully and try out men of promise at every possible opportunity. If it is to be the policy of the Institute to have

> young men here on temporary appointment, this would enable us to be in much better position to watch the field.
>
> In my opinion the ideas set forth are so important for the future of our Department that it is my intention to present them to the Curriculum Committee of our Board of Trustees at its meeting next month, after I have had an opportunity to discuss them further with you next week.

Accordingly, Eisenhart presented on December 17 to the Curriculum Committee of the Board of Trustees a statement "on certain matters of policy in connection with the relation of Princeton University to the Institute", a copy of which was kindly given to me by A. W. Tucker. One paragraph reproduces in substance, even partly in wording, the first one quoted above. In conclusion, Eisenhart states that he is presenting this statement "with the expectation that you will approve of the position which I have taken...". It was indeed "approved in principle" by the committee. Obviously the latter was empowered to do so and to speak in the name of the Board of Trustees. Had it been solely advisory, Eisenhart could only have asked the committee to recommend to the board that it approve of his position. I am not aware of any other statement by university authorities addressing this question, again prior to 1963.

As already mentioned, Eisenhart was at the time dean of the faculty. Tucker pointed out to me that, in the organization of the University, this position was next in line to the presidency and that there was in fact no president in charge at that time: Hibben had retired in June 1932 and Dodds would be nominated and become president in late spring 1933. During the academic year 1932–1933, there was only an acting president, namely the Chairman of the Board of Trustees, E. D. Duffield, living in Newark, who mainly took care of off-campus, external affairs. Under those circumstances, Eisenhart was in fact addressing the Curriculum Committee as the chief academic officer of the University.

Although Flexner had not mentioned it in his formal report, he was of course acutely aware of another powerful factor for the rapid growth of the Institute, namely the anti-Semitic policies of the Nazi regime, without which the Institute could hardly have attracted Einstein, Weyl, and von Neumann. This was in fact only the beginning of the Institute's involvement with the migration of European scholars to the U.S. It is a well-known fact that Veblen played a prominent role in helping European mathematicians who had to leave Europe to relocate in the United States.[4] He, Einstein, and Weyl,

[4]See in particular the articles by L. Bers, D. Montgomery and N. Reingold in *A Century of Mathematics in America, Part I* (editor, P. Duren, with assistance of R. A. Askey and U. C. Merzbach), Amer. Math. Soc., Providence, R.I., pp. 231–243, pp. 118–129, pp. 175–200, respectively.

through a network of informants, were well aware of many such cases and often aided in a crucial way by offering first a membership, sometimes with a grant from the Rockefeller Foundation.

At the official Institute opening on October 1, 1933, the school already had over twenty visitors. The level of activities was high from the beginning. While emphasizing the importance of the freedom to carry on one's own research, and the opportunity of making informal contacts and arrangements, the early yearly *Bulletins* issued by the IAS list an impressive collection of lectures, courses and seminars. Among those given in the first four years, let me mention: A two-year joint seminar on topology by Alexander and Lefschetz, followed by a two-year joint course on topology, a joint seminar (extended over several years) by Veblen and von Neumann on various topics in quantum theory and geometry, a course and a seminar by H. Weyl on continuous groups (the subject matter of the famous *Lecture Notes* written by N. Jacobson and R. Brauer), followed by a course on invariant theory, courses and seminars by M. Morse in analysis in the large, a two-year course by von Neumann on operator theory, lectures on quantum theory of electrodynamics by Dirac, on class field by E. Noether, on quadratic forms by C. L. Siegel, and on the theory of the positron by Pauli. In 1935 H. Weyl started and for a number of years led a seminar on current literature. There was also of course a weekly joint mathematical club. The membership steadily increased and Veblen could state around 1937 that in Fine Hall there were altogether approximately seventy research mathematicians and an intense activity. This figure included the members and visitors of the University, too. There was no physical separation in Fine Hall between the two groups, which intermingled freely.[5] Many faced the familiar dilemma of having to choose between attending lectures or minding one's own work. There were also some grumblings that all this was too distracting for the graduate students. The trustees, mindful of the financial aspect, were asking for some limitation and even a reduction of the number of members; Veblen apparently was not too receptive. Almost from the start, Princeton had become a world center for mathematics, the place to go to after the demise of Göttingen.

That the Institute had in this way a considerable impact on mathematical research in Europe and in the United States needs hardly any elaboration. Less evident, and maybe less easy to imagine nowadays, is its role in the improvement of the conditions in American universities by the sheer force of the example of an institution providing such exceptional conditions and opportunities to faculty and visitors. In 1938 Flexner was pleased to quote to the trustees from a letter written to him on another matter by the secretary of the AMS, Dean R. G. D. Richardson of Brown University: "... *The Institute*

[5]For many recollections about Fine Hall at this time, see *The Princeton Mathematics Community in the* 1930*s. An Oral History Project*, administered by C. C. Gillespie edited by F. Nebeker, 1985, Princeton University (unpublished, but available for consultation).

has had a very considerable share in the building up of the mathematics to its present level.... Not only has the Institute given ideal conditions for work to a large number of men, but it has influenced profoundly the attitude of other universities."

The School of Mathematics developed along lines certainly consonant with the vision of the founders, as outlined in the first documents, but not identical with it. Underlying the original concept was a somewhat romantic vision of a few truly outstanding scholars, surrounded by a few carefully selected associates and students, pursuing their research free from all outside disturbances, and pouring out one deep thought after another. Einstein, Weyl, and Veblen soon decided they were not quite up to that lofty ideal and that the justification for the Institute would not be just their own work but, even to a much greater extent, to exert an impact on mathematics, in particular mathematics in the United States, chiefly through a vigorous visitors program. The visitors (called "workers" initially, "members" from 1936 on) were to be mathematicians having carried out independent research at least to the level of a Ph.D. and to be considered on the strength of their research and promise, regardless of whether or not they were assured of a position after their stay at the Institute. Furthermore, their interests did not have to be closely connected to those of one of the faculty members. Originally it was intended that the Institute would also have a few graduate students (but no undergraduates) and would grant degrees. It was officially accredited to do so in 1934. But already then, Flexner stated that it had been done because this seemed a wise thing to do, but it would not be a policy of the Institute to grant degrees, earned or honorary. Indeed, it has so far never done so. This view was confirmed in the 1938 issue of the yearly *Bulletin*, which stated that the Institute had discarded undergraduate and graduate departments on the ground that these already existed in abundance.

In short, the School of Mathematics had very early taken in many ways the shape it still has now, albeit on a different scale, at any rate for the visitors program. It was called School of Mathematics, although its most famous member was not a mathematician. In fact, when asked which title he would want to have, Einstein chose Professor of Theoretical Physics. However, it had been understood from the start that the school would also include theoretical physics. Internally, it was sometimes referred to as School of Mathematics and Theoretical Physics and there were always some visitors specifically in theoretical physics. The faculty had contemplated early on the addition of theoretical physicists; in particular Schrödinger was suggested by Weyl in 1934 and then also by Einstein. Dirac was also mentioned. But the director felt that he could not increase the faculty in the school: He was at the time starting two other schools, in economics and politics and in humanistic studies. Moreover, the financial situation caused some worry and he and the trustees felt some caution was called for. Still, Dirac was a visiting professor in 1934–1935 and Pauli the following year. Later, Pauli spent the war years at the Institute and was offered a professorship in 1945. He was interested but felt he could not commit himself before he had gone back at least for a while to Zürich, where his position had been kept open for him. He stayed at the Institute for one more year with the official title of Visiting Professor, but functioning as a professor and chose later to go back definitely to Zürich. The first real expansion in theoretical physics took place under the first half of Oppenheimer's directorship. As theoretical physics grew at the Institute, the two groups operated more and more independently from one another until it was decided, in 1965, to separate them officially by setting up a School of Natural Sciences. In the sequel, "School of Mathematics" will be meant in the narrow sense it has today.

The Institute developed first very informally. As already stated, Flexner relied for mathematics largely on outside advice, mainly that of Veblen. He had to: "*Mathematicians, like cows in the dark, all look alike to me*", he had said to the trustees at the January 1938 meeting. But this was to be an exception. He had already much more input in the setting up of the School in Economics and Politics and he expected fully it would be so in most aspects of the governance of the Institute. The correspondence with Veblen had shown already some differences of opinion on the eventual shape and running of the Institute, but they were not urgent matters at the time and could be overlooked while dealing with the tasks at hand, on which Flexner and Veblen were usually fully and warmly in agreement. However, as the Institute grew, differences of opinion between the director and some trustees on one hand, and the faculty on the other, became more apparent and relevant. The former liked to view the Institute as consisting of three essentially autonomous schools. They were willing to let each one run its own academic affairs; but there was a rather widespread feeling that professors were often conservative, parochial, not really able to see the Institute globally. Besides it was wrong for them to get involved in administrative matters (after all, Flexner had so often heard professors complain about those duties, which take so much precious time away from research and there he was offering them the possibility of having none...). On the other hand, the faculties of the three schools, which had been chosen quite independently and did not know one another, began to meet, to discuss matters of common interest, to compare views and problems and as a consequence to develop some feeling of being parts of one larger body. Understandably, they wanted to have at least a strong consultative voice in important academic matters. This came to a head when Flexner appointed two professors in economics without any faculty consultation. Added to earlier grievances, it led to such an uproar that Flexner had to resign. But, at a more basic level, there was no attempt to reconcile these two rather antagonistic attitudes in order to arrive at a *modus vivendi* offering a better framework to resolve any conflict that might arise again. None did arise under the next director, Frank Aydelotte (1939–1947), who earned the confidence of the faculty by his way of handling Institute matters (but, as a counterpart, less than unanimous approval from the trustees). Some conflict did surface, not to say erupt, under the next two directors, J. Robert Oppenheimer (1947–1966) and Carl Kaysen (1966–1976). Fortunately, except in one case to which I shall have to come back, these disputes had comparatively little visible impact on the workings of the School of Mathematics, as unpleasant and distracting as they were to its faculty, so that with relief I may pronounce these matters as outside the scope of this account and ignore them altogether. To conclude this long digression, let me add that a prolonged, in my opinion largely successful, effort was made over several years and concluded in 1974 to set up some Rules of Governance for handling in an orderly way between trustees, faculty and the director all aspects of the academic business of the Institute. There has been no such crisis under the present director, Marvin L. Goldberger (1986–), nor under the previous one, Harry Woolf (1976–1986).

In the fall 1939, a new chapter in the life of the Institute began with the moving of the Institute into the newly built Fuld Hall, on its own grounds. In preparation for this change, the school had begun to build up a library, aided in this first of all by Alfred Brauer, whom Weyl had taken as his assistant for this purpose. (Brauer did the same later on, on a bigger scale, for the Mathematics Department of the University of North Carolina at Chapel Hill.) In spite of the war, the Institute operated normally, although some professors were engaged in war work, albeit on a somewhat reduced scale. The influx from Europe increased and, again this had a direct bearing on the school: Siegel was given permanent membership, converted to a professorship in 1945. Kurt Gödel, after having been a member for about ten years, became a permanent member in 1946 and a professor in 1953. Why it took so long for Gödel is a matter of some puzzlement. There was of course unanimous admiration for his achievements and some faculty members had long favored giving him a professorship. The reluctance of others reflected doubts not on his scientific eminence, but rather on his effectiveness as a colleague in dealing with school or faculty matters (Siegel has been quoted to me as having said that one crazy man (namely himself) in the school faculty was enough) or on whether they would not be too much of an imposition on him. As a colleague of his in later years, I would say I found that, his remoteness not withstanding, he would acquit himself well of some of the school business, hence that those fears were not all well founded. On the other hand, I have to confess that I found the logic of Aristotle's successor in more difficult affairs sometimes quite baffling.

After the war, the activities of the school and its membership increased gradually. There was a conscious effort to have members from Eastern Europe or East Asia, in particular Poland, China, India. 1946 was also the beginning of the first (and so far only) venture of the Institute outside the realm of purely theoretical work, namely the construction of a computer under von Neumann's leadership. This has been described in considerable detail by H. Goldstine in his book,[6] to which I refer for details. The computer was used for a few years by a group working on meteorology and von Neumann wanted this to become a permanent feature at the Institute. But the faculty did not follow him. Even the faculty members who had a high regard for this endeavour in itself felt that it was out of place at the Institute, especially in view of the fact that there was no related work done at the University. The computer was given to the University in the late fifties.

Of the first faculty, Alexander resigned in 1947, remaining for some time as a member, Einstein became Professor Emeritus in 1946, Veblen in 1950 and Weyl in 1951. Siegel resigned in 1951 to return to Germany. Added to the faculty in 1951 were Deane Montgomery and Atle Selberg, who had been permanent members since 1948 and 1949 respectively, followed in 1952 by Hassler Whitney.

I came to the Institute in the fall of 1952, not knowing really what to expect. The only recommendation I can remember having received was to appear now and then at tea. This may have been prompted by memories of more formal days, but I soon realized that they were not counting heads. Instead, I found a most stimulating atmosphere, many people to talk to, and suggestions came from many sides. Let me indulge in some reminiscences of those good old days, with the tenuous justification that it is not out of order to describe in this paper some of the experiences and impressions of one visiting member.

F. Hirzebruch, whom I had known in 1948 when he spent some time in Zürich, came once to my office to describe the Chern polynomial of the tangent bundle for a complex Grassmannian. It was a product of linear factors and the roots were formally written as differences of certain indeterminates; Hirzebruch proceeded to tell me how to interpret them but he could not finish: they looked to me like roots in the sense of Lie algebra theory and this was just too intriguing for me to listen to any explanation. An extension to generalized flag manifolds suggested itself, but it was not clear at the moment whether this was more than a coincidence and wishful thinking. A few days later however, it became clear it was not and that marked the start of our joint work on characteristic classes of homogeneous spaces, to which we came back off and on over several years. Conversations with D. Montgomery and H. Samelson led to a paper on the ends of homogeneous spaces. A Chinese member, the topologist S. D. Liao, lectured on a theorem on periodic homeomorphisms of homology spheres he had proved using Smith theory. Having the tools of "French topology" at my finger tips, I tried to establish it in that

[6]H. H. Goldstine, *The computer, Part III*, Princeton University Press, Princeton, N.J., 1972.

framework, succeeded and then, by continuation, obtained new proofs of the Smith theorems themselves. This was the beginning of an involvement with the homology of transformation groups. Of much interest to me also was the seminar on groups, let by D. Montgomery, including his lectures on the fifth Hilbert problem, solved shortly before by him, L. Zippin and A. Gleason, and the contacts with H. Yamabe, his assistant that year.

At the University, Kodaira was lecturing on harmonic forms ("a silent movie" as someone had put it. The lectures were perfectly well organized, with everything beautifully written on the blackboard, but given with a very soft, low-pitched voice which was not so easy to understand.) Tate was lecturing on his thesis in Artin's seminar. The topology at the University gravitated around N. Steenrod, and his seminar was the meeting ground of all topologists. Among those was J. C. Moore, whom I had looked for immediately after my arrival with a message from Serre. This was the beginning of extensive discussions, and a friendship which even moved him to put his life and car at stake by volunteering to teach me how to drive.

My discussions with Hirzebruch went beyond our joint project. He was at the time developing the formalism of multiplicative sequences or functors, genera and experimenting with reduced powers, the Todd genus and the signature. In the latter case, this was soon brought to a first completion after Thom's results on cobordism were announced. Sheaf theory, in particular cohomology with respect to coherent sheaves, had been spectacularly applied to Stein manifolds by H. Cartan and J.-P. Serre; Kodaira, Spencer, Hirzebruch were naturally looking for ways to apply such techniques to algebraic geometry. So was Serre, of course. Being in steady correspondence with him, I was in a privileged position to watch the developments on both sides, as well as to serve as an occasional channel of communication. The breakthroughs came at about the same time in spring 1953 (I shall not attempt an exact chronology) and overlapped in part. Serre's first results were outlined in a letter to me, to be found in his *Collected Papers* (I, 243–250, Springer-Verlag, Berlin and New York, 1986); included were the analytic duality and a first general formulation of a Riemann–Roch theorem for n-dimensional algebraic manifolds. It was soon followed by the analogue for projective manifolds of the Theorems A and B on Stein manifolds. Spencer and Kodaira gave in particular a new proof of the Lefschetz theorem characterizing the cohomology classes of divisors. Soon came a vanishing theorem, established by Kodaira via differential geometric methods and by Cartan and Serre via functional analysis. Attention focussed more and more on the Riemann–Roch theorem, whose formulation became more precise, still with no proof. During the summer, we parted, I to go to the first AMS Summer Institute, devoted to Lie algebras and Lie groups (6 weeks, about thirty participants, roughly two lectures a day, a leisurely pace unthinkable nowadays) and then to Mexico (where I lectured sometimes in front of an audience of one, but not less than one, as Siegel is rumored to have done once in Göttingen, a rumor which unfortunately I could not have confirmed).[7]

Back at the Institute for a second year, I found again Hirzebruch, whose membership had also been renewed. The relationship between roots in the Lie algebra sense and characteristic classes had been made secure, but this whole project had been left in abeyance, there being so much else to do. Now we began to make more systematic computations, using or proving facts of Lie algebra theory and translating them into geometric properties of homogeneous spaces. Quite striking was the equality of the dimension of the linear system on a flag variety associated to a line bundle defined by a dominant weight and of the dimension of the irreducible representation with that given weight as highest weight. Shortly after, I went to Chicago, described this "coincidence" to André Weil, and out of this came shortly what nowadays goes by the name of the Borel–Weil theorem. After I came back, Hirzebruch was not to be seen much for a while, until he emerged with the great news that he thought he had a proof of the Riemann–Roch theorem. This was first scrutinized in private seminars and found convincing. I also provided a spectral sequence to prove a lemma useful to extend the theorem from line bundles, the case treated by Hirzebruch, to vector bundles. A bit later, Kodaira proved that Hodge varieties are projective. All this, and the work of Atiyah and Hodge giving a new treatment of integrals on algebraic curves, completed a sweeping transformation of complex algebraic geometry. Until then, it had been rather foreign to me, with its special techniques and language (generic points and the like). It was quite an experience to see all of a sudden its main concepts, theorems and their proofs all expressed in a more general and much more familiar framework and to witness these dramatic advances. This led me more and more to think about linear algebraic groups globally, in terms of algebraic geometry rather than Lie algebras, an approach on which I would work intensively the following year in Chicago, benefitting also from the presence of A. Weil.

During that second year, I also gave a systematic exposition of Cartan's theory of Riemannian symmetric spaces and got personally acquainted with O. Veblen, on the occasion of a seminar on holonomy groups he was holding in his office. I had of course no idea of his role in the development of the Institute, nor did I know about Flexner and his avowed ambition to create a "paradise for scholars". But I surely had felt it was one, or a very close approximation, so when I was offered a professorship in 1956, I was strongly inclined to accept it. It raised serious questions of course. I realized that, viewed from the inside, with the responsibilities of a faculty member, paradise might not always feel so heavenly. I had also to weigh a very good

[7](Added in proof) B. Devine just drew my attention to the interview of Merrill Flood by A. Tucker in the collection referred to in footnote five above, according to which such an incident did indeed take place once in Fine Hall.

university position (at the ETH in Zürich) with the usual mix of teaching and research against one entailing a "total, almost monastic, commitment to research", (as someone wrote to us much later, while declining a professorship). In fact, the offer had hit me (not too strong a word) while I was visiting Oxford and in a conversation the day before, J. H. C. Whitehead had made some rather desultory remarks about this "mausoleum". To him it was obviously essential to be surrounded by collaborators and students at various levels. I also had to gauge the impact on my family of such a move. But, after some deliberation and discussions with my wife, who left the decision entirely to me, I felt I just could not miss this opportunity.

My professorship started officially on July 1st, 1957, but I was already here in the spring. I found Raoul Bott, with whom I had many common interests. Sometime before, Hirzebruch and I had made some computations on low-dimensional homotopy groups of some Lie groups and, to our surprise, some of our results were contradicting a few of those contained in a table published by H. Toda. There ensued a spirited controversy, in which the homotopists felt at first quite safe. Bott was very interested; he and Arnold Shapiro, also at the Institute at the time, thought first they had another proof of Toda's result on $\pi_{10}(G_2)$, one of the bones of contention, but a bull session disposed of that. Later, Bott and Samelson confirmed our result. Eventually, the homotopists conceded. At the time, I had not understood why Raoul was so interested in those very special results, but I did a few months later when he announced the periodicity to which his name is now attached: Our corrections to Toda's table had removed a few impurities which stood in the way of even conjecturing the periodicity.

There was also a very active group on transformation groups around D. Montgomery who, with the Hilbert fifth problem behind him, had gone back fully to his major interest. My involvement with this topic increased, culminating in a seminar held in 1958–1959.

But I was now a faculty member in mathematics (together with K. Gödel, D. Montgomery, M. Morse, A. Selberg, H. Whitney, as already mentioned, Arne Beurling, who had joined in 1954, and A. Weil from fall 1958 on) and had to have some concerns going beyond my immediate research interests. Foremost were two, the membership and the seminars. As regards the former, it was not just to sit and wait for applicants and select among them, but also of course to seek them out. Weil and I felt that in the fields somewhat familiar to us, a number of interesting people had not come here and I remember that for a few years, in the fall we would make lists at the blackboard of potential nominees and plan various proposals to the group. In this way, in particular, we contributed not insignificantly to the growth of the Japanese contingent of visitors, which soon reached such a size that the housing project was sometimes referred to as "Little Tokyo" and that a teacher at the nursery school found it handy to learn a few (mostly disciplinary) Japanese words.

After a few years however, there was no significant "backlog" anymore and no need to be so systematic. As to the seminars, there were first some standard ones, like the members' seminar and the seminar in groups and topology, led by D. Montgomery. Others arose spontaneously, reflecting the interests of the members or faculty. We felt that the Princeton community owed it to itself also to supply information about recent developments and that beyond the graduate courses offered by the University and the research seminars, there should be now and then some systematic presentations of recent or even not-so-recent developments. In that respect, J.-P. Serre, a frequent fall term visitor during those years, and I organized in fall 1957 two presentations, one on complex multiplication and a much more informal one where we wrestled with Grothendieck's version of the Riemann–Roch theorem. As soon as he arrived, Weil set up a joint University–Institute seminar on current literature, thus reviving the tradition of the H. Weyl seminar, which he had known while visiting the Institute in the late thirties, and had also kept up in Chicago. The rule was that X was supposed to report on the work of Y, Z, with $X \neq Y, Z$. Later on, the responsibility for this seminar was shared with others. It was quite successful for a number of years, but was eventually dropped for apparent lack of interest. As I remember it, it became more and more difficult to find people willing to make a serious effort to report on someone else's work to a relatively broad non-specialized audience. Maybe the increase in the overall number of seminars at the University and the Institute, at times somewhat overwhelming, was responsible for that, I don't know.

During those years, algebraic and differential topology were in high gear in Princeton. In 1957–1958 J. F. Adams was here, at the time he had proved the nonexistence of maps of Hopf invariant one (except in the three known cases). Also Kervaire, while here, proved the non-parallelizability of the n-sphere ($n \neq 1, 3, 7$) and began his joint work with J. Milnor. In fall 1959 Atiyah and Hirzebruch developed here (topological) K-theory as an extraordinary homology theory, after having established the differentiable Riemann–Roch theorem; Serre organized a seminar on the first four chapters of Grothendieck's EGA. During that year, Kervaire, then at NYU came once to me to outline, as a first check, the construction of a ten-dimensional manifold not admitting any differentiable structure! M. Hirsch and S. Smale were spending the years 1958–1960 here, except that Smale went to Brazil in 1960. Soon Hirsch was receiving letters announcing marvelous results, so wonderful that we were mildly wondering to what extent they were due to the exhilarating atmosphere of the Copacabana beach, but they held out. (At the Bonn Tagung in June, as the program was being set up from suggestions from the floor, as usual, the first three topics proposed were the proofs of the Poincaré conjecture in high dimension by Smale and by Stallings and the construction of a nondifferentiable manifold by Kervaire; Bott, freshly arrived and apparently totally unaware of these developments, asked whether this was a joke!)

During these first years at the Institute, my active research interests shifted gradually from algebraic topology and transformation groups to algebraic and arithmetic groups, as well as automorphic forms. That last topic was already strongly represented here by Selberg, and had been before by Siegel. This general area was also one of active interest for Weil, and it soon became a major feature in the school's activities. Without any attempt at a precise history, let me mention a few items, just to give an idea of the rather exciting atmosphere. I first started with two projects on algebraic groups, one with an eye towards reduction theory, on the structure of their rational points over non-algebraically closed fields, the other on the nature of their automorphisms as abstract groups. Some years later, I realized that Tits had proceeded along rather similar lines and we decided to make two joint endeavours out of that. But I was more and more drawn to discrete subgroups, especially arithmetic ones. Rigidity theorems for compact hermitian symmetric spaces, hyperbolic spaces and discrete subgroups were proved by Calabi, Vesentini, while here, Selberg and then Weil. It is also at that time that I proved the Zariski density of discrete subgroups of finite covolume of semisimple groups. Weil was developing the study of classical groups over adeles and of what he christened Tamagawa numbers. I. Satake, while here, constructed compactifications of symmetric or locally symmetric spaces. It became more and more imperative to set up a reduction theory for general arithmetic groups. The Godement conjecture and the construction of some fundamental domain of finite area became prime targets. The first breakthroughs came from Harish-Chandra. I then proved some results of my own; he suggested that we join forces and we soon concluded the work published later in our joint *Annals* paper. This was in summer 1960. The next year and a half I tried alternatively to prove or disprove a conjecture describing a more precise fundamental domain and finally succeeded in establishing it. Combined with the other activities here and at the University, this all made up for a decidedly upbeat atmosphere. But in 1962 rumors began to spread that it was not matched by equally fruitful and harmonious dealings within the faculty. Harish-Chandra, who was spending the year 1961–1962 here, asked me one day, What about those rumors of tremors shaking the Institute to its very foundations? We were indeed embroiled in a bitter controversy, sparked by the school's proposal to offer a professorship to John Milnor, then on the Princeton faculty.

Before we presented this nomination officially, the director had indeed warned us, without being very precise, that there might be some difficulty due to the fact that Milnor was at the University, and we could hardly anticipate the uproar that was to follow. The general principle of offers from one institution to the other and the special case under consideration were heatedly debated in (and outside) two very long meetings (for which I had to produce minutes, being by bad luck the faculty secretary that year). A number of colleagues in physics and historical studies stated that it had always been their understanding that there was some agreement prohibiting the Institute to offer a professorship to a Princeton University colleague. In fact, the historians extended this principle even to temporary memberships. Fear was expressed that such a move would strain our relations with the University, which some already viewed as far from optimal. In between the two meetings, the director produced a letter from the chairman of the Board of Trustees, S. Leidesdorf, referring to a conversation he had participated in between Flexner and the president of the University, in which it had been promised not to make such offers. He viewed it as a pledge, which could be abrogated only by the University.

Those views were diametrically opposite to those of the mathematicians here and at the University, which were in fact quite similar to those of Eisenhart in the letter quoted earlier or in his statement to the curriculum committee, both naturally and unfortunately not known to us at the time. He really had said it all. First of all, the school used to give sometimes temporary memberships to Princeton faculty. This was on a case-by-case basis, not automatic, and it had never occurred to us to rule it out *a priori*. We also felt that our relations with Fine Hall were excellent and would not be impaired by our proposal. In fact D. Spencer had told us right away we should feel free to act. D. Montgomery stated that Veblen had repeatedly told him, in conversations between 1948 and 1960, that there had never been such an agreement. J. Alexander, asked for his opinion, wrote to Montgomery that he had never known of such an agreement (whether gentlemanly or ungentlemanly). He also remembered certain conversations in which an offer to a university professor was contemplated, or feared by some university colleague, conversations which would have been inconceivable, had such an agreement been known. Finally he had "no knowledge of deals that may have been consummated in 'smoke-filled rooms' or of 'secret covenants secretly arrived at.' All this sort of stuff is over my depth." A. W. Tucker, chairman of the University Mathematics Department, consulted his senior colleagues and wrote to A. Selberg, our executive officer, that in their opinion (unanimous, as he confirmed to me recently) the Institute should be free to extend an offer to Milnor. Of course, were he to accept it, this would be a great loss, but any such "restraint of trade" was distasteful to them and could well prove damaging in the long run. It would be much better, they felt, if the University would answer with a counteroffer attractive enough to keep Milnor. The point was repeatedly made that, when two institutions want the services of a given scholar, it is up to the individual to choose, not up to administrators or colleagues to tell him what to do; also, as Eisenhart had already pointed out, that such a blanket prohibition might be damaging to the recruiting efforts of the University.

In the course of the second faculty meeting a colleague in the School of Historical Studies, the art historian Millard Meiss, stated it had indeed been his understanding there was such an agreement; he noted that the mathematicians and his school acted differently with regard to temporary memberships; he felt the rule had been a wise one in the earlier days of the Institute, but was very doubtful it had the same usefulness today. Accordingly, he proposed a motion, to the effect that the faculty should be free to extend professorial appointments to faculty members of Princeton University, with due regards to the interests of science and scholarship, and to the welfare of both institutions. He also insisted that this should occur only rarely. This motion was viewed as so important ("the most important motion I have voted on in the history of the Institute", commented M. Morse) that it was agreed to have the votes recorded by name, with added comments if desired. It was passed by fourteen *yes* against four *no*, with two abstentions.

After this, it would have seemed most logical to take up the matter with the president of the University, R. Goheen, but nothing of the kind was done at the time and the tension just mounted until the trustees meeting in April. There, as we were told shortly afterwards by the director, the Milnor nomination did not even come to the board: The trustees had first reviewed the matter of invitations to Princeton University faculty, with regard to the Meiss motion, and had voted a resolution to the effect that the agreement with Princeton University to refrain from such a practice was still binding.

In this affair we had worked under a further handicap: In those days, it was viewed as improper to talk about a possible appointment with the nominee before he had received the official offer (nowadays, the other way around is the generally accepted custom). Consequently, none of us had ever even hinted at this in conversations with Milnor. But he had heard about it from other sources and it became known that he would have been seriously interested in considering such an offer. The director and the trustees may not have felt so fully comfortable with their ruling after all. At any rate, they soon proposed to offer some long-term arrangement to Milnor, whereby he could spend a term or a year at the Institute during any of the next ten years. This was of course very pleasant for Milnor, and we gave this proposal our blessing, but it fell short of what we had asked for. Finally, eighteen months later, in October 1963, we were informed that, following instructions from the trustees, the director had taken up the matter of general policy with President Goheen in January 1963 and we received a copy of a letter written on January 21, 1963 by President Goheen to the director, outlining one. Although cautious in tone, it allowed one institution to extend an offer to a faculty member of the other, after close consultation "*to the end of matching the interests of the individual with the common interests of the two institutions to the fullest extent possible.*" In conclusion, he urged that "*this agreement supplant any specific or absolute prohibition that we may have inherited from our predecessors.*"

Right after the next trustees meeting the director wrote to Goheen on April 22, in part: "*The Trustees asked me to tell you that they welcome your letter, and that they have asked me to let it be a guide to future policy of the Institute.*" As far as I know, the matter was never reconsidered and this agreement is still in force. At the time we were apprised of this (October 1963), it would have therefore been "legally" possible for us to present again our proposal, although Milnor was still a Princeton faculty member.

But we could not! During 1962–1963, we had asked for two additions to our group; they had been granted and no chair was available to us anymore. How had this come about?

This experience had left strong marks. It was not just the decision of the trustees, but the way the matter had been handled and the breakdown in relations within the faculty (also contributed to by conflicting views on some nominations in the School of Historical Studies), the ruling from on high by the board, without bothering to have a meaningful discussion with us, bluntly disregarding our wishes, as well as those of the faculty as expressed by the Meiss motion, all this chiefly on the basis of a rather flimsy recollection of the chairman of the board, promoted to the status of an irrevocable pledge. Some of us were wondering whether to withdraw entirely into one's own work or to resign, and were sounded out as to their availability. One Chairman, who had for some time wanted to set up a mathematics institute within his own institution, toyed with the idea of making an offer to all of us. We still had the option of making another nomination and there were indeed two or three names foremost on our minds. But just choosing one and presenting it would not suffice to restore our morale. Something more was needed to help us rebound. It was Weil who suggested that we present two nominations instead of just one, as was expected from us. After some discussions, we agreed to do so and nominated Lars Hörmander and Harish-Chandra.

This took the rest of the faculty and the director completely by surprise. The latter did not raise any objection on budgetary grounds. He also made it clear at some point that if granted, this request would have no bearing on faculty size for the other groups. Since our nominations were readily agreed to be scientifically unassailable, it would seem that our proposal would go through reasonably smoothly, but not at all. Our request had been addressed by A. Selberg, still our executive officer, directly to the director and the trustees, bypassing several steps of the standard procedure for faculty nominations, which seemed unpracticable in the climate at the time, and also not fulfilling one requirement in the by-laws. And it is indeed on grounds of procedure that the director and some colleagues raised various objections. There was overwhelming agreement on the necessity of major changes in our procedure for faculty appointments. The question was whether this review should precede or follow the handling of our two nominations. Again, this grew into a full-size debate and we did not know how our proposal would fare at the

April trustees meeting. There, as we were told at the time, the director recommended to postpone the whole matter, but the trustees, after having heard Selberg present our case, voted to grant our request under one condition, namely that a faculty meeting be held to discuss our nominations. This was really only to restore some semblance of formal compliance with the by-laws, and they were anxious that this matter be brought with utmost dispatch to a happy conclusion, so that the Institute would soon regain its strength and some measure of serenity. This meeting was held within a week and the offers were soon extended.

Harish-Chandra accepted quickly, Hörmander after a few months. Finally, this sad episode was behind us. We felt and were stronger than before and could devote ourselves again fully to the business of the school. In fall 1963 there were the usual seminars on members and faculty research interests. Harish-Chandra started a series of lectures, which became an almost yearly feature: every week two hours in a row, most of the time on his own work, i.e., harmonic analysis on reductive groups (real, later also p-adic), documenting in particular his march towards the Plancherel formula. He was not inclined to lecture on other people's work. One year however he did so, he "took off", as he said, viewing it as some sort of sabbatical, and lectured on the first six chapters of Langlands' work on Eisenstein series (then only in preprint form). There were also some seminars on research carried out outside Princeton: I launched one on the Atiyah–Singer index theorem, for non-analysts familiar with all the background in topology. Eventually, R. Palais took the greater load and wrote the bulk of the *Notes* (published in the *Annals of Math. Studies* under his editorship). The following year, there was similarly a "mutual instruction" seminar on Smale's proof of the Poincaré conjecture in dimensions ≥ 5. Still, we felt some imbalance in the composition of the membership and the activities of the school. Of course, there is no statutory obligation for the school membership to represent all the main active fields of mathematics. In any case, in view of the growth of mathematics and of the number of mathematicians, as compared to the practically constant size of the school (the membership size hovering around 50–60 and that of the faculty around 7–8), such a goal was not attainable anymore. Nevertheless, it has always been (and still is) our conviction that the school will fulfill the various needs of its membership best if it offers a wide variety of research interests, and that this is a goal always to keep in mind and worth striving for, even if not fully reachable. For this and other reasons we decided in 1965 to have more direct input in part of the work and composition of the school by setting up a special program now and then. This idea was of course not to have the school fully organized all of a sudden, rather to add a new feature to the mathematical life here, without supplanting any of the others. Such a program was to involve as a rule about a quarter, at most a third, of the membership, with a mix of invited experts and of younger people. It would often be centered on an area not well represented on the faculty, but not obligatorily so. We did not want to refrain from organizing a program in one of our fields of expertise, if it seemed timely to gather a group of people working in it to spend a year here. It was of course expected that such a program would include a number of seminars for experts to foster further progress, but we also hoped it would feature some surveys and introductory lectures aimed at people with peripheral interests, and would also facilitate to newcomers access to the current research and problems. Pushing this "instructional" aspect a bit further, we also decided to have occasionally two related topics, hoping this would increase contacts between them.

The first such program took place in 1966–1967 and was devoted to analysis, with emphasis on harmonic analysis and differential equations. In agreement with the last guideline stated above, the second one (1968–1969) involved two related topics, namely algebraic groups and finite groups. As a focus of interaction, we had in mind first of all the finite Chevalley groups and their variants (Ree and Suzuki groups). They played that role indeed, but so did the Weyl groups and their representations, as can be seen from the *Notes* which arose from this. The third program (1970–1971) centered on analytic number theory.

In 1971, again with an eye to increasing breadth and exposure to recent developments, another activity was initiated here, namely an ongoing series of survey lectures. In the sixties and before, the dearth of expository or survey papers had often been lamented. The *AMS Bulletin* was a natural outlet for such, first of all because the invited speakers for one-hour addresses are all asked to write one. But this did not seem to elicit as many as one could wish and various incentives were tried, with limited success. It had always seemed to me that most of us are cold to the idea of just sitting down to write an expository paper, unless there is an oral presentation first. But the example just mentioned showed that this condition was not always sufficient. Already in my graduate student days, I had been struck by some beautiful surveys in the *Abhandlungen des Math. Sem. Hamburg*. They were usually the outgrowth of a few lectures given there. This suggested to me that one might have a better chance of getting a paper if the prospective author were invited to give some comprehensive exposition in a few lectures, not just one. However I had done nothing to implement such a scheme, just talking about it occasionally, until the 1970 International Congress in Nice. There K. Chandrasekharan, then president-elect of the IMU, told me he wanted to set up a framework for an ongoing series of lectures sponsored by the IMU, to be given at various locations, with the express purpose to engender survey papers. Would I help to organize it? Our ideas were so similar that we quickly agreed on the general format: A broad survey, for non-specialists, given in four to six one-hour lectures, within a week or two. Expenses would be covered, but the real fee would be paid only upon receipt of a manuscript suitable for inclusion in this series. A bit later, I suggested as an outlet for publication the *Enseignement Mathématique*, mainly for two reasons: First, it is in some way affiliated to the IMU, being the official organ of the International Commission for Mathematical Education. Second, it has the rare, if not unique, capacity to publish as a separate monograph, sold independently, any article or collection of articles published in that journal.

The first two such sets of lectures were given at the Institute in the first quarter of 1971, by Wolfgang Schmidt and Lars Hörmander (who was a visitor, too, having resigned from the faculty in 1968), both soon written up and indeed published in the *Enseignement Mathématique.* But a difficulty arose with our third proposal, namely to invite Jürgen Moser, then at NYU, to give a survey on some topics in celestial mechanics. From the point of view of the IMU, these lectures were meant to promote international cooperation. Accordingly, the lecturer was to be from a geographically distant institution, so that the invitation would also foster personal contacts. They felt that we did not need an IMU sponsorship to bring Moser from NYU to the Institute. They certainly had a point. On the other hand, it was also a sensible idea to have such a set of lectures from Moser. In the school, we were really after timely surveys, whether or not they were contributing to international cooperation, while this latter aspect was essential for the IMU. Also, they wanted of course to have such lecture series be given at various places and their budget was limited. Since we planned to have about one or two per year, our requests might well exceed it, so that some difficulties might be foreseen also on that score. We therefore decided to start a series of similar lectures of our own, and to call them the Hermann Weyl Lectures, an ideal label, in view of Weyl's universality: It was a nice touch to be able on many occasions to trace so much of the work described in those lectures to some of his. We planned to publish them as a rule, though not obligatorily, in the *Annals of Mathematics Studies.* Otherwise, the conditions and format of the lectures were to be the same. Our series started indeed with J. Moser's lectures, resulting in an impressive two-hundred page monograph. For a number of years, the H. Weyl lectures were a regular feature here, at the rate of one to two sets per year. As to their original purpose, namely to bring out survey papers, I must regretfully acknowledge that our record is a mixed one, and that the list of speakers who did not contribute any is about as distinguished as that of those who did. Maybe Moser's contribution was a bit daunting, although F. Adams and D. Vogan rose to the challenge, even topping its number of pages (slightly in the former case, largely in the latter). Overall, the high quality of the monographs growing out of the H. Weyl lectures has made the series very worthwhile. Their frequency has declined in recent years. Since we started this, "distinguished" lecture series have sprung up at many places. Also, symposia, conferences and workshops on specific topics have proliferated, often leading to publications containing many surveys or introductory papers. There is indeed nowadays quite a steady flow of papers of this type so maybe the need for our particular series has decreased. One of the nice features of the Institute is that we need not pursue a given activity if we do not feel it fulfills a useful function in the mathematical community. So we may well leave this one in abeyance and revive it whenever we see a good opportunity.

In 1966 C. Kaysen had taken up the directorship and found the school faculty in good shape. He thought that, at least with our group, he would not face requests for new appointments. But we pointed out to him that our age distribution was a bit unfortunate and would later create some problems, with retirements expected in 1975, 1976, 1977, and 1979. Therefore it might be desirable to consider some advance replacements; also that some minimal expansion might be to the good. He agreed. In 1969 Michael Atiyah joined the faculty. Originally, this appointment had been meant to be an expansion, but it was not anymore, after Hörmander had resigned in 1968. Later, we made offers successively to John Milnor and Robert P. Langlands, who came to the faculty in 1970 and 1972 respectively.

In the sixties, considerable progress was made in the general area I had already singled out as a very strong one here: Algebraic groups, arithmetic groups and automorphic forms, number theory, harmonic analysis on reductive groups. Much of it was done here, but also at the University by G. Shimura, and by R. P. Langlands who was there for three years. It continued unabated, or even at an increased pace, after Langlands joined us. This whole general field had become such an active and important part of "core mathematics" that it was all to the good. However, that was not matched by activities of similar scope in other areas and created some imbalance, accentuated by Atiyah's resignation in 1972. For reasons already explained, in our view it was not in the best interest of the school in the long run and to correct it by increasing activities in other areas became a concern. There were two obvious means to try to remedy this: the special programs and new faculty appointments. But they were not available to us during the energy crisis and the immediately following years. The financial situation of the Institute was worrisome and we had not even been authorized to replace Atiyah. Also, we had not been able to take care completely within our ordinary budget of the special programs, which entailed invitations to well-established people. We always had had to get some outside support, besides our standing NSF contract, and that was hard to come by in those years. But we resumed both as soon as it became possible: Enrico Bombieri came to the faculty in 1977 and Shing-Tung Yau in 1980, broadening greatly its coverage. We had also to wait until 1977 for the programs but have had one almost every year from then on.

In 1977–1978, our program was devoted to Fourier integral operators and microlocal analysis with the participation in particular of L. Hörmander and M. Kashiwara. This was again an attempt to increase contacts between two rather different points of view, in this case the classical approach and the more recent developments of the Japanese school around M. Sato. It led to a collection of papers providing a mix of both. The next one was on finite simple groups and brought here a number of the main participants to the collective enterprise to classify the finite simple groups. 1979–1980 was the year of the biggest program to date, on differential geometry and analysis, in particular nonlinear PDE. The number of seminars was somewhat overwhelming. Several were concentrated at the end of the week, so as to make it easier for people in neighboring (in a rather wide sense including New York and Philadelphia) institutions to participate. Roughly speaking, the main activities were subdivided in three parts: differential geometry, minimal submanifolds, and mathematical physics, with seminar coordinators L. Simon for the second one, S. T. Yau for the other two. A remarkable feature of the third one (devoted to relativity, the positive mass conjecture, gauge theories, quantum gravity) was the cooperation between mathematicians and physicists, probably a first here since the early days. Two volumes of *Notes* resulted from this program.

There was none the following year but then, in 1981–1982, we had one on algebraic geometry, at least as big as the previous one. Again, seminars were also attended by visitors from outside, two even coming from Cambridge, Massachusetts: D. Mumford and P. Griffiths would visit every second or third week for two to three days, each to lead one of the main seminars. We had decided to concentrate on the more geometric (as opposed to arithmetic) aspects of algebraic geometry, since we intended to have in 1983–1984 a program on automorphic forms and L-functions. But even with that limitation, it was of considerable scope (Hodge theory, moduli spaces, K-theory, crystalline cohomology, low-dimensional varieties, etc.). Griffiths' seminar also led to a set of *Notes*. This was again very successful but the evolution of these seminars betrayed a natural tendency, namely to try each time to improve upon the previous one, leading not unnaturally to bigger and bigger programs. As already stated, our original intention had been to add an activity, not to suppress any, and we began to wonder whether these programs, carried out at such a scale, might not hamper somewhat other important aspects of the mathematical life here, such as variety, informality, the opportunity for spontaneous activities and unplanned contacts, quiet work, etc. So we decided to scale them down a bit. Again, this was not meant as a straightjacket; rather, that the initial planning would usually be on a more modest scale. But, if outside interest would lead to a growth beyond our original expectations (as is the case with the present program on dynamical systems), we would of course do our best to accommodate it. We were aided in fact in our general resolve by the emergence of the Mathematical Sciences Research Institute at Berkeley: Big programs are an essential feature there and they have more financial means than we to carry them out. There is no need to compete for size.

S. T. Yau had resigned in 1984 and was soon replaced by Pierre Deligne. The retirements we had warned C. Kaysen about had caught up with us for some time and our group was reduced to six, two fewer than the size we were entitled to at the time, so that we had the possibility of making two appointments. We were anxious to seize this opportunity to catch up with some new major trends in mathematics. There had been some very interesting shifts in the overall balance of research interests, partly influenced by the development of computers, notably towards nonlinear PDE and their applications (with which we had lost first-hand contact after Yau's resignation), dynamical systems, mathematical physics, as well as an enormous increase of the interaction with physicists, the latter visible notably around string theory and conformal field theory (CFT). These last two topics were very strong at the University, but underrepresented here (not only in the faculty, but also in the membership). As a first attempt to improve this situation, I suggested in fall 1985 to E. Witten to give at the Institute a few lectures on string theory aimed at mathematicians. They were very well attended, so that the next logical move was to think about organizing a program in string theory and to ask Witten whether this seemed to him worth pursuing and, if so, whether he would agree to help, first as a consultant and then as a participant. That same year, we made two successful offers to Luis Caffarelli and Thomas C. Spencer, thus increasing considerably our range of expertise in some of the "most wanted" directions.

The first question put to Witten was not entirely rhetorical, given the abundance at the time of conferences and workshops on these topics. But it was agreed after some thought that a year-long program here would have enough features of its own to make it worth trying. A bit later, an expert to whom I had written about it warned that, in view of the usually rather frantic pace of research in physics, this might be all over and passé at the time of the program (1987–1988); but it seemed to us there was enough new mathematics to chew on for slower witted mathematicians to justify such a program on those grounds (later, that expert volunteered to eat his words). Anyway, we went ahead. The program had originated within the School of Mathematics, but the School of Natural Sciences became gradually more involved and eventually contributed to the invitations. In fact, the borderline between the two schools became somewhat blurred, the physicists D. Friedan, P. Goddard and D. Olive being members in mathematics, while the mathematicians G. Segal and D. Kazhdan were invited by the School of Natural Sciences. A primary goal of this program was to increase the contacts between mathematicans and physicists and to help surmount some of the difficulties in communication due to differences in background, techniques, language and goals. Accordingly, we had invited several mathematically minded physicists and some mathematicians with a strong interest in physics, all rather keen to contribute to the dialogue. The program was very intense, too, with an impressive array of seminars, notably many lectures on various versions of CFT, and many discussions in and outside the lecture rooms.

Our last two appointments, succeeded by that of E. Witten in the School of Natural Sciences, have quickly made the Institute a major center of interaction between physics and mathematics and also increased significantly the membership in analysis. Altogether, the school faculty seems to me to be about as broad as can be expected from seven people. I hope it is not just wishful thinking on my part to believe that by its concern for the school and its own work, it is well on its way to maintain a tradition worthy of the vision of the first faculty.

The reader will have noticed that, from the time I came to the Institute, this account is largely based on personal recollections and falls partly under the label of "oral history", with, as a corollary, an emphasis or maybe even an overemphasis on the events or activities I have been involved with or witnessed from close quarters. Even with those, I have not been even-handed at all and this paper makes no claim to offer a balanced and complete record of the school history and of all the work done there.[8] Such an undertaking would have brought this essay to a length neither the editors nor the author would have liked to contemplate. Also absent is any effort to evaluate the impact of the school on mathematics in the U.S. and beyond: How much benefit did visitors gain? How influential has their stay here been on their short-range and long-range activities? What mathematical research was carried out or has originated here? How important has been the presence and work of the faculty? These are some of the questions which come to mind. To try to answer them would again have had an unfortunate effect on the length of this paper. Besides, an evaluation of this sort is more credible if it emanates from the outside, at any rate not solely from an interested party of one. Moreover, as a further inducement for me to refrain from attempting one, two evaluations of relatively recent vintage do exist. First, a report by a 1976 trustee–faculty committee, whose charges were to review the past, evaluate the Institute and provide some guidelines for the future. Its assessment was based in part on the letters of a number of scholars and on the answers (over five hundred from mathematicians) to a questionnaire sent to all past and present members on behalf of that committee. Second, one by a 1986 visiting committee, chaired by G. D. Mostow. Both, though not exempt from

[8]In that connection, let me mention that *A Community of Scholars. The Institute for Advanced Study, Faculty and Members 1930–1980*, published by the Institute for Advanced Study on the occasion of its fiftieth year, contains in particular a list of faculty and members up to 1980 and, for most, of work related to IAS residence.

criticisms, conclude that the School of Mathematics has been successful in many ways. As a brief justification for this claim and without further elaboration, let me finish by quoting from a letter written in 1976 by I. M. Singer to the chairman of the review committee, Martin Segal, who was happy to share it with the committee:

> Their [the members'] stay at the Institute under the guidance of the permanent staff affects their mathematical careers enormously. Their contacts with their peers continue for decades. They leave the Institute, disperse to their universities, and carry with them a deeper understanding of mathematics, higher standards for research, and a sophistication hard to attain elsewhere.
>
> Such was the case when I was here twenty years ago. Last fall when I signed the Visitors' Book I turned the pages to see who was here in 1955–1956. Many are world famous and they are all close professional friends. I notice the same thing happening now with the younger group. Before I came in 1955, the Institute was described to me as I am describing it to you. It remains true now as it has been for the last thirty years.

In preparing this article I benefitted from the use of some archival material. I thank E. Shore and M. Darby at the Institute for their help in dealing with the Institute archives and R. Coleman at the University for having kindly sent me copies of some documents in the University archives. I am also grateful to A. Selberg and A. W. Tucker for having shared with me some of their recollections, and especially to D. Montgomery for having done so in the course of many years of close friendship.

During most of his career, Edgar R. Lorch has been connected with Columbia University. In 1924, he entered Columbia as an undergraduate. He received his B.A. in 1928 and his Ph.D. in 1933, writing a dissertation under the direction of J. F. Ritt. He was appointed to the Columbia faculty in 1935, and he served there until his retirement in 1977. He was Chairman of the Department at Barnard College in 1948–1963 and at Columbia in 1968–1972. His research has focused on operators in linear spaces, normed rings, and topology. Among his many publications is a research monograph entitled Spectral Theory.

Mathematics at Columbia during Adolescence

EDGAR R. LORCH

"Now, really, these French are going too far. They have already given us a dozen independent proofs that Nicolas Bourbaki is a flesh and blood human being. He writes papers, sends telegrams, has birthdays, suffers from colds, sends greetings. And now they want us to take part in their canard. They want him to become a member of the American Mathematical Society (AMS). My answer is 'No'." That was the reaction of J. R. Kline, the AMS secretary, as he strode out of the Society's office on the third floor of Low Library. Kline was a charming person, especially warm with us younger colleagues. It was always a pleasure to be in his company and as we walked from Miss Hull's office to the Faculty Club for lunch or to the afternoon session of the Society in Pupin Hall he would unfailingly tell me some anecdote on one of our flamboyant members. One of them, concerning Norbert Wiener, deserves retelling here.

It seems that the Klines and the Wieners had adjacent summer cottages on a lake in New Hampshire. It was Norbert's habit every summer to swim from his dock to a small island not too far away in the middle of the lake. Thus he would convince himself that his physical capacity did not lag behind his mental sharpness. On these swims, JRK would keep company in a rowboat carrying on a conversation with the convex body which was slowly progressing to the goal. Trying, as usual, to keep the initiative within his own hands, especially since, as he approached the island, he was becoming quite winded, Norbert puffed out his trump card: "Kline, who are the five greatest living mathematicians?" And JRK quietly: "That is an interesting question. Let's see." And he mentioned without delay or difficulty four names. Then, full stop. "Yes, yes, go on," burbled NW, not having heard the name of his favorite candidate. But JRK, with delicate humor, never revealed the identity of Mr. Quintus.

There was a sequel to the Bourbaki episode. About that time the leading mathematical societies signed reciprocity agreements allowing any member of one contracting society to become on demand a member of another. Entrance into the Society was attempted for NB under reciprocity and led to an astonishingly large correspondence (See Everett Pitcher, *A History of the Second Fifty Years, American Mathematical Society, 1939–1988*, AMS Centennial Publications, Vol. I, pp. 159–162.) Today's younger mathematicians cannot easily imagine the heat produced by the episode.

Up to the fifties, Columbia was a Times Square for mathematics, a meeting place for the entire Northeast corridor. This was natural since so many meetings took place on our campus. The beautiful Society office, presided over by the beautiful Miss Hull, was here; the treasurers of the Society seem to have been Columbia people (were we really more honest than the others?), the Society library was here, more or less mixed up with the Columbia collection. We were really privileged at Columbia, and it broke our heart when the Society at the tender age of 60 or so decided to leave the nest and start life on its own in Providence. That was a bit before our department decided that enough was enough and it was time to become modern.

Among the very first of the visitors I remember at Columbia was G. D. Birkhoff who came in the summer of 1929 to teach in our summer session. How many of today's mathematicians have ever taught during the summer? GDB radiated power and good will, and being in his company was a privileged way of starting a career. I was a first year graduate student at the time, a very critical period for a young person. In his lectures, GDB had an unconscious knack for associating himself with substantial stage props, both "im grossen" and "im kleinen." In his course entitled "Mathematical Elements of Art," we navigated from Greek and Japanese vase forms (a rather obvious and easy subject) to the writing of poetry via formulas in which the listener could test and grade himself against Keats and Shelley. Then at the end of the course, there was music, in which I was particularly interested. Due to the tightness of the program, there was only one day left over for this subject. Full of expectation, I went to 202 Hamilton Hall in 88° temperature (plus humidity), and there I found a magnificent Steinway grand piano, all eleven feet of it, in its imperial ebonized glory. What was in store for us now? Well, precisely nothing. GDB spoke in a general way about a variety of things but never, I mean never, was the piano touched.

I remember another episode some years later when Birkhoff gave one of the inaugural lectures for the founding of the Institute for Advanced Study (IAS).

He had already mobilized on the board an astonishing quantity of symbols when he stopped short, looked up and down, and said with surprise, "But there is no colored chalk here." After an inaudible gasp of consternation on the part of his Princeton hosts, a young local professor got up and raced out of the room. GDB proceeded. After a brief pause, the young man reappeared, a bit breathless but also sheepishly jubilant, carrying a lovely box which bounded an 8×10 matrix of a rainbow assortment of chalk. Birkhoff looked at him over his spectacles and said, "That's all right. I don't plan to use it," and went on with his exposition.

As a young student, I was fully aware of the exceptional role played by Columbia in the first years of the Society. Indeed, the original name could well have been the Columbia Mathematical Society. Four of the first seven presidents of the AMS were associated with Columbia. J. H. Van Amringe held a professorship at Columbia over the period 1863–1910 and was Dean of the College from 1896 onward. "Van AM" was a popular teacher who inspired the creation of some old Columbia student songs [Archibald, *A Semicentennial History of the American Mathematical Society 1888–1938*, Amer. Math. Soc., Providence, RI, 1938, pp. 110–112]. He was the first president of the New York Mathematical Society (now the AMS) in 1888. G. W. Hill had close ties with Columbia but worked for much of his career at his home in West Nyack, New York. He lectured on celestial mechanics at Columbia in 1893–1895 and in 1898–1900. He was president of the AMS in 1895–1896. His fundamental contributions to the theory of the lunar orbit earned him an international reputation. Hill's differential equation is now well known in celestial mechanics. R. S. Woodward, the fifth president of the AMS (1899–1900), taught mechanics and mathematical physics at Columbia during the years 1893–1904. He was an astronomer and geographer of first rank who later served as President of the Carnegie Institution of Washington (1904–1921). Thomas S. Fiske was educated at Columbia and was on the faculty from 1888 to 1936. After founding the AMS (NYMS) as a graduate student in 1888, he became its seventh president in 1903–1904.

The offices of the Society at the very beginning must have been the desk of Professor Fiske. When I came to Columbia some thirty-six years later, the Society had its quarters in space provided by the University. Still later, when I was book review editor for the *Bulletin*, I remember making frequent visits to its beautiful sunny quarters on the third floor of Low Memorial Library, which, until the Butler Library was built, housed the main university collection.

An older professor of stature at Columbia in the twenties and thirties was Edward Kasner, a delightful, kind man who had done distinguished work in differential geometry. We used to share an office together, and in my mind's eye, I still see him so well coming in at 10:50 on a chilly fall day, peeling off his topcoat, jacket, and sweater, then putting the jacket back on in preparation for his lecture in fundamental concepts. As a last step in the preparation, he would turn his back to me, pull an envelope from his jacket containing his false teeth, and snap them on audibly. Then forth to the fray.

Kasner's course for M.A. candidates was very popular yet very elementary. He spent a great deal of time working with large numbers. I do not know how many class days were spent estimating the number of grains of sand on the earth. His favorite large number was 10^{100}, well beyond any number arising in the physical universe. He asked his two-year-old nephew what name to give this monster, and the little boy gurgled "google." The name stuck.

As the reader may guess, Kasner was not without his idiosyncrasies. He loved nature and hiking and would regularly walk up Riverside Drive to the New Jersey ferry, cross the river (cost, one nickel) and climb the Palisades, the top of which was covered by a respectable "wild" forest. On each of these walks, and this he recounted to me at least ten times, he would dig a hole at the base of some tree and bury a nickel. Why? So that he would never find himself *depourvu* of ferry fare on his return. Come now, you younger mathematicians who are ostentatious about your pecularities, let's see you match that.

Mathematics departments have their ups and downs, and during the twenties, honesty requires one to admit, Columbia was much on the down side. The administration was keenly aware of the situation and was just as keenly proceeding to do something about it. The rule of action here at our University based on the rule of thumb "il ne faut pas se prendre pour de la merde" is to start at the top creating an ordered list of the world's greatest mathematicians, to make offers starting at number one, and to see what happens. Well, here is one thing that happened as it was told to me some years later by one of the more talkative members of our department. Hermann Weyl received a princely offer. It was discussed, and special conditions were made and agreed upon. One of them was that his assistant, a young woman named Lulu Hoffman, was also to come to Columbia. This raised a problem immediately because Columbia had only male professors. However, the problem was easily solved. Dr. Hoffman was to teach in Barnard. This actually took place, and at Barnard she was the first woman mathematics teacher. The Columbia-Weyl bargaining went on, and finally Weyl decided not to accept. As he put it, and this is the part about which my talkative colleague insisted, Weyl pointed out that Göttingen was the center of the mathematical universe, that he was very happy there, and that he did not wish to change things by accepting Columbia's offer. We fellow instructors used to laugh wholeheartedly picturing Hermann Weyl, on a deck chair of the *SS Bremen* or *Hamburg* crossing the ocean to New York with the center of gravity of world mathematics following obediently some one hundred yards behind the propeller's wash.

There were other attempts by the authorities to obtain the services of a very distinguished man, but these were equally unfruitful. (However, Columbia was nowhere near winning the university sweepstakes for the greatest number of successive turndowns.) It was then decided to engage brilliant promising younger scientists. In this way, the department was enriched by the presence of Bernard Koopman from Harvard and Paul Smith from Princeton. That was an astute move on the part of the university, which paid off handsomely.

Paul was a topologist to the marrow. He was very quiet and very concentrated. His compass always pointed towards Princeton with its solar system of topologists. He was not the one who said, "Whenever I see a derivative it gives me nausea," but he probably thought it. He held some beliefs with a strange intensity. One was love and reverence for Vermont and all it stood for. He had a summer home there. One of his great regrets was that he had not been born there instead of New Hampshire. It was in the late forties that Paul was instrumental in bringing Sammy Eilenberg into the department with consequences for its development and emphasis that lasted decades.

Koop, or Bernie, as we called him, had a completely different personality. In society, he was lively, wide-ranging, playful, and mordant. He loved to open a conversation, size up the strength and weakness in his fellows, and needle them on. My close contacts with him were of the greatest value to me in opening up new horizons, in encouraging me, and in planning some steps of my future.

During the thirties, the mathematicians and the physicists would eat lunch together at the Faculty Club every day at a round table with a normal capacity of six but with as many as eleven trying to reach their plates. The physicists included Rabi, Quimby, Kusch, Fermi, Lamb, and Townes, also on occasion Szilard or Teller; the mathematicians were Ritt, Koopman, Smith, A. C. Berry and myself. There were also Schilt and Eckert from astronomy and Selig Hecht from biophysics. At these lunches, no holds were barred, no subject was taboo. The only rule was no shop talk. The game was to produce the most froth. In this, Rabi and Koopman were the leaders. Alas, WWII put an end to our daily intercourse, and all concerned were the losers.

Koopman was heavily involved in questions of statistical mechanics and kept in constant touch with both G. D. Birkhoff and John von Neumann, who were both super specialists in the subject. On the occasion of one visit to Princeton in the fall of 1931, Koop learned that von Neumann, using one of Koop's ideas, had given a proof of the mean ergodic theorem, based entirely on the theory of unitary transformations in Hilbert space. Tremendously excited, Koopman passed on this bit of news to Birkhoff, indicating proofs. Presumably, Birkhoff did not comment in detail, but, harnessing all of his powers, succeeded during the next weeks in proving a theorem giving convergence almost everywhere as against von Neumann's weaker convergence in the mean. He immediately set about sending in his proof to the *Proceedings*

Paul Smith

Joseph F. Ritt

(Photograph of Paul Smith courtesy of David Plowden/Columbia College Today. Photograph of Joseph F. Ritt reprinted from *Biographical Memoirs*, Vol. 29, 1956, with permission from the National Academy Press, Washington, DC.)

of the NAS where it was published one year before von Neumann's corresponding result. Let us be more precise. GDB's results were communicated on November 27 and December 1 and appeared in the December 1931 *Proceedings.* Von Neumann's proof came out in the January 1932 issue. This brought on a near collision of our two meteors, and Koopman had to work hard to extricate himself and them—which he did in an explanatory article in the *Proceedings* written by himself and von Neumann (May 1932).

I was puzzled and irritated by Koop's attitude towards Bourbakism. Here was a movement which in my mind had been of such inestimable value in uprooting the stuffy leftovers of nineteenth century mathematics, and he, for his part, was persistently deriding it. I think there was a Dedekind cut in time on who became a Bourbakist and who on the other side was doomed to wander about in the once flourishing oases of the previous century. And I, for one, seemed to fall right at the cut or just to the right of it. As we enthusiasts grew up on our side of the cut, we collected some fifteen to twenty "fascicules" of the great man, read him, and cursed him roundly for his style (to read Bourbaki is like chewing hay), and were grateful. Naturally, the movement was overdone. The second generation of Bourbakists included some educationalists who promptly put the "new math" into the grade schools where there was an overkill. I am reminded of a cocktail party in Rome at which a mother of a fourth grade hopeful came to me and proudly announced, "My son has started studying "insiemistica." I was at first puzzled by what she meant, but pulling the word apart, it became all clear: insieme + mistica, that is, the mystique of sets (oder so etwas)!

On August 1, 1914, my father, who most of his life had been a loyal subject of King George V of England, discovered that he had made a serious mistake in finding himself and his family in Frankfurt, Germany. Within forty-eight hours of the declaration of war (WWI), he was arrested and marched off to a concentration camp in Berlin, called "Ruhleben" (life of peace), where he met hundreds of fellow Britishers who were destined to be his stablemates for the coming months. One of these camp mates was James Chadwick who discovered the neutron in 1932. (Ruhleben was the Berlin racecourse. When war was declared, racing was stopped, the stables were emptied, and the empty race course which was surrounded by its high fence to keep out the nonpaying public was adjudged an ideal place to keep the unlucky Englishmen.)

The fortunes of war determined that in 1918 I found myself in Englewood, N. J. where I was duly enrolled in the excellent public schools. It was there, sometime later, that I came to know from a distance an older upper classman who stood out from his peers. His name was Marshall Harvey Stone. Upon finishing high school, I was admitted next door to Columbia in 1924 as a pre-engineer. It was the dean, Herbert Hawkes, a student of J. W. Gibbs, who called me in one day after my advanced calculus course and pointed out that to him I looked more like a future mathematician than an engineer. That was close to the first time that I realized that one could make a livelihood following our Muse. The next semester, I met my first mathematician full on: J. F. Ritt, in differential equations, and it was a revelation. My decision had by now been made and in my senior year I entered graduate study by taking theory of functions (real and complex variables) with Thomas Scott Fiske.

There were at the time some 150 students registered in a more or less loose way in the graduate program. First year graduate courses had a population of sixty or so and for the first time after four years of living in the all male desert of Columbia College, there were women in the class. This cohort of 150 or so students sifted itself out over the years. Some went into the secondary school system or "ended up" at the Bureau of Standards. At the time, Columbia was producing one or maybe two Ph.D.'s a year. I have been given to understand that in the thirty preceding years there had been circa five woman doctorates. I recount here with reluctance and embarrassment an incident which was communicated to me without intermediaries. F. N. Cole, in giving advice to his successor at Barnard College, told him, "Don't ever employ any woman in your department. They'll give you only trouble".

T. S. Fiske was a kind, courteous, and distinguished person. Extremely handsome with his full mane of silver-white hair, his very ruddy complexion crowned by a sharp nose, and dressed always like the governor of the state rather than as a college professor, he imposed his personality on his class, which followed in awe. However, he had long ago given up his research activities, and it was an open secret that if one was to learn function theory, one had to do it on one's own. I don't remember many ε's appearing on the board and I am ready to swear that he never divided ε by 2 or by n in order to accommodate many clients in a proof. On the complex level, he made us read what he affectionately called "my little book" (*Functions of a Complex Variable*, 97 pp., John S. Wiley, 1907), but it was clear that to learn the subject one had to read Konrad Knopp or Osgood. Some years later, after his retirement, three of us younger instructors were assigned to his office. There we found two very heavy dumbbells (evidently hefting the fledgling AMS was not demanding enough for his young muscles) and, unless I am dreaming, a mounted head of a moose, presumably culled on a hunting expedition in the woods of Maine.

The basis of the graduate program consisted of three courses: real and complex variables, algebra, and projective geometry. Algebra was given by W. Benjamin Fite, a group theorist who right to the end contributed papers on his subject. The text used was Dickson's *Modern Algebra.* Inflicting such a book on students was most certainly not an act of kindness. It was awful. Fite taught the class as if we were reading Xenophon's *Anabasis.* Two pages every lesson, during which he reproduced the proofs on the board line by line as they appeared in the book. If Dickson used i and j as subscripts, the professor never made the mistake of using p and q instead. Fite was an exceptionally kind and sweet man. One almost forgave him his pedagogical deficiencies. My algebraic horizons were opened three years later when I read van der Waerden. I am proud to say that I gave the first course in "modern algebra" at Columbia in the spring of 1938 using this marvelous book. We also used to call it "abstract algebra." In fact, one of my students, Robert Schatten, raised questions with me on the first day as to whether the course was abstract enough for him, who evidently was anxious to get to the heart of the matter without foreplay. Schatten had a very disconcerting habit of calling his shots, sometimes years in advance. He was seldom wrong.

The course in projective geometry was given by a younger man, George Pfeiffer. It was based on Veblen and Young and was a good course. As we all know, that kind of course disappeared from the graduate curriculum of most universities. I gave the course at Columbia the last time it was offered. There was a spirit in the department which encouraged the younger members to broaden themselves by giving courses away from their main track. I took much advantage of this attitude over the years. I remember, in particular, giving the only course ever given in our department on mathematical logic. It was the summer of 1950. The heat was unbearable. All doors and windows were open. Next door, the great Jean Dieudonné was lecturing on group theory. Not lecturing but thundering. Since my class had heard his entire exposition in addition to mine, I offered to let my students take his final examination as well as my own.

The most vibrant mathematician at Columbia, and nationally recognized during the thirties and forties was Joseph F. Ritt. Here was a highly original and introspective thinker who developed his ideas and obtained his problems by reading the opera of the past great: Jacobi, Abel, Liouville. A tremendous worker, beset by poor health, he labored in solitude seldom "rubbing elbows" with contemporaries. His work was in a highly classical spirit, and since he did not need the recent mathematics of the twentieth century, he did not learn it. On many occasions, he questioned me on the theory of measure and integration, but although he seemed interested, he was evidently satisfied with the Riemann integral and more recent advances were nice but not too important. In some cases, he was contemptuous of recent trends. Thus, as a longtime worker using only real or complex numbers, he referred to finite fields as monkey fields.

When I was a young instructor (in the post depression one could remain at this level for six to eight years), I came to be quite intimate with Ritt. In fact, I was for many years his closest colleague. He had forgiven me for having dropped the earlier classical interests to which he had introduced me and to have turned my attention to linear spaces. Around 1941, I showed him the proof I had devised that the only complex normed algebra which is a field is the set of complex numbers. He was thrilled. (Gelfand's paper "Normierte

Ringe" containing this theorem did not reach our library until 1942 due to the German invasion of Russia. Of course, Mazur's earlier announcement of a proof was unknown to me.) This result helped to reconcile him to the power of modern methods. Ritt was a proud man and was much upset as years went on that no prize was awarded to him. In laughing about this misfortune, he would recite to us the epitath that he had composed for his tombstone:

Here at your feet J. F. Ritt lies;
He never won the Bôcher prize.

A principle at Columbia was that after receiving the Ph.D. one had to go off on a fellowship for a year or two before coming back to Columbia to become an instructor at $2700 teaching twelve hours a week, including trigonometry. The standard places to go to receive this coat of varnish were Harvard and Princeton. I applied and obtained a National Research Council Fellowship and was soon on my way to Harvard to study under M. H. Stone. There I met a fellow Fellow, Deane Montgomery, and we used to break up our life of continuous daytime study by meeting in his furniture-free apartment at night sipping beer cross-legged on the floor. The following year, I received an offer from the IAS to be von Neumann's assistant.

One of the perks for being a professor at the Institute was to have an assistant. The work load placed on this person's shoulders varied from ε to $1/\varepsilon$ depending on the professor involved. I went to Oswald Veblen for an indication of what would be expected of me. Veblen quickly, and with a modicum of annoyance, described four categories of activity:

1) Follow JvN's lectures, take notes, complete proofs, prepare mimeograph sheets of them, distribute them to the auditors.

2) Assist in the editing of the *Annals* of which JvN was leading editor. Prepare all accepted manuscripts for the printer. (Give all instructions: Greek, boldface, German, etc. Indicate displayed formulas.)

3) The *Annals* were being printed in the USA for the first time and no longer by Lütke and Wolf in Nazi Germany. The assistant was to go to Baltimore two afternoons a week to teach the printers how to set up subscripts, superscripts, etc.

4) JvN was at the time still writing up his many 100-page papers in German. The assistant was to translate, type up, and prepare these many papers for publication.

Veblen added with firmness that the above were the normal duties of the assistant but it would be fair game to add other duties which could not at the moment be foreseen. (I myself questioned the need of a translator at the time. Von Neumann had been lecturing in most fluent English (modulo some idiosyncrasies: "infinite serious") and seemed more than at ease. I was present at an after-lecture party in Harvard in 1934 where someone mentioned

Lewis Carroll's *The Hunting of the Snark.* Von Neumann and Wiener who stood nearby were set on fire by this spark and began to recite at "il più presto possibile" some 150 lines of the poem. So far as I could tell, the race was a dead heat.)

I went back home in a rather downcast mood. Upon arrival in New York, I found that Columbia had awarded me a Cutting Traveling Fellowship, worth $1800, which allowed me to travel freely to any and all countries and to devote all of my time to my studies. I reluctantly turned down the offer of the Institute and eagerly accepted the traveling fellowship which allowed me to spend nine months in the intimate company of Frederic Riesz in Szeged, Hungary. Here I had lunch (2 hours) and dinner (over 2 1/2 hours) with this kind great genius five times a week. In addition, during Carnival we would meet three nights a week at a hotel where I danced with the local talent until 3 A.M. I was told later that the work load originating from von Neumann's stellar position at the Institute was parceled out to four distinct people. I felt certain that each of the four young people who filled these positions were reasonably tired at the end of the day from their paramathematical activities.

During the critical years shortly after 1950, Columbia was the home of a distinguished group of stars including Claude Chevalley (who was said to have refused admission into his linear algebra course to anyone who had previously studied matrix theory), and Harish-Chandra, who stayed briefly before going to the IAS. Then there were at various times the French visitors: Hadamard, Denjoy, and Brillouin in physics. I remember an evening in the nine-room apartment of Leon Brillouin at Columbus Circle where he had on view ten to fifteen of the most spectacular Modiglianis (three full-sized canvasses per room) that one can imagine. An anecdote on Denjoy is in order. He was giving a series of about six lectures to an audience that started with a substantial number and plunged to a bare three graduate students after four lectures. And these three decided to go on strike claiming that their situation was untenable. Consternation in the department. Finally, the strikers, after much urging, agreed to go back to the lecture hall but on one condition: that Denjoy should cease lecturing in English and switch over to French.

Even earlier, there were several younger colleagues starting brilliant careers and contributing much newer-generation strength: Francis Murray, Ellis Kolchin, and Walter Strodt. It was said of Murray that any course that he taught became, in short order, a course on linear operators in Hilbert space. Kolchin and Strodt developed many ideas launched by J. F. Ritt in his ground-breaking work in algebraic differential equations.

An account of the "early" years at Columbia would not be complete without mentioning those outstanding mathematicians in the New York area who should have been members of our faculty and whose distinction earned them the title of Corresponding Members of our department. I am thinking principally of Jesse Douglas and Emil Post. Douglas' work towards solving the

Plateau problem led to his receiving the most distinguished of the many minimum wage prizes that society dangles before our profession: the Fields Medal. Its presentation at the Oslo Congress in 1936 is still sharp in my mind. Douglas himself was not present (maybe, like Bourbaki, he was too busy working at home on his problems) and Norbert Wiener stood in his stead, radiating personality, as he was listening to the glowing citation and as he was photographed by bevies of Norwegian newsmen. That afternoon a few local newspapers, not quite understanding the last minute change of cast, printed the story of Professor Jesse Douglas accepting the Fields Medal and showed with it the glowing photo of Norbert.

Emil Post was another one of us, although his manner was so soft-spoken and his subject so distant from our interests that no one paid much attention to him. Little did we know that we were in the company of a great person of mathematical logic.

Like other older American institutions of higher learning, Columbia changed from being a mere college to a university at the end of the nineteenth century. The Faculties of Political Science, of Philosophy, and of Pure Science were founded in 1880, 1890, and 1892, respectively. It is not a coincidence that the American Mathematical Society was founded during this same period. The fundamental underlying impulse was much the same. It is of some interest to note that, from the point of organizational structure, both the Society and the University had very much in common during the early years: a direct simplicity, a lack of superstructure, a type of growth that was not induced but to a large extent just happened. Yet each was taking care of those things that mattered. The two organizations were like siblings growing through a glorious adolescence and each one leading a protected existence. The cooperation between the two was close. Meetings of the Society were held in classrooms, members slept in dormitories, dinners were held on campus. The University, for its part, encouraged young students who heard the call of the Muse to take the critical step. Mathematics was a calling. The large broth of graduate students was allowed to simmer on its own. The chosen few surfaced by virtue of their gritty perseverance. The Society, on its side, had its six or seven officers. Miss Hull took care of the office. There were few publications, and three young ladies read proof for these. A library developed by accident through exchanges and was housed on an upper floor, where it would not be in anyone's way. The younger people got to know their brilliant elders, who seemed to enjoy their company.

A few years after the termination of WWII, this relaxed and slow moving laissez faire came to an end for both the Society and for the University, represented in our case by the Mathematics Department. The two siblings put behind their adolescence and became energetic, forward looking, and also aggressive institutions. The Society moved out and settled in Providence on its own real estate. Meetings were transferred from classrooms to hotel grand ballrooms seating a thousand or more. Lucky the person who knew five percent of the attendees. Members slept in four-star hotels at $70 per night. Publications multiplied. All the ills of society were fair game for discussion at Council meetings. The services of an Executor Director to act as a chief executive officer of a large corporation were obtained.

The Columbia department, for its part, underwent a parallel transformation. In the first place, mathematics became a profession, like law or dentistry. The department was awarded its own building, thus protecting its members from being contaminated by a stray philosopher or professor of English. Then graduate admission was strictly supervised. Each year some fifteen or more students were admitted to the Ph.D. program with the expectation that eighty percent of them would get a degree in four or five years. These students received free tuition and a stipend to "live." The teaching of calculus was revolutionized. Instead of having sections of twenty freshmen taught by impoverished graduate students who had "been around" for several years, the young were herded into large classrooms of eighty or one hundred and were lectured at by an expert in automorphic functions who had a platoon of graduate students as assistants. The professors applied to the National Science Foundation for grants which allowed them to lighten their teaching load and exempted them from the drudgery of teaching in summer session. Professors freely boasted of their contract appeal. The administration of the department was being carried out by a staff of five secretaries, some of whom would even type in TEX.

"Run like a country store," you could say of both the Society and the Columbia department some fifty years ago, whereas now they resemble more closely a highly efficient mail-order house. However, it is not necessary for us either to sink into nostalgia for the good old times or to swear by the leading edge of progress toward the future.

Both systems allow the greatest freedom in grappling with mathematics, in following the Muse. And what counts more for us than to consecrate ourselves to that Goddess of which Schiller, had he been a mathematician, would have sung:

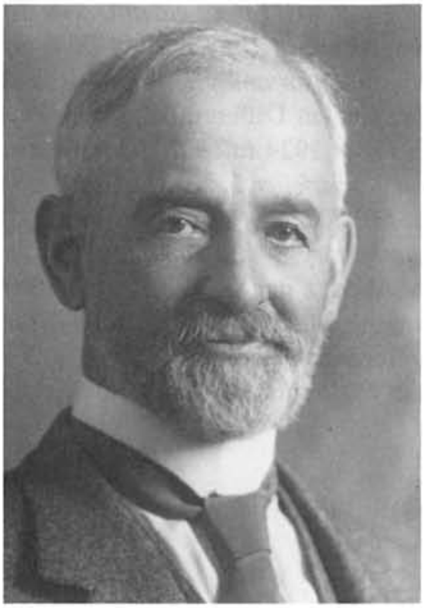
$\mathrm{M}\alpha\theta\eta$ schöner Götterfunken, Tochter aus Elysium

Dirk Jan Struik was born in Rotterdam and graduated from Leiden University. From 1917 *to* 1924, *he was assistant at the Technical University of Delft and collaborated with J. A. Schouten in his work on tensor analysis. This led to his doctoral thesis,* Grundzüge der mehrdimensionalen Differentialgeometrie, *at Leiden in* 1922 *under W. van der Woude. From* 1924 *to* 1926, *he visited the Universities of Rome and Göttingen with a Rockefeller Fellowship, and from* 1927 *to his retirement in* 1960, *he taught at M.I.T. His main scientific interests have been in differential geometry and the history of mathematics. Among his books are* Einführung in die neueren Methoden der Differentialgeometrie *(with J. A. Schouten),* Yankee Science in the Making, A Concise History of Mathematics, Lectures on Classical Differential Geometry, The Land of Stevin and Huygens, *and* A Source Book in Mathematics 1200–1800.

The MIT Department of Mathematics During Its First Seventy-Five Years: Some Recollections

DIRK J. STRUIK

The Massachusetts Institute of Technology was chartered in 1861 and opened its doors in 1865. At this Boston engineering school the teaching of mathematics, for many years, was directed by John Daniel Runkle, pupil and protegé of Benjamin Peirce of Harvard, first at its Lawrence Scientific School, where he graduated in 1851, then for many years at the Nautical Almanac office in Cambridge. He was the right-hand man of William Barton Rogers, the founder and first president of the Institute, and both men set their stamp on its whole educational policy. When Rogers had to take leave of absence, between 1870 and 1878, Runkle was president, in which function he was able to weather the severe financial crisis of 1873. He introduced several laboratory courses, had women admitted as students, and after 1878 devoted much of his energy to the teaching of mathematics. In this he was first assisted by Dr. William Watson, in charge of descriptive geometry (in the accepted tradition of the French Polytechnique), later by George A. Osborne and after 1884 by Harry Walter Tyler, an MIT graduate in chemistry



Harry W. Tyler

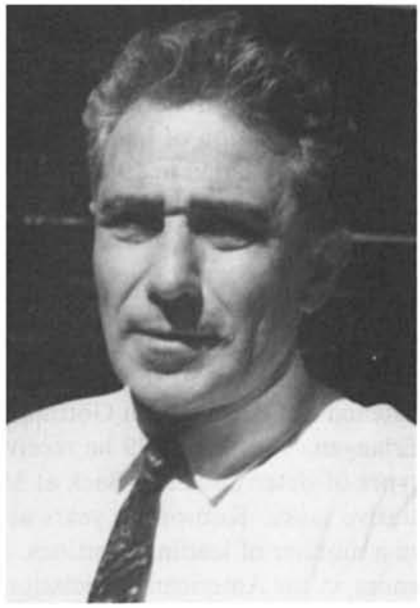
Clarence L. E. Moore

Henry B. Phillips
1941

Frederick S. Woods

(Photographs courtesy of the MIT Museum.)

Philip Franklin
1949



Norman Levinson

Jesse Douglas

Eric Reissner

(Photographs courtesy of the MIT Museum.)

who turned to mathematics and passed through the ranks from assistant to a full professorship in 1893.

Runkle saw the mathematics department strictly as a service department for the instruction of budding engineers, on a par with the language instruction. When he died in 1902, Tyler succeeded him as head of the department, a position he held until 1930. This department was section III of course IX, General Studies, when I joined it in December 1926. I remember Tyler as a greying, very correct, gentleman of middle size, with short beard and mustache, kind but disciplined, with a keen eye for administrative and educational efficiency. He belonged to a newer generation than that of Runkle, had learned some of the modern mathematics obtainable in Europe, having listened to Felix Klein in Göttingen and to Paul Gordan and Max Noether in Erlangen, where in 1889 he received his Ph.D. (his thesis dealt with certain types of determinants). Back at MIT he applied himself mainly to administrative tasks. Known for years as "Secretary of the Faculty", he was active in a number of leading positions, in the American Academy of Arts and Sciences, in the American Association of University Professors (AAUP), even in the Appalachian Mountain Club. But, having tasted a bit of modern mathematics, he was no longer satisfied in keeping his department purely as a service establishment for the teaching of undergraduates. Supported by the energetic president Richard McLaurin, Tyler saw to it that the mathematics department was considerably enlarged and creative scientific work encouraged by judicious appointments, like those of Moore, Phillips, Woods and Hitchcock. He taught for many years a course in the history of science together with his colleague, W. T. Sedgwick, the biologist and public health authority. The *Short History of Science* (1917) by Tyler and Sedgwick was one of the first such books in the English language, republished in a revised edition of 1939. Because Sedgwick had died, Tyler found as co-author another biologist-colleague, Robert P. Bigelow.[1]

Geometry, in its many forms from projective and differential geometry to quaternions and tensors, was popular with this first generation of Tech men engaged in research. First of all, there was C. L. E. Moore, "research advisor for mathematics of course IX." Clarence Lemuel Elisha Moore, Ohio born, with a Ph.D. from Cornell (1904), had traveled for a year in Europe, where he was profoundly influenced by E. Study in Bonn and by C. Segre in Turin—as was Julian Lowell Coolidge at Harvard. From 1904 on he had been on the teaching staff at MIT and had published a number of papers on projective and differential geometry, some in collaboration with others. A tall, lumbering, heavily built man, with poor eyesight, always willing to listen to others and to encourage younger men, he enjoyed with them the results of their studies. He was of particular support to young assistant professor Norbert Wiener,

[1]Incidentally, I had not, as the preface claims, "read the complete manuscript and made suggestions." I only offered suggestions on the mathematics.

who, in the days I came to Tech, had already done fundamental work on Brownian motion and harmonic analysis, Wiener being one of the first in this country to understand the importance of Lebesgue integration also for fields of applied mathematics. Despite these achievements Wiener remained uncertain of himself, being a man of many moods and disturbed by the fact that so far little attention had been paid to his work, especially in the USA. Wiener himself, in his autobiography,[2] remembers Moore as a "tall, slightly awkward, humorous and kindly man, with the human gift of affection and love of mathematics." Moore could not always follow Wiener—for that matter, who could?—after all, he was no expert in the more subtle forms of modern analysis. In my own case he could see exactly what I was doing, had even applied tensors in his research; his admiration for Ricci was such that he had Miss Richardson, his secretary, type out the whole of Ricci's *Lezioni sulla teoria delle superficie*, a rare, lithographed book of 1898—those were the days before Xerox.

A paper Moore wrote, in collaboration with his colleague Phillips, was on linear distance in projective geometry (1912), a paper I liked because it ties in, as Moore showed, with those cases in (imaginary) developables where ds^2 is the square of a linear form. He also published on surfaces in more dimensional space with E. B. Wilson, for a while head of the physics department before he went, in 1922, to the Harvard School of Public Health as statistician.

Moore died in 1931. We lost in him a mentor not easily replaced. His memory is kept alive by an instructorship in his name.

Wiener also pays his respect to Henry Bayard Phillips, a North Carolinian with a Ph.D. from Johns Hopkins (1905), who came to MIT in 1907. A widely read man, productive both in pure mathematics and in its applications, he drew Wiener's attention to the statistical mechanics of Willard Gibbs, which led Wiener to the discovery that the Lebesgue integral can play a role in matters of statistics, such as in Brownian motion. We saw already Phillips' interest in geometry. He wrote several textbooks, the one that always interested me was that small-sized book on differential equations (course M22), because it contained an abundance of pretty little problems in mechanics and physics. Some were a bit of brain teasers and there were instructors (myself included) who had trouble finding the solution. Moral dilemma: Shall we pick the brain of a colleague, perhaps of Phillps himself? Humiliating. Shall we hope that a clever student finds the solution first? Not quite cricket, as the British say. Let's try once more, OK now, and we can face our class with steady eye....—Wiener calls Phillips an individualist, and he certainly had philosophical ideas of his own, ideas I could not always follow, but that is neither here nor there.

[2]N. Wiener, *I am a Mathematician*, MIT Press, Cambridge, MA, 1964.

Another geometer was Frederick S. Woods, easy going yet dedicated, with a devotion to the Klein tradition. Like Tyler and so many others who would build a strong mathematical climate in the USA, like Osgood (for Harvard), Van Vleck (for Wisconsin), White (for Vassar), and Cole (for Columbia), he had joined the Gideon band of young mathematicians who in the 1880s had crossed the Atlantic in order to find in Europe what was not yet to be found in their homeland. Woods received his Ph.D. in 1895 under Klein himself, on a thesis about minimal surfaces in what we now would call Minkowski space $(++-)$. Most of his further work remained in the Klein tradition. Arriving in 1895 at MIT as an assistant professor (this was at "Boston Tech", not at the present monumental establishment across the river in Cambridge, dating from 1916, when McLaurin was president), he met as a colleague Frederick H. Bailey, a Harvard graduate, and began to collaborate with him on a series of textbooks that had a wide circulation. Among them was the two-volume *Course in mathematics*, published first 1907–1909, in which the calculus was taught didactically interlaced with algebra and analytical geometry, thus discarding the traditional boundary between these fields (going back, probably unconsciously, to the Leibnizian origins). In different modifications and reprints these "Woods and Bailey" books have been used for years all over the USA. There even was a French edition, a *Mathématiques générales* (1926).

Woods also published other books of interest, such as *a Non-euclidean geometry* of 1911 and a *Higher Geometry* of 1922, the latter still a very readable introduction to such Kleinean notions as line and pentaspherical coordinates.

Woods succeeded Tyler as head of the department in 1930, and left it to Phillips in 1934. He stayed on as an honorary lecturer. He died in 1950.

Still another geometer, or better geometer-algebraist was Frank L. Hitchcock, a Harvard graduate of 1910, the year in which he joined the department at MIT. Originally a chemist, much of his work was on the applied side; like Phillips he wrote a text on differential equations with nice little problems, but new for use in applied chemistry, with Clark E. Robinson as co-author. A modest gentleman, almost self-effacing, friendly, very helpful to students, a hard worker (I read that he published 200 papers) he may not have expected that his work would be useful in computer programming, but there is indeed a Bairstow–Hitchcock method of finding complex roots to polynomials (paper of 1944 by Hitchcock).

Hitchcock's thesis was on vector functions, and much of his mathematical work was dedicated to quaternions and their offspring. They were popular with this "older" generation at MIT, and not only here. We meet in this subject an old, let us call it, Anglo-Saxon hobby. Born under the famous Dublin bridge, quaternions were welcomed under the Stars and Stripes by Benjamin

Peirce, where they led him to the composition of the *Linear Associative Algebras* (1870), the first original mathematical book written in the USA. Then Gibbs, at Yale, crippled the poor quaternions and got in vectors a better insight into Maxwell's theory, as did Heaviside in England. Gibbs' method was explained by his pupil, E. B. Wilson (whom we already met at MIT in the 1920s) in a book of 1901 widely known as "Gibbs–Wilson". Quaternions etc. continued to fascinate American mathematicians; in 1910 both president and treasurer of the "International Association for promoting the Study of Quaternions" were Americans, the one in Ontario (A. Macfarlane), the other in Illinois (J. B. Shaw). Gibbs' dyadics led to Ricci's tensors, also at MIT; in the MIT *Journal of Mathematics and Physics* we find papers by several authors on this topic. Vector analysis was taught at MIT from a book of 1909 by a man with the dismal (Nantucket) name of Coffin, later replaced by a book by Phillips (1933).

In 1922 the department felt strong enough in its research efforts to publish, with the assistance of the administration and some members of other departments, this *Journal of Mathematics and Physics*. Here Moore, Wiener, Franklin and others could publish their results. It could show the mathematical world at large that MIT had reached a certain confidence in the field of the exact sciences.

Among the papers in the early issues of the journal we find some by Joseph Lipka. Lipka, Polish born, was a student of Edward Kasner at Columbia University, where he received his Ph.D. in 1912. He continued to work in that Kasner specialty of geometrical considerations related to classical dynamics, in particular the so-called natural families of curves in n-space. Lipka traveled to Italy and Levi Civita, represented MIT at the 700th anniversary of Padua University, but died of an operation soon after his return. This was in 1924, and he was no more than forty years of age. Since I came to MIT in 1926, I never met him. But he was remembered mainly through his conducting the mathematical laboratory they had at MIT (M54), reflected in his *Graphical and Mechanical Computation* (1918), often referred to as Lipka's Tables. This was still the time of the slide rule, and other mechanical computers such as harmonic analyzers. Wiener and several members of the electrical engineering department under Vannevar Bush had plenty of new ideas, which eventually led to the electronic computers. But that came later.

I have still to mention Lepine Hall Rice, with poor eyesight that grew worse, which led to his being pensioned off (or so I hope, pensions in academia were not what they are now, at any rate in the leading universities; some came from private foundations like that established by Carnegie). Rice's specialty was determinants. He left his large collection of reprints to me and they remained for years under my care—the MIT library had little use for reprints. Then Providence led me one day, while walking on Belmont Hill, to a grandson of the Thomas Muir who wrote the four-volume *Theory* *of Determinants* (1906–1923). He was living on the hill and after a question or two I received permission to have the reprints transferred to the family of that great expert on determinants.

Among the other members of the faculty there were men who concentrated on undergraduate teaching, a task taken seriously at MIT ever since Runkle's days. I remember Frederick H. Bailey, co-author of the Woods and Bailey books, Dana P. Bartlett who also taught least squares and had written a text about them, Nathan P. George, George Rutledge, author of papers relating to numerical calculation, and Leonard Macgruder Passano, the most colorful of the team.

Passano was a Baltimore man, and not only the author of some mathematical textbooks, but also of a school text on the history of Maryland and of several essays and plays ("A Family Affair", "Zimri the Kind", etc.). Tall, immaculately dressed, with neatly trimmed beard and spats, he saw himself as a man of the world, which he showed by having a large reproduction of Manet's *Olympia* (or was it Goya's *Maya*?) above his desk in his office. He had been at MIT since 1902. He could be witty; on one occasion when plans were discussed to strengthen the applied side of the department he opposed it, probably wanting also his Euclid bare: "Mathematics, the queen of the sciences, should not become its quean."

2

Among the younger men, men of my age, I found, apart from Wiener (whom I already had met in Göttingen), Samuel D. Zeldin, Raymond D. Douglass and Philip Franklin. Zeldin, born in Russia, had come to the USA sufficiently prepared to obtain his Ph.D. at Clark in 1915. His specialty was continuous groups, but after he joined the MIT staff in 1919 he concentrated more and more on teaching undergraduates, who appreciated his kindness and warmth. Douglass came from the University of Maine, wrote some papers with Rutledge, who also supervised his doctoral thesis (1931), but also was primarily a teacher, somewhat of a drillmaster, but an excellent and popular one at that—he had been in the Navy himself. Teaching remained, as I said, an important task and good teaching counted much in promotion. On the whole, I believe the students were satisfied; among the complaints about poor teaching heard at MIT during the years I do not remember many directed against the mathematics department.

New courses were added (and some courses were dropped, as those on elementary mathematics), such as the one of Wiener on harmonic analysis and the one of my own on differential geometry and tensors. There remained gaps, of course; even Harvard had them. To fill omissions to a certain extent a reading course (M90) was added, where students could study special subjects under the tutelage of a willing professor. The first Ph.D. was conferred on

James E.Taylor (1925), who went to Pittsburgh; the second to William Fitz Cheney (1927), whose thesis was on tensors, supervised by Moore. Cheney sat in on my lectures in the first year and was helpful in correcting my English ("don't try to make jokes in a language you don't fully control"). He was great on mathematical puzzles and was a popular performer at Open House. For many years he headed the department at the University of Connecticut at Storrs. The third Ph.D. was Carl Muckenhaupt—see Wiener's autobiography.

Philip Franklin was Wiener's brother-in-law, having married Norbert's sister Constance, a mathematician in her own right. The two men had met during the war on the Aberdeen proving grounds. Phil was an even-tempered, mild, humorous man, "of almost Mr. Chips proportions", as Dean Harrison said in 1965 at his funeral service—and a mathematician of many parts. His Princeton Ph.D. thesis of 1922 was in the Veblen topology field and a contribution to the four-color problem. Coming to Harvard first and then to MIT he brought a new field to Cambridge. "Franklin", Marshall Stone writes, "gave us [Harvard] our first systematic introduction to topology." In the MIT Journal of 1933–1934 he extended his studies to the six-color problem for one-sided surfaces. He was well versed in many fields of geometry, algebra and analysis. In 1936 he lectured before the American Mathematical Society on transcendental numbers. In the early 30s he published with Moore a set of papers on algebraic Pfaffians. Since Franklin brought topology to MIT in his "analysis situs" form, and Wiener in its "point-set-Lebesgue" form, we see that it came to the Institute through two brothers-in-law.

I have always had the feeling that living in the shadow, so to speak, of his overwhelming brother-in-law, cramped his style. At any rate, he devoted much of his time to the writing of eight excellent textbooks, such as his *Differential Equations for Electrical Engineers* (1931) and *Methods of Advanced Calculus* (1944).

Wiener's activities need not be discussed here at any length, since you can find them described in often fascinating detail in his autobiographical *I am a Mathematician* (1956). A Harvard Ph.D. of 1913 in mathematical logic (he was not yet twenty), he had joined the teaching staff at MIT in 1919 after a stay in Europe on a traveling fellowship. He was not only the most original thinker of our group with the most extended interests and a quick mastery of new topics, but through his many travels was also personally acquainted with many outstanding mathematicians in America and Europe. In his work, abstract mathematics blended with its applications to physics and engineering (later also to medicine), and in turn received much inspiration from workers in this field. In discussions with his student, Claude Shannon, and members of the electrical engineering department he laid the foundations of communication theory, and in discussion on harmonic analysis and the computers of that time, such as Bush's differential analyzers, he paved the way to the modern electronic types of computers, as well as to what he himself later would baptize cybernetics. All of this is in my—and many other people's—recollection connected with the picture of Norbert rambling along the corridors of the Institute, entering offices and labs, while buttonholing colleagues to test his newest ideas or worrying about Hitler or the latest foolishness of the State Department.

With the physicists, and especially with Manuel Sandoval Vallarta, scion of an ancient Conquistadores family of Mexico, who was remarkably well-informed, Wiener used to talk relativity and quanta. This was the time that Vallarta collaborated with the Abbé Georges Lemaître, a pupil of Eddington, on cosmic rays and the expanding universe. I participated in many of those discussions, a result of which was a paper that Wiener and I published in our MIT Journal, a paper in which we tried, using a theory of invariants by E. Cotton, to construct a partial differential equation embracing, by suitable normalization, both relativity and the Schrödinger equation. Rereading it recently, I was pleased by our referring to the then just-published five-dimensional theory of Kaluza-Klein, a theory recently found attractive to astrophysicists.

Wiener also gave a course on the history of philosophy. I never sat in on it, but we had many talks. I always enjoyed the way he had of grasping those elements in the philosophies of the past relevant to understanding the science of the period, or of vital importance to the present (and future) state of science. Hence, his admiration for Leibniz.

His teaching was erratic; many students could not follow him. But for the happy few with mathematical enthusiasm, like Levinson, Paley or Shannon, he was a lasting inspiration.

The electrical engineering department, aware of the role that the theory of probability was playing in telephone and related traffic problems, invited Thornton C. Fry of the Bell Telephone Laboratories in New York to lecture on this subject. The content of these lectures, held during 1926–1927, was highly appreciated; their substance can be found in Fry's book of 1928. When Fry returned to Bell, the mathematics department was asked to take over the course. In a reckless moment I volunteered; it was an adventure since all I knew was what I had picked up in a course at Delft. But it was good fun. At first I was only three lectures ahead of my class, but studying the books of Bertrand, Czuber, Fréchet and Coolidge gave me the chance to supplement Fry. My students shared my enthusiasm and the course became an annual event (M76). Eventually other departments organized their own courses and we could replace Fry with more rigorous material, in this case, Uspensky.

That probability came so late as a regular subject may seem strange today. Equally strange is the fact that there were no seminars or colloquia in the mathematics department. Those who had traveled knew them; the Hilbert seminar at Göttingen had a certain fame. After talking it over with Tyler and

Moore, I discussed the plan for a joint Harvard-Tech seminar with Marshall Stone, then a younger man at Harvard, and Stone was sympathetic.

We ran into a snag. Harvard, at that time, had already for many years a strong department with such leading men as Osgood, Bôcher, Huntington, Coolidge and Birkhoff, with promising younger mathematicians, and with some experience in seminars. But there existed an old resentment due to attempts by Harvard to "take over" Tech, and some of the older men had taken it personally. Moreover, the idea of "parity" did not appeal to some Harvard men. A colloquium, yes, but a Harvard one with MIT people graciously invited to attend. I talked it over with Moore. I still see him with his long body slumped in his chair, visor over his eyes, hands behind his head. "Well, Struik," he grinned, "I know these men. For them the MIT is still the vocational school down the river, and on top, there is still the old resentment. Go ahead, accept the offer, and everything will be straightened out in the long run."

And so it was. For years we had seminars at Harvard and MIT with sometimes excellent lectures and members of both institutions equally welcome. After 1948 Brandeis came in, together with an increasing number of specialized seminars.

Among the lectures I remember with great pleasure was J. A. Schouten from Delft. He came in 1931 and talked about the then new subject of spinors. He had a way of starting with a whole series of definitions—"*entia non sint multiplicanda sine necessitatem*", whispered Norbert into my ears—you recognize Occam's razor. The "necessity", of course, came up soon enough. Another lecturer was Felix Bernstein, in a seminar on probability, in which Eberhard Hopf also participated. This was the time of axiomatization of probability in set theory and of the discovery of ergodic theorems. Hadamard also lectured, although he had difficulty in being admitted to the USA because of his communist sympathies; he had just visited Brazil and was excited about the ferns he had seen.

This was a lively time, and a time in which the mathematics department was greatly strengthened, due to new appointments, more than once from the ranks of excellent graduate students. The whole of MIT was changing with the new Compton administration. Karl T. Compton, who became president in 1930, was an outstanding physicist with a long record of achievements mainly in electronic research. He understood fully that a modern engineering school can only be first grade if it is also a leading school of science, which at that time meant mainly chemistry, physics and mathematics. Thus began the transformation of MIT from a still essentially undergraduate college into a research institute of the first rank, but also maintaining or improving its educational facilities. Through the appointment of Vannevar Bush to the vice presidency Compton obtained the cooperation of this aggressive, administrative and engineering genius. Things were moving, and so was the mathematics department.

Just as the electrical engineering department had been emancipated from the physics (already in 1902), the mathematics department obtained its independence from General Studies in 1933 and changed from IX–C to course XVIII. The graduate courses attracted outstanding students. Among those who eventually joined the faculty we find George Wadsworth, Prescott D. Crout, and Norman Levinson, all with doctorates from MIT. The rising reputation of Norbert Wiener—in 1934 he received, with Marston Morse, the Bôcher prize—was a great attraction for students, and also for established mathematicians to accept appointments, as did William Ted Martin and Robert H. Cameron. Crout and Wadsworth did their best work after the period I am dealing with, Crout in computational research in applied fields, Wadsworth in meteorology and oil geology (using ideas of Wiener). Levinson, who started in electrical engineering, became one of Wiener's most brilliant disciples. His work covers many fields of analysis, in nonlinear equations and in prime number theory. Some of his results appeared in his *Gap and Density Theorems* of 1940.

Another follower of Wiener, equally outstanding, was Raymond Paley, a young Englishman fresh from Hardy and Littlewood. He had long sessions with Norbert, showing "a superb mastery of mathematics as a game"; a result of this collaboration was the influential *Fourier Transforms in the Complex Domain* (1934). Paley was as reckless in sport as in mathematics; he found an early death while skiing in the Canadian Rockies. "If he had not come to an untimely end he would be the mainstay of British mathematics at the present time", wrote Wiener in 1956.

A particularly original mind was that of Claude E. Shannon, who started in 1933 as a student, got his Ph.D. in 1940, became a member of the faculty until he left for the Bell Telephone Laboratories, then returned in 1959 as a permanent member of the faculty. Under Wiener's and Bush's inspiration he wrote his *Mathematical Theory of Communication* (1949), which, with his later work, has made him one of the creators—if not the creator—of information theory, developing his ideas from his observation that switching circuits in automatic telephoning can be based on the algebra of logic. Making communication engineering possible, Shannon's work has turned toward the age of the modern computer, as did that of Bush and others at MIT.

Norbert Wiener was, as we see from all this, pretty much the center of research in the department of those days. From outside, as I said, came William Ted Martin, also influenced by Wiener and turning his research into function theory of more variables. Martin left MIT to head the department at Syracuse, but returned to MIT to head the department of mathematics, where he succeeded Phillips in 1947 as efficient head of the department.

No wonder that Phillips, in his report on the department of 1935, could write that it "is now regarded as one of the strongest in the country, both in teaching and research." In 1933 the visiting committee, pointing out that all principal fields of mathematics were covered except perhaps number theory, concluded, perhaps with some exaggeration, that Cambridge had become "the most inspiring mathematical center in America."

The first woman Ph.D. in mathematics at MIT was Dorothy Weeks; her thesis of 1933 was on coherency matrices. For many years she was on the faculty of Goucher College in Maryland.

Among those who came to the faculty and eventually left were Robert H. Cameron, Jesse Douglas, Eberhard Hopf, and Otto Szász. Cameron, a Cornell Ph.D. (1932) like Martin, with whom he co-authored some papers, was influenced by Wiener and was at MIT from 1935 to 1945, when he left for the University of Minnesota. I remember with pleasure the department picnics he and his lady organized, one or two at Walden Pond. Hopf was a Ph.D. from the University of Berlin (1930), had worked in celestial mechanics, and had come to Harvard, I believe, to study with Birkhoff. Through Wiener's influence he became a member of the MIT department, where he stayed from 1932 to 1936. One well-known result of his stay was the Wiener–Hopf equation, expressing Hopf's ideas on cosmic radiation joined to Wiener's insight into prediction theory. Hopf returned to Germany in the Hitler period, attracted by a good professional offer, not because Nazi philosophy appealed to him or his wife. Needless to say, we did not like his choice.

This is the place to remember Jesse Douglas, nervous, emotional, and a remarkably sensitive mathematician. Like Lipka a Kasner graduate of Columbia, he was at MIT from 1930 to 1936. An analyst with a subtle feeling for its geometrical side, he did his best known work on existence theorems in the problem of Plateau, for which he received the Fields medal in 1936 and the Bôcher prize in 1943. We had long discussions, in which he worried about the papers by his rival Tibor Radó. He was fond of anecdotes, some quite funny, mixed with his own special little prejudices; my experience, he said, is that geometers are as a rule nice fellows, and analysts are nasty. He had his own lifestyle which did not include coming to class on a regular schedule, so that Phillips, who stuck to the Runkle discipline of conscientious teaching, had to let him go, to my and others' regret. He lived mostly on fellowships, but spent the last ten years of his life at CCNY. He died in 1965.

With Otto Szász we come to the refugees who came from Central Europe after 1933, often after a sojourn in England or France. It was again Wiener who, through his multifold connections, took much of the initiative in bringing mathematicians over and finding places for them—after all, he was a Jew and knew anti-Semitism from personal experience. Some of us also did our best, with hospitality or by signing affidavits required by the immigration authorities. Placing was not easy; the depression was not over yet and there was still a good deal of anti-Semitism in American academia. Among those who came and became members of the staff, I remember the fine personality of Otto Szász, a Hungarian versed in the methods of his teacher and friend Lipot Fejér in questions pertaining to Fourier analysis in its widest sense. He left in 1936 for Cincinnati. Equally welcome was Witold Hurewicz, topologist and student of Brouwer in Amsterdam, who enriched his field by the influential concept of homotopy groups. He was the second brilliant mathematician at MIT to lose his life in an accident: Paley died in Canada, Hurewicz died in the Yucatán, on a sightseeing trip after attending an international conference in Mexico City (1956).

Hurewicz became a permanent member of the faculty, and so did Eric Reissner, of the Technische Hochschule in Berlin; he obtained his Ph.D. at MIT and remained there, investigating problems in mechanics and elasticity, often questions of bending and buckling of plates. He thus became a force in building the applied side of the department. C. C. Lin came in somewhat later.

There were several others who came and went, like Antoni Zygmund, exponent (with S. Saks) of the Polish approach to analytic function theory. But I think back with particular pleasure to the lectures of Stefan Bergman on functions of several complex variables, not only because of his enthusiasm, but also because of the plastic way he combined his analytical developments with geometrical illustrations of four-dimensional figures. Felix Bernstein came for a while, then became involved in the mathematics of population genetics, on which he lectured at a conference on probability we had at MIT in December 1933; Eberhard Hopf also presented a paper, with some interesting ideas he had on the relationship between causality and probability. These were the days of the foundation in set theory of probability (Kolmogorov's book in the *Ergebnisse* is of 1933) and of the formulation of ergodic theories, in which Birkhoff and Wiener participated; Bernstein's paper was one indication of how stochastic ideas were penetrating all fields of science, as Fry's book had already shown for engineering. The conference took place at the occasion of the yearly meeting of the American Mathematical Society, then held in Cambridge. (The annual dinner was held at the Walker Memorial at MIT with Julian Lowell Coolidge as toastmaster; he was good at it: "Hi, hi, the gang's all here!") Indeed, we met many very bright but economically very unhappy men in those 1930s—men and women. Emmy Noether also visited MIT; the department should have given her an appointment. I do not know why it did not work out.

So much was going on at MIT that I will not go into more details—only mention some names to revive old memories: Sammy Saslaw, Harold Freeman, Nat Coburn and Norman Ball, Shikao Ikehara and Yak Wing Lee. The awarding of Ph.D. degrees was now an annual event; other talented students had to be satisfied with bachelor's or master's degrees, often going on to complete their studies elsewhere.

Among my own students in differential geometry I like to mention Domina Spencer, now well-known as an author of books on mathematics and engineering with her late husband, Parry Moon of the engineering department, Hsin P. Soh and Alfonso Nápoles Gandara. Soh came to us with rather erratic mathematical knowledge but interesting ideas on relativity. But, with a fellowship we got for him, Franklin, Vallarta and I supporting it, he published a paper in the MIT journal of 1932–1933 in which he outlined a field theory based on a complex Riemannian line element, the real part expressing gravitation, the imaginary part electromagnetism. Soh left us for China at the time of Japan's open aggression, which upset him very much. I wonder what happened to him; attempts to find out have failed. If a colleague in China reads this paper, can he tell me something about Soh's later life?

Nápoles was a Mexican with Aztec blood in his veins, not a Castillian like Vallarta. He was a pupil of Sotero Prieto, who against many odds, had been trying to introduce modern mathematics into Mexico. Nápoles, after his return to Mexico, continued, not without success, Sotero's work at the Universidad Nacional where he became head of the department. He invited me to lecture in the summer of 1934—foreign lectures were still quite a novelty at the time, so that my lectures received a remarkable publicity, even my blue eyes were mentioned. My visits, later repeated, may have helped to increase interest in, and respect for mathematics in Mexico. But the honor of creating a school of creative mathematicians goes to Solomon Lefschetz, who spent many months during the years at the Universidad.

After the excursion to Mexico I spent my sabbatical year, 1934–1935, in the Netherlands, where I collaborated with Schouten on our two-volume book on tensors and their application to Riemannian geometry, and with him visited a symposium on tensors in Moscow.

One other mathematical excursion worth mentioning occurred at that time, that of Wiener to Beijing, then called Peiping, also at the invitation of one of his students, in this case Y. W. Lee. You can read about his adventures in his autobiography. When he returned, he could speak Chinese, and tried it out on Chinese students, who told us that his Chinese was very good. Norbert was always good at languages. Curiously enough, he stayed away from Russian, although his father had been a professor of Slavic languages and a translator of Tolstoy.

We now have arrived at the 1940s and the distinguished role MIT mathematicians have played during the war years. But this part of the story I must leave to others.

Wilfred Kaplan has been associated with the University of Michigan since 1940. After completing his Ph.D. at Harvard in 1939 under the guidance of Hassler Whitney, he spent a year at the College of William and Mary before accepting T. H. Hildebrandt's bid to come to Ann Arbor. His research has concerned the topology of curve families, dynamical systems, and complex function theory. He is the author of influential textbooks on mathematics for engineering students, including Advanced Calculus, Ordinary Differential Equations, and Operational Methods for Linear Systems. *For many years he has played an active role in the AAUP, serving for a time on the national Executive Committee.*

Mathematics at the University of Michigan

WILFRED KAPLAN[1]

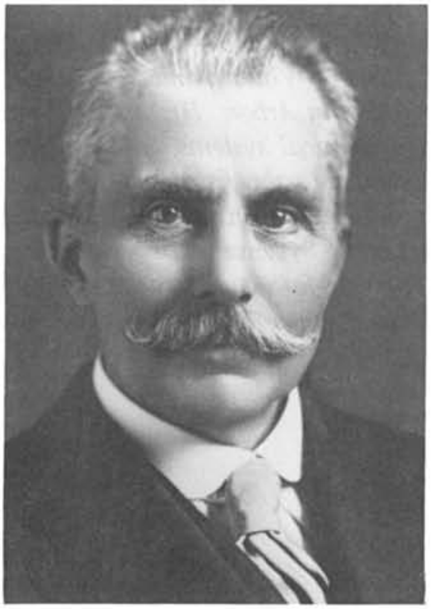
INTRODUCTION

This article is confined to the story of the Mathematics Department in Ann Arbor. For the period up to 1940 an excellent history appeared in [2]. This provides much detail about the professors and curriculum. Because of the availability of this source, the early period will be treated rather concisely.

THE PERIOD TO THE END OF WORLD WAR II

The first hundred years. The University of Michigan traces its beginning back to 1817, when a Catholepistemiad of Michigan was created in Detroit [**4**, Chapter 1]. The primitive conditions, however, prevented realization of the plan until 1837, when regents were appointed for an institution in Ann Arbor. It took four more years before buildings could be erected and five professors appointed. On September 25, 1841 instruction began, with seven students in classes taught by two professors: the Reverend George P. Williams for mathematics and science, the Reverend Joseph Whiting for Greek and Latin.

[1]The present article has been prepared, in accordance with advice from the editors, as a shortened version of an article on file at the departmental office in Ann Arbor, including a list of all faculty from 1841 to 1988.



Alexander Ziwet
(about 1920)

George Y. Rainich
(about 1930)

Raymond L. Wilder
(about 1930)

Theophil H. Hildebrandt
Chairman, 1934–1957
(about 1940)

(Photographs of A. Ziwet and R. L. Wilder courtesy of Michigan Historical Collections and photograph of T. H. Hildebrandt courtesy of University of Michigan News and Information Services.)

By 1854 there were sixty-three freshmen and a comparable number in the three higher classes and Professor Williams was being assisted by professors from other disciplines. The curriculum covered algebra, geometry (Legendre), trigonometry, analytic geometry, calculus.

In 1863 Williams became Professor of Physics and Edward Olney was appointed Professor of Mathematics. Until 1872 Olney and one instructor did the teaching. From 1872 to 1877 the staff gradually rose to five. The curriculum expanded slightly, with encouragement to those who wished to pursue topics such as quaternions, calculus of variations and calculus of finite differences. Olney wrote several textbooks for the courses. By 1887 there were courses on projective geometry and the theory of functions, including elliptic functions.

The University's library had started with 3707 volumes (purchased for $5000), covering many fields. It offered little in mathematics and grew very slowly. A major improvement came in 1881 when a complete set of Crelle's Journal was donated.

From 1888 on the department expanded steadily. Notable additions were Alexander Ziwet and Frank N. Cole (Ph.D. Harvard, 1886), the first Ph.D. in mathematics to join the department. Both were much involved with the New York Mathematical Society, which became the American Mathematical Society (hereafter referred to as the Society) in 1894.

Cole was inspired by Felix Klein, whose seminar in Germany he attended in 1883–1885. In 1885–1886, as a graduate student at Harvard, he lectured on the new geometric function theory. He came to Michigan in 1888 and remained until 1895, when he went to Columbia. His years at Michigan were especially productive, yielding eight papers on group theory and a translation of E. Netto's *Theory of Substitutions*. This work stimulated further important work in the field. While in Ann Arbor, Cole had as student and colleague G. A. Miller, who later had an active role in the Society and a distinguished career in group theory. From 1896 to 1920 Cole was secretary of the Society. Concerning his career one is referred to [**1**], especially pp. 100–103.

Alexander Ziwet was on the publication committee of the Society from 1898 to 1912 and was vice-president in 1903–1904. His career at Michigan lasted from 1888 to 1925. He did much to improve the courses and the library, donating a large personal collection of books. He also left a bequest of about $20,000 to the University. The Ziwet Fund has supported a series of Ziwet lectures by outstanding mathematicians, beginning in 1936. From the obituary in the *American Mathematical Monthly* (vol. 36, 1929, p. 240), we quote: "Professor Ziwet was outstanding as a scholar and teacher. His range of knowledge was not limited to mathematics, especially from the applied point of view, but extended to many sections of pure mathematics, history of mathematics and the humanities. As a linguist he was perhaps unsurpassed by any member of the University faculty.... He was a potent influence in the University, not only for high ideals in connection with engineering education, but also in the promotion of graduate work and research."

Another important figure was James W. Glover (Ph.D. Harvard, 1895), who was in the department from 1895 to 1937. In 1902 he offered the first courses in actuarial mathematics and over the years did much to build a strong program in this area. The interest in insurance mathematics had arisen much earlier: John E. Clark and Charles N. Jones were department members from 1857 to 1859 and 1875 to 1888, respectively, who later had careers with insurance companies.

We also mention Walter B. Ford (Ph.D. Harvard, 1905), who wrote on asymptotic series and summability theory; he was active in the Society, holding various posts; he was in the department from 1900 to 1940, but continued to be active in research until his death in 1971 at the age of 96. He was concerned about the college-level curriculum, wrote several textbooks for it, and was a great supporter of the Mathematical Association of America (MAA), of which he was president in 1927–1928. Early investments in IBM made him very wealthy and he gave generously to many philanthropies, as well as to the Chauvenet Fund of the MAA. In 1973, after his death, his son Clinton B. Ford gave a large sum to the MAA to create the Walter B. Ford Lecture Fund. The obituary (*Amer. Math. Monthly*, vol. 78, 1971, pp. 1094–1097) refers to his high standards for exposition: "A doctoral candidate under his supervision could always expect to prepare at least twenty drafts of his dissertation before its linguistic format would be approved."

Louis C. Karpinski (Ph.D. Strasbourg, 1903) was in the department from 1904 to 1948 and had a distinguished career in history of mathematics. Clyde E. Love joined the department in 1905 and had wide influence through his textbooks.

By 1908 the department had grown to twenty: four professors, two junior professors, five assistant professors, nine instructors. The curriculum included Fourier series and spherical harmonics, ordinary and partial differential equations, theory of substitutions, theory of numbers, theory of invariants, potential theory, courses for teachers.

In 1909 Theophil H. Hildebrandt (Ph.D. Chicago, 1910) joined the department and remained until 1957. A student of E. H. Moore, he did important work in functional analysis and integration theory. For example, in 1923 he gave the first general proof of the principle of uniform boundedness for Banach spaces, before the work of Banach and Steinhaus (1927). In 1928 he published a basic paper on the spectral theory of completely continuous transformations (compact operators) on Banach spaces, completing earlier work of F. Riesz. His pioneering research in these developing areas of analysis is described by Dunford and Schwartz, *Linear Operators, Part I* (*Interscience*, 1958).

Tomlinson Fort was in the department from 1913 to 1917; he was active in the Society for many years. Harry Carver joined the department in 1916 and had a distinguished career in statistics; he was in many ways a pioneer in the development of this field in the U. S., having personally started the *Annals of Mathematical Statistics* and taking a leading part in the founding of the Institute of Mathematical Statistics. He remained until his retirement in 1961.

Beginning in 1901 there was a gradual separation of mathematics instruction for engineering students, with Ziwet in charge. This lasted until 1928, when the engineering department was absorbed in the original Mathematics Department in the college of arts and sciences. It should be remarked that at the University of Michigan, as at many other universities, the question of who should teach mathematics to engineering students has remained a bone of contention over all the years; many Engineering College professors have taught "engineering courses" indistinguishable from mathematics courses.

Around 1920 the curriculum expanded by introduction of courses in applied mathematics: vector analysis, hydrodynamics, elasticity, celestial mechanics; also courses in infinite series and products, divergent series, history of mathematics, graphical methods.

Wooster W. Beman functioned as chairman of the department from 1887 to 1922. He was succeeded by Joseph L. Markley, who held the title for only four years, when Glover became chairman. During Markley's term, there were several important additions to the staff: James A. Shohat, Ruel V. Churchill, Cecil C. Craig, Ben Dushnik. Shohat made important contributions to analysis, including a book on the moment problem written with J. D. Tamarkin; he was in the department from 1924 to 1930. The other three remained in the department until retirement. Churchill did much for the applied mathematics program and had wide influence through his books on applied analysis. Craig did important work in statistics. Dushnik was active in set theory.

Glover also brought in some new talent: George Y. Rainich and Raymond L. Wilder in 1926; Walter O. Menge in 1925; William L. Ayres and Arthur H. Copeland in 1929. Rainich, in relativity theory, and Wilder, in topology, did much to strengthen the teaching program and research, especially through seminars. Ayres was also an outstanding topologist and was active in the Society; he left the department in 1941. Menge strengthened the actuarial program; he remained until 1937. Copeland made important contributions to probability theory.

In many ways Hildebrandt, Rainich and Wilder brought the department to a higher level of breadth and seriousness. Each had appreciation for mathematics far beyond his special field and encouraged students and younger staff in all fields.

Most of Rainich's papers and his greatest achievements were on the theory of relativity. In a series of papers in the 1920s he showed that the mathematics of the general theory which Einstein had made to supply a model for gravitation, also supplied one for electromagnetism. Rainich ran an "orientation seminar" for advanced undergraduate and beginning graduate students, covering a broad spectrum of topics; he had a remarkable talent for building enthusiasm of the young students and encouraging them to make careers in mathematics. He and his wife, Sophie, often entertained new and old members of the department at their home and thereby did much to bring the new ones into the life of the department. As émigrés from Russia they brought cultural breadth to university life.

Wilder was a topologist of first order, a product of the R. L. Moore school and (as seemed to follow axiomatically) a superb teacher, using the Socratic method to let the students do the discovering. He had twenty-five Ph.D. students. Wilder was a pioneer in the development of the topology of manifolds. By methods of algebraic topology, he extended to higher dimensions many of the results of set-theoretic topology in the plane and 3-space. Some of his best achievements are found in his AMS Colloquium Publication *Topology of Manifolds* (vol. 32, 1949). He had very broad interests in topology and hence could recognize new talent of great variety. He also promoted interest in logic and foundations through a very popular course. He was president of both the Society (1955–1956) and the MAA (1965–1966).

Hildebrandt did much to encourage work in the rapidly developing area of functional analysis. He was very active in the Society and in the MAA, serving as president of the Society in 1945–1946. In 1929 he was the first recipient of the Chauvenet Prize, given for a 1926 paper on "The Borel theorem and its generalizations." In recognition of his leadership as chairman over twenty-three years, the T. H. Hildebrandt Research Instructorships (later Assistant Professorships) were introduced in 1962. He also had a great interest in music, acquiring a degree in that field in 1912 with the organ as specialty. A testimonial to him after his death in 1980 stated: "He was more than an outstanding scientist and enthusiastic expositor of mathematics; he was a leader who took a deep interest in the personal as well as the mathematical growth of his students and colleagues."[2]

In 1934 Hildebrandt became chairman and further appointments were made: Edwin W. Miller in 1934, specializing in set theory—he died of a

[2]The testimonial is from the minutes of a meeting in Fall 1981 of the faculty of the College of Literature, Science and the Arts of the University of Michigan. The memorial was drafted by George E. Hay and Cecil J. Nesbitt.

heart attack in 1942; Paul S. Dwyer in 1935, working in statistics; Sumner B. Myers in 1936, in differential geometry; Robert M. Thrall and Cecil J. Nesbitt, algebraists, and Robert C. F. Bartels in applied mathematics, in 1937–1938; Herman H. Goldstine in 1939, in functional analysis. The last five named had noteworthy career changes: Myers turned to functional analysis, Thrall to operations research, Nesbitt to actuarial mathematics, Bartels in 1967 became the first director of the University's computing center, Goldstine went on leave in 1941 but did not return, having been drawn into basic research on digital computing with John von Neumann (see his article "A Brief History of the Computer," which has appeared in Part I of *A Century of Mathematics in America*, American Mathematical Society, 1988, pp. 311–322).

Over the years 1922–1941 courses were steadily added, so that at the close of the period all the main branches of mathematics were covered, with a fair number of courses at the graduate level. The department had its first Ph.D. in 1911: W. O. Mendenhall, who wrote on divergent series under the guidance of Ford. By 1922, eleven doctor's degrees had been granted; by 1941, ninety. Among the recipients were Ralph S. Phillips and Charles E. Rickart, both students of Hildebrandt in functional analysis.

As a fitting climax at the end of the first 100 years, the department sponsored a two-week Topology Congress in June 1940, with Wilder and Ayres as organizers. The speakers included S. Eilenberg (who then joined the department), E. Van Kampen, S. Lefschetz, H. Whitney, S. Mac Lane, C. Chevalley.

The war years. As elsewhere, World War II had a devastating effect on the University and, in particular, on the mathematics program. Enrollments diminished and some faculty took leaves for military research. There were several military training programs on campus, such as the Air Force Meteorology Program and the Navy's V-12 Program.

The department did add a few professors at the time: Edward F. Beckenbach, in analysis, who remained only two years; Wilfred Kaplan, who had come for the Topology Congress; George E. Hay, in applied mathematics, who later became chairman (1957–1967); Erich Rothe, in functional analysis; the topologists Samuel Eilenberg and Norman E. Steenrod.

There were also several Ph.D. students finishing up, who helped to sustain interest in research. Among these were L. J. Savage, who went on to a career in statistics, and S. Kaplan in topology.

One unusual by-product of the war was a seminar on meteorology, bringing together several professors including G. Y. Rainich, the physicist G. Uhlenbeck, the geologist R. L. Belknap, the aeronautical engineer A. Kuethe. A motivation was to work on a topic that might have practical applications and help the war effort.

Those who were on leave for military research and those who joined the department after the war, having done such research, gained breadth by the experience and their subsequent research and teaching showed a better understanding of such topics as control systems, operations research, applied statistics, communication networks.

Immediately following the war the enrollments shot up and soon the returning GIs appeared, bringing a highly motivated group of students.

Research discussion groups. There were many formal and informal gatherings to discuss research. A Mathematics Club meeting monthly has existed for about 100 years; according to Jones [**3**, p. 9], this began prior to 1891 in Prof. Ziwet's parlor. In [**1**, p. 50], Cole in 1891 referred to a "Mathematical Society of the University of Michigan." In [**6**, p. 3], Wilder tells of a secret mathematics club of about twelve members, including professors from physics and philosophy, meeting during the period 1927–1934. The writer also recalls a similar club organized by S. Eilenberg about 1940, called "The Gauss Group."

Eventually the secrecy was abandoned. In the 1940s regular colloquia were held and research seminars arose in ever-increasing numbers.

The Postwar Years

The professional staff and research activities. Under Wilder's leadership, the University of Michigan quickly grew to be a major center of topology, with such leaders as Eilenberg,[3] Steenrod, Bott and Samelson. There were many Ph.D.'s in the field, including S. Smale in 1957 (winner of a Fields medal in 1966). In real and functional analysis Hildebrandt and Myers provided strength; they were followed by E. H. Rothe, L. Cesari, P. Halmos and many others. Complex analysis took on great vigor following a two-week international conference in 1953; this was the beginning of an important "Finnish connection," involving many visits of faculty to and from Finnish universities—F. Gehring, who joined the department in 1955, became a leader in this enterprise. Number theory was fostered by W. J. LeVeque, D. J. Lewis and others. Applied mathematics had a strong old tradition in the department. Statistics continued as a strong interest, but a separate department was formed in 1969. Logic was promoted by Wilder through a very popular course and book on foundations; the program was sustained by R. Lyndon and others. A small group, including P. Jones and C. Brumfiel, ran a program for the training of secondary-school teachers of

[3] Major joint work of Eilenberg and Mac Lane was a by-product of Ziwet lectures given by Mac Lane in 1941. During one of these lectures (on group extensions) Eilenberg suddenly left the room! The audience wondered whether something was wrong, but later learned that he had just then realized the important connection between group extensions and topology. In the following days Eilenberg and Mac Lane were often found conferring intensely.

mathematics. In algebra and algebraic geometry strength gradually developed, with R. Brauer in the department from 1948 to 1952. Actuarial mathematics had an ancient tradition, created by Glover; this was continued by Nesbitt, A. L. Mayerson, C. H. Fischer and D. A. Jones. For some years Michigan's program in actuarial mathematics was generally considered the strongest in the country.

The instructional program. The table below gives basic information on the growth of the department and of mathematics instruction up to 1985. In column 2 the count is made at the beginning of the fall term and includes visitors but excludes regular staff on leave. The count is affected by the creation of a Computer Science Department in 1965 (later, in 1983, attached to the Electrical Engineering Department) and of a separate Statistics Department in 1969. For column 5 comparable figures are not available for 1940–1960. Although large lecture sections have been used for some second- and third-year courses, the freshman calculus course has been taught only in small sections, generally by teaching assistants, with some coordination by professors.

Growth of the Department, 1940–1985

1	2	3	4	5	6
Year	Departmental Roll		Under-graduate Majors	Graduate Students	Ph.D.s Granted in Previous 5 years
	Staff	Teaching Fellows			
1940	35	8	53		29
1945	35	11	32		21
1950	48	20	80		24
1955	61	34	56		56
1960	63	71	165		43
1965	84	116	290	303	66
1970	65	112	281	250	105
1975	64	80	114	165	101
1980	64	81	74	123	63
1985	66	92	111	131	61

The scope of the undergraduate program has broadened over the years to reflect increasing use of mathematics in engineering and the sciences, especially the social sciences, and the revolution in computing. The following are typical: courses in advanced mathematics for engineers, linear programming, algorithms, operations research, numerical analysis at various levels, mathematics for the social sciences. The concern for K–12 mathematics education has led to expanded offerings in teaching of mathematics, including summer institutes and in-service programs for teachers.

In order to meet the needs of gifted students, two special sequences of courses were introduced in the post-Sputnik era: one proceeding somewhat more rapidly through calculus and differential equations; the other an honors sequence over the first three years. The enrollments in these sequences in 1970 and in 1985 were: rapid sequence: 1970, 218 and 1985, 196: honors sequence: 1970, 73 and 1985, 20. In 1970, 152 students graduated with majors in mathematics; in 1986, 64 did so.

A general master's degree has been offered for many years. Recently specialized programs were introduced in applied mathematics, secondary mathematics education and scientific computing. The number of degrees granted in 1970 was 101 and in 1985 was 24.

The actuarial program produced about 400 graduates from 1903 to 1940, about 300 from 1940 to 1960 and about 200 in each succeeding decade. Among these is the distinguished mathematician T. N. E. Greville, who received a Ph.D. in 1933. (See a forthcoming history of the actuarial profession in North America, titled *Our Yesterdays*, by Jack Moorhead.)

The Ph.D. program has been a major interest of the department. Since 1940 it has been supervised by a committee which has considered entrance requirements, progress of candidates and evaluation of dissertations.

Among those receiving the Ph.D. at Michigan are many who have gone on to successful careers in mathematics. These include W. Feit, 1955, M. Jerison, 1950, D. J. Lewis, 1950, J. R. Munkres, 1956, R. Phillips, 1939, F. Raymond, 1958, S. Smale, 1957, L. J. Savage, 1941, J. R. Schoenfield, 1953, E. H. Spanier, 1947, F. L. Spitzer, 1957.

Since 1960, the Sumner B. Myers Prize has been awarded to those whose dissertations have been deemed outstanding.

Other aspects of department life. Various endowed lecture series have brought outstanding mathematicians to the department for a week or more. The oldest of these is that named after Alexander Ziwet; the first lecturer was Edouard Čech in 1936 and the series has provided twenty-six visits up to 1986. The series named after George Y. Rainich has had three speakers: Lipman Bers in 1983, Michael H. Freedman in 1986, Richard M. Schoen in 1988.

During the academic year there has long been a tradition of a weekly colloquium speaker, very often a visitor. In addition fifteen or more specialized seminars have flourished, from beginning graduate to advanced research levels.

The Mathematics Club has long preserved special customs. The meetings are held in the evenings and are also social occasions, with refreshments served. The talks are expository, aimed at a broad audience, and are often followed by vigorous discussions. Each meeting opens with a reading of the minutes of the previous meeting. Then come unannounced "three-minute talks," presented by staff or students, on some novel ideas found in research or teaching. Finally (and this may be an hour later) the announced speaker is allowed to take over. The informality and spirit of fun at these evenings have done much to build *esprit de corps*.

In 1952 the *Michigan Mathematical Journal* was initiated, under the leadership of Rainich. He became an early fan of "desktop publishing" when he discovered that the new departmental typewriter could justify margins; what better way to exploit this than in a new mathematics journal? (The machine was very primitive by modern standards, and the result was unattractive. A better printing process was soon found which did not, however, justify margins.) From 1954 to 1975 George Piranian was editor; his high standards and dedicated labor put the journal on firm ground. He was succeeded by Peter Duren (1976–1977) and by James Kister and Carl Pearcy with some alternation in the following years.

In 1964 *Mathematical Reviews* moved from Providence to Ann Arbor. Its presence has brought other mathematicians to the area and provided a lively association with the department.

The department has generally avoided being embroiled in political matters. However, one member of the faculty, H. Chandler Davis, was a victim of the "McCarthy period" of the 1950s. He cited the First Amendment to the Constitution as a basis for refusing to testify to a congressional committee investigating communism. He was dismissed from the University in 1954 by action of the regents. There was widespread faculty criticism of this step. A later investigation by AAUP led to a censuring of the University. Davis has described the events of this period in his article "The Purge", which has appeared in Part I of *A Century of Mathematics in America* (American Mathematical Society, 1988), pp. 413–428.

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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.

Albert C. Lewis was a student at the University of Texas at Austin from 1963 to 1975, receiving B.A. and M.A. degrees in mathematics and a Ph.D. in history of mathematics. Supported by a Humboldt Fellowship in 1975–1976, he did research in Europe on the work of H. Grassmann. He then helped to found the Archives of American Mathematics, serving as Curator of the History of Science Collections at Texas from 1977 to 1981. For the last five years he has worked with the Bertrand Russell Editorial Project at McMaster University in Hamilton, Ontario. His publications include articles on Grassmann, Russell, and the Texas mathematicians Halsted and R. L. Moore.

The Building of the University of Texas Mathematics Faculty, 1883–1938

ALBERT C. LEWIS

INTRODUCTION

In 1938, when the American Mathematical Society was celebrating its semicentennial, R. L. Moore of the University of Texas at Austin was president of the society. Also in that year, R. H. Bing, a twenty-four-year-old high school teacher in Palestine, Texas, took one of Moore's summer courses for teachers. In 1973, Bing returned to Texas after twenty-eight years at Wisconsin and four years later became the second professor at Texas to be elected president of the American Mathematical Society. These personal honors of Moore and Bing also mark periods of growth and of attention at Texas to achieving or regaining high standing among departments across the country.

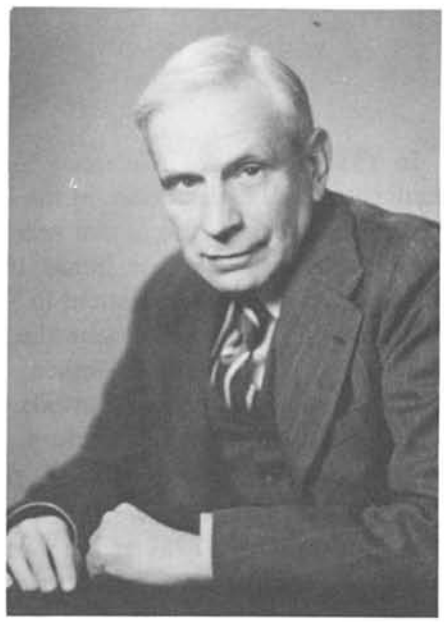
R. L. Moore, a student at Texas from 1898 to 1901 and a member of the faculty from 1920 to 1969, has to play a leading role in any complete history of mathematics at Texas as the most famous member of the department.[1] But there were mathematicians, principally Harry Y. Benedict and Milton B. Porter, whose names were relatively unknown outside of Austin but whose associations with Texas were just as long and intimate as Moore's and whose

[1]There is a fairly substantial literature on R. L. Moore and the Moore method of teaching. The principal ones are [Traylor], [Wilder 1976], and [Wilder 1982].

G. B. Halsted
ca. 1896

M. B. Porter
1912

R. L. Moore
ca. 1905



H. S. Vandiver
ca. 1955

(Photographs courtesy of the University of Texas at Austin, Archives.)

contributions to mathematics at the university were, in their way, essential to achieving the success it has had. And then, most important, there was George Bruce Halsted, who started it all.

This account focuses on faculty relationships and recruitment for the first fifty-five years, including some of the influences leading to comings and goings of faculty members. It does not attempt to include the many other aspects that would go into making up a complete history of the department: students, libraries, buildings, curricula, visitors, relationships with other departments at the university, and other external influences. And, the most important caveat, it talks around the mathematics which is at the center of the lives of the people described here. Most of the principals in this account are well represented in standard references or other works cited here. The subject at hand is confined to their institution building.

The years spanned by this account are rather well covered, in both their problems and their triumphs, by materials at the University Archives of The University of Texas at Austin, including the nationally-oriented Archives of American Mathematics. Documents have been preserved dealing with topics which in more modern times might be considered too sensitive or controversial even to put in writing in the first place, let alone be preserved. This is fortunate for those trying to explain the development of a department—or, for that matter, of any social group—since it is often the calamity that marks a turning point, for better or worse. The signal for the historical researcher that he is nearing the end of this revealing archival vein occurs when he finds a letter written in the 1920s to the university president at the bottom of which the president has written "Answered by telephone."

THE FIRST SCHOOL OF MATHEMATICS: A FALSE START

The Texas Constitution of 1876, re-expressing a concern stated as early as the 1827 Constitution under Mexico, called for the establishment "as soon as practicable" of "a University of the first class." It also provided one million acres of West Texas grazing lands, supplemented in 1883 by another million, as an endowment for the University and the Agricultural and Mechanical College of Texas.[2]

A faculty could not be formed, however, until 1883. The leader in getting to that point was the very active president of the first Board of Regents, Ashbel Smith, who was born in Hartford, Connecticut and obtained a medical degree from Yale in 1828. He came to Texas in 1837, served the government of the Republic of Texas in several capacities, and helped prepare the way for the annexation of Texas to the United States in 1845. Among his subsequent activities, he served in the Mexican War, was appointed to the board

[2]Detailed accounts of the legislative history are provided in [Lane], [Benedict], and [Griffin].

of visitors of the U.S. Military Academy at West Point in 1848, and was appointed a juror for the 1876 centennial celebration in Philadelphia. As one of many doctors who joined the Texas militia, he played one of the leading roles in the defense of Texas during the Civil War. A lifelong bachelor, as a member of the Texas legislature, he was credited in large measure with the establishment of the Texas school system.[3] To be charged with the founding of a university modelled after the University of Virginia was probably seen by him as the crowning contribution to his services to Texas at the age of 77—Thomas Jefferson was in the same decade of his life when he proposed and pressed for the Charter for the University of Virginia.

Smith wrote to professors and presidents at the older universities, relying especially on Virginia, soliciting advice and nominations for a faculty. Even at this early stage, the mathematics professorship seems to have given more trouble than the others—at least most other appointments appear to have been handled in a unanimous fashion and not brought to a recorded vote, let alone two votes. Apparently, the Board of Regents initially agreed to seek senior people whose reputations were well established and who would thereby bring immediate prestige to the University. Thus in 1882 the minutes record that agreement was reached on establishing one professor in the "School of Mathematics Pure and Applied" at $3,500.[4]

The vote of the board in November was General LeRoy Broun, 1; Professor Bruce Halsted, 1; General Kirby Smith, 2; Professor Alexander Hogg, 2. Since no choice was made, a second ballot was held with the results of General LeRoy Broun, 4 and General Kirby Smith, 2.[5] The military titles serve as a reminder of the postbellum atmosphere of academia to which many former officers returned after the war. Edmund Kirby Smith, for example, originally from Florida, was a general in the Confederate Army, having graduated from the U.S. Military Academy in 1845 where he also taught mathematics from 1849 to 1852. In 1883, he taught mathematics at the University of The South in Sewanee. Alexander Hogg, from Virginia, received his A.M. degree from Randolph-Macon College and was the first professor of pure mathematics at Texas A&M College (now Texas A&M University). He left Texas A&M in 1879 to become a civil engineer with a railway company.[6]

Halsted, born in Newark, New Jersey, was a promising 29-year-old graduate from Princeton University, and at that time an instructor there. His major publications up to that date included several on logic and geometry and a textbook on mensuration. He later described what he was doing at Princeton in

[3][Handbook].

[4]Minutes of the Board of Regents, 17 August 1882. Unless otherwise stated, all original archival sources cited in this paper are either in the University Archives, which also handles the Archives of American Mathematics, or in the Eugene C. Barker Texas History Center. Together these repositories form a part of the General Libraries at The University of Texas at Austin.

[5]Minutes of the Board of Regents, 16 November 1882.

[6]On Smith, see [Wakelyn]. On Hogg and Smith, see [Geisser].

what were probably much the same terms he submitted to Texas. Upon obtaining his Ph.D. at Johns Hopkins University under J. J. Sylvester in 1879, "after further study at the University of Berlin, [he] was called to Princeton in 1881 to plan and inaugurate a system of post-graduate instruction in mathematics, and having established it, he remained to give instruction...." His only connection with the South at this point appears to have been that his mother was from South Carolina. His father was a lawyer who worked in Washington D. C. during the war and was a friend of Abraham Lincoln.[7]

William LeRoy Broun best fit the requirements of the board. Born in 1827 in Virginia, with an M.A. from Virginia in 1850, before the war he had been a professor of mathematics at the University of Georgia and afterwards a professor of physics and astronomy. This was followed by the presidency of the State College in Georgia. He had been Professor of mathematics at Vanderbilt University in Nashville for the past seven years. During the war, he commanded the Richmond arsenal, a part of the Confederate Ordnance Bureau, where he became friends with J. W. Mallett, formerly a professor at the University of Virginia, now the chairman of the new faculty at Texas.[8]

When Broun acknowledged his official notice of election in November 1882, he wrote Smith asking what the date of opening was, what buildings would be completed at that time, what provision had been made for obtaining scientific apparatus, and what would be the available income from the University's endowment. The following month he asked for assurance that the salary would not be reduced after he arrived in Texas and asked, "Is the term of office for 'good behavior and satisfactory performance of duty', or is it for a term of years only?" Finally, in January 1883, Broun accepted the position. On 31 August, he arrived in Austin on the same train from New Orleans that Mallett was on.[9]

Neither of them were to stay in Austin for long. Mallett left after one year to return to the University of Virginia. Broun was elected chairman of the faculty in his place in May 1884 but stayed for only a short period, having already given notice in January that he wished to resign because of his daughter's sickness. Some of the regents wished to take advantage of this opportunity to split the chair of pure and applied mathematics and to have a separate chair of applied mathematics which would include engineering. Regent Simkins, in particular, said that he heartily endorsed any such move towards "practical attainments" and "the sooner we begin, the better for the University."[10] Under Broun, the applied mathematics offering as given in

[7][Halsted 1893]. *The New York Times*, 3 July 1871. There is no collection of Halsted papers at Texas, or any known elsewhere, and thus we have only his letters in other collections. A few brief biographical articles exist: [Tropp], [Lewis 1973], and [Lewis 1976].

[8][Lane, p. 268], [Broun].

[9]Ashbel Smith papers: Broun to Smith, 29 and 28 December 1882, 20 January 1883.

[10][Vandiver F, p. 441]. Smith papers: Regent Thomas D. Wooten to Smith, 26 January 1884; E. J. Simkins to Smith, 8 May 1884.

the catalogue consisted of applications of calculus to mechanics and physics "for those who have completed the course in Pure Mathematics."

Another regent's views seem to have had more immediate effect. Thomas D. Wooten, a medical doctor who moved to Texas from Kentucky in 1865, wrote Smith that he wanted to consider younger people for Broun's position.

> I think their vim and enterprise would suit the genius of our people better and are more likely to stand by the university as a means of their own advancement and success. I am not disposed to favor any more confederate... [*illegible*]. But am in favor of selecting if possible living moving progressive men, men who will accept the situation and the university as it is, accept it as their own and willing to work for it stand by it identify themselves with it and the state and people of the state and if they are not likely to have such a fealty we don't want them. I believe it might be well to require them to take some such oath and thus require them to live on Texas soil the year round. As it now is they can hardly wait until the close of the session to get away, as though they had to escape some pestiferous clime or moral infection. So far as I now feel they may all go to the devil or any where else they may choose to go.... Broun has not for some time come up to the true measure of a great man in my estimation. He suggests in his last conversation that we ought to have a president. I think he felt his inability to head the institution and grapple with the situation....

When Ashbel Smith died in 1886, Wooten became chairman of the board and remained on the board as its head for the record period of nearly eighteen years.[11]

It was in 1884 that William Sydney Porter ("O. Henry") came to Austin and a time, as one of his biographers has put it, when the notation 'Gone to Texas' placed beside a man's name made it suspect that "he was on the verge of bankruptcy, unwanted marriage, tuberculosis or some other disaster."[12] A proper university in the capital city would go far to overcoming this sort of image and Regent Wooten's urging to take a chance with willing younger people who would grow with the university was followed—albeit without the formal oath requirement—in choosing the next head of mathematics.

George Bruce Halsted, 1884–1902: A Faculty of One

At the board meeting in August 1884, Halsted was elected for the professorship at a salary of $3,500 plus $500 for housing. Though Halsted never

[11]Smith papers: Wooten to Smith, 24 January 1884.

[12][O'Connor, p. 18].

seemed to shy from blowing his own trumpet, he probably did get good letters of recommendation from Princeton and Johns Hopkins. These might well be the letters he had printed up in 1883 in a twenty-five-page pamphlet, *Some Testimonials and Credentials of G. B. Halsted*, which included letters by Sylvester, Simon Newcomb, C. A. Young, Josiah Royce, and a former student, Henry B. Fine. Fine had just received his A.M. degree and was to become a professor of mathematics and dean at Princeton and president of the American Mathematical Society in 1911. Fine wrote,

> ...through [Halsted's] influence I was turned from the Classics to Mathematics, and under his instruction or direction almost all of my mathematical training has been acquired. From personal experience, as well as from what I know of the general opinion of his Princeton pupils, I can testify that Dr. Halstead [sic] has the gift, so rare among teachers, of throwing a charm about the very difficulties of his subject, and of awakening real enthusiasm in all who have the least aptitude for it.[13]

Once established in Texas, Halsted wrote back to Princeton leaving no doubt with his former classmates that he had made the right move:

> My lines have fallen here in pleasant places, and I am actively happy as the official head of pure science in a state larger than the German Empire.... I am thoroughly in love with Texas and have purchased ten thousand dollars worth of its soil. I have not yet married and so am open to engagements. My salary here is four thousand dollars for nine months at two hours a day, and besides I have furnished to me an assistant who is paid two thousand dollars a year; so you see that monetarily my position is better than that of the President of Princeton.[14]

Halsted was to marry and raise three sons in Austin. His salary, however, did not grow and, in fact, was to be reduced fourteen years later in unhappier times.

As for the more "practical" side of mathematics which Regent Simkins urged, a new instructor, Alvin V. Lane, was appointed to handle applied mathematics courses given in parallel with pure mathematics and designed as preparation for engineering: "Engineering, Surveying, Mechanical Drawing, etc." Lane was made an associate professor in 1885, but he left in 1888 to join a Dallas bank and was replaced by T. U. Taylor, a graduate of the University of Virginia, who stayed for forty-eight years and came to embody

[13] Letter dated 25 September 1882 as printed in the copy in Princeton University Library.

[14] *Decennial Record, Class of 1875* (Princeton, 1885), p. 43.

engineering at Texas. The following year, the catalogue listed applied mathematics with Taylor as a division under Halsted's School of Mathematics. The next year, the School of Applied Mathematics was listed in a completely separate fashion, but Halsted's domain still had the more general name of School of Mathematics. Halsted's title, however, changed from Professor of Pure and Applied Mathematics to Professor of Pure Mathematics, while Taylor became Associate Professor of Applied Mathematics. The courses offered were non-overlapping. Halsted made a motion at the November 1894 faculty meeting that the regents be asked to "formally and officially" separate the School of Applied Mathematics from the School of Pure Mathematics and to put Taylor in "complete charge" of the former.[15] Whatever the difference may have been between the official catalogue statement and the actual working arrangement, such discrepancy characterized the relationship between pure and applied mathematics (or, more precisely, between two groups of mathematicians classified under these rubrics) for most of the history of the department(s), even after they were officially merged in 1953. In 1896, however, the official distinction was evident. The School of Pure Mathematics under Halsted was followed in the catalogue by the School of Applied Mathematics under Taylor.

Beginning in 1888, Halsted had a succession of students from Texas schools who specialized in mathematics under him and either became instrumental in shaping the future of mathematics at the university or led distinguished careers elsewhere. In 1888, M. B. Porter, from Sherman, Texas, entered the university after attending various schools, followed the next year by H. Y. Benedict, with little formal education, from land in West Texas settled by his parents when they came from Kentucky. Leonard E. Dickson took his first course under Halsted in 1890 and was in the same sophomore class as George Washington Pierce. Florence P. Lewis obtained her bachelor's degree in 1897. R. L. Moore came from Dallas in 1898 from a good school background and after having studied W. E. Byerly's *Differential Calculus*, the text used at the university.[16]

Benedict, as a fellow (i.e., teaching assistant) and then tutor (a full-time, post-graduate teaching position) in Halsted's department in 1892 and 1893, taught freshman courses, and, after getting his M.A. degree in 1893, worked at the University of Virginia astronomical observatory. He credited T. U. Taylor, under whom he had taken undergraduate and graduate courses in applied mathematics, for guiding him in general and getting him this job at Taylor's alma mater. After two years, he was able to enter Harvard where he

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[15]Faculty minutes, 6 November 1894.

[16]R. L. Moore papers: Halsted to Moore, 18 February 1898. [Vandiver H 1961], [Archibald] on Dickson, [Greenwood 1988]. For references to classes and grades, here and following, the source is the records in the Registrar's Office of The University of Texas at Austin.

attended lectures by Maxime Bôcher, Byerly, B. O. Peirce, and Osgood, and obtained his doctorate in 1898.[17]

Porter graduated from the university in 1892 and, after tutoring on a sugar plantation, went to Harvard where he obtained a Ph.D. in 1897 under Bôcher. He then returned to Texas for two years as an instructor before going to Yale.

Dickson and G. W. Pierce obtained their B.S. degrees from Texas in 1893 and their M.A. degrees the following year. Pierce went on to get a Ph.D. at Harvard under J. Trowbridge and W. C. Sabine, and continued as a professor of physics there until his retirement in 1940. Dickson was the first ranked graduate of the "academic departments" at Texas in 1893 and delivered the oration at the commencement: "A plea for pure science."[18] He had been a fellow in Halsted's department in his last year but submitted a letter of protest to Halsted which the latter passed on directly to the regents. (One cannot be certain that Halsted did not request Dickson to make the protest.) Dickson said he was getting paid as a fellow ($300) for doing the teaching work of a tutor. Halsted requested the creation of a tutor for his department "so that classes could go on this year as every other year of the University's existence." Regent Wooten agreed after consultation to try to provide for a tutor "with all the pay at the disposal of the executive committee." The university's treasury had only $200 at the time, but the next $100 to come into their hands would go to make up the total of $600 needed, "the Committee being anxious to accommodate you." Presumably this was done—Dickson continued teaching through the rest of the 1893–1894 year.[19] We shall return to Dickson shortly.

Florence P. Lewis studied for part of 1900 at the University of Zürich, left Texas the following year for a $1,000 position in Mississippi,[20] and then returned to teach at Texas for 1902–1903. In 1913, she received her doctorate from Johns Hopkins and taught for most of her career at Goucher College.

W. L. Prather resigned from the regents in 1899 to become president of the university, as he deemed it his "duty" to do so. In 1895, the university had taken up what W. L. Broun called for when he left in 1884, the establishment of an office of president appointed by the regents, instead of a chairman or president elected by the faculty. The president preceding Prather, Winston, left under a cloud, and rather fierce competition ensued to fill the position. For a time, a major contender for the position was Dudley G. Wooten whose father, T. D. Wooten, was president of the Board of Regents. He withdrew his candidacy in July 1899 at the same time his father left the board.[21]

[17] H. Y. Benedict papers: Benedict to Taylor, 2 June 1933; lecture notes, 1895–1898.

[18] Commencement Program, 21 June 1893.

[19] Benedict papers: Halsted to Regent T. M. Harwood, 21 September 1893.

[20] Halsted, "The School of Pure Mathematics," *The University Record*, June 1901, p. 146.

[21] T. S. Henderson papers: G. Winston to Henderson, 22 June 1899; D. G. Wooten to Henderson, 12 July 1899; Prather to Henderson, 7 October 1899.

After his graduate year at Texas, Dickson went to Chicago where he was one of E. H. Moore's first doctoral students. After getting his degree in 1896 and spending time briefly at the Universities of Paris and Leipzig, he went to the University of California as an assistant professor in 1899. In April of that year, Dickson wrote Judge Clark, who, as proctor at Texas, acted as a useful and widely trusted intermediary between students, faculty, and regents:

> Upon learning that Dr. Porter had been appointed to *Yale* instructorship I dropped a line to Prof. Halsted asking if they could not manage to keep Porter perhaps as Assistant Professor; as the former had so intimated his intention of recommending advance next year.... It has occurred to me that Texas could afford to make an assistant professor at least—and that the many ties binding me to Texas people would make it very congenial for me there. But I write this confidentially to you—as there is no need for me to go begging for a place! Of course I could not accept an instructorship under my present status here and offers east. Last spring I had an offer at Michigan and later an increased offer there—and came *near* going. The Chicago people, with all of whom I have most cordial relations, have corresponded considerably about my going there—but I see no need of changing except for a *better* position, which they could not offer, as the expected *vacancy* there did not occur.
>
> Please say nothing of my willingness to be considered—until (granting, of course, Porter's resignation) the authorities are willing to provide an adjunct [assistant, in modern terminology] professorship. In the latter case I can get the warmest support from the heads of the departments at Columbia, Michigan, Chicago, California, Indiana, etc.[22]

Dickson agreed to a three-year appointment as associate professor at Texas, beginning in the summer of 1899, but then took up an offer in April 1900 to go to Chicago as an assistant professor. This was taken by some regents and faculty members as at least a gross insult if not morally and legally wrong. Regent Cowart, from his Washington, D.C. office, wrote the chairman of the board, T. S. Henderson, that he was "disgusted" with Dickson's resignation: "It seems we are fated to develop men of extraordinary mathematical genius, and then other institutions of learning appropriate them as soon as they show any promise." Cowart also wrote to Proctor Clark: "I am simply disgusted with the infernal way that some of our professors have of coming before us constantly and clamoring for an increase of salary, and when any prize glitters before their eyes in a northern University they quit us without any

[22]Memorabilia of the University of Texas: Clark—Dickson to Clark, 1 April 1899.

excuse. I think Dickson's conduct is simply infamous, and I intend, if I can, to have a set of resolutions adopted characterizing it as it should be."

The regents took their retribution by not paying Dickson for the last two months he taught—the remainder of the spring term. Dickson attempted to get this money with the intervention of a lawyer from his hometown of Cleburne, Texas, but the records do not show if he ever received it. There is no record of Halsted's stance in this affair. T. M. Putnam, who had come to Texas as an instructor from California, apparently with Dickson, left with him for Chicago and was one of his first doctoral students there. During Dickson's last brief tenure in Texas, he was R. L. Moore's calculus instructor.[23]

No official announcement of this unexpected vacancy was made by the university, but Dickson's leaving and its circumstances soon became widely known. Cowart thought that they could get Porter if they could "put him on an equality" with "G. A. L. Halstead," and he tried personally to get him to take Dickson's place at the same rank and salary and thought for a while he had succeeded. "I consider him," Cowart wrote later to Henderson, "fully the equal of Dickson, and in a few years I think we could arrange it so that he could be put in charge of that school instead of the Barnum who now disgraces it."[24]

"Cowart has gone too far," Prather wrote to Henderson, "in asking Dr. Porter to accept Dr. Dickson's place." The university may not have the money to pay this salary, and it may not be the "wisest arrangement" at present. Whether the university backed up the offer or not, Porter did not then come to Texas. It has been conjectured that he was put off from returning because of the Dickson incident. Perhaps it is significant that when he did return, it was to take Halsted's place.[25]

Cowart, in referring to Halsted as "that Barnum," may have had in mind an incident from several years previous on another front. Arthur Lefevre, originally from Baltimore and a graduate of the University of Virginia, received a degree in civil engineering from Texas in 1895, at age thirty-two, and in 1894 started as an instructor in the School of Pure Mathematics. In 1896, he published a book, his only publication in mathematics, *Number and Its Algebra*, which referred to Halsted but which went substantially beyond the latter's work on the subject. Apparently, a symposium based on the book was to be held. C. S. Peirce was invited to attend such an event sometime

[23]Henderson papers: Prather to Regent Henderson, 6 April 1900; Cowart to Henderson, 21 April 1900, 8 May 1900, 10 May 1900; W. J. Ramsey to Henderson, 6 January 1901. Benedict papers: Cowart to J. B. Clark, 31 May 1900.

[24]Henderson papers: Cowart to Henderson, 21 April 1900, 8 May 1900. Evidently, "G. A. L." is Cowart's evocation of a form of insult whereby a man is called a girl. In a letter of 6 November 1901, he says of another displeasing man that he "would have made a fine girl."

[25]Henderson papers: Prather to Henderson, 22 May 1900, [Greenwood 1988, p. 13].

before 1898.[26] In 1897, Lefevre appealed to the Board of Regents to try to counter what he saw as injustices being done to him by Halsted and by a president, Winston, who avoided involvement. Lefevre's appeal provides one person's view of what it was like to work as a colleague with Halsted. Lefevre says that Halsted encouraged him to oppose an order of the president's increasing the number of sections of freshman mathematics. Halsted himself, according to Lefevre, said that he had already lost $500 of his salary over casual oppositions and thus was timid about doing battle over this. But the president claimed to Lefevre that Halsted had advocated the increase all along with him and he was just supporting the head of the school. In a related incident described by Lefevre, Halsted had spent an hour in Porter's sophomore classroom with the main purpose of criticizing Lefevre's preparation of the freshmen. Apart from the inappropriateness of the action, a large proportion of the freshmen had not even taken freshman mathematics at the university, according to Lefevre, but had been admitted with credit from an "affiliated school" accredited by Halsted himself. Also, Lefevre claimed that the whole university knew that Halsted had, in his classes, charged Lefevre with unacknowledged appropriations in his book from Halsted's work.

In May 1902, on Halsted's recommendation, E. H. Moore was willing to accept R. L. Moore as a student at Chicago, especially since Halsted ranked him, with equal training, as superior to Dickson. But there were, E. H. Moore replied, no fellowships available for the coming year.[27] Thus Halsted proposed to hire Moore as a tutor. Moore had been a fellow the previous year but since he would no longer be a student he would have to be appointed tutor. There was such a position open since one of the current tutors, E. P. R. Duval, had resigned to become an instructor at the University of Oklahoma. The president and regents had another candidate in mind, Mary Decherd, a school teacher who had been recommended in one letter to the regents as a "relative of Governor Sayers and... from one of the oldest and best families in Bastrop County." Letters of recommendation also came from the Commissioner of the General Land Office and from a former principal of Austin High School.[28] After Moore had left Texas to teach at a high school, Halsted wrote to him:

> I raised the five hundred dollars to pay for you here with me, and made the proposition to Mr. Brayther, and he rejected it. ... Of course I made a fuss about it.

[26] [Eisele, p. 70] where "MS 183" should be "MS 229." Max H. Fisch has provided this reference.

[27] R. L. Moore papers: copy (made by Halsted?) of a letter of E. H. Moore to Halsted, 15 May 1920.

[28] Henderson papers: W. E. Maynard to Henderson, 25 April 1902; Charles Rogan to Henderson, 5 June 1902; C. S. Potts to Henderson, 10 June 1902.

> He sent a letter after me, saying that after the present session my "services would not be required" in the University, and threatening, if I divulged his villainy, to cut off the remainder of this year's salary.
>
> Of course that would put a stopper on my work in getting out my book. So you see there is no hope for you here. What should I do?

And, after several more letters offering suggestions to Moore for a position for the next year, Halsted wrote: "I have also another praise of your work coming out in the next number of the great Educational Review, and your future is assured. I wish I could say as much for my own."[29]

A praise appeared in the *Educational Review* for December[30] but an even more direct one appeared in the October issue of *Science* in an article ostensibly about the Carnegie Institution. Halsted's terms of praise were somewhat self-defeating: "And finally among the sifted [sic] few who have the divine gift and the divine appreciation of their gift, the exquisite bud in its tender incipiency may be cruelly frosted." After citing Moore's work on Hilbert's axioms of geometry, which had gotten E. H. Moore's attention, Halsted pressed the point further:

> This young man of marvelous genius, of richest promise, I recommended for continuance in the department he adorned. He was displaced in favor of a local schoolmarm. Then I raised the money necessary to pay him, only five hundred dollars and offered it to the President here. He would not accept it. ... The bane of the state university is that its regents are the appointees of a politician. If he were even limited by the rule that half of them must be academic graduates, there would be some safety against the prostitution of a university, the broadest of human institutions, to politics and sectionalism, the meanest provincialism.[31]

The regents at their meeting on Saturday, 6 December, unanimously adopted a resolution "that in their judgment the interest of the institution requires that [Halsted] should be removed and that his place be declared vacant from this date, his salary to be paid for the current month." On Sunday, official word was sent to Halsted via the janitor. T. U. Taylor later wrote simply that Halsted's services were terminated "on account of misunderstandings." "He was," Taylor added, "too free in his criticisms of the University authorities...." This was evidently just the last straw in what

[29]Moore papers: Halsted to Moore, 8 September 1902; Halsted to Moore, 13 November 1902.

[30]"The teaching of geometry," *Educational Review* **24**(1902), 456–470.

[31]"The Carnegie Institution," *Science* (n.s.) **16**(1902), 644–646.

had become an increasingly strained relationship between Halsted, on the one side, and the regents and Prather, the former regent, on the other. In themselves, Halsted's remarks were only the public expression—though admittedly in Halsted's usual purple style—of a situation which even Regent Cowart had expressed in private: "I don't see how a Board of Regents in with every administration can keep out of politics."[32]

In April of 1903, Moore, teaching high school in Marshall, Texas, received word that he would have his Chicago fellowship. In a 1972 interview, Moore said that he could not analyze his relationship to Halsted, that his appreciation of Halsted was not something he could explain, but that, nevertheless, he was certain there was "no one at all who I wish had been professor of mathematics [at Texas] instead of Halsted." Halsted himself would probably not be at a loss for words to describe the nature of his value to someone like Moore. Though Halsted did do some original and influential mathematics, especially in the axiomatic treatment of non-Euclidean geometry, he was primarily a prolific writer of expository papers and textbooks, and a teacher. In an essay he published in 1876, while a fellow in mathematics at Johns Hopkins working under Sylvester, Halsted described the rise of three separate men in place of the single classical mathematician: the writer of research papers, the teacher, and the reader—"the last class including the writers of non-original treatises and all textbooks." Halsted probably saw himself already in 1876 as a reader, as one of those who, "wishing to be of most use to their race, carefully read these memoirs, and after long and patient study of them, digested them into connected treatises, supplying the missing links and making them really part of the available mental wealth of the world."[33]

Causes for Halsted's dismissal other than the airing of dirty linen have been indicated in a document prepared in 1951 by the Dean of Arts and Sciences in response to a suggestion that an instructorship be named after Halsted. After consulting with the two most senior members of the university community at the time, Porter and W. J. Battle, the dean reported his findings:

> Both Professor Battle and Professor Porter are in agreement that Professor Halsted fully deserved the dismissal he got. According to Dr. Battle, Halsted was associated with Edwards (Biology), Everhardt (Chemistry), and McFarlane (Physics) in an effort to discredit the services of Messrs. Waggener, Wooldridge, and Wooten of the Board of Regents. Several of these men were discharged for their campaign, but Professor Halsted was continued on the Faculty with his salary reduced by $500 for each of three years. The

[32]Minutes of the Board of Regents for 6 December 1902. [Taylor, p. 87]. Benedict papers: R. E. Cowart to J. B. Clark, 7 May 1900.

[33]Moore papers: Halsted to Moore, 14 April 1903. Moore in an interview with A. C. Lewis on 1 November 1972. A similar statement by Moore is reported in [Traylor, p. 20]. Halsted, "Modern mathematicians as educators," *Nassau Literary Magazine*, November 1876, p. 98.

> final act which appeared to have led to his dismissal was stated by Dr. Battle and Dr. Porter to have been Halsted's "stuffing the ballot box" in connection with his candidacy for president of the Texas Academy of Science.
>
> Dr. Battle feels that under no conditions should any fund be named in honor of G. B. Halsted. The record seems clear enough in the matter to support this conclusion.[34]

It should be noted that this memorandum was composed at a time when its author was attempting to diminish Moore's influence at the university and the proposal for the instructorship had been initiated by H. J. Ettlinger who could be regarded as staunchly in Moore's camp. Though Battle was professor of Greek from 1893 to 1949 and had served in administrative positions beginning with Dean of Arts, which included mathematics, in 1908, corroboration for either of these points—discrediting the named regents or stuffing a ballot box—has not been found. There were problems associated with the departures of the professors Battle lists but the precise natures of them seem at least as unclear from the existing record as in Halsted's case. Some facts are not quite right: Alexander P. Wooldridge, who was secretary to the Board of Regents during Halsted's time, and Leslie Waggener, who was chairman of the faculty and president, were never regents. Wooten, as has been mentioned above, was a regent from 1881 to 1899. It is true that Halsted's 1884 salary of $3,500 had become $3,000 by 1902, presumably this happened in 1898, or before, judging from what Lefevre reported in the incident described above. Though Halsted, as the professor with greatest seniority, might have been entitled to more, $3,000 was still the top salary, which five of the eighteen full professors received, and President Prather's salary was $3,333.34. The local newspaper reported at the time that there had been a *general* reduction of salaries "several years ago" and that it was rumored that Halsted would resign then. Furthermore, no mention of either of these two points against Halsted has yet been found in the voluminous archives from that period which are not devoid of documentation of what were taken to be scandalous matters (for example, Porter's supposed liaison, treated below). Thus it does not seem advisable to attach much weight to this late and superficial gathering of evidence by someone not exactly disinterested.[35]

Whatever the reason for his dismissal, by the end of 1902, Halsted could well have agreed with the observation made by Mallet, former chairman of

[34] Moore papers: copy of memorandum to file, C. P. Boner, 24 October 1951.

[35] Ettlinger interview with A. C. Lewis, 26 May 1975. Henderson papers: "Salaries September 1, 1901–August 31, 1902." *Austin American Statesman*, 11 December 1902, also quoted in [Traylor, p. 36] (where "Regent Lomax" should read "Registrar Lomax").

the faculty: "Texas can send a man up higher, and let him down lower, than any other region on the face of the earth."[36]

PORTER AND BENEDICT, 1902–20: AN ERA OF DIPLOMACY

R. L. Moore recalled in 1972 that "there was a big difference between the personalities of Halsted and Dickson" and that Porter and Benedict "did not appreciate Halsted." Of all Halsted's students at Texas, Moore was probably the closest personally. Their correspondence continued through Halsted's succession of unhappy jobs and up to Halsted's death in 1922, and it covered a wide range of subjects, both mathematical and personal. Now, as far as the university was concerned, a new beginning could be made and Prather was quick to try to bring Porter back to Texas once again. He could report, just four days after Halsted's dismissal, that Porter had responded to his offer of an associate professorship but was holding out for professor.

In place of Halsted, the regents and administration were probably ready for a head of mathematics whom they could be well assured in advance would be less cause for concern and now, thanks largely to Halsted, they had qualified people available who were Texas alumni, such as Benedict and Porter. We have an overview of this period from J. W. Calhoun (B.A. 1905, M.A. 1908), originally from Tennessee, who entered the university in 1901. Starting as a tutor in pure mathematics in 1905, he continued as a teacher and administrator at the university until his death in 1947. He described this early transition period in a history written about 1946 in response to "vigorous and somewhat heated" discussions following H. S. Vandiver's transfer from the pure to the applied mathematics department to avoid working with R. L. Moore. According to Calhoun,

> ...President Prather...was very fond of Benedict and had a high opinion of his ability [and] desired to appoint him to the place. T. W. Gregory was at that time a member of the Board of Regents and desired to have M. B. Porter appointed. Benedict, who was a classmate of Porter at the University of Texas and had been his roommate in Divinity Hall at Harvard, also wished Porter to be appointed. Porter [who] was then an Assistant Professor at Yale would not leave for less than a full professorship (and as at that time there could be only one professor in a department) Benedict was willing to accept an associate professorship in order to have Porter come as a Professor.

On the surface, this would appear to be a good way to sow the seeds of more problems. In fact, this was the beginning of a new era of diplomacy

[36][Mallett, p. 17].

in which the mathematics faculty played a more normal role in the university and gradually began to seek new members who were graduates of other institutions.[37]

Benedict had already proved himself useful as a moderating influence in a difficult time and was not to be overlooked. Benedict had been asked by Prather to take over the School of Pure Mathematics immediately after Halsted's dismissal. In 1906, the School of Applied Mathematics, last seen in 1903 when it was effectively absorbed by the engineering department, was revived. It now also included astronomy, and Benedict was made chairman of it, assisted by C. D. Rice, who had also been a student under Halsted. Its offerings overlapped pure mathematics and were in some cases cross-listed with pure mathematics, but the intent—besides providing Benedict a full professorship—was the mathematical training of engineering students. At the same time, an engineering college was established with Taylor as dean. According to Calhoun, Taylor "did not wish engineering students taught Mathematics by women," and it was Taylor who proposed the school be headed by Benedict. Benedict was now earning \$2,400 compared with Porter's \$2,500 salary. Thus began Benedict's move into administration where he was to rise through the ranks: chairman, director of the extension division, dean, and, finally, president.[38]

Porter, while an instructor at Yale from 1899 to 1902, came to know Edward Lewis Dodd, then working on an M.A. degree in mathematics. Dodd, born in Cleveland, Ohio, received his doctorate from Yale in 1904. In 1907, Dodd had been teaching at the University of Illinois for a year and came to Texas at Porter's invitation as an instructor in pure mathematics at \$1,600. Dodd wrote that, in advanced mathematics, "I am perhaps best prepared in function theory, vector analysis and differential equations." "I wish," he added,"to be as useful to the University of Texas as possible, and will gladly prepare myself to teach any course that may be desired." His first publication was in 1905. In 1911, he tried to obtain a position back at Yale but was unsuccessful. He then became more mathematically active. In 1912, he offered for the first time at Texas a course in actuarial mathematics, which, according to the catalogue, was "modelled after Broggi's *Traité des assurances sur la vie avec développements sur le calcul des probabilités*." This followed the establishment the previous year of a School of Business Training. Dodd wrote to President Mezes in the spring of 1913: "Since last spring, I have written

[37]W. J. Battle on Calhoun in [Handbook]. [Calhoun, p. 2].

[38]Benedict papers: Prather to Benedict, 8 December 1902. Presidents' papers/College of Arts and Sciences/Pure Mathematics, 1907–1929 [=PM 1907–1929]: Taylor to President Houston, 9 January 1907; Houston to Benedict, 13 June 1907; Houston to Porter, 13 June 1907. [Calhoun, p. 2].

eight papers for publication, on the general subject of mathematical probability with special attention to the theory of measurement and statistics."[39] Dodd thus began his rise through the ranks to become in 1923 Professor of Actuarial Mathematics. His reputation in the actuarial field was such as to attract the attention of R. L. Wilder who came to Texas in 1921 to study with him.

After completing his doctorate at Chicago in 1905, R. L. Moore taught at the University of Tennessee for one year and then went to Princeton, Halsted's alma mater, as instructor. While there, Moore made enquiries about joining the Texas faculty. Oswald Veblen, with whom Moore had worked at Chicago and who came to Princeton in the same year as Moore, wrote also on Moore's behalf pointing out that Moore had seven young men ahead of him at Princeton (including Veblen himself) and that he would have better prospects at Texas. He added that "in his speciality, the foundations of geometry, he is one of the best men in the country."[40] Moore did leave Princeton in 1908 but for Northwestern instead of Texas. In 1911, he moved to the University of Pennsylvania.

An apparently minor, personal, incident in 1910 was probably the closest thing to a scandal during M. B. Porter's watch as the senior mathematician at Texas. There is evidence that during Porter's absence from the campus a faculty member, not in mathematics, told others that Porter had had some sort of illicit affair with a married woman. The actual accusation, which appears not to have caused any lasting damage, does not have any historical relevance to the present account, but the way in which it is referred to in the existing documents provides a valuable illustration of a combination of gentility and frankness that probably characterized the handling of such potential crises. Porter wrote to President Mezes expressing concern about the damage the "slanderous stories" might have for the reputations of the woman and the university. A colleague from another department also wrote to the president in support of Porter's good character:

> It is a monstrous thing that members of the University Faculty should directly or indirectly traduce the character of their associates and trample in the mud the fair name of an excellent and innocent woman, whose husband was not here to protect it.
>
> In the South ordinarily such things have but one ending, but this must by all means be avoided. The gravity of the situation is such as to cause me much anxiety.[41]

[39] Presidents' papers/[PM 1907–1929]: Dodd to Porter, 8 May 1907; carbon copy Mezes to J. Pierpont, 30 January 1911; Dodd to Mezes, 15 April 1913.

[40] Presidents' papers/[PM 1907–1929]: Moore to Houston, 6 September 1907, Veblen to "Dear Sir," 29 April 1908.

[41] Presidents' papers/[PM 1907–1929]: Porter to Mezes, 13 September 1910; W. B. Phillips to Mezes, 13 September 1910.

On a less personal issue, Arthur Lefevre, in his capacity in 1912 as Secretary for Research of the Organization for the Enlargement by the State of Texas of Its Institutions of Higher Education, published a critique of the present state of affairs at the university, especially calling attention to the fact that though doctoral programs were announced "a few years ago" in the catalogue, no one has completed one. He attributed the root cause to inadequate support:

> The average salary paid the teaching force of the University of Texas thirty years ago was double the present average salary. How could an intelligent man demand of the University of Texas, in its present circumstances, the first-class research and manifold services to the general public which have come to be essential characteristics of the modern university?[42]

Lefevre's study, prepared on behalf of an alumni group, did not make numerical comparisons with other universities and did not go into the fact that the "teaching force" was being increased at the lower end of the salary scale. One sign that the mathematics instructors' salaries, at least, offered by Texas were still competitive, in spite of the absolute decline he noted, is the addition in 1913 of a new instructor from Harvard to the School of Applied Mathematics, Hyman J. Ettlinger, at the usual rate for Texas of $1,200.[43] Raised in St. Louis, Ettlinger had been very active in Jewish affairs from his school days, through Washington University in St. Louis, and at Harvard University where he obtained his M.A. in 1911 and was to get his Ph.D. in 1920 under G. D. Birkhoff. It was about the time of Ettlinger's Ph.D. degree that Harvard instituted its *numerus clausus* which set a limit on the number of Jews admitted and which Norbert Wiener described as killing "the last bonds of my friendship and affection for Harvard." There were thirty Jewish students at Texas when Ettlinger arrived there, and he helped establish a Menorah Society for them. In 1915, a rabbi was appointed to the Board of Regents. If there were any problems caused by Ettlinger's being Jewish, either at Harvard or at Texas, he made nothing of them in his later reminiscences. This is not to say there were no problems, but he claimed there was only one incident in sixty-one years. It occurred around 1917 when Ettlinger was an assistant football coach at Texas. In an argument with a caretaker who refused to unlock a door so that Ettlinger could get a referee's whistle, the caretaker said that he would not do it for anyone "and certainly not for a Jewish...," whereupon the 210-pound Ettlinger floored him. He would have ignored the man's remark, Ettlinger has said, but for the fact

[42][Lefevre, pp. 42–43].

[43]Presidents' papers/College of Arts and Sciences/Applied Mathematics, 1909–1913: copy of telegram from Rice to Ettlinger, 14 June 1913. Salaries at Kansas were comparable to those at Texas but in [Price, pp. 187–188] it is maintained that the national scale was rising and that Kansas suffered as a consequence.

that his football players witnessed it and he felt a lesson was needed on the spot. Ettlinger was accused of using less violent but still physical tactics in departmental controversies of subsequent years, but that goes beyond the scope of this account. Moore, smaller than Ettlinger in stature but a proficient boxer at Princeton who kept himself in shape, was also known on occasion to make aggressive use of his physical capabilities in academic disagreements.[44] In Texas, at least, the successful use of such nonverbal language need not detract from one's reputation. In fact, for an established male scholar, it adds a certain cachet which can probably only help one's reputation outside the scholarly world. Still, it seems that no other department at Texas has had members with quite this reputation.

The first doctoral degree in mathematics was earned by Goldie Prentis Horton in 1916 with a thesis entitled "Functions of limited variation and Lebesgue integrals" done under Porter's supervision. She came from Elms, Texas, as an undergraduate in 1904, received a bachelor's degree in 1908, and then alternated teaching in schools with earning a master's degree at Smith College and studying at Bryn Mawr. She accepted a tutorship at Texas in 1913. In 1917, she was promoted to instructor at $1,000. (Porter's salary was then $3,000, Dodd's $1,900, and, to select another but more senior instructor, Mary Decherd's was $1,200.) Dodd, as chairman of pure mathematics, had appealed in 1916 for more staff to help cope with increases in enrollment. In the primary undergraduate course, the enrollment was 675 split up into 22 classes, and there were seven advanced classes.[45]

This increase in enrollment was soon met with a substantial increase in teaching force, at least of those who were below the rank of associate professor. Besides Porter, Dodd, and Calhoun (transferred to pure mathematics the previous year), in 1915–1916 there were nine others listed in pure mathematics including student assistants. Three years before, there were only three besides Porter and Dodd in pure mathematics. There was no such increase in staff for applied mathematics, and, in fact, its total number stayed fairly constant for the next twenty years.

Continuing in an expansionist direction, Texas hired A. A. Bennett, an instructor at Princeton, as an adjunct professor in pure mathematics in 1916. Veblen, Bennett's doctoral supervisor at Princeton, wrote Porter that Bennett had published in the *Annals of Mathematics* and "has wider knowledge of mathematics than any man of his age whom I know." Princeton would try to keep him, Veblen said, but "I am not sure, however, that they will be able to meet your offer...." Veblen also mentioned others that Texas might

[44][Wiener, pp. 271–272]; [Greenwood 1986]. Ettlinger interview with A. C. Lewis, 6 February 1974. [Traylor, pp. 71–72, 89, 127].

[45]Presidents' papers/[PM 1907–1929]: W. J. Battle to Calhoun, 26 April 1916, Dodd to President Vinson, 16 October 1916.

be interested in, but on the back of this letter Porter wrote, "I vote for Bennett." Once the offer was made, Veblen wrote again to Porter saying that he would recommend Bennett take the offer, unless Princeton could duplicate the position and salary, and volunteered more information about him: "no doubt you will be satisfied with Bennett... the only handicap which he has is a certain rapidity of utterance—and this I should think would be less of a drawback in the less effete atmosphere of Texas."[46]

Applied mathematics also increased its staff at this time by the addition of Paul M. Batchelder from New Hampshire. He received an M.A. from Princeton in 1910 and took courses under G. D. Birkhoff. When Birkhoff moved to Harvard, Batchelder went with him and, after two years teaching at Northwestern University, in 1916 obtained his doctoral degree under him. W. F. Osgood recommended Batchelder to Texas and Harvard sent a biography which included an evaluation Birkhoff wrote in 1913: "While I should not characterize him as a man of unusual powers of original investigation, I feel he is possessed of a clear insight and that he has the unusual gift of clear presentation." Birkhoff also said that he found Batchelder likable and "well worth while as a friend." Ettlinger wrote to the chairman of his department at Texas that Batchelder "would make us a good man": "I knew him fairly well. His temperament is very much like Barrow's [David F. Barrow, Ph.D. Harvard, 1913, instructor at Texas, 1914–1916], quiet and retiring. His health at one time was precarious." Batchelder was promptly offered $1,300, and he joined the faculty as an instructor in both pure and applied mathematics. He retired in 1954 as an associate professor.[47]

R. L. Moore and H. S. Vandiver, 1920–1938: New Beginnings

The global confrontations of World War I seem to have had no appreciable effect on the development of the mathematics faculty, but 1917 saw a political battle in Texas which greatly influenced the future place of the university within state politics. This event also helped to eventually bring more emphasis to rewarding faculty members for scholarly achievement rather than for seniority. Though there were undoubtedly supporters for this notion before on the campus, an attack by the governor on the independence of the university and its Board of Regents provided an impetus for this reform.

James E. Ferguson, a banker, had been elected governor in 1914 as a supporter of business and a rescuer of the small farmers who were major victims of the depression of 1913–1914. The trouble appears to have begun

[46]Presidents' papers/[PM 1907–1929]: Veblen to Porter, 24 April 1916 and 8 May 1916.

[47]Presidents' papers/Dean of Arts and Sciences/Applied Mathematics, 1913–1919: Osgood to Rice, 12 July 1916; Ettlinger to Rice, 22 July 1916; typed sheet from Harvard University [?], July 1916; Graff (Secretary to the President) to Rice, 26 July 1916.

by his insistence on a direct say about the university's budget in all its details. When there was resistance, he declared that the regents of this "autocratic University" were in for "the biggest bear fight in Texas." The university president, R. E. Vinson, wrote from the depths of uncertainty to a potential faculty member in June 1917 that the "Governor of Texas has vetoed the entire University appropriation for the next biennium [1917–1919], because the regents failed to accede to a demand of his that four members of the faculty and I be discharged from the University... feel yourself entirely free to accept other employment, in case we are not able to keep the University open."

A coalition of opposition forces joined the supporters of the university and brought about successful impeachment proceedings against Ferguson in the legislature in August of 1917. New regents were appointed by the acting governor and all but one of the fired professors were reinstated.[48]

Just as at other state universities in the country which went through similar catastrophes, there was the positive effect of delineating and maintaining—at least for a period—the boundaries of authority between the governor, on the one side, and the regents and university administration on the other.[49] Perhaps in the long run, it also caused a more vigorous campaign to take advantage of the increasing appropriations being granted the university to attract high quality faculty from outside. None of the mathematics faculty were among those fired or threatened in 1917 and none of the senior faculty, at least, left. In fact, new members were added during the next three years. The post-war increase in the student population and the consequent demand for mathematics opened up jobs across the country.

Pure mathematics in 1919 had been doing without the services of A. A. Bennett who had been taking leaves of absence each year since 1917 to do government war-related work on computation of ballistics tables. He offered to resign in 1919, but the university was willing to try to retain the connection. The applied mathematics faculty needed to be augmented to meet the new demands, and two instructors were added in 1919. One, A. E. Cooper, was a doctoral student of L. E. Dickson at Chicago and helped with Dickson's three-volume *History of the Theory of Numbers* (Carnegie Institute, Washington, D.C., 1919–1934). When an offer of $1,600 was made to Cooper, he at first said that his present position for a private company paid twice that and he asked if he would be getting more later. Evidently, the university stood firm,

[48]Presidents' papers/[PM 1907–1929]: Vinson to T. M. Simpson of Chicago, 4 June 1917. [Frantz, pp. 72–81], [Fehrenbach, pp. 638–639], [Gould].

[49][Price, pp. 184–186] recounts a similar experience at The University of Kansas in the period 1917 to 1924.

even when Cooper then said that his company increased his salary and offered a "tentative promise of future partnership."[50]

In the middle of the new hiring move in 1919, the chairman of applied mathematics, Rice, discovered that they might lose Ettlinger in a raid from quite unexpected quarters. Porter had offered Ettlinger an associate professorship in pure mathematics to take the place of Bennett at an increase of $500 without telling either Rice or the Dean of the College of Arts, Benedict. Ettlinger, however, did tell these two and Rice appealed to President Vinson indicating that Ettlinger felt he could not refuse the advancement. Vinson referred the protest to Benedict who replied that he did "not feel disposed to stand in Ettlinger's way or to indulge in internal competition." Taylor, as dean of engineering, also objected when he heard about it. Thinking that Cooper had rejected the offer of instructor, Taylor assumed that would leave only one person (Rice) where three were provided for.

> The work of Applied Mathematics is all *prescribed* work for engineering degrees while a large part of that in pure mathematics is elective.
>
> The transfer of Professor Ettlinger at this time will seriously cripple and almost paralyze the work in engineering. I believe the reason for the transfer is the monetary consideration, and I call your attention to the fact that Professor Ettlinger has been drawing three hundred dollars from athletics in addition to his other pay.

The arrangement settled on had Ettlinger move to pure mathematics (with no promotion, whether with an immediate salary increase is not clear) and Calhoun move from pure back to applied. Both were to become chairmen of their respective new departments the following year. There appears to have been no serious interruption to the smooth relationship between departments which continued to share faculty and cross-list courses.[51]

Negotiations were uneventfully completed before the end of 1919 for Clark Milton Cleveland, a graduate in civil engineering from the University of Mississippi, to come to Texas as an instructor in applied mathematics at a salary of $1,600. While at Texas he worked as a part-time graduate student under R. L. Moore and received his doctorate in 1930. He retired as a full professor in 1962.[52]

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[50]Presidents' papers/[PM 1907-1929]: Bennett to Vinson, 15 September 1919. The A. E. Cooper papers contain notes relating to the *History*. Presidents' papers/Dean of Faculty/Applied Mathematics, 1919-1924: Rice to Taylor, 3 September 1919; three letters from Cooper to Vinson, 11, 13, and 15 September 1919.

[51]Presidents' papers/Dean of Faculty/Applied Mathematics, 1919-1924: Rice to Vinson, 17 September 1919 with added note by Benedict; Taylor to Vinson, 22 September 1919; Vinson to Calhoun, 18 December 1919.

[52][Greenwood 1970]. Presidents' papers/Dean of Faculty/Applied Mathematics, 1919-1924; Cleveland to Taylor, 18 November 1919; telegram from Vinson to Cleveland, 8 December 1919.

What the initial contacts with Moore were are not known, but, as indicated above, as early as 1907 he had indicated to friends at Texas that he would welcome returning. He was made an associate editor of the *Transactions of the American Mathematical Society* in 1913. In January 1919, Moore, at the University of Pennsylvania for the past eight years, wrote Rice, who had been one of his graduate instructors at Texas, recommending hiring Anna M. Mullikin, currently working towards her doctorate and "one of the best students I ever had." Apparently, Moore was juggling several possibilities at this juncture. In April, the University of Minnesota was attempting to attract him as an associate professor at a salary of $2,500. Moore asked for $3,500 and was told that was out of the question and that the maximum was $3,000. "We have several men in mind," the chairman, W. H. Bussey (who had been a doctoral student of Dickson at Chicago), wrote, "and I do not know what the final decision will be." Whom they wanted seemed clear, however, towards the end of April, when the president at Minnesota asked Moore for a meeting. In May, E. H. Moore wrote R. L. Moore: "I don't know what they are offering DJ. I think surely they would have given you $3000. If by any slip they don't get DJ I think you will be called. —I note what you say, and hope they give you $3000 at Pennsylvania." He added in a postscript, "I think I can understand how you feel averse to leaving Pennsylvania, when the department is now becoming stronger steadily, for Minnesota." But the decision had been made before E. H. Moore wrote his letter. Dunham Jackson was appointed professor by Minnesota. "If Jackson had not accepted, you would have been offered a Professorship here," Moore was told by Bussey.[53]

Moore thus spent another year at Pennsylvania before negotiations with Texas began. He wrote to Porter on 29 March 1920, probably in response to some preliminary enquiry, that he "would give very serious consideration to an offer of associate professor, at a salary of $3000" and asked "what the chances would probably be for further advancement in rank and salary, whether there will be some opportunity to give advanced courses, and whether the library contains the back numbers of the more important periodicals." "It is true," he granted, "that I would like to return South and Texas, in particular, has a decided attraction for me." President Vinson wrote Moore on 3 April 1920 offering the $3000 associate professorship. His student, Anna Mullikin, who was to get her doctorate in 1922 from Pennsylvania, though still under Moore, was appointed as instructor at Texas in July 1920. When Moore returned to the pure mathematics department as an associate professor, Mary Decherd, the person who had displaced him eighteen years

[53]Presidents' papers/[PM 1907–1929]: 18 January 1919. Moore papers: Bussey to Moore, 16 April 1919, 22 April 1919; Burton to Moore, 26 April 1919; E. H. Moore to Moore, 27 May 1919; Bussey to Moore, 22 May 1919.

before, was still there, though now as an instructor, and would remain until her retirement in 1944.[54]

Moore's first doctoral student at Texas was Raymond L. Wilder. In the fall of 1921, Wilder came to Texas with an M.S. degree from Brown University. R. G. D. Richardson had suggested Wilder for any vacancy that Porter and Dodd might have, and a Brown University statement about Wilder included the fact that his principal work was in accounting. Porter asked President Vinson to offer an $1,800 instructorship "at once lest we lose him."[55]

Though it was Dodd he came to work with, Wilder has told how he decided to take some additional mathematics and Dodd suggested Moore's course. Wilder introduced himself to Moore during registration:

> I soon realized that he was very negative about my enrolling in his course...I had two counts against me, as I analyzed it later. One was that I was a Yankee. The second was that I was an actuarial student, and what in the world was an actuarial student doing taking a course from Moore? Well, this went on for some time, and I didn't want to give up. He finally made the mistake of asking me, "What is an axiom?" I had pretty good training at Brown, and I knew what an axiom was. His guard was down, and I think he, in utter frustration, said, "OK, go ahead, and take the course." Actually, I wasn't really in the course until I proved what we called Theorem 15 in those days.[56]

Wilder eventually became one of the three Moore students, with Bing and G. T. Whyburn, who were elected presidents of the American Mathematical Society.

Two years later, Wilder had his Ph.D. and an offer of $2,750 to go to Oklahoma A&M (now Oklahoma State University). Moore tried to keep him at Texas. Benedict thought he could be given $2,400, and Moore and Porter went together to argue their cause with Acting President T. U. Taylor. But Taylor—with his many years of experience vis-à-vis pure mathematics and an emphasis on seniority in service to the university rather than current academic market value—made an offer to Wilder of only $2,200. Taylor maintained that pure mathematics was "well-equipped in man-power having such top-notchers as Porter, E. L. Dodd, Moore, Ettlinger, A. A. Bennett,

[54]Presidents' papers/[PM 1907–1929]: Moore's letter and Porter to Vinson, 1 July 1920, regarding Mullikin.

[55]Presidents' papers/[PM 1907–1929]: Richardson to Porter and Dodd, 23 February 1921; Porter to Vinson, 12 March 1921; Vinson to Wilder, 14 March 1921; Wilder to Vinson, 17 March 1921.

[56]Remarks at the presentation breakfast of The University of Texas at Austin Mathematics Award honoring the memory of Professors R. L. Moore and H. S. Wall, San Antonio, 24 January 1976, recorded and transcribed by Lucille E. Whyburn. The award (to William T. Eaton) and the event was organized by H. J. Ettlinger. More details of Wilder at Texas are given in [Bing].

and also such Instructors as Miss Goldie Horton." "I recall," he continued, "that Miss Horton has had the Ph.D. degree for many years and is yet an Instructor. I also recall the fact that the Administrative Council has been very kind to the Department of Mathematics ["departments" now instead of "schools"] and gave Professor Moore a jump in salary that broke the Southern record." Taylor pointed to another faculty member offered $3,500 by the same Oklahoma institution and claimed he could not offer him more of a raise though his services to his department were more necessary than Wilder's to pure mathematics. Next September, Wilder tendered his resignation so that he could accept an assistant professorship at Ohio State University.[57]

The higher administration appeared for a while to be unenthusiastic about providing for Moore's further advancement. In September of 1921, Benedict, as Dean of Arts and Sciences, conveyed his balanced recommendations for promotions to President Vinson:

> With some misgivings... I append a list of those persons who seem to me to be superior to some men, to say the least, who are now ranked above them, and who ought, therefore, sooner or later, to be promoted in rank: ... Cooper, Dodd, Calhoun, ... The cases of Bennett and Moore demand special comment. Moore is perhaps the more talented; Bennett the more persistent. Both are starred men in Cattell's authoritative list [*American Men of Science*]. (They and [the zoologist] J. T. Patterson constitute our only three live stars, an almost disgraceful situation.) They have been here but a short time; they would have to be made professors purely on scientific merit; but their promotion, particularly if accompanied by a statement of policy in regards the Ph.D. and its accompaniments, would tone up the situation among the "intellectuals." But what about making three full professors in mathematics at once?[58]

The wavering stance of the letter hints at the potential for discontent over what some might regard as an unseemly haste and indeed it took two more years before any of these promotions in pure mathematics came about.

The university could not complain at this time about salaries, at least as far as averages went. Benedict made a comparative study in 1921 from which he concluded that "the salary scale in the College of Arts and Sciences compares very favorably with that found at other state institutions. Our averages are as good as any in the case of associate professors, adjunct professors, and

[57]Presidents' papers/[PM 1907–1929]: text for telegram from Moore to Porter, 1 September 1923; copy Taylor to Wilder, undated; "Acting President" Taylor to "Acting Acting President" Sutton, 10 September 1923; Wilder to Splawn, 8 September 1924.

[58]Presidents' papers/Dean of College of Arts and Sciences, 1923–1924: Benedict to Vinson, 15 September 1921. Porter had also been starred in the first edition of Cattell.

instructors. We fall behind California and Wisconsin in professors' averages because of a few $6,000 and more salaries in those institutions."[59]

In the summer of 1923, before Wilder went to Ohio State, that institution made inquiries to see if Moore himself could be tempted away. Dodd came close to leaving the same year when he took a leave of absence to teach at Williams College.[60] Finally, both Dodd and Moore were made full professors in 1923. In the spring of that year, the university's endowment of grazing land was enhanced when Santa Rita #1, the university's discovery oil well, blew in.

Approval was given in 1924 by the administration for hiring Harry Schultz Vandiver from Cornell as an adjunct professor in pure mathematics at $2,800 to take the place left temporarily by Batchelder. The latter had just been made adjunct professor, perhaps as a security against permanently losing him to Brown University, where he went to teach for the year 1924–1925. Vandiver had been recommended to Porter by L. E. Dickson with whom Vandiver, like Cooper, had worked on the *History of the Theory of Numbers*. By 1924, Vandiver had twenty-three publications on number theory starting with a collection of problems and problem solutions in the *American Mathematical Monthly* from 1900 to 1904. It was through these problems that he got to know G. D. Birkhoff, and the two co-authored Birkhoff's first paper in 1904. Vandiver came to epitomize pure mathematics at the university to an even greater extent than Moore. Whereas Moore played two of the three roles Halsted had posited in 1876, the teacher and original writer, Vandiver played mainly that of original writer. Though they had about the same number of years in academic positions, Vandiver, retiring in 1966 at age 84, and Moore in 1969 at 86, Vandiver took leaves of absence for research, while Moore had ten times as many doctoral students as Vandiver and devoted himself to regularly teaching undergraduate as well as graduate courses. Vandiver had the further distinction of being almost entirely self-taught and having no degrees, or even a high school diploma, until he was awarded an honorary degree in 1946 by the University of Pennsylvania.[61]

Bennett, who was chairing the Department of Pure Mathematics in 1922–1923, remained an associate professor and did not participate in the promotions of that year. In January of 1925, the three full professors of pure mathematics—Porter, Dodd, and Moore—requested the administration raise the salaries of Bennett and Ettlinger to $3,600. They pointed out that Texas Technological College had offered "one of our staff" $3,750. The requested raise was approved by the regents, but by the time it was to take effect later in

[59]Presidents' papers/College of Arts and Sciences, 1921–1924: report of 10 June 1921.

[60]Presidents' papers/[PM 1907–1929]: copy of R. D. Bohannan to Moore, 4 June 1923; Dodd to Vinson, 8 April and 23 April 1922.

[61]Presidents' papers/[PM 1907–1929]: Acting President Sutton to Vandiver, 16 July 1924. [Greenwood 1983], [Vandiver H 1963].

1925, Bennett had been attracted back to Brown University, his alma mater.[62] He is the only member of pure mathematics of adjunct rank or higher who came to Texas between 1902 and 1938 and ever left for another university. (There were two such in applied mathematics, J. N. Michie and C. A. Rupp.)

Porter had worked towards the establishment of a graduate faculty since at least 1920. In 1925, thanks in good part to his efforts, this was accomplished. That same year, R. G. Lubben, a student since 1916, received his doctorate under Moore and stayed on as a member of the faculty until 1959 when he retired due to illness. In 1927, Gordon T. Whyburn, who had his B.A. and M.A. in chemistry from Texas, became Moore's third doctoral student, and his brother, William M. Whyburn, became Ettlinger's first. A report on the department of pure mathematics stated that as of September 1927 there were 1,068 students in 42 freshman sections, and 135 students in higher classes, not counting those in astronomy or aeronautics, which were taught in pure mathematics in 1927–1928. Also, National Research Council Fellowships had been received by Lubben for study in Göttingen and by W. M. Whyburn for work at Harvard. On his return, W. M. Whyburn went to the University of California at Los Angeles where he served as chairman from 1937 to 1944. In 1927, W. T. Reid received an M.A. degree on a subject suggested to him by Dodd, and went on to get a doctorate with Ettlinger in 1929.[63] In 1925, Lucille Smith entered the university to major in English, and worked as a computer (on the Monroe calculator) for Vandiver. She took a course with Moore, became interested in mathematics and, as Mrs. G. T. Whyburn, in 1936 she obtained an M.A. degree under Moore.

With pure mathematics thriving, the perennial question of its relationship to applied mathematics was raised again in 1926 by T. U. Taylor. "Several years ago," he wrote to President Splawn, "the Engineering Faculty unanimously recommended that the Department of Applied Mathematics be included in the College of Engineering like the Department of Drawing.... The Department was created during the Houston Administration solely for this purpose." His recommendation to fix this "illogical" situation was to appoint Benedict professor of astronomy and transfer him to pure mathematics and then transfer the applied mathematics department to engineering. Benedict's response was that he had long thought the departments should be "fused." Calhoun, still in applied mathematics, requested that the president give the matter careful consideration.[64] The only outcome of the consideration was to change the name to Department of Applied Mathematics and Astronomy.

[62]Presidents' papers/Dean of Arts and Sciences, 1924–1929: note by Benedict [?], undated, "Bennett and Ettlinger...". Presidents' papers/[PM 1907–1929]: Porter, Dodd, Moore to President and Dean [January 1925].

[63][Vandiver H 1961], [Greenwood 1988, p. 14]. Presidents' papers/[PM 1907–1929]: memorandum, 16 July 1928.

[64]Presidents' papers/Dean of Faculty/Applied Mathematics, 1924–1929: Taylor to Splawn, 1 March 1926; Benedict to Splawn, 10 March 1926; Calhoun to Splawn, 13 March 1926.

Taylor died in 1941, and the interdepartmental situation remained essentially unchanged until the shotgun merger of the departments in 1953. The new building, then housing the new group, was named after Benedict.

Benedict's rise through the administrative ranks reached its pinnacle in 1927 when he succeeded Splawn as president of the university. Benedict's fellow student in Halsted days, L. E. Dickson, contributed a brief encomium for the occasion:

> All are familiar with his success as dean, due to his unerring judgment, rare talents as an executive, and deep affection for the University. But I wish to emphasize the fact that the man having all these essential qualities is also a scientist. This is the age of science. Himself an astronomer, Benedict is just the man to make a success of the new astronomy observatory so amply endowed. ... A university is no longer counted as a great one unless it is a center of research in the various sciences. And only then does it serve adequately the needs of modern life. Benedict is the ideal man to steer the University of Texas toward greatness.[65]

The department of applied mathematics began to grow in 1928. Ernst George Keller, a graduate from Chicago, was added as an adjunct professor at $2,800. When he went on a leave of absence for the following year, a replacement, Homer Vincent Craig, was hired at $2,000. The budget for the year 1931 of the Great Depression shows that Benedict was receiving $10,000 (presumably his total salary as president), Calhoun (only part time in mathematics) $4,000, Cooper $3,750, Cleveland $2,800, Keller $2,617, and Craig $2,600. In 1932, R. N. Haskell was appointed an adjunct professor. Haskell had been recommended by Griffith C. Evans at Rice Institute (now Rice University) as an "attractively married" mathematician who had published two papers, including his 1930 thesis, with Evans on potential theory.[66]

A physics professor and Dean of the College of Arts and Sciences evaluated the effect of Craig and Haskell in a report of 1950:

> Both of these men are inspiring teachers. Both of them do an excellent job of teaching the fundamentals of the subject. Both are interested in their students and spend a lot of time with their students at any and all hours. The major swing of science students

[65] *The Alcalde*, 27 November 1927.

[66] Presidents' papers/Dean of Faculty/Applied Mathematics, 1924–1929: Calhoun to Benedict, 25 June 1928; Benedict to Calhoun, 28 August 1929. Presidents' papers/Budget and Departments/College of Arts and Sciences/Applied Mathematics and Astronomy, 1929–1939: Calhoun to Benedict, 12 August 1931; Cooper to Parlin, 8 September 1932; Evans to Cooper, 3 September 1931.

> from Pure Mathematics to Applied Mathematics is attributable in considerable measure to these two men.[67]

On the pure mathematics side, G. T. Whyburn was awarded a John Simon Guggenheim Memorial Foundation Fellowship for 1929–1930 and he and his wife went to Vienna and worked with Hans Hahn. On his return, he went to Johns Hopkins and in 1934 from there to the University of Virginia. Moore gave the Colloquium Lectures of the American Mathematical Society for 1929 (published in 1932, revised in 1962, and reprinted in 1970), became Visiting Lecturer for the American Mathematical Society for 1931–1932, and was elected to the National Academy of Sciences in 1931 after a practically unanimous vote of the mathematics section. Vandiver received a Guggenheim fellowship for 1927–1928, in 1931 was awarded the Cole Prize in the theory of numbers by the American Mathematical Society, and delivered the Colloquium Lectures for 1935 (unpublished). In 1934, he was elected to the National Academy of Sciences.[68]

Porter and Goldie Horton collaborated on a textbook, *Plane and Solid Analytic Geometry* (Edwards Brothers, Ann Arbor), and when it was published in 1934 they were married. They both continued in the department, until he retired in 1945 and she, after teaching part time from 1958, retired in 1966.[69]

People from Benedict's day can vividly recall when they learned of his death in 1937. Apart from being a well-liked president, it happened in a way that seemed in keeping with his hard-working West Texas upbringing. As one who was a student at the time put it, he "dropped dead from a heart attack on the sidewalk in front of the old YMCA building, a center of campus activity for many generations. In another world you would have expected him to have fallen dead behind a plow or in his Fordson tractor seat." Calhoun took over as acting president, and, in spite of a reputation as a tight-fisted comptroller of the university, put through the first faculty pay raise in many years. Just the previous year, the pure mathematics department was having problems holding on to its instructors. Porter, Moore, Dodd, Ettlinger, and Vandiver unsuccessfully petitioned the dean for an extra $900 to enable them to keep C. W. Vickery, a 1932 Moore doctoral student, as a full-time instructor "to help with the excessive size of freshman classes." In the summer of 1937, O. H. Hamilton, who had just received his doctorate with Ettlinger, declined an

[67]Presidents' papers/C. P. Boner, five-page memorandum to file, 23 December 1950. Boner's main purpose was to document his belief that R. L. Moore was no longer a positive influence for mathematics at Texas. In a conversation with the author on 20 March 1975, Boner, who died in 1979 at age 79, shed no light on this matter, or the Halsted instructorship incident cited above, and only recounted some earlier and friendlier memories of Moore.

[68][Whyburn], [Archibald, pp. 21, 39, 73]. G. D. Birkhoff papers (Library of Congress): letter to members of the mathematics section of the academy, 27 April 1930.

[69][Greenwood 1972], [Vandiver H 1961].

offer of $900 for a half-time instructorship so that he could accept an offer from Oklahoma A&M.[70]

For 1935–1936, pure mathematics had as instructors, besides Vickery, several others who had recently received doctorates with Moore: Edmund C. Klipple (1932), Robert E. Basye (1933), and F. Burton Jones (1935). Robert E. Greenwood, who had received his B. A. in 1933, was the only instructor who was not a Moore student. After receiving his M.A. and Ph.D. from Princeton, he was to return in 1938 as an instructor in applied mathematics and was one of those who helped to mediate between the two groups on the occasions when their relations deteriorated. He retired as a full professor in 1981.

Money for the top level of faculty was somewhat more forthcoming in 1937 than for instructors. The Texas legislature approved a bill establishing a category of Distinguished Professor and providing $6,500 salaries for nine months to three "nationally distinguished" faculty members to be nominated and voted upon by the graduate faculty. With 58 voting by ballot for the first recipients of this honor, the historian Eugene C. Barker received 33 and Moore 29. The other nominees, geneticist T. S. Painter with 23 votes, zoologist J. T. Patterson with 11, and Vandiver with 7 did not have majorities. Apparently, these results were handed on by Acting President Calhoun to the Board of Regents who then selected Patterson as the third. Vandiver sent in a late ballot for Moore and for the geologist E. H. Sellards, and Vandiver in turn received votes only from Dodd and Porter in pure mathematics. There is no record of Moore taking part in the vote but, in addition to Vandiver, his other colleagues in pure mathematics, Dodd, Porter, and Ettlinger, also voted for him. Later alignments of this group make it tempting to read more into Moore's nonparticipation and the failure of Ettlinger to vote for Vandiver than the documents support, but it is at least clear that there was no complete reciprocity in supporting each other. On the other hand, relationships had clearly not broken down to the extent they were to do by ten years later when it was unlikely either Vandiver or Moore would support each other for anything favorable.[71]

Whatever the relationships were before the balloting, the outcome did not sit well with Vandiver who appealed to the president in 1939. He maintained that reputations could be damaged by the implication that those not selected as Distinguished Professor were in fact not distinguished. "There is in my opinion," Vandiver wrote, "no individual in the faculty or any group of individuals who are at present in a position to estimate the national or international reputation of any particular member of the faculty." Vandiver

[70][Frantz, p. 139]. Presidents' papers/Dean of Arts and Sciences/Pure Mathematics, 1929–1939: Vickery to Parlin, 8 October 1936; Hamilton to Regents, 10 June 1937.

[71]Presidents' papers/General Administration/Distinguished Professors, 1937–1938; [Greenwood 1983, p. 20].

requested that the president make this determination himself and enclosed biographical information, including lists of research grants and publications. He was appointed a Distinguished Professor in 1947.[72] Benedict's death in 1937 seems to have marked the end of a period of relative harmony in the mathematical community at Texas.

POSTSCRIPT

The University of Texas had passed its fifty-year milestone in 1933. In 1939, the president's office sent a questionnaire to the faculty which invited them to evaluate to what extent the university was "a University of the first class" as called for by the 1876 constitution. To the key question, "Does Texas have a University of the First Class?", Dodd answered "No," Ettlinger "Yes, but it can be improved," and Porter "No." There is no record of responses from Moore or Vandiver (who was on leave for part of the year). Porter's replies stand out among all the faculty responses because he simply and directly called for just those practical reforms which did, in fact, take place before long. The university "needs more first class professors. Fellowships are also needed." To the question "Is the intellectual atmosphere conducive to having a University of the first class?", Porter replied "No." "There should be more $6,500 professors for people that deserve them." There was "a lack of understanding and appreciation of high grade research." "I believe there should be a senate to decide important questions of policy and that competent committees should be appointed to assist the Deans in the selection of new professors above assistant. There should be an aggressive and well-equipped Dean of the Graduate School...." He noted at the end, "That skill in teaching should receive full recognition goes without saying. I believe this is less apt to be overlooked than other qualifications."[73]

Though Porter participated in the founding of the graduate school, he never held a higher administrative position than departmental chairman. During his most active years, he might have done much for the university at a higher level, but, as it was, the mathematics faculty was the main beneficiary and arguably came closer than any other department at the university to the high standard he expressed.

ACKNOWLEDGMENTS

In addition to the cited interviews, interviews with the following have provided background information: Anne Barnes, R.H. Bing, Robert E. Greenwood, James M. Hurt, Richard P. Kelisky, R. G. Lubben, and Lucille E. Whyburn. Help in locating material has been provided by the archivist

[72] Presidents' papers/General Subject/Vandiver, 1939. Vandiver to Rainey, 20 October 1939.
[73] Presidents' papers/General Policy/University of the First Class, 1939.

of the Archives of American Mathematics, Frederic F. Burchsted, and by the reading-room staff of the Eugene C. Barker Texas History Center. Uta Merzbach and R. E. Greenwood provided valuable critiques of early drafts. Professor Greenwood has been the departmental memory for many years and has been principally responsible for the faculty memorial resolutions (listed under his name in the bibliography) which show a personal and sensitive acquaintance with the subjects. The present account seeks to provide an archival complement to these biographies.

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