Ivan Niven was born in Vancouver, Canada. Following his undergraduate work at the University of British Columbia, he was a student of L. E. Dickson at the University of Chicago, receiving his Ph.D. in 1938. Following a research appointment at the University of Pennsylvania, he taught at the University of Illinois and at Purdue University before settling at the University of Oregon in 1947. Recently he served as President of the MAA. He is known both for his research in number theory and for his skill as an expositor. His books include Irrational Numbers, The Mathematics of Choice, *and (with H. S. Zuckerman)* An Introduction to the Theory of Numbers.

The Threadbare Thirties

IVAN NIVEN

The nineteen thirties were not threadbare for everybody. Full professors had assured positions providing a comfortable, albeit not opulent, standard of living. To be sure, there were salary cuts of 10 to 15 percent about 1932 or 1933 at virtually all institutions, but this was in a period of falling prices, so on balance the professors were relatively well off. By contrast, many young American mathematicians found the thirties somewhat trying, if not downright grim, as also did the flood of emigrés who left Europe for various reasons: for their personal safety, for better opportunities, or out of sheer disgust with the turn of events on the continent.

Although the term "refugees" is often applied to the mathematicians who came from Europe in the years 1933–1942, I shall use the word "emigrés." While some of the newcomers were refugees, many were not, Kurt Gödel and Otto Neugebauer for example. I have seen John von Neumann listed as a refugee. But he came to the United States in 1930, three years before Hitler came to power. A Privatdozent in Germany, von Neumann was offered a professorship at Princeton.

It would take considerable investigation to determine exactly who was a refugee, and who was not. From my point of view, the distinction is not important.

For convenience, the term "thirties" denotes the years 1930–1942, a period when too many Ph.D.'s in mathematics were chasing too few jobs. About 1943 the situation reversed itself and soon there were lots of jobs, not all in mathematics *per se* but in war-related work such as operations analysis for the Air Force, meteorology, cryptanalysis, the development of radar, and the de-Gaussing of ships (so as to avoid attracting magnetic mines). This was very honorable employment, because World War II was regarded as a just war to rid the world of the scourge of Hitler and his allies. Nobody was claiming, certainly nobody was *proclaiming*, that it was immoral to do war work.

Not being a historian, I cannot write history. But I can write more about the life and times of the thirties than just a collection of personal reminiscences. This account is based on written documents of those times, and on information gleaned from conversations and correspondence about the experiences of others as well as my own. Many sources were available to me, partly because I am an ardent meeting-goer, not only to counterbalance the relative isolation of the professorship in Oregon, but also because I have always had an interest in other mathematicians as well as what they are doing in their fields.

My years since 1947 in the rural Northwest were preceded by studies and teaching in the Universities of Chicago (Ph.D. 1938), Pennsylvania, Illinois, and Purdue, followed by several leaves from Oregon to visit larger mathematical centers. An undergraduate at the University of British Columbia in the early thirties, I myself am an immigrant, a naturalized citizen of the U.S. since 1942. To observe the process of becoming a citizen, my friends the late Reinhold and Marianna Baer went along with me to the Federal Courthouse in Danville, Illinois, a court where many an emigré scholar from the University of Illinois with upraised right hand took the oath to uphold the Constitution of the United States, and thenceforth was a foreigner only in part. (My American wife never had to swear to uphold the constitution, but she does anyway.)

The story of the emigrés has been chronicled frequently over the years. We cite a few books available on the subject [**8**, **9**, **27**]. Another, [**13**], devoted to the migration of artists, composers, playwrights, and novelists, presents quite candidly a measure of dissatisfaction with American culture and mores. There are full scale biographies and autobiographies of Richard Courant [**19**], Marc Kac [**14**], Jerzy Neyman [**17**], and Stanislaw Ulam [**26**], as well as shorter pieces on Lipman Bers [**2**], John von Neumann [**27**], and Olga Taussky-Todd [**1**]. The important commentary by Peter D. Lax [**15**] assesses the considerable mathematical impact of the emigrés. There also are recent historical analyses of the migration by Colin R. Fletcher [**10**], and Robin E. Rider [**22**]. Another, by Nathan Reingold [**20**], is being reprinted in this volume. Reingold's article concentrates on the emigration of mathematicians to the United States; Fletcher is concerned with Great Britain, the other country that made a substantial effort to help the emigrés. Rider's article discusses the United States and Great Britain, and physicists as well as mathematicians. The name index in the last five pages of Rider's paper includes the names of mathematicians and physicists who emigrated to Great Britain and the United States in the period 1933–1945.

Arnold Dresden [7] has a list of 129 emigrés, including a few mathematical physicists and economists as well as mathematicians, who came to the United States from 1933 until the first half of 1942. Although Dresden has a convenient year-by-year ordering of the newcomers, his list is not quite complete, understandably, since his paper was published in 1942.

The story of the emigrés is part of the thirties, but only part. My account is concerned with what mathematical life was like in America then. What was the situation here when the emigrés came? Paul Halmos, in his autobiography [**12**], opens a lot of windows, and so does Garrett Birkhoff [**4**], who, although ostensibly discussing leaders of American mathematics from 1891 to 1941, offers penetrating comments in passing about the strengths as well as the limitations of mathematical education and research in that period.

Over and above this material in print, I have been helped immeasurably by friends who lived through that era and whose conversation and correspondence about the thirties has not only added to my knowledge about the era, but also corrected a few misimpressions I had as well as confirming many of my recollections. I list their names at the close of this article. Any quotation with no reference number attached is taken from a letter from one of these correspondents.

LIFE IN THE THIRTIES

In the Great Depression of the nineteen thirties, business failures were widespread, leading to unemployment and severe privation for a large fraction of the population. Unemployment rates were high throughout the period, as much as 25 percent at times. Bank failures brought great hardship to the depositors, for there was no such thing as deposit insurance, which is standard today. There were no food stamps or unemployment benefits. Many of the elderly were in severe straits, for although a few employers had pension plans, there was no general retirement program like the Social Security System, which was only being developed at the end of the thirties. Studs Terkel [**25**] has collected and edited some dramatic descriptions of life in those days through a collection of oral histories of some of those hit hard by the Depression.

In the thirties, financial appropriations to state institutions were slashed by legislatures; in private schools, income from endowments and gifts from donors dropped sharply. Most college and university faculties took salary cuts of 10 to 15 percent around 1932 or 1933, and even so it was necessary in some schools to terminate a few junior faculty members. Increased teaching loads were occasionally reported, as also was the dropping of some courses from the curriculum for budgetary reasons.

Because universities and colleges were hard-pressed for funds, in most cases there were no paper graders, no travel money to attend meetings (whether or not a paper was being given), no sabbaticals, no secretarial help with the typing of papers or correspondence (except for department heads), not even stamps for correspondence; no institution-sponsored health insurance or life insurance. Regularized retirement programs were few and far between. Thus fringe benefits were very meagre compared to today, when they augment an academic salary by as much as 30 percent or more.

Research grants for mathematicians were very rare; the National Science Foundation was not created until the early fifties.

There were 65 Ph.D.'s given in mathematics in 1938. Fifty years later, the annual number is more than ten times as large, and the whole mathematical community is proportionately larger, even though the population of the country has scarcely doubled over the five decades.

In 1938, virtually all the Ph.D. degrees were awarded to citizens of the United States and Canada. Today almost half the degrees go to foreign nationals. Lynn Steen, in his retiring MAA presidential address, said that the number of American students taking Ph.D. degrees in mathematics in 1987 was less than 40 percent of what it was 15 years ago.

Although the number of male Americans taking Ph.D.'s in mathematics has decreased sharply in the last 15 years, the number of females among the Americans earning Ph.D. degrees has remained virtually constant. Thus the percentage of females has risen from about 10 to about 20 over the past 15 years, although the actual number of women held fairly steady.

How many women were there 50 years ago among the Ph.D. graduates? The answer is at least 14 percent of the group of 65 in 1938. The precise percentage is not readily determined because the list of names published in the *Bulletin of the AMS* in May 1939 included many names with initials only and a surname. However, nine of the 65 names (14 percent) were clearly identifiable as females.

Although the historian Alan Palmer has written that "The greatest revolution in the twentieth century world has been the changed status of women in society," his conclusion could hardly be substantiated by the data mentioned above. Women have made striking advances in other fields, by comparison. In law, 38 percent of the degrees were earned by women in 1985, up from 7 percent in 1971. Even more remarkable, 49 percent of the degrees in accounting were awarded to women in 1985, up from 10 percent in 1971.

Even if we go back to even earlier times, the percentage of women among mathematicians was not remarkably smaller than today. R. G. D. Richardson, secretary of the AMS at the time, reported [**21**] in 1936 that of the 1286 Ph.D.'s in mathematics graduated in the United States from the time of the first Ph.D. at Yale in 1862 up to 1934 inclusive, 168 were women; that's 13 percent.

Teaching loads were higher in the thirties. In schools with Ph.D. programs, three courses was a common load for younger faculty members, except at a very few major private institutions with slightly lighter loads. But four courses was a common load at institutions with only a Master's degree program or no graduate work at all. Emeritus Professor M. Wiles Keller writes that when he went to Purdue University in 1936, "most of the staff taught 18 hours per week." He added that loads of 15 hours were possible, presumably for a few more scholarly professors. This differential in teaching loads for scholars was not uncommon: Ralph P. Boas reports that at Duke University he was given only 3 courses "as an incentive to research," where 4 courses was the nominal teaching load. Similarly, Abraham H. Taub writes that he went to the University of Washington as an instructor in 1936 with a teaching load of 13 hours a week, where the normal teaching load was 15 hours a week. He was given a "research" allowance.

According to the AMS Survey [**3**, Part II, p. 21] headed by A. A. Albert, teaching loads in the midfifties were still around 10 or 11 hours per week for younger faculty members in the major state schools with Ph.D. programs.

Department heads in the thirties, secure in positions they could hold as long as they wished, rarely consulted more than a small inner circle of professors, if that, about significant decisions on hiring, tenure, and promotion. They could control graduate admissions and graduate assistantships and fellowships, or delegate this control to trusted colleagues. Very few departments had a formal committee structure. In short, the department heads could be, and many were, autocrats, benevolent in varying degrees. This system could be very effective if a strong department head was brought in to build up a lagging department, to offset the danger of mediocrity perpetuating itself. Nevertheless, there is greater justice in the modern practice of a periodic review of department heads.

One of the most significant changes in the last fifty years has been the attitudes of students. There were no student evaluations of faculty in those days; the students were being evaluated, not the faculty. The students treated the faculty with a measure of deference; the injunction to "Challenge Authority," often seen today on T-shirts and bumper stickers, was observed frequently in the political arena, but rarely in academia. Even though the percentage of failing grades was noticeably higher 50 years ago, students did not challenge professors who failed them. A badly performing student accepted a failing grade with docility as a rule.

The grade of F was given much more freely in the thirties than today. M. Wiles Keller writes that as of 1936 at Purdue University "a failure rate of 30 percent was not unusual" in lower division classes. This remarkably high rate was lowered at Purdue within a few years, and should not be taken as commonplace in colleges and universities in the thirties. Nevertheless, it was not regarded as improper to have a 10 to 15 percent failure rate in a class.

UNEMPLOYMENT

In the thirties we all knew from personal experience that there were Americans with Ph.D.'s in mathematics who were unemployed, and others who were employed but in positions not closely related to a research degree in mathematics — high school teaching for example. There is one study that yielded some hard data on the numbers. In 1933 the Mathematical Association of America appointed a commission to determine "as accurately as possible the present situation" on the employment of Ph.D.'s in mathematics. E. J. Moulton **[16]**, reporting for the Commission, wrote that "there were about 40 or 50 Doctors of Philosophy in mathematics who had not, as of October first (1934) found employment reasonably satisfactory to them."

The Commission reached this conclusion on the basis of evidence obtained from 50 leading universities in the country. Thus 180 mathematicians with Ph.D.'s were identified who were seeking positions for 1934–1935. Out of this group, the employment status on October 1, 1934 of 149 persons was tracked down. Here is the breakdown: 14 were unemployed; 5 had work "which in no way related to their special training"; 18 held assistantships at low pay (they were continuing their employment as graduate students, in effect); 12 were in government service or business "where their mathematical training was a direct asset"; 12 had fellowships to continue their studies; 88 had teaching positions (21 in universities, 53 in colleges, 2 in normal schools, 2 in junior colleges, and 10 in high schools or academies).

Apart from the first three groups $(14+5+18)$, the 112 others were viewed by the Commission as having "obtained positions more or less satisfactory to them." This is surely debatable: of the three Ph.D.'s teaching in high school in the thirties whom I knew well (then or later), not one regarded it as truly satisfactory employment, but makeshift, and all three of them worked very hard to get into university work. This is not to suggest that it was easy for a Ph.D. in mathematics to even get a high school post in the thirties. Then, as now, there were requirements laid down by state departments of education; also a school system strapped for funds might not, within its structural salary scale, have the additional funds for an individual with a very large number of university course credits; and finally, the other teachers were not always enthusiastic about bringing in someone who outclassed them in mathematical knowledge and understanding.

Contrasted with the above estimates of 40 to 50 Ph.D.'s who had not found satisfactory employment by October 1934, the secretary of the American Mathematical Society, R. D. G. Richardson reported [**20**, p. 323], that there were 75 American Ph.D.'s in mathematics out of work in 1935.

Salaries and Ranks

"Grade inflation" is well-known, with classes getting more high grades and fewer low grades, causing a rising grade point average. Not so well-known perhaps is "rank inflation," as revealed in the distribution of ranks in departments of mathematics. There was a four rank system 50 years ago, with fresh Ph.D.'s spending several years at the rank of instructor, compared with a three rank system in widespread use today. This factor alone causes rank inflation, but there is more to it than that. Consider the University of Illinois as an example. In 1939 there were 29 faculty members with Ph.D.'s active in the Department of Mathematics: 4 full professors, 2 associate professors, 5 assistant professors, and 18 instructors (actually 8 instructors and 10 "associates," a rank peculiar to Illinois, equivalent to senior instructor perhaps). In early 1988, in a total faculty of 99, the distribution of ranks was quite different: 70 professors, 15 associate professors, 9 assistant professors, and 5 teaching associates. Looking at the full professorship figures alone, that rank now accounts for 71 percent of the Department of Mathematics, compared to 14 percent in 1939. A similar inflation in the percentage of professors has occurred in many universities, although the change may not have been as great as at the University of Illinois. As an aside, 4 of the 29 faculty members at Illinois in 1939 were women; the corresponding figures for 1988 are 15 out of 99.

Although mathematicians are better off financially in 1988 than in 1938, it is generally thought that academic salaries have not kept pace with the advances made by many other professional people over that period, such as physicians, lawyers, dentists, and accountants. How much better off are the mathematicians? The standard entry-level salary in the thirties was \$1800, compared to at least \$25,000 today. Inflation has devalued the dollar by a factor of about 8 to 1, so that the \$1800 of 1938 is the equivalent of about \$14,400 today. A beginning salary of \$25,000 is 74 percent higher.

In comparing salaries then and now, note that Federal income taxes are higher today than in the thirties. On the other hand, there are many more sources of summer income today, other than just summer school teaching. There are two additional factors favoring the situation in modern times. First, the value of fringe benefits in 1988 amounts to 25 or 30 percent of salary, compared to a paltry figure in the thirties. In many schools fringe benefits were unheard of. The other factor is the inflation in ranks. Bright young mathematicians can expect to rise faster through the ranks today, thus getting into the higher salary brackets more quickly. From the data given above about the distribution of ranks at the University of Illinois, it is clear that the institution is supporting mathematics more handsomely than in earlier times.

Annual raises are not uncommon today. By contrast, Emeritus Professor Charles E. Rickart of Yale commented to me recently that there were periods when he got scarcely any raises at all except when he was promoted.

One explanation for the "Rickart phenomenon" is that, just as today, many institutions had established a minimum salary for each rank, so that for example every assistant professor would have a salary no less than, say, \$2800. Budgets being extremely tight, the minimum salary at each rank tended to be a maximum, with all raise money reserved for those getting promotions.

Many mathematicians of that era have interesting stories to tell about promotions and raises, except for those who don't care to recall that period in too much detail.

Mark Kac had two years of very temporary positions following his Ph.D. in 1937. Then he got an instructorship in Cornell University in 1939, with a promotion to an assistant professorship in 1943. In his autobiography [**14**, p. 99] Kac comments, "At the time of this promotion I had about 25 publications. How times have changed!."

The case of the distinguished mathematician George Pólya illustrates how slow promotions were even in the World War II era. Although Pólya had been a professor at the École Polytechnique Fédérale in Zürich from 1928–1940, he came to Stanford in 1942 as an associate professor. His promotion to a full professorship came only in 1947, when Pólya was 59! It seems unlikely that he was held back by prejudice, for the department head at Stanford from 1938–1953 was Pólya's fellow Hungarian and co-author Gabor Szegö. Harold Bacon, an Emeritus Professor at Stanford who was on the faculty during that whole period, writes that the year Pólya was hired "was a war year, and the budget constraints, then and in the next few years, were exceedingly tight. It was only possible to offer him the salary, and therefore the rank, of associate professor.... I am certain that the financial situation was the only serious obstacle to his having been brought here at the higher rank." Budgetary problems lay heavily on the academic world in the U.S. for at least 20 years beginning in 1930.

The Research Atmosphere

There was a leisurely pace to mathematical life in this country in the thirties, except at a few active graduate centers. Garrett Birkhoff [**4**, p. 50] has written, "Whereas today at least ten seminars in Cambridge compete for attention, each concerned with a different subarea of mathematical research, there was then (the 30s) only one weekly colloquium in Greater Boston. It was attended also by research-oriented MIT staff members and (often) by a contingent from Brown." Harvard was the center for this seminar.

Ralph Boas (Ph.D. Harvard, 1937) notes that as late as 1950 when he went to Northwestern, research was not easy because, "grants and really reduced teaching loads hadn't been invented.... Those of us who were serious about research stuck close to work. We didn't have much time for recreation or social life in contrast to the old-timers. When I came to Northwestern (in 1950), there was a staff of about 20, and the department as a whole was publishing 3 or 4 papers a year; 10 years later, there were around 30 people, publishing 20 or 30 papers a year."

Boas also has this to say about finding time for research, "... many of us didn't have families and the distractions that go with them. Some (mathematicians) married wives who took care of them and left them free to do research, an arrangement I don't approve of." I don't approve of it either, unless a woman freely and knowingly chooses to follow that pattern of life and does not do so because of pressure from her family or her religious group. Since the world is so vastly different in 1988, it is important to have on the record that the prevalent lifestyle in America fifty years ago was based on an unwritten but commonly understood contract between husband and wife that it was his responsibility to work outside the home to support the family financially, and her responsibility to manage the household. Of course, there always had been cases where both husband and wife worked outside the home, but these were in the minority. (Mary L. Boas, the wife of Ralph, is now a Professor Emeritus of physics.) "Responsibility to manage the household" meant doing everything possible to free the husband for his work, in a very full sense. For example, in 1939 Hans Rademacher had an auto even though neither he nor his wife could drive; a friend drove them around occasionally. I offered to teach Rademacher, who was in his forties at the time, how to drive. After thinking it over he astonished me by proposing that rather than teaching him how to drive he would prefer that I teach his wife. So I taught his wife to drive in that spring of 1939. Hans Rademacher never did take up driving, to my knowledge. He spoke frequently about the need for large blocks of uninterrupted time to think about mathematics.

The Great Depression had the beneficial effect of raising the level of instruction in mathematics, as the surplus of good mathematicians filtered down to less prestigious schools. Apart from the 20 or so top departments of mathematics and a very few others, there had been a tradition of faculty advancing through the ranks without having produced any creative mathematical work beyond the one or two papers evolving from the Ph.D. dissertation. There was no pattern of regular seminars or colloquia, or even a pattern of keeping up with mathematics. (For example, faculty members at the University of British Columbia gave no talks outside of class.) But the severe competition created by the shortage of jobs in the thirties along with the coming of the emigrés altered that pattern of mathematical inactivity. Young mathematicians set their sights on continuing participation in the advances of the field. In many schools there was almost a dichotomy between the "young Turks" and the old-timers. The number of universities with active seminar and research programs grew from around 20 to 50 or more just from 1930 to 1950.

There is general agreement with an observation of Ralph Boas that "I think that the new Ph.D.'s who had the hardest time in the 1930s were the ones who finished a few years before we did." That means Ph.D.'s from the early thirties. In many cases the first few years after the Ph.D. were spent moving from place to place, with gnawing uncertainty from year to year about what the future held. Baley Price, with a Ph.D. from Harvard in 1932, spent 5 years in temporary positions at Union College, Rochester, and Brown before settling into what would now be called "a tenure-track position" at the University of Kansas. D. H. Lehmer, a Professor Emeritus at Berkeley, wrote "I took my Ph.D. (at Brown) in 1930, but I got my first real job in 1934 at Lehigh University because someone died that summer. These four years were full of disappointments, but there were interesting episodes too. We got to know a good many unemployed mathematicians and we were no worse off than the average." Both D. H. Lehmer and Leo Zippin were listed in *Scripta Mathematica* in volume 4 (1936), pp. 87–93, 188–195, 283–389, 330–334, among seventeen leading young (under 40) mathematicians in the U.S., and both of them had years of uncertainty in the thirties.

The late J. W. T. Youngs (Ph.D., Ohio State, 1934) taught in St. Paul's, a private high school for boys, for several years in the thirties following his doctor's degree, all the while hoping to get back into university work. He succeeded and went on to a very successful career in analysis and later, combinatorics. He was chair of the department (Chairman, in those days) at Indiana University for several years.

These cases illustrate a special phenomenon of the thirties: the many mathematicians who did not get to follow the careers their graduate professors had led them to expect, at least not without a delay of several years of uncertainty in the critical period immediately after the completion of their graduate work. Some were lost to mathematics and others never reached their full potential.

Recollections from the Thirties

Everett Pitcher, Ph.D. in 1935 at Harvard with Marston Morse and long-time secretary of the AMS, wrote, "Graduate students (at Harvard) were not married. Graduate students lost their financial support if they married. One of my mentors (Ph.D. Chicago about 1924) started at Harvard and had to leave when he married." There were other schools at that time, Pennsylvania for example, having that same rule of no financial assistance for married graduate students.

The depressed salaries in academia nudged some young mathematicians toward industrial positions, which was, in terms of the general welfare of the country, not necessarily a negative trend. The switch to industry was not difficult when it followed a leave of absence from a university to do war work. One example was Leon (Leonidas) Alaoglu, who like me was at that time a Canadian with a Ph.D. in 1938 from Chicago. After a year at Pennsylvania State, Leon spent three years at Harvard as Benjamin Peirce Instructor. In 1942 he went to Purdue University as an instructor, at a lower salary than he had at Harvard.

By 1942 Alaoglu had published his landmark paper (*Annals of Mathematics*, 1940) containing the basic theorem which now bears his name: The closed unit ball in the dual space of a Banach space is compact in the weak-star topology. The paper had developed out of Leon's graduate work with Lawrence M. Graves at Chicago. And while at Harvard, Leon wrote two papers jointly with Garrett Birkhoff in which they established general ergodic theorems on semigroups of linear operators. In 1944 Leon left Purdue on leave of absence to work with the United States Air Force as an operations analyst in World War II. At the conclusion of the war Leon could have returned to Purdue but still as an instructor and at the same salary he had when he left to work for the Air Force. I remember well how scornful of Purdue he was, as he moved out of academic life. He was very successful in industrial work, for within a few years he was a scientist in the Research and Development Division of the Lockheed Aircraft Corporation. While there, Alaoglu participated in the colloquia and seminars at nearby Cal Tech. After his untimely death, an annual Leonidas Alaoglu Lecture was established there in his honor.

Saunders Mac Lane came close to taking a high school teaching position. The circumstances were these: with his baccalaureate degree from Yale and a Master's from Chicago, Mac Lane completed his Ph.D. work at Göttingen in 1933, one of the last Americans to go to Germany for graduate work in mathematics before World War II. Next there was a one year post at Yale, and then Mac Lane was looking for a position in the spring of 1934. Jobs were very scarce, so he interviewed for a position at Exeter, a well-known private high school of the first rank. However, an opportunity opened up for a Benjamin Peirce Instructorship at Harvard, enabling him to stay in the university system. It is interesting to contemplate what course his career might have taken if this leading American mathematician had gone to Exeter instead of to Harvard.

Eugene Northrop, a 1934 Ph.D. from Yale, was on the job market along with Saunders Mac Lane in the spring of that year. In his case he could find no satisfactory university or college position, so Northrop took a teaching position in a private secondary school in the northeast. Northrop never returned to the career in teaching and research at the university level he had originally contemplated, although he later taught, and played a leading role, in the lower division program at the University of Chicago.

Nathan Jacobson, Ph.D. in 1934 from Princeton with Wedderburn, taught at Bryn Mawr in 1935–1936 as a result of the death of Emmy Noether on April 14, 1935, a very sad loss for mathematics. Here is Jacobson's description of the situation, taken from an autobiographical article which will be included in his forthcoming Collected Mathematical Papers, "Since a number of courses by Emmy had been announced for the following year, Professor Anna Pell Wheeler, the chairperson of the department at Bryn Mawr, had to find someone who could give these courses. She offered me a lectureship for the year 1935–1936." The next year, 1936–1937, Jacobson held a National Research Council Fellowship which he spent mostly at Chicago working with A. A. Albert. I recall a special series of lectures on algebra that he gave that year. The star that he became in the mathematical world was beginning to shine. And yet, three years after his Ph.D., Jacobson was once again looking for a regular position in the spring of 1937.

He comments, "Perhaps it is appropriate to describe the employment situation at that time. First, this was in the depth of the Great Depression. Salaries declined in some instances and there were very few new positions. Moreover, for the new Jewish Ph.D.'s the situation was further aggravated by anti-Semitism that was prevalent, especially in the top universities — the only ones that had any interest in fostering research."

Although the early thirties were apparently tougher than the late thirties, the depression was not really over for mathematicians until 1943 or so. Paul Halmos, well-known as an eminent mathematician and a great expository writer and lecturer, with a Ph.D. from Illinois in 1938, sent out over 100 letters that spring inquiring about possible openings, with no luck at all. As he recounts on page 80 of his autobiography **[12]**, the University of Illinois kept him on for a year at a salary of $1800 and a teaching load of 15 hours per week.

Another example of hardship is that of the late Thurman S. Peterson. He was unemployed in the fall of 1938, living with his parents in Los Angeles, when an opportunity arose for him at the University of Oregon because of a sudden, debilitating illness of the department head. Peterson and I were colleagues at Oregon for many years. Here's the rest of the story: after his undergraduate years at Caltech and a Ph.D. at Ohio State in 1930, Peterson held a temporary post at the University of Michigan for two years, and then got a stipend at the Institute for Advanced Study. After two years there, he was slated to be an assistant to one of the senior members of the Institute in 1934–1935. However, in the spring of '34 an inquiry came in to the Institute about a suitable candidate for a teaching post at a private high school for girls in the Philadelphia area. Teaching mathematics to high school students did not have great appeal to Peterson, but he was urged strongly by the Director of the Institute to apply for the post, so as to open up a stipend there for some mathematician in need. Accordingly, Peterson did apply, and after an interview was awarded the position. Years later, he told me that four years of that work was all that he could stand, getting farther and farther away from university life. In 1938 Peterson quit, with no other employment in sight, and that takes us back to his unemployed status in Los Angeles in the fall of 1938.

THE COMING OF THE EMIGRÉS

Clearly there was not a prosperous expanding economy for mathematicians in America when the emigrés arrived on the scene. Jobs were very scarce until 1942 or so, and suddently became plentiful in 1943 with war work of one kind or another. For example, the University of Oregon had four active faculty members in 1942–1943, contrasted with eighteen the next academic year. The sharp increase was caused by the creation by the Federal government of specialized military training programs at scores of universities and colleges. The shortage of college-level teachers of mathematics became so severe that retired faculty members were asked to help out with the load: the distinguished Hans Blichfeldt at Stanford University for example.

The United States, a country of immigrants and their descendants, had over the years welcomed many foreign mathematicians into its universities: J. J. Sylvester at Johns Hopkins, Heinrich Maschke and Oskar Bolza at Chicago, J. D. Tamarkin at Brown, to mention just an influential few. The difference in the thirties was that the mathematicians came in from Europe in such large numbers that they could not be absorbed comfortably in an already depressed job market. How many came? Arnold Dresden [7] lists a total of 129 emigrés year by year from 1933 through the first half of 1942. To give some idea of the relative magnitude of this number, we contrast it with an estimate by Garrett Birkhoff concerning the approximately 937 academic mathematicians with Ph.D.'s in the United States and Canada in 1933, that "of these probably less than 150 were active in research" [**4**, p. 67].

Peter Lax gives a "necessarily partial" list of 57 "illustrious immigrants," including a few who came just after World War II [**15**, p. 132]. Of the more than 100 active mathematicians who came to America over a few years, at least 50 were eminent in research. Understandably, these world-class scholars wanted to continue their work in research settings, which meant at some 25 Ph.D. degree granting schools. Given the severe financial situation, it was out of the question for these institutions to create 50 professorships. Whatever hiring these universities were doing was mostly at the instructor level.

The outcome, as might be expected, was that many an outstanding emigré had to swallow his pride and take what was available, with great financial hardship in many cases. Here are a few examples.

Stanislaw Ulam, who had taken his Ph.D. in Poland in 1933, came to the University of Wisconsin as an instructor in 1941.

Alfred Tarski was appointed to an instructorship at Berkeley in 1942, at age 40.

Richard Courant went to New York University in 1934 as a professor, with an annual salary of $4000, contrasted with his previous salary equivalent to about $12000 as Director of the Mathematical Institute at Göttingen.

Max Dehn, a distinguished topologist who was the first mathematician to solve one of the 23 famous problems posed by Hilbert at the turn of the century, fled eastward across the USSR via the trans-Siberian railroad, and found his way to Pocatello, Idaho. He got a position at Idaho State College there in 1941 at a salary of $1200. Later he moved to a position at the Illinois Institute of Technology. Carl Ludwig Siegel [**23**] commented later that "... although the experts were well aware of his reputation, the dearth of financial support for research facilites at that time (in the U.S.) made it impossible to provide him (Dehn) with a position suited to his abilities. The more prestigious universities thought it unbecoming to offer him an ill-paying position, and found it best simply to ignore his presence." I'm not sure whether that is an accurate and fair assessment of the situation.

In recent years I have heard criticisms similar to that of Siegel, that surely the United States could have offered better positions to Emil Artin, Emmy Noether, Hans Rademacher, Antoni Zygmund, and others. Perhaps so, but the problem was not simple. For one thing, the department heads were very troubled by the dilemma of whether to fill any opening with an emigré or a young American, as Reingold [**20**] has made clear in his essay.

Although Siegel is right in his comment about "the dearth of financial support for research facilities," there is an ironic aspect to this. In the 1920s the Rockefeller Foundation had contributed funds to help bring the Mathematical Institute at Göttingen to its position of world leadership. At that time and into the early thirties there was no center in the United States that could compare with Göttingen, according to Saunders Mac Lane [**19**, p. 130] and others. With degrees from Yale and Chicago prior to his Ph.D. from Göttingen in 1933, Mac Lane is in a good position to make comparisons.

Hans Rademacher, a distinguished figure in analysis and analytic number theory, had been a full professor of mathematics at the University of Breslau in Germany from 1925 to 1933. He was dismissed by the Nazis because he was a member of the International League for the Rights of Man and president of the Breslau chapter of the German Society for Peace. Migrating to America, Rademacher spent the year 1934–1935 at the University of

Pennsylvania under a joint grant from the Emergency Committee of Displaced German Scholars (later Displaced Foreign Scholars) and the Rockefeller Foundation. In 1935, at age 43, he was invited to stay on, as an assistant professor. Presumably this rank was assigned to him because the University lacked the funds to do better. In any event, Rademacher accepted and kept hoping year after year for a rank in keeping with his achievements. In the preface to Volume I of Rademacher's Collected Papers [**11**, p. xvi], the editor Emil Grosswald writes, "In those years, the length of faithful service to the institution (University of Pennsylvania) and not professional excellence, was the main criterion for promotions — a fact that was forcefully explained to the somewhat surprised assistant professor by a most self-assured dean."

The University promoted Hans Rademacher from an assistant professor to a full professor in 1939, to meet an outside offer from a comparable university. There was a slight hitch when the offer came in, since the considerable salary raise needed to keep Rademacher amounted to virtually the entire dollar amount for raises for the whole department. John R. Kline, the head of the graduate program in mathematics, pressed the University administration very hard that it would be a serious mistake to lose such an outstanding scholar who was at the same time a superb teacher. Kline succeeded; funds were found and Rademacher stayed on. His Ph.D. students in the years since have contributed a great deal to analytic number theory and cognate topics. I know the story well because Herbert Zuckerman and I had both chosen, as postdoctoral fellows, to spend the year 1938–1939 at Pennsylvania working with the most accomplished assistant professor I have ever seen.

Although the emigrés were generally received with friendship and cordiality, there was one unhappy incident. In the late thirties, Richard Courant of New York University was invited to Yale University to speak. It fell to Einar Hille "painfully to disinvite him, not because of antisemitism, but rather xenophobia among the graduate students who felt keenly the 'unfair,' as they saw it, competition of outstanding foreign mathematicians for the few available jobs." This quotation is taken from a letter dated September 23, 1987, to Peter Lax of NYU from Asger Aaboe, Professor of the History of Science at Yale. Peter Lax drew it to my attention, commenting that he had learned of the incident from Richard Courant several years earlier. I requested permission from Asger Aaboe to quote this passage from his letter, and he kindly agreed.

Richard Courant was a colorful and controversial figure, as the biography by Constance Reid [**19**] shows. He was a remarkable mathematical leader, coming as he did to this country in 1934 after losing his position as Director of the Mathematical Institute in Göttingen, and subsequently developing mathematics at New York University from a modest position to "Göttingen in New York."

One incident in Courant's career focuses on a cultural difference between Europe and the United States. In Europe there was in many places a strong tradition of graduate students and even younger faculty members serving as assistants to professors in the course of pursuing their own studies, in a kind of apprenticeship system. The biographies of Courant and Neyman [**19**, **17**] confirm that quite clearly.

A contretemps arose in the writing of the well-received book, *What Is Mathematics*? by Richard Courant and Herbert Robbins [**6**]. The book was copyrighted in the name of one author only, Courant. In the preface to the book, written and signed by only one of the authors, Courant thanks his co-author for his work in the preparation of the book. Herbert Robbins, a young American with a Ph.D. from Harvard, had participated in the writing on the understanding, at least on his part, that he was to be a co-author, not just a person thanked in the preface.

Also, six very capable young men are credited in the preface with helping "in the endless task of writing and rewriting the manuscript," in addition to another who wrote a first version from a course of lectures by Courant. It is quite customary for American authors to express thanks to friends and colleagues who have *read* critically a first draft of a manuscript produced by the author(s), but usually not who have *written* and *rewritten* it. However, Courant was quite accustomed to having his younger colleagues assist in the writing of his books. There was nothing irregular or unusual about it, since it was part of his cultural academic background. He had intended to be the sole author of *What is Mathematics.*

For further details on this cultural conflict, see the references [**1**, pp. 283–298] and [**19**, pp. 223 ff.].

The Emergency Committee

Gaining entry to the United States was not automatic or easy. The open door policy for European immigrants of the early years of the century was replaced by a quota system in the Johnson-Reed Act of 1924 which restricted immigration from any country to 2 percent of the number of persons from that country already in the U.S. according to the 1890 census. By the 1930s the quotas were heavily oversubscribed, so that when Lipman Bers went to a U.S. Consular Office in Paris he was told [**2**, p. 276] "Register and come back in fifteen years." However, the Immigration Act of 1924 had a specific exemption of university teachers from the quotas. The exemption applied only to those with specific assurance of a teaching position in the United States. Most of the well-known, established European scholars came in under this exemption. In the case of Bers yet another avenue was used to gain entry. He says [**2**, p. 277], "I literally owe my life to Mrs. [Franklin D.] Roosevelt. She convinced her husband to issue special emergency visas to political refugees and intellectuals caught in France after the defeat of the French and considered particularly endangered. (At that time, nobody had any idea that millions of people would be killed just for being Jewish!) Committees in New York and Washington were making lists of people who ought to be given these visas. All we had to do was identify ourselves." Lipman Bers, having fled from Paris ten days before it fell to the conquering German Army, was in the southern part of France, not occupied by the Germans, when he got a telegram instructing him to go to the U.S. Consulate in Marseilles for a visa.

As early as 1933 there was concern in the United States over the plight of displaced German scholars. A committee was formed, later to be named the Emergency Committee for Displaced Foreign Scholars, with the purpose of locating positions in the U.S. for mathematicians so they could enter the country under the university teaching exemption to the quota restrictions of the immigration laws. The article of Nathan Reingold [**20**] has a full description of the work of this committee, which, by the way, included among its members the presidents of 17 colleges and universities [**22**, p. 116].

The one American mathematician singled out more than any other "for his untiring help to so many" [**15**, p. 129] was Oswald Veblen. As to Great Britain, Robin E. Rider [**22**, p. 159] has written that, "Under G. H. Hardy's influence, Cambridge found room for eighteen mathematicians," a remarkable achievement.

The Emergency Committee wanted to save scholarship, so they were interested primarily in helping distinguished scholars to find a place in this country. It was natural for these scholars to try for the universities with well-established graduate schools and research programs. The department heads in these schools were faced with a dilemma in filling any opening they had. Should they give employment to an outstanding mathematician from Europe or to a young American? Nathan Reingold [**20**], in preparing his detailed account, had access to much of the correspondence on this problem between various leaders of American mathematics. The young mathematicians in the U.S. were not troubled by any such dilemma. For the most part they did not feel threatened by the emigrés, perhaps because the newcomers were older. The young Americans on the job market were looking for instructorships, with entry-level salaries. Furthermore, it became clearer with every passing year that the European contingent was adding a lot to scholarship here, to understate the case. For example, the German reviewing journal, *Zentralblatt für Mathematik*, began to deteriorate in the midthirties because Nazi racist policies were being forced on the journal, so that, for instance, the reviewing staff should be "racially pure." This trend led to the decision here to establish a reviewing journal in this country, although there was concern whether American mathematicians had adequate financial support and general mathematical strength to launch such a journal properly. Enter Otto

Neugebauer, who came to Brown University in 1939, having played a key role in the founding of *Zentralblatt*. His knowledge and experience were of crucial importance in getting *Mathematical Reviews* started off on the right foot.

Standards in Teaching and Research

The emigrés had, for good reason, a high opinion of the educational system in Europe, and were somewhat skeptical, to put it mildly, of that in America. They recognized the quality of the best institutions in the United States, and consequently wanted to be affiliated with those schools, and were somewhat scornful, mostly covertly scornful, of the others. Garrett Birkhoff [**4**, p. 51] has pointed out that "Even in the 1930s, mathematics concentrators (majors) in many small colleges only got to calculus in their senior year!" You don't have to come from Europe to lift an eyebrow about this, as Birkhoff's exclamation mark shows.

One of the central differences between the United States and continental Europe was the contrasting emphasis on teaching and research. Whereas the Europeans thought of a university as a research institution primarily, professors in many American universities were supposed to be "teachers first and publishers of sophisticated research second," as Garrett Birkhoff put it [**4**, p. 73]. Department heads in the United States were often heard to say "this is a teaching university," making the emphasis abundantly clear. The distinguished American four-year colleges such as Swarthmore, Oberlin, Reed, Amherst, Williams, and the many others had no direct counterparts in continental Europe. Nathan Reingold [**20**, p. 318] writes that very few of the emigrés recognized, as one of them wrote later, that "It takes a long time for anyone not born or brought up in this country to realize... that... the primary aim of a college... is to educate members of a democratic society, that it includes among its functions the training of mind and character, of social attitude and political behavior." Mindful of the awesome power of the masses in an open one person–one vote society, the United States deliberately arranged for a much higher percentage of college age young people to attend college or university than in any other country.

On the other hand, the European emigrés quite obviously had a powerful educational background in mathematics. "Compared to the United States, there seems to be a difference of two or three years in specialized education, due perhaps to a more intensive schooling system during the Gymnasium (high school) and college years." This quotation is from a paper on the life of John von Neumann written jointly by S. Ulam, H. W. Kuhn, A. W. Tucker, and Claude Shannon [**27**], four leading mathematicians of our times. The Europeans not only had this two or three year advantage as students, but also it appears that those Europeans who worked their way up into university positions had more time for research than their counterparts in America. The Canadian mathematician J. C. Fields, famous for conceiving of and giving financial support to the Fields medals, estimated in 1919 that "the average U.S. or Canadian college professor taught 400 hours a year, as compared with 100 in the more advanced European countries" [**4**, p. 51].

The emigrés had a positive influence here. We cannot be certain what caused the reductions in teaching loads in mathematics, resulting in at most a two-course load in all Ph.D. degree granding universities now, where previously only faculty members at a few schools had enjoyed this privilege. It should be a right, not a privilege, for while a professor can teach a three-course load *and* continue a program of research and publication, *and* work with Ph.D. students, there is a price to pay somewhere.

The faculty member, if not a genius, has to put in at least a 50-hour work week, and has to cut service work to the school to a minimum. Service on any of those time-consuming committees, such as monitoring the progress of students on probation, or developing a new approach to the group requirements or other conditions for the baccalaureate degree, is out of the question unless the professor is willing to be seriously overburdened.

But if one way or another the professor does manage to evade these heavy-duty service assignments, there may well be resentment among other faculty members who feel that a disproportionate share of the service load is thrown on them. Since, except for the few years following my Ph.D., I was always being pressed to serve on committees, the advent of the two-course load was a godsend to me, even though it did not arrive at my university until 18 years after my Ph.D.

In retrospect, since mathematical research can be viewed as an international competition, the teaching loads assigned to young American research mathematicians in the 1930s, especially in the state universities, were too large to enable them to compete with foreign scholars not so encumbered. One suspects that the arrival of the emigrés heightened the perception of the need for reduced course loads.

Most important of all, the emigrés added beyond measure to the mathematical research and scholarship in this country. The United States already had a group of active mathematicians, many of world class. Peter Lax [**15**, p. 129, 130], after giving a list of American mathematicians who were active during the twenties and thirties, comments that, "The quality of the list is extremely impressive, the quantity a little small for a country the size of the United States."

Thus mathematics in this country was energized by an infusion of talent from Europe, adding to the considerable strength already here. This led to world preeminence. The early years of the climb to preeminence, 1930–1942, were unfortunately a period of financial hardship and uncertainty for many mathematicians.

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