Reminiscences about the 1930s

Ralph Phillips

Saunders Mac Lane has recently lectured on American mathematics, giving a bird’s-eye view from the vantage point of a well-established mathematician. I would like to present a worm’s-eye view of American mathematics in the 1930s from the perspective of a beginning mathematician. For me it was not a pretty picture.

I entered UCLA in 1931, about 3 years after its metamorphosis from a teachers’ college in central LA to a university in Westwood. The faculty was essentially the same with the addition of two or three new mathematicians, principally Earl Hedrick and William Whyburn. Hedrick was an imposing and charismatic figure, an excellent administrator and a flamboyant teacher; in time he became president of the American Mathematical Society (AMS). Among mathematicians he is best known for his translation of Goursat’s Cours d’analyse. Whyburn was a student of R. L. Moore, an eccentric Texas mathematician who, together with his students, dominated the field of point-set topology for many years. He popularized the idea of laying out a course in the form of progressively inclusive lemmas and theorems and having his students prove the material. The pace is fairly slow, but such a course is great for developing a student’s ability to do mathematics. I had the good fortune to take courses of this kind from three of Moore’s students: Whyburn at UCLA and later Ayres and Wilder at Michigan.

After finishing a year of graduate studies at UCLA, majoring in both mathematics and physics, I went on to the University of Michigan as a graduate student in physics in 1936. I immediately attached myself to Samuel Goudsmit, who soon afterward encouraged me to get out of physics and into mathematics. At that time, the graduate work in mathematics was essentially in the hands of W. L. Ayres, T. H. Hildebrandt, G. Y. Rainich, and R. L. Wilder. Ayres made a name for himself in point-set topology but gave up on research in mid-career to become chairman and later dean at Purdue. Hildebrandt was a well-educated mathematician, a student of E. H. Moore at Chicago; his research started out very well but slowed down when he became chairman of the department, a position he retained for 23 years. Rainich was a very nice man who was especially kind to me. He specialized in relativity theory and was pleasantly surprised when, toward the end of his life, his work received a lot of attention. Wilder worked in topology; of the four, I believe that he left the most substantial mathematical legacy. Both Hildebrandt and Wilder were presidents of the AMS.

I wrote my thesis under Hildebrandt in the (at that time) rapidly developing field of functional analysis. Hildebrandt was socially awkward, an organist in his church, a man of strict moral judgments and of many prejudices. Along with most other mathematics chairmen of that period he would not hire a black and believed that there should be a quota on the number of Jews in a department. However, his was a generous quota: there

Ralph Phillips, born in 1913, taught at several universities before ending up at Stanford in 1960, where he later became Robert Grimmett Professor of Mathematics. He retired in 1983. He succeeded Einar Hille as the leading protagonist of semigroups in analysis. In 1960 he began a 25-year collaboration with Peter Lax on scattering theory for hyperbolic systems of partial differential equations. His recent work on automorphic functions, with both Peter Lax and Peter Sarnak, is a combination of partial differential equations and analytic number theory. He is a member of the American Academy of Arts and Sciences.
were at least three Jews in the mathematics department at that time. By contrast, it should be noted that in many mathematics departments the quota on Jews was at most one (see [3], p. 182). To this day I am sure that Hildebrandt liked me personally and thought well of my mathematical abilities. Yet he felt obliged to warn schools where I applied for a position that I was Jewish, knowing that it would probably eliminate me as a candidate. I suppose that had he acted otherwise, many of his fellow chairmen would have felt betrayed.

I had a Rackham Fellowship during the 2 years I was in the Mathematics Department at Michigan and for the following year (1939–1940), which I spent at the Institute of Advanced Study. I had written to von Neumann for permission to spend the year at the Institute and he had replied that he looked forward to collaborating with me. I suppose it was naive of me to take his reply literally, but I did and was disappointed when I was unable to see much of him. I have recently learned (see [2], pp. 194–197) that 1939 was a troubling time for von Neumann, both for personal reasons and because he was already deeply involved in war work.

Nevertheless, my stay at the Institute was very rewarding. I took courses from von Neumann and Hermann Weyl and wrote three papers, one in collaboration with Salomon Bochner. I remember Bochner as a kind and friendly man, still troubled by scars inflicted by Nazi anti-Semitism. There was a very good group of young mathematicians at the Institute that year: Warren Ambrose, Hugh Dowker, Paul Erdős, Paul Halmos, Walter Strodt, and Henry Wallman. Among our other activities, we organized a seminar on almost periodic functions. I can report that Erdős was just as eccentric then as he is now. At the end of the year, Oswald Veblen (a professor at the Institute) asked me to go to the University of Washington to replace Abe Taub, who was to visit him the next year. I accepted with the understanding that I would have a fellowship waiting for me at the Institute the following year.

The reason I accepted was that I was becoming anxious about obtaining a permanent job. This was at the height of a 10-year depression, and there were almost no academic positions available for anyone — especially for Jews and foreigners. Talented mathematicians from Europe like Carl Loewner and Fritz John were fortunate to obtain jobs in universities that did not emphasize research. Witold Hurewicz, who arrived in the United States in 1938 with a well-established international reputation, spent a year looking for a job; he had given up and was on his way to China when at the last moment he got an offer from Chapel Hill. The job situation for young Jewish American Ph.D.’s was equally bad. Many of them with Jewish-sounding names changed their names to more acceptable ones, even at the cost of forgoing credit for their earlier publications.

In the summer of 1940 I visited Stanford, hoping to get a job. J. D. Tamarkin was there at the time and recommended me to Gabor Szegö, who was the chairman of the Mathematics Department. Szegö, who was Jewish, later told Peter Lax that he tried to hire me but that the appointment was blocked by Professor Manning because I was Jewish.

At the University of Washington I was one of three new instructors, one of whom was clearly not a research mathematician. In addition to the required 15-hour-a-week load, I taught a beginning topology class. At the end of the year I was the one instructor who was not kept. What made it all worthwhile for me was getting to know my future wife, Jean, who was a teaching assistant at the time.

In the summer of 1941 Jean and I set out for the Institute, stopping off at Ann Arbor on the way. Hildebrandt almost offered me a job but abruptly stopped the negotiations when I informed him of my living arrangements. I do not attribute this to anti-Semitism. However, while we were in Ann Arbor I got a telegram from H. B. Phillips, the chairman at MIT, asking me if I would accept a position there; in those days this was tantamount to an offer. This came at the urging of Ted Martin, who had heard about me from Bochner. I, of course, telegraphed back my willingness to come to MIT, but my reply was never acknowledged. Ted Martin later told me that Phillips had in the meantime been informed by Hildebrandt that I was Jewish and that this ended the offer. I should mention that this was not an isolated event at MIT. A couple of years earlier, Norman Levinson came up against the same prejudice, and it was only because G. H. Hardy intervened on his behalf that he was eventually appointed. Hardy is said to have confronted the provost of MIT, asking him whether he was running a scientific or a theological institute [1].

At the end of the summer, we arrived at Princeton, looking forward to a year at the Institute. But this was not to be. The country was just beginning to prepare for the war in Europe, and the universities were being asked to train officers for the military. This happened in the summer and there was suddenly a shortage of qualified mathematicians. I was approached by Solomon Lefschetz who was empowered to offer me an instructorship at Harvard. "Approached" is probably not the right word, for he made it clear that if I did not accept, my future in mathematics would be very bleak; so I ended up at Harvard in 1941. Incidentally it should be noted that although Lefschetz was Jewish, he was not above engaging in a mild form of anti-Semitism. He told Henry Wallman that he was the last Jewish graduate student that would be admitted to Princeton because Jews could not get a job anyway and so why bother [1]. For the most part, the already established Jewish mathematicians whom I came in contact with (Bochner, Rainich, Szegö, Tamarkin) were very supportive but were limited in what they could do by the prevailing culture.

During my year at Harvard the younger analysts organized a seminar on functional analysis. Gelfand’s paper