EXPERIENCES AT MICHIGAN, 1949 – 1970

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Introduction This is a rather tricky memoir to write: I intend it to be of some general interest to members of the University of Michigan Mathematics Department, and yet it must necessarily be but one slice of the truth about the Department during that period, as seen through my eyes. I'll try to make it neither too personal nor too impersonal.

I came to Michigan at age 26, and the ensuing years were the most formative of my life, so I remember many events vividly; but I have never been a good raconteur and my memory is rapidly worsening as I approach 80, so there will be not only many gaps in what I recount, but very likely some (unintentional) falsehoods as well.

First let me introduce myself. I grew up in Boulder, graduated from the University of Colorado, did graduate work at Cornell with thesis direction from Mark Kac, Barkley Rosser and Paul Erdös, and was a Peirce Instructor at Harvard for two years before coming to Ann Arbor. During my years at Michigan, I was a Fulbright Research Scholar, learning from Kurt Mahler at the University of Manchester, during 1951-52, then a Sloan Fellow at University College London (Harold Davenport) and Göttingen (C. L. Siegel) during 1957-59, a visiting professor at Colorado during 1963-64, and Executive Editor of Mathematical Reviews during 1965-66. I was Chairman of the department from 1967 to 1970, when I accepted a visiting appointment at the Claremont Graduate School, and liked California so well that I stayed on there until 1977, when I became Executive Director of the American Mathematical Society. I remained such until I retired in 1988. Most of these events, but not quite all, are irrelevant to the rest of my story.

For brevity, I will hereafter use 'the department' as shorthand for the University of Michigan Mathematics Department.

Before McCarthyism This may seem a strange way to partition time, but in fact that political development had a substantial effect on the department and on me, as I will recount later.

I believe an unusually large number of us came as instructors in September of 1949: Don Darling, Joe Lee, Jack Lohwater, Imanuel Marx, Chuck Titus, Joe Ullman and I. Ann Arbor was then chock full of veterans attending college on the G. I. Bill, and the Darlings, and my wife and I, were forced to take apartments in Ypsilanti the first year. Don was a probabilist from Cornell, and my thesis was in probabilistic number theory, so we hit it off well. He was also an avid bridge player, but despite his mathematical background he was convinced that a maleficent spirit always dealt him bad cards, and he groused so vehemently about his chronic bad luck that a number of us refused after a while to play with him. He went on to UC Irvine in about 1970.

Jack Lohwater was a WWII veteran in the Navy, and had sustained a severe head injury in the Pacific. He came from Columbia. He was rather too fond of belittling others, both directly and to third parties, and he created hard feelings in a number of quarters over the years. For some reason I was never, as far as I know, the object of his put-downs, and we were still good friends when I succeeded him at MR in the Sixties.

I didn't know Marx (as he preferred to be called) or Joe Lee as well as some others, as both failed to get tenure and left within a few years. Marx, I recall, was the fastest gun in the West with the Sunday *Times* crossword puzzle; he seemed to fill in as fast as he could read the clues. And he was the subject of one of the many stories about the occasional gaffes and ineptitudes of Ted Hildebrandt, the long-time chairman of the department: Ted informed Marx that he would not be given tenure while they were sitting in adjacent stalls in the men's room on the third floor of Angell Hall.

Chuck Titus had remained at Syracuse for a year after his PhD. A student of Loewner's, he refused to belong to any 'school', and worked somewhere between topology and analysis. Joe Ullman, from Stanford, worked principally in function theory but was interested in individual problems in other areas. These two men were the only ones who remained at Michigan until retirement, and are probably well known to the reader.

The big guns in the department when we youngsters arrived were, from my point of view, Ted Hildebrandt, Ray Wilder, Richard Brauer, and Ruel Churchill. Sumner Myers had a lesser reputation, but was generally regarded as the heir apparent to Hildy, as chairman. Ray Wilder, who held the only named chair in the department, headed a vigorous group of younger topologists, including Edwin Moise, Gail Young, and Hans Samelson, and of course Norman Steenrod had only recently departed for Princeton, so this was probably the strongest tradition in the department. Brauer was pretty much alone in algebra and went on to Harvard two or three years after I came; one of his (inadvertent) contributions to the future of the department was the training of one of his students, Donald J. Lewis, on whose thesis exam I sat during my first year. Classical applied mathematics had rather little prestige in American mathematics at that time, this being the heyday of the axiomatic method and there being no computers, but Churchill was well known for his textbooks in the subject and was the leader of a group including Bob Bartels, Earl Rainville, and Chuck Dolph. This group were housed in West Engineering, and I had the pleasure during my first year of sharing an office with Bartels. Bob Thrall was an erstwhile algebraist who had become more interested in 'modern' applied mathematics, such as operations research, linear programming, and mathematical psychology, and Cecil Nesbitt and Carl Fisher continued the tradition in actuarial mathematics. Maxwell Reade and George Piranian were the among the midlevel classical analysts; later the first became strongly involved in graduate student recruitment and support and the latter in the editorship of the Michigan Mathematics Journal.

I've obviously not listed the whole department above, but these are most of the people who stand out in my mind 50-odd years later. Within the next few years there were some other newcomers of special interest to me, including Jack McLaughlin, Raoul Bott, and Roger Lyndon. Jack and I shared an office (3216 Angell) for 16 years after he arrived in 1950, and we were naturally close friends. He was enormously respected by his students for the clarity and thoroughness of his lectures, and held in awe for the speed with which he delivered them. All three of these newcomers joined a (highly) social group, which also included Titus, Dolph, Darling, Reade and me, and our spouses, and which had a reputation, I believe, as a pretty hard-drinking crowd. Five or six of us were of almost exactly the same age, and our joint thirtieth birthday party was such a rousing success that Raoul returned from Harvard ten years later for the fortieth, in hopes that it would be equally memorable.

Chan Davis and all that Life in the department was exactly what one would expect in a mathematics department for the first four or five years I was there, filled with teaching, research, and as little administrative and committee jobs as one could manage. My only committee assignment that I recall during that period was on the committee we referred to as the Space Cadets, charged with planning for unification of the two portions of the department, then in Angell and West Engineering. The hope at that time was that we would all move into West Engineering at some future date; that unification did not occur, of course, for well over 40 years.

Our peaceful life was interrupted in about 1954 by the heightening cold war and the witch hunting by Senator Joe McCarthy and his brethren of the House UnAmerican Activities Committee. One of the latter was Michigan's Rep. Clardy, who headed a subcommittee that held hearings in various places in the state, including the University. His subjects of interest in Ann Arbor were a member of the Medical School, a biologist, and Chandler Davis of the department. Chan had come as a new PhD from Harvard a few years before; I had already known him there, as a graduate student with strongly held social and political views rather to the left of my own. He had written and/or distributed some political literature around the campus that Clardy's committee charged him with, and the committee demanded to know whether he was a member of the Communist Party. Chan refused to answer, not on the usual grounds of the Fifth Amendment but on his right to free speech under the First Amendment. This naturally raised the ire of the committee, and he was eventually convicted of contempt of Congress and served a one-year sentence, following which he eventually emigrated to Canada. This was bad enough, but the Regents compounded the offense by summarily discharging Davis in August of the year of his hearings, which I believe was 1954. There had already been many heated meetings of the University Senate on this whole problem, and several of us mathematicians, headed by Sumner Myers, had met with the LS&A Executive Committee in an attempt to get fair treatment for Chan's position, and LS&A faculty meetings and the University Senate fairly boiled that fall when the faculty became aware of what was generally regarded as cowardly behavior by President Hatcher and the Regents. In particular, there was a good deal of resentment over the fact that Chan was given no severance pay, while the timing of his dismissal made it essentially impossible for him to get a new job quickly.

This whole event was more complicated than as I have recounted, in part because I don't remember everything and partly because the full account is available elsewhere. But one consequence was of great significance to me: because I had spoken out rather strongly in the Senate meetings on Chan's firing and on the severance pay issue, I was appointed as the junior member of an ad hoc Senate Committee on Severance Pay, charged with recommending a policy on this subject to the Regents. This assignment , which lasted for an entire academic year and resulted in a Regent's bylaw identical with our recommendation, was my first close contact with faculty members other than mathematicians, and it considerably broadened my understanding of the university and academia. Also, I soon found myself on more and more College and University committees having little or nothing to do with mathematics. I'm not sure that anyone other than Chan himself was more affected by his dispute with authority, long range, than I was. By the early 1960s my principal focus had really shifted from almost exclusive concentration on research in number theory to a wide variety of academic and administrative matters.

The Number Theory Program When I came to Michigan there was one number theorist in the department, Leonard Tornheim, who had been a student of L. E. Dickson as I recall. Leonard left within a couple of years to work for an oil company in California, so then there was just me. This amply indicates the status of the theory of numbers in American mathematics at that time; like applied mathematics, it was in such a quiescent state in comparison with algebra, topology and the abstract and axiomatic approach to mathematics that it was regarded by many as a near-dead subject. And Dickson, who had directed the theses of most of the younger American number theorists, all in the arithmetic theory of quadratic forms, was the author of the only American books on number theory, and they did not really succeed as books that would attract graduate students. (My mistake: there was one book, by Uspensky and Heaslet, that was pretty good but very elementary.) But the only really attractive publication in English, on the research level, was Hardy and Wright's elegant tome, first published in about 1940 I think, and research activity in the subject was centered in Germany (Siegel, Hasse, Landau, etc.) and England (Hardy, Littlewood, Mahler, Davenport, etc.)

So by the early 50s I realized that the absence of a modern textbook available to American students was a real problem in popularizing the subject, and I set about to provide one. It eventually appeared in 1955 in two volumes, one entirely elementary and the second containing material from a variety of branches of number theory in which research was currently being conducted in Europe. Soon after, Ivan Niven and Herbert Zuckerman published a somewhat similar book, and I like to think that between us we had some influence on the development of number theory in this country. In any case, by that time I was regularly teaching a year-long course in the subject.

When Don Lewis returned to the department after a period at Notre Dame the program began to grow. We hired a very talented young man from California, Harold Stark, but had to let him go after a few years for lack of publications; to our great embarrassment he settled a famous and long-standing question on factorization in quadratic domains the next year, and of course has gone on to an outstanding career. We also began to get a number of visitors from Europe, including Kloosterman, Turán, Rényi, Davenport and Halberstam. And I started to think about a new writing project, to bring up to date Dickson's *History of the Theory of Numbers*. This had been published in about 1908, and MR didn't start until 1940, with the intervening period covered only in the German journals *Jahrbuch der Fortschritte der Mathematik* and *Zentralblatt für Mathematik*. And even this coverage had been rather spotty, as the *Jahrbuch* plunged in quality during the 1920s and the *Zentralblatt* started only in 1929 or so, and later was marred by Nazi ideology and the war. By 1960 Don Lewis and Harold Davenport and I had developed a proposal to produce a substantial history of the period in question, consisting partly of reviews of the books and papers published then and partly of survey articles on the various branches of number theory, each to be written by one of a team of about a dozen experts who had agreed to participate, including such names as Turán, Gel'fond, Rankin, Erdös, and of course Davenport. It was an ill-fated effort; I presented the idea to the Council of the AMS, hoping to get a recommendation for financial support by the NSF, but the Council declined to give one, feeling that this would concentrate too much money in one branch of mathematics. So it was no surprise when the NSF also declined the proposal. (Later, when computers became available, I persevered in this direction by compiling *Reviews in Number Theory, 1940-72*, with financial support and clerical and computer assistance from the AMS itself.)

By this time Hugh Montgomery had joined the department, and several young number theorists had begun untenured appointments, so the program continued to prosper while I was on leave in London, Göttingen, and Colorado during the late 50s and early 60s; by the mid-60s and thereafter I was hardly a participant.

Mathematical Reviews In 1960, MR had fallen on hard times: it was understaffed, and this was exacerbated by the fact that the Executive Editor, Sydney Gould, was devoting all his time (and interest) to the Society's translation program in Russian and Chinese. So a large backlog of unreviewed material had piled up in the Providence office, and when the number theory history project folded, Gordon Walker, then the Executive Director of the Society, invited me and Walter Heyman — whose plans for a lecture tour had also collapsed — to work at MR assigning articles to reviewers. A couple of years later Gould was replaced by Jack Lohwater (who since leaving Michigan had been at Rice), and by 1965, through truly prodigious effort, Jack had managed to make the journal almost current, except in mathematical physics. But the enthusiasm on the part of Brown mathematicians to participate as consultants at MR had waned; that department was just too small to provide all the help needed. So the AMS Council decided that the editorial offices should be moved to be adjacent to a fresh and much larger mathematics department, and when I agreed to assist in the change, Ann Arbor was chosen as the new site. Only the head of the clerical services, Ray Goucher, elected to move from Providence to Ann Arbor, so I took a leave in the spring of 1965, hired four women to head up departments such as proof reading and library, and we all went to Providence to learn the job. In the meantime the University had made available a building on Fourth Street to house the new offices, and provided considerable remodeling (it had been a brewery); much later MR was moved to two or three other locations in Ann Arbor but eventually returned to this building, where it currently remains. In June the contents of the MR office were packed up and shipped west, and somehow we managed to produce only one slightly tardy issue before getting back to full speed. I remained as Executive Editor until the following summer, and returned to the mathematics department in the fall of 1966.

Comm.-Comm. This section is not exactly part of U-M history, but it may be of some historical interest to mathematicians nonetheless because, of the many AMS committees I served on over the years, the Committee to Monitor Problems in Communication, or Comm.-Comm. as it was usually called, probably had the greatest consequences for the broad mathematical community, even though there are few modern traces of it to be found.

The year was 1966, when computers were young. In the Providence office Gordon Walker, banking on his earlier experience working for American Optical, had installed a phototypesetting machine called the Photon that read punched paper tape to produce pages of mathematics without the usual hand typesetting. The NSF had established an Office of Science Information Services to further the application of computers to scientific publishing, abstracting, and indexing, and the American Chemical Society was busy computerizing *Chemical Abstracts* as fast as the state of the art would allow. Walker, an imaginative and forward-looking person, was well aware of these developments outside the mathematical world, and he persuaded the AMS Council to create Comm.-Comm. and to provide the support needed to make a real push toward modernization of AMS activities. Members of the committee were Alex Rosenberg, Dan Zelinsky, Burton Colvin, Wallace Givens, Walker (*ex officio*) and, I suppose because of my work on MR, I was named chairman. We met many times over several years, with meetings lasting one or two full days. Here are a few of the products of our work.

At that time it was customary for authors to receive 50 or 100 free offprints of journal articles, and to distribute same to a list of colleagues. Many mathematicians had filing cabinets full of offprints received this way, while others had only the relatively few they had especially requested. At the urging of the committee the Society instituted its Mathematical Offprint Service (MOS), in which subscribers provided profiles of their interests according to the MR classification system, and the Society obtained sets of offprints from publishers and distributed them to the appropriate subscribers, the matching of articles against individuals being done on the Society's Digital computer. (MOS lasted five years or more, finally achieving about 1400 subscribers, but was still too labor-intensive to break even, as priced. With modern computers it would be viable now, if it were needed.)

From MOS we learned very quickly that the MR classification system was too coarse for the purpose, and was somewhat dated, and in the summer of 1969 the NSF funded a two-month-long working session, held in what was then the Men's Union in Ann Arbor, to build a more suitable system. Something like 100 mathematicians from around the country spent a few days each, in small groups, suggesting changes and designing classifications for individual areas, and Rosenberg and I oversaw the work and put the final product together. It was immediately adopted by MR, and with rather minor changes this is the system still used.

It was also the committee's idea to prepare collections of reviews from MR in selected branches of mathematics. Profiting from my experience with the number theory history, I thought it would be wise to start with a more popular field, and succeeded in persuading Norman Steenrod to initiate the series, with his *Reviews in Topology*. That was mostly hand work, but for the second collection, in number theory, the computer was used to provide forward references with each item to later articles that referred to it. Six or eight collections were published before MR had so thoroughly computerized its database that such paper products became obsolete. In the meantime, of course, the AMS office had also been making more and more use of computers, in its publishing, clerical, and business operations, and has always been one of the pioneers among scientific societies in this respect.

Recognizing that the AMS is principally, though not exclusively, concerned with research in pure mathematics, Comm.-Comm. obtained NSF funding for a Commission on a National Information System in the Mathematical Sciences, operating under the auspices of the Conference Board of the Mathematical Sciences. So there were representatives from most of the societies belonging to the Conference Board, including SIAM, MAA, Operations Research Society, Association for Symbolic Logic, etc. We held several meetings, but nothing ever came of it; each society has its own way of doing things, and there just wasn't enough impetus to support the large effort that would have been required to unify procedures that were either already in place or not even wanted by the various groups.

In the early Sixties publication had commenced of *Science Citation Index*, and commercial publishers quickly recognized that they could use the concept of 'core journals' (those containing articles getting many citations in the relevant literature) to strongly increase prices of such publications. Alarmed by the strong rise in prices in mathematics journals, Comm.-Comm. caused a list of journal prices in mathematics to be published in the *Notices*. This list was considered sufficiently useful that it was updated several times in later years, but eventually had to be terminated when Gordon & Breach Publishers sued the AMS (and the American Physical Society) for defamation or some such thing, since their journals were among the most expensive on the lists.

Comm.-Comm. remained as a standing committee of the Society for many years, and was instrumental in encouraging the Society to make a number of useful innovations; it was finally dissolved only in about 2000.

Chairing the Math Department George Hay had succeeded Hildebrandt as chairman in 1957, and when his second five-year term expired I was appointed to succeed George. I believe it is true that I came in with the strong support of the department, and for a year or so all went smoothly. But this was a time of turmoil: students were up in arms about the Vietnam War, and civil rights issues were distressing the entire country. Blacks had rioted in Detroit, and black students marched through Angell Hall, throwing a vile-smelling liquid here and there and tossing drawers-full of mathematics department library cards on the floor. Students and a few ultra-liberal faculty members pressured the Literary College to relax standards, eventually forcing the elimination of foreign language and other minimum-curriculum requirements; all this of course led to many long and argumentative College faculty meetings, with a lot of participation by math department members.

So it was a time of unrest; but I don't think that the above facts and incidents would have led to a serious division within the department, since on the whole mathematicians tend to have rather similar social attitudes. But they caused some stress on everybody's part, and this probably exacerbated a problem then unique to the math department. This requires some explanation.

The University had been growing rapidly ever since World War II, first because of the GI Bill and then (partially because of that) because a steadily larger fraction of young people completed high school and then went on to college. With the establishment of the NSF in the late Fifties (after Sputnik went up) it became government policy to encourage students interested in science and mathematics. The resulting growth in math department enrollments led in turn to the appointment of many new instructors (and when that rank was effectively abolished, assistant professors), and those of the young faculty who did a creditable job could fully expect to be promoted to tenure.

But by the late Sixties, projections supplied by the Department of Education showed clearly that enrollments would be leveling off until the baby boom hit the colleges, and the math department faculty, who had mostly been hired since 1945, would have fewer students and experience very few retirements for at least the next decade. So the department Executive Committee realized that it would simply not be possible to more or less routinely promote all the young faculty members to tenure, from then on. (This problem was unique to mathematics at that time simply because we were the only ones to look carefully at the demographic data; other departments experienced the reality a bit later.) The Executive Committee decided to hold a meeting of the tenured faculty to explain the situation, and unfortunately we held it at my house, and at night. It was not intended as a clandestine meeting, but it was so interpreted by the nontenured faculty and even by some of the older members.

At about the same time the E.C. informed some of the assistant professors that they would not be promoted, and should begin looking elsewhere. One of them claimed that he was being treated unfairly, and carried his protest to the College level. In the course of time this created a schism in the department, and I found myself the object of criticism from many quarters. At more or less the same time I became divorced, and when I received an invitation to accept a one-year visiting appointment in the newly-formed mathematics department at the Claremont Graduate School, in California, I found it too attractive to refuse. I had intended to return to Michigan, but somehow never did.

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