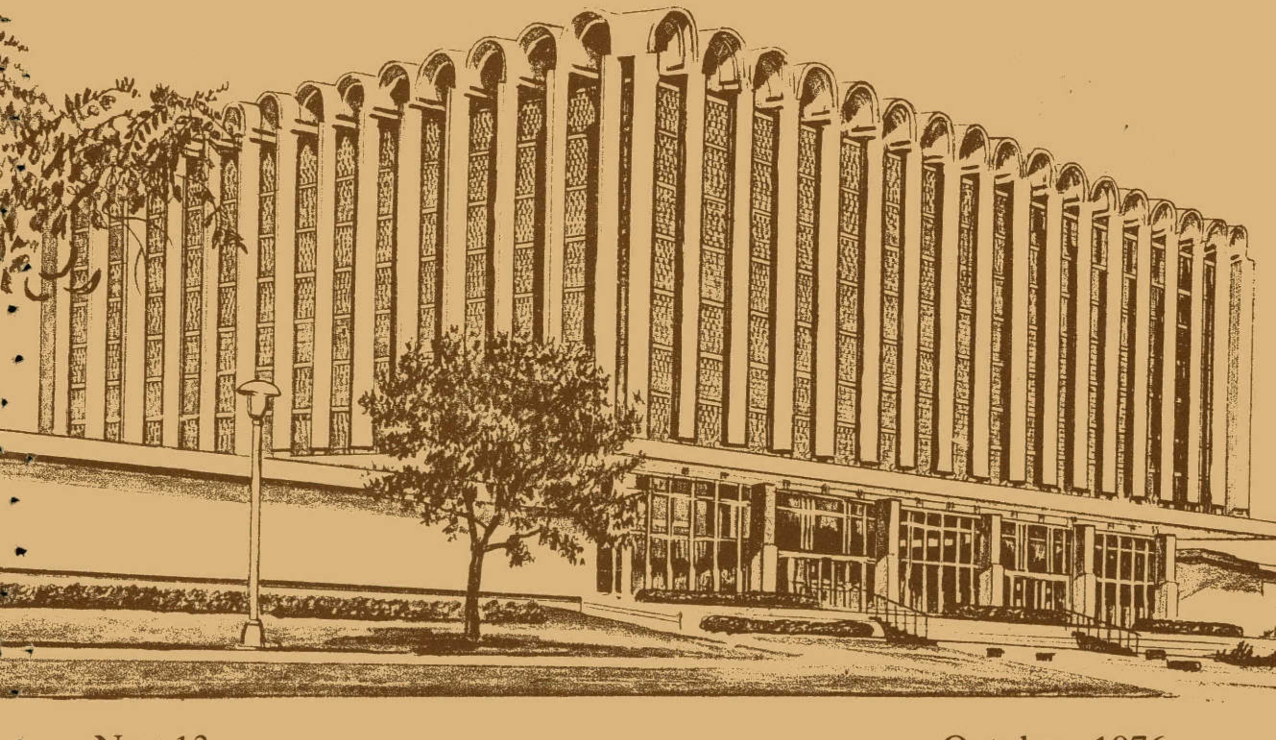
GRADUATE STUDIES TEXAS TECH UNIVERSITY

Men and Institutions in American Mathematics

Edited by
J. Dalton Tarwater, John T. White, and John D. Miller



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Glenn E. Barnett, Executive Vice President

Graduate Studies No. 13
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PREFACE

This collection of papers represents the proceedings of the conference on the History of American Mathematics held at Texas Tech University on 28, 29, and 30 May 1973. This conference was the first in a series of six conferences to be held at various universities in the State of Texas under the general title: American Mathematical Heritage. The series has received partial support from the Texas College and University Bicentennial Program.

The second conference was the American Mathematical Heritage Symposium on the History of Statistics and Probability held at Southern Methodist University, 27, 28, and 29 May 1974; and the proceedings of this conference will be forthcoming. The third conference in Algebra was held at The University of Texas at El Paso in 1975. Other conferences in Topology, Complex Analysis and Applied Mathematics are planned for The University of Texas at Austin, Rice University, and The University of Texas at Arlington, respectively.

The three main articles presented at this conference were by Marshall Stone, Salomon Bochner, and Garrett Birkhoff. Illness prevented Professor Bochner from attending; his papers were read by Professor R. O. Wells, Jr.

The theme of the conference was appropriately the title of Professor Marshall Stone's paper, "Men and Institutions in American Mathematics," and for this reason, it appears as the title article.

With the exception of papers by Kenneth May and Robert Thrall, these proceedings contain the papers presented at the conference. Some are faithful copies, others have been shortened to achieve spatial and conceptual balance. Robert Thrall's paper has appeared elsewhere, and Kenneth May's paper is, unfortunately, unobtainable. Bochner's paper appeared in the *American Mathematical Monthly* in a more complete version.

This conference was funded by Texas Tech University, and the proceedings were audiotaped and are reposited in the Texas Tech University Southwest Collection.

We gratefully acknowledge permission to use in the essay *Mathematical Americana* material from the article "Continuity and Discontinuity in Nature and Knowledge," Dictionary of the History of Ideas, copyright 1973 by Charles Scribner's Sons.

J. Dalton Tarwater
John T. White
John D. Miller

MEN AND INSTITUTIONS IN AMERICAN MATHEMATICS

MARSHALL H. STONE

History serves several purposes. First of all, it aims to preserve a record, necessarily incomplete and hence selective, of significant events of the past. It serves to create and embody traditions that play an important part in our social thinking. More important than this, however, is the hope that from well-written history one can draw some lessons for the future, finding in the record of past behavior indications of how people are likely to behave in the future or guides as to what their behavior should be in complex situations. In our concern for the future of mathematics, the chief problem is to create circumstances propitious for the study and discovery of mathematical truths. This seems to be a compelling reason for reviewing the history of American mathematics on this and other occasions.

I am not a historian and am in no position to offer a paper that meets the criteria of adequacy and precision properly applied to historical writing. What I can do and what I propose to do is merely to supply future historians with evidence drawn from a rich experience dating from the autumn of 1919, the year when I first came in contact with some of the leading figures in American mathematics. It has been my good fortune to know a great many of those who conceived and carried out the ambitious plans that have resulted in putting America in the forefront of world mathematics. It has been my privilege to work closely with many of them in the development of mathematics at several leading universities and in some of the most effective mathematical and scientific organizations. In choosing the title "Men and Institutions in American Mathematics" for a paper based largely on personal recollections, I seek to suggest a theme that will give some unity to these reminiscences. Indeed, I firmly believe that the principal problem for mathematical historians is to examine the men who were prominent or influential at different stages and to investigate the institutions in and through which they worked. Men create institutions with certain purposes in view but are in turn confined by the very institutions they create. It is this interaction between men and institutions that is one of the major concerns of history.

The chief and most stable institution in the world of mathematics has been for at least a hundred years the department of mathematics in a college or university. For teaching or creating mathematics, it is the department that provides the milieu in which a mathematician does his daily work and finds many of his most intimate associations. This was not always the case, inasmuch as in earlier times many famous mathematicians were supported by private sponsors or by learned academies in posts designed for consultation and research rather than for teaching. The careers of Euler and Lagrange furnish well-known examples. However, when science had won a place in the university curriculum, increasing the demand for courses in mathematics, the department of mathematics began to take the form we know and eventually assumed responsibility not only for teaching but also for research. Historically, departments of mathematics have provided nuclei around which have grown other institutions, affording opportunities for intellectual activity of a less limited or local character. In the United States the first organization in which mathematics and mathematicians had a prominent place was the National Academy of Sciences. The Academy was founded by the Congress at the time of the Civil War not only for immediate practical purposes but also in response to the desire of American scientists for a national organization comparable to the Royal Society or the prestigious European academies. Among the leaders in the Academy were such mathematical scientists as Benjamin Peirce (Professor of Mathematics at Harvard and a prominent algebraist), Simon Newcomb (grandfather of the distinguished contemporary Hassler Whitney), the astronomer George Washington Hill, and others.

In those earlier times, we must take note of the formation of the first American mathematical journals. They were associated with the departments of mathematics in two universities, Johns Hopkins and the University of Virginia. The Johns Hopkins University was founded with a commitment to research as well as to teaching. The University's principal mathematician, Sylvester, was brought to Baltimore from England. He promptly started the *American Journal of Mathematics,* which has had a distinguished and uninterrupted record of publication ever since. The *Journal* has at times received support from outside sources but has been published by the University throughout its existence. The *Annals of Mathematics,* founded somewhat later by Ormond Stone, was published first by the University of Virginia, but later was taken over by Princeton University, where it has remained to this day. These two distinguished journals, both with international reputations, have been followed by many others, with many recent additions being devoted to such special branches of pure or applied mathematics as logic, algebra, topology, number theory, functional analysis, numerical analysis, probability, combinatorial theory, computer science, and so on.

In 1888 the founding of our first mathematical society marked the beginning of a new epoch in American mathematics. Starting as a local association of mathematicians, the New York Mathematical Society quickly became the nucleus of a national organization and was renamed the American Mathematical Society in 1894. The Society has been the leading body in American mathematics ever since. It has provided a national forum for the communication and publication of mathematical research and has been an effective instrument for promoting research and related professional activities.

The Society was created and led by a succession of distinguished American mathematicians, many of whom it was my privilege to know personally. Among these were Professor Thomas Fiske, one of the principal organizers of the New York Mathematical Society and later its president; and Professor F. N. Cole, who was a longtime secretary of the Society. When I was a young man, both were members of the faculty of Columbia University. Professor Fiske already was serving there when the New York Mathematical Society was formed, but Professor Cole came to the city from the University of Michigan somewhat later.

Professor Fiske was chairman of the mathematics department when I received and filled my first fulltime appointment as an instructor in mathematics. I do not recollect having met him before taking up my duties, although I may have when, as a boy, I visited the Columbia campus with my father (Harlan Fiske Stone was Dean of Columbia Law School 1910 to 1923). My appointment was discussed primarily with another mathematician, Professor Herbert Hawkes, then Dean of Columbia College. However, I recall very clearly visiting Columbia University when I was still a Harvard graduate student and calling on Professor Cole in his office in the quarters of the Society. The University had welcomed the Society on its campus, making available office space in a small, red-brick building that has since been torn down and is known now only through photographs. At a later time, the Society's headquarters were removed to Providence, Rhode Island, during the term of Dean R. G. D. Richardson of Brown University, who succeeded Professor Cole as Secretary. My conversation with Professor Cole was also the occasion for making the acquaintance of Miss Caroline Seely, his office secretary and assistant, who for a great many years served the Society as secretary with efficiency and widely appreciated dedication. The fact is that, in administering the affairs of the Society, the secretary and those who assist him are very influential and develop extensive contacts with members of the Society. Professor Cole and Miss Seely were no exceptions to this rule. My own relations with Secretary Cole's two successors, Dean Richardson and Professor J. R. Kline of the University of Pennsylvania, were particularly close inasmuch as I was active in Society affairs during their terms of office. Indeed, as President of the Society (1943-44), it was my privilege to work in very close cooperation with Secretary Kline. Whatever the other officers may contribute to the formation of policy, it is the faithful and devoted service of the secretary that guarantees the execution of policy in all detail. American mathematics, indebted to the Society for so much of its collective achievements, thus owes the deepest gratitude to the lengthening line of its distinguished secretaries.

One of the most significant early decisions in the Society's history resulted in the foundation of its *Transactions*. It is said that when the American Mathematical Society first began considering the publication of a research periodical, there was strong opposition from members interested in the *American Journal of Mathematics*. Apparently there was a definite and widespread feeling that the *Journal* functioned primarily as a "house organ" of the Johns Hopkins University and therefore had to be considered inadequate for America's growing need of research publication. The discussion became more heated, reaching a climax in a meeting at which the final vote was to be taken. At this point Professor Maxime Bôcher of Harvard University, exercising his talent for diplomacy in a characteristic way, was inspired to ask the opponents whether they would object if the Society were to publish the transactions of its meetings. Put in this light, the problem quickly received a favorable solution in the calmer atmosphere Bôcher thus had created. For this and other details of the Society's history, it is easy to refer to the first of the two *Semicentennial Volumes* (1938) prepared under the editorship of Professor R. C. Archibald of Brown University, an excellent historian of mathematics who was devoted to the cause of the Society. For dates and other details, this volume is a handy source. It is thus unnecessary for me to dwell on the immensely important role the Society has played in the development of American mathematics.

The Society, having been created largely with the aim of promoting research, was not organized to become active in other areas of professional interest, such as the teaching of mathematics. It was therefore quite natural for other organizations to spring up from time to time as need for them became apparent. Thus, the Mathematical Association of America was founded in 1915 by a group interested in the problems of teaching. The Association has developed strong activities in mathematical education, with a focus on collegiate problems. It has undertaken a worthwhile program of publication with the *American Mathematical Monthly* as its journal and the *Carus Monographs* as a distinguished series of books. Recently the *Monthly* has increased its level of sophistication and has been publishing more ambitious expository articles devoted to modern topics such as sheaf theory and the foundations of differential geometry. The two societies have cooperated closely. There is, in fact, an extensive overlap of membership, making joint publication of a directory a practical undertaking.

Still other groups formed as various specialized interests grew stronger and sought independence. The American Statistical Association and the Association for Symbolic Logic were among the earlier such organizations. The Society for Industrial and Applied Mathematics was founded somewhat later and was followed by others with ties to various applied fields. Many of these organizations maintain excellent relations with the American Mathematical Society, arranging joint meetings and symposia. Cooperation among these societies with overlapping mathematical interests is mediated also by the National Research Council, inasmuch as many of them are represented in the Division of Mathematical Sciences. It should be recalled that the Council was created during World War I when there was need to expand the activities for which the National Academy of Sciences had been chartered by Congress during the Civil War. The Division of Mathematical Sciences, however, was not split off from the Division of Physical Sciences until after World War II. The Council and the National Academy thus have afforded the various mathematical societies and associations an important opportunity to work together in activities arising out of governmental and public affairs.

During World War II, mathematicians were confronted with professional problems created by selective service, manpower controls, and military needs for scientific and technological innovations. None of the existing mathematical organizations could function effectively by itself in seeking solutions to these problems. The American Mathematical Society took the lead in forming a representative War Policy Committee to handle these war-related problems. Whatever success the Committee may have had in promoting the effective use of mathematicians in wartime service, there was some sentiment for converting it into a policy group that would function in peacetime to coordinate the efforts of the various mathematical organizations in their common cause. The example of the American Institute of Physics suggested that the mathematicians needed a similar unifying organization, particularly because there were clear prospects that the government would be playing a more important and influential part in scientific affairs in the coming decades. Whereas there was insufficient backing for any move to create an analogous institute for mathematics, the less ambitious decision to modify and continue the Policy Committee was made. As the years went by, this arrangement was found to be relatively ineffective and was given up; eventually it was replaced by the stronger and more formal Conference Board of Mathematical Sciences. The Board is certainly needed if mathematicians are to handle satisfactorily the increasing number of problems created by policies of the federal government, but whether it will receive the necessary backing and support to become a permanent institution remains to be seen.

One of the important lessons of the wartime experience of 1942 to 1945 was the realization of the increased dependence of the government and its military agencies upon scientific and technological advice. These agencies were strongly desirous of maintaining the contacts with science that had developed during World War II. When the National Defense Research Council was disbanded, the Army, Navy, and Air Force assumed some of its functions and developed their own research and development agencies. These agencies sought to keep their ties with science and mathematics by negotiating contracts for the performance of various kinds of scientific work, including fundamental research in areas far removed from the immediate interests of the military. At the same time, there was a strong desire on the part of many leading scientists to create a civilian agency charged with the promotion of science. In response to their presentation of the arguments for government support, Congress created the National Science Foundation. As a result, very substantial support began to flow from government through both civilian and military channels to universities, research institutes, and scientific societies. Mathematics, by sharing with the sciences in this support, was accelerated significantly in its development over the past 25 years. Recent events suggest that mathematicians have come to depend a little too much on federal funding of fellowships and research activities. A careful analysis of the effects of this support of mathematics by the National Science Foundation and the Department of Defense would be worth undertaking. Until someone can examine all the details, any discussion will be tentative and, to some extent, speculative. I shall therefore leave this very significant aspect of recent American mathematical history without further comment.

Let me now turn to a more personal part of my narrative. Since 1919 I have had long personal associations with two of our leading universities, and shorter or less important associations with several others. Thus I can tell something of the growth and influence of the departments of mathematics at Harvard and Chicago. I shall start, however, by saying something about Princeton, because these two institutions cannot be mentioned without also mentioning Princeton — both the University and the Institute for Advanced Study.

For a long time Cambridge, Chicago, and Princeton were the three principal centers for mathematics in the United States. Without losing their positions of eminence, they have been joined since by Berkeley; many other institutions have brought their mathematics departments to such a degree of excellence that they are rivals if not peers of the four leaders. The early primacy of Harvard, Chicago, and Princeton and the extent of their influence upon the development of American mathematics can be gauged by the doctorates they have granted and observing the role their former graduate students have played in creating other centers and helping to bring mathematics to full sophistication in this country. For illustration it is quite sufficient to mention two names. R. L. Moore received his doctorate from the University of Chicago and went on to make Austin a center of mathematical learning and research. The second name, which also has its Texas associations but is mentioned here in a quite different connection, is that of Griffith Evans, who was a Harvard doctor in mathematics. Professor Evans was undoubtedly the prime mover in raising Berkeley to its present position among American mathematics departments. It is easy to multiply such examples in order to illustrate the tremendous seminal role played by our three pioneer departments.

I cannot write at length about Princeton because my contacts there were quite limited, particularly when compared with those I enjoyed at Harvard and eventually at Chicago. The salient fact about mathematics at Princeton was the brilliant leadership of Professor Oswald Veblen, who came there with a doctorate recently earned from the University of Chicago. Ably supported by Henry Burchard Fine and Luther Pfahler Eisenhart, both influential in the administration of the University, Veblen succeeded not only in making Princeton a great department but also in helping create the Institute for Advanced Study. To these tasks he brought an insight into the significant trends in mathematical research and a remarkable astuteness in attracting first-class mathematicians and guiding them along the path of cooperation. Among the distinguished mathematicians he gathered together at Princeton were J. W. Alexander and Solomon Lefschetz, two of the creators of modern topology. In retrospect, it is amazing to see what topology was more than 60 years ago when Veblen and his colleagues started to form the Princeton "school." What a contrast with what topology has since become. I recall vividly the state of topology at Harvard when I was a graduate student there. There had been no course offered in the subject and hardly more than passing reference made to it in courses on other subjects until 1923-24. In that year, Philip Franklin came to Harvard as an instructor after receiving his Ph.D. at Princeton. I remember that his dissertation was only 11 pages long. He gave us our first systematic introduction to topology, lecturing for just one semester on elements of such modest appearance that one hardly could have believed them destined to flower into what is known today as topology. (Henri Lebesgue's lectures a year later at the Collège de France, the first he gave on topology, I believe, were equally rudimentary).

Although Princeton's pioneering in topology was perhaps its greatest mark of distinction, analysis and algebra were well represented. It suffices to mention the names of George Birkhoff, John von Neumann, Einar Hille, Salomon Bochner, and James McShane, all of whom lectured on analytical subjects at Princeton for various periods of time, and Wedderburn and Artin, who carried forward the work in algebra with similar distinction. I believe that of the analysts, Professor Bochner spent the longest time at Princeton, inasmuch as he remained there from the early thirties until his retirement and the formation of a new association with the Rice Institute. Professor Wedderburn served at Princeton from the time he came to America from Great Britain until the end of his academic career. Probability and statistics were well represented at Princeton in the persons of Feller, Wilks, and Tukey; logic was taught by Alonzo Church. Veblen himself, despite his early interest in combinatorial topology, remained devoted to geometry in a broader sense. Seconded by Eisenhart, he gave a strong impetus to teaching and research in differential geometry at Princeton.

In the early thirties, the mathematics department at Princeton suffered an upheaval that was more violent than it may have seemed at the moment, and yet was far less destructive than it might have been. I refer, of course, to the effects produced by the creation of the Institute for Advanced Study. Professor Veblen himself had had no small influence in creating the Institute. It was he who detached from Princeton a distinguished segment of its mathematics department as a nucleus for the Institute's first faculty. Thus Princeton lost Veblen, Alexander, and von Neumann to the Institute where, with Einstein and Herman Weyl, they inaugurated the School of Mathematics. Marston Morse soon joined the School of Mathematics in a move from Harvard to Princeton. These and later appointments gave the Institute a predominantly mathematical character that has persisted to the present despite a growing number of appointments in other fields, especially in theoretical physics during Robert Oppenheimer's incumbency as director. It is said that the first director, Dr. Abraham Flexner, decided upon mathematics for the initial emphasis because he found essential agreement among mathematicians as to the achievements and relative standing of their colleagues, something that was more difficult to find in other fields.

In its earliest days, the Institute had no buildings but was housed in Fine Hall. This had the effect of concealing from any but the most perceptive observer the cleavage in the Princeton department and making it appear that, on the contrary, Princeton had gained mathematical strength by the institute's additional appointments and its own replacement moves. Eventually, however, physical separation between the Princeton department and the School of Mathematics as well as the fundamental difference between their respective goals left no ambiguity as to their mutual relations. Legally independent of each other, they became functionally independent as well. In combination, the two institutions today form one of the world's most powerful mathematical centers. Had Princeton University not made a determined effort to maintain its mathematics department at the same high level it had reached before losing three of its most distinguished mathematicians to the Institute, the picture might have been quite different. By a series of judicious replacements and distinguished junior appointments, Princeton succeeded in strengthening and expanding its department and maintaining its position of leadership. The Institute for Advanced Study likewise has strengthened its School of Mathematics, adhering strictly to the original intent of the founders that it should be an international center for research and individual study. Because the school cannot be discussed adequately without emphasizing its international role, I shall leave further remarks concerning it for the following paper.

In the autumn of 1919, I entered Harvard College as a freshman. The members of the mathematics department at that time were Professors Osgood, Coolidge, Bouton, Huntington, Birkhoff, Graustein, Walsh, and Beatley (who had a joint appointment with the School of Education). During the terrible epidemic of Asiatic influenza a year or so earlier, Harvard lost both Professor Maxime Bôcher and Professor Gabriel Marcus Green, a young and very promising mathematician interested in projective differential geometry. Professor Charles Bouton, who was my teacher in analytic geometry (a year's course at that time), was a specialist in the Lie theory of differential equations. I always believed that he had studied with Sophus Lie but never have tried to confirm this belief.

Professor Bouton had an important influence on my career. In the spring he advised me to enter the junior course in advanced calculus if I could somehow pass an examination in sophomore calculus. Because I had read some calculus during the midyear examination period when I probably should have been preparing more thoroughly for my examinations, I arranged to take the finals in calculus about to be set by a young instructor, Dr. Marston Morse. When I passed, I gave up a major in chemistry and concentrated on mathematics instead. Morse left Cambridge for a few years, but returned as a member of the faculty later on and remained until his appointment to the Institute of Advanced Study in the thirties.

Professor Oliver Kellogg, who joined the department at that time, was my teacher in mechanics. His specialties were potential theory and integral equations. In collaboration with G. D. Birkhoff, he later published the first explicit fixed point theorem in functional analysis. Professor George Birkhoff was my principal teacher and the director of my thesis for the Ph.D. He was certainly the leading American mathematician of that time, enjoying a firmly established international reputation. He had left Princeton in 1912, despite his personal friendship with Oswald Veblen, accepting an assistant professorship at Harvard when he was already a professor at Princeton. This decision doubtless reflected his views as to Harvard's preeminence and the opportunities he eventually would enjoy there as *primus inter pares*. Perhaps he was drawn back also by his memories of undergraduate days spent at Harvard. It is certain that the Harvard Mathematics Department had a record to be proud of and had set out under the leadership of Osgood, Coolidge, and Bôcher to reach greater heights than it had attained previously in the times of Benjamin Peirce and William Elwood Byerly (Harvard's first doctor in mathematics and a long time member of the department).

The development of American mathematics between 1890 and 1925 or 1930 was influenced strongly by the experience of young Americans who had pursued their postgraduate studies in European universities. There they found a concern for research that was rare in American institutions of those days. Many of them returned to America resolved that advanced study and research should become a departmental and university responsibility rather than one left to individual inclination and choice. In order to continue their own research in a propitious atmosphere, they projected changes in their own departments and joined together in launching the American Mathematical Society. Osgood, Coolidge, Bouton, and Bôcher all had shared this experience. Osgood took his doctorate at Göttingen, where he knew and greatly admired Felix Klein. Julian Coolidge studied both in England and on the Continent, where he acquired a lasting taste for the geometry of the Italian school.

The vision of such young mathematicians must have been known at an earlier, less favorable time to men like Benjamin Peirce, yet American departments of mathematics had remained service departments. From the earliest colonial days, America had been in touch with mathematics in England and also on the Continent, and had kept abreast of the major advances there. Calculus, for instance, was taught here very shortly after it was discovered. However, the thought that research should be an integral part of a department's work and not left to a member's individual choice as something to be done only after he had performed his teaching and other duties was still a novel thought in the nineties. The young men coming back from Europe put their minds and their hearts into the effort to plant this thought and to bring it to fruition. As early as the first decade of the twentieth century the University of Chicago already was giving research a new place and opening up new paths in American higher education. When Oswald Veblen, R. L. Moore, and G. D. Birkhoff took their degrees at Chicago, they were forerunners of the coming generations who would study for the doctorate in America rather than abroad.

Long before I started my own studies at Harvard, the transformation envisaged by Osgood, Coolidge, Bôcher, and their colleagues was far advanced. To be sure, mathematics was expanded greatly and the conditions for research were made still more favorable during the thirties, but the Harvard of my student days could not have offered more opportunity or encouragement to a youth eager for study and learning. At the beginning of my junior year, I even had the temerity to call on Professor Birkhoff at his home and ask him to suggest a problem for me to investigate in an area that had already attracted my interest. Eventually he directed my dissertation.

In the early twenties the mathematics courses offered at Harvard comprised a basic core supplemented by a variety of advanced courses and seminars reflecting the research interests of members of the faculty. The freshman courses were three half-year courses in solid geometry, trigonometry, and algebra, and a full year course in analytic geometry. There were two courses in calculus — one open to sophomores and a second, more advanced one usually taken by juniors. There were intermediate courses in mechanics, projective geometry, and probability. The course in probability and statistics was not given regularly, but was offered occasionally by either Professor Coolidge or Professor Huntington, both of whom had a strong interest in it. After I joined the department in 1927 as an instructor, I taught this course once myself. In retrospect, it may seem very strange that there was such a meager offering in statistics at a great university like Harvard. It is true, nevertheless, that there was no organized instruction on an adequate scale in probability and statistics at Harvard until a separate Department of Statistics was created after World War II.

The more advanced courses began with Mathematics 12 and 13, which were devoted to functions of real and complex variables. Both courses usually were taken by first year graduate students and a number of senior honor students. The more advanced courses included differential equations, dynamics, potential theory, differential geometry, and algebraic geometry. It is remarkable that there was no course in which Lebesgue integration appeared as a topic. Professor Osgood dwelt at great lengths on uniform convergence in Mathematics 12 in a language and spirit that brought confusion to many a student; but he never mentioned the Lebesgue integral. Neither did Professor Birkhoff, although he certainly was familiar with the concept and eventually applied it in a sophisticated way in proving his celebrated ergodic theorem. Indeed although de la Vallée Poisson had lectured on the Lebesgue integral at Harvard in 1917 or 1918, I had to wait to learn about it until 1923 or 1924 when I was enrolled in an analysis seminar conducted by Professor J. L. Walsh. Incidentally, it was in this seminar that I suddenly saw how an idea of Alfred Haar could be developed into my doctoral thesis. After introducing Marcel Riesz's technique for the summation of infinite series a few months later, I was able to complete the thesis with very little guidance from my director, Professor Birkhoff. (I can recall consulting him on only two occasions when I needed advice or encouragement.)

Another branch of mathematics that was represented poorly in the Harvard curriculum in those days was algebra and number theory. There was no advanced course offered, although I recall that the catalogue of courses listed one on Galois theory given by Professor Osgood. However, he did not lecture on the subject while I was a student.

To sum up, the curriculum as I knew it at Harvard in 1919 to 1924 was strong in classical analysis and geometry along with some of their applications. It had not felt yet the pressure for a more advanced and sophisticated approach to mathematics that was already building up in Europe; but it was soon to do so.

With the addition of younger mathematicians during the twenties and thirties, the department gradually shifted its interest in directions consistent with current trends. Heinrich Brinkmann, a fellow graduate student who had come from Stanford, spent a postdoctoral year at Göttingen attending Emmy Noether's lectures in 1923-24, and returned to Harvard the following year as an instructor. He was an accomplished algebraist and differential geometer with an unusual gift for exposition. While he was at Harvard, he gave advanced courses in his specialties, but remained in Cambridge for only a few years before going to Swarthmore.

After a postdoctoral year in Paris and two years as an instructor at Columbia, I returned to Harvard as a member of the faculty in 1927 and taught there until 1946, except for a brief two-year interlude at Yale in 1931 to 1932, a Guggenheim Fellowship in 1936-37, and wartime leaves in 1943 to 1945. During those years, Professor Osgood retired, Professor Kellogg died, Professor Morse was called to the Institute for Advanced Study, and Professor Graustein was killed in an automobile accident. The department was kept at strength by a series of new appointments that brought D. V. Widder, Hassler Whitney, Saunders Mac Lane, Garrett Birkhoff, Lars Ahlfors, Lynn Loomis, and George Mackey to Harvard as faculty members. Professor Ahlfors' service in the department covered two periods separated by several years as a professor in Helsinki; Professors Whitney and Mac Lane eventually resigned, as I did also, to take up positions elsewhere. Professor Widder retired recently, but the others are still at Harvard.

An important event in the history of the department was the appointment, made jointly with the physics department, of a professor of mathematical physics. This move reflected the foundation-shaking discoveries in atomic physics and the relativistic theory of space, time, and gravitation, all of which placed a higher premium than ever on the skillful use of mathematical concepts and techniques. The new appointment was proposed and considered at a time when preoccupation with the new quantum physics had reached a peak. The earlier interest in relativity had engaged the attention of Professor G. D. Birkhoff, who offered a course in that subject, which I attended as a student. The mathematical basis in differential geometry was sufficiently familiar so that no urgent need was felt in either the mathematics or the physics department for a special appointment. Quantum theory was different because of the wide variety of mathematical tools employed, some of them unfamiliar to many physicists.

I recall vividly the first days when group theory and wave mechanics came into the physicist's comprehension of the atom. Group theory had been essential for crystallographers, but other physicists had very little knowledge of groups and were surprised and perhaps a little alarmed to find the need for groups in calculating atomic spectra and formulating the general principles of the new physics. They turned to the mathematicians for information and advice. Although the need for similar help in dealing with wave mechanics was less urgent, the emerging importance of the Hilbert space formulation led physicists to look at familiar mathematics in a new light that was strange to them. Here, too, they needed consultation with mathematicians. Under these conditions, Heinrich Brinkmann and I felt called upon to offer courses in group theory and orthogonal series, respectively, that might be helpful to the physics department. It soon became evident, however, that a theoretical physicist familiar with the new developments should be brought to the Harvard faculty. I do not recall in detail the discussions in the mathematics department of the proposed joint appointment or the choice of an appointee. The choice fell first upon Robert Oppenheimer. When he could not be persuaded to leave California, Harvard invited J. H. Van Vleck, who accepted and continued his distinguished career without interruption until his retirement. His later associations were with the Department of Engineering Sciences and Applied Physics, which was created after World War II. He played a leading part in its development as member and chairman.

The changes that took place in mathematics during the twenties and thirties likewise found expression in events within the department. Many of these changes reflected the progressive penetration of mathematics by algebraic concepts that can be traced back to Dickson, Speiser, and Emmy Noether and, of course, beyond them to their precursors, among whom Benjamin Peirce has an important place. Other changes corresponded to the spreading exploration of the axiomatic method, of which Professor E. V. Huntington was a prominent proponent. The younger mathematicians who had enjoyed early contacts with these trends and realized to some degree their significance for the future of mathematics began to treat them in their lectures and books. The brilliant circle that grew up around Emmy Noether in Göttingen found a remarkable expositor in B. L. van der Waerden. His *Moderne Algebra* provided the means for establishing the new algebra as a firmly placed part of the advanced curriculum. Young Frenchmen who went to Göttingen for higher studies returned to France dissatisfied with the classical traditions of their own country. They soon rebelled against them under the leadership of André Weil and with some encouragement from Élie Cartan. Banded together under the pseudonym of Nicholas Bourbaki, they formulated and put in motion an ambitious program for elaborating a completely revised exposition of mathematics in the modern spirit. This immense, painstaking labor has gone forward more or less steadily ever since, and continues unabated. The enterprise was menaced by the Second World War (during which, as a precautionary measure, a complete duplicate set of Bourbaki's manuscripts was left with me for safekeeping), but survived the threat.

At Harvard in the early thirties there were four younger mathematicians who, without formal associations and without an explicit program of action, began to remold the core of the Harvard curriculum into a more modern shape. These were Hassler Whitney, Saunders Mac Lane, Garrett Birkhoff, and I. Whitney's major interest lay in topology, but this led him naturally into studies of linear dependence and other algebraic topics, where his original treatment has had a continuing influence. He was able to introduce his ideas into his courses, particularly those offered to graduate students. Both Mac Lane and Garrett Birkhoff were algebraists. Birkhoff then was engaged actively in pioneer investigations in universal algebra and lattice theory. He pressed for the introduction of a new undergraduate course in algebra and was given an opportunity, which he used with Saunders Mac Lane, to develop such a course. It was not very long before they published their celebrated text, *A Survey of Modern Algebra*, which has done for undergraduate instruction in America what van der Waerden's book was already beginning to do for graduate instruction in algebra. Later, of course, they published a much revised version of this influential book under a different title, *Algebra*.

While my colleagues were thus introducing more algebra and topology of a modern kind into Harvard's mathematical curriculum, I had a similar opportunity to modify the real variable course, Mathematics 12. I felt that the emphasis that Osgood had placed on uniform convergence should be greatly diminished and that new emphasis should be introduced on point-set topology and Lebesgue integration. As a basis for such a modern treatment of analysis, I chose to start with the algebra and set-theory needed in the theory of cardinal, ordinal, and real numbers, and to do this with an informal, axiomatic approach. Thus my course contained many introductory topics of an algebraic nature. In this, I was encouraged by frequently hearing Norbert Wiener proclaim his thesis that harmonic analysis was, in essence, the theory of the abelian group of translations of the real line, and by familiarity with the work being done currently on ordered, normed, topological, and formally real fields. I was unsuccessful in doing much with Lebesgue integration or in treating the subject in a way I considered satisfying, partly because of lack of time. As a starting point, ideas developed by Daniell and Norbert Wiener appealed to me, but they were not brought into acceptable form until 1948, when I was able to summarize my treatment in a series of four notes published in the Proceedings of the National Academy of Sciences. The parallel between this theory and the one adopted by Bourbaki is all the more remarkable because they were developed completely independently, except for some considerations presented in the fourth note in response to a query put to me by André Weil. With the passage of time, the innovations described above appear very modest indeed. However, they were seminal despite the fact that they lay hidden under the subsequent innovations incorporated in the curriculum of today.

While recalling innovations with which the Harvard Mathematics Department had some connection, I should not fail to mention a small part it played in the history of the electronic computer. During the thirties, Cambridge was the center of a lively development in the computing art. At M.I.T. Professor Vannevar Bush was engaged in a program for developing a differential analyzer capable of solving ordinary differential equations of considerable complexity. The success of this program inspired curiosity and interest in the local scientific community.

I was motivated to include in a course on harmonic analysis a discussion of integraphs, harmonic analyzers, and other analogue computers. Indeed, in working on multiplicative systems needed for studying the axioms for Boolean algebra, I noted that it might be possible to carry out the time-consuming calculations more quickly and with less risk of error if an electrical device easily adapted to changes in the multiplication tables could be built. Thus I was receptive already to Professor Howard Aiken's mature proposal for an all-purpose digital computer when it was announced a little later. When he sought assistance from a fund left to the University by Professor Arthur Kennelly, famous as a discoverer of the Kennelly-Heaviside layer, his application needed the approval of the mathematics department as provided in the terms of the gift. The department voted unanimously to approve the grant, and, thus, lent its support to the development of the first modern digital computer. Before the new computer could be built and put into operation, however, Professor Aiken was called for duty in World War II as a naval reservist and was diverted to other activities until the U.S. Navy realized the importance of his invention. Meanwhile, the University of Pennsylvania began developing a computer based on electronic switching instead of the mechanical switching that Aiken had in mind.

Professor Aiken pursued his original concept for many years and built several versions of his machine even though the electronic computer promised to be faster and more powerful.

In bringing my Harvard reminiscences to a close, I would like to describe the manner in which the mathematics department conducted its affairs. It was a small department. Both the undergraduate body and the graduate school were small by present-day standards; the number of mathematics students was correspondingly small as well. For a long time, the department had no office; its secretary worked only part time, on loan from Professor Coolidge who was entitled, as Master of Lowell House, to the services of a secretary. In 1945, there was some talk of giving the department permanent quarters in a new science building, but nothing happened. The department had to continue to use a small office that had been made available in the old Fogg Art Museum, but then acquired quarters in the former Geographical Institute. Only recently the department has found at last a suitable place in one of Harvard's newest buildings. In order to transact its business, the department usually met in the home of the chairman. Its procedures were thoroughly democratic. Every member was free to participate in the discussion, which usually resulted in a consensus. All votes were voice votes, with members expressing themselves in reversed order of seniority.

The department placed great emphasis on teaching. Every member was expected to teach an undergraduate course. The standard teaching load was nine hours per week, with a proportionate reduction made when Harvard introduced its tutorial system. When Professor G. D. Birkhoff was named Cabot Fellow (in response to offers made to him by the Institute for Advanced Study and Columbia University), his teaching load was reduced, but he was still expected to teach an undergraduate course. Members of the department often gave courses at Radcliffe College, then a separate institution, bringing their weekly load up to 12 hours. In my own case, this was a regular practice. Other junior members of the department did likewise, but the senior members did so much less frequently. There was always a strong emphasis on research in the department. Promotion was dependent in part upon a member's capacity in discovering mathematics as well as his ability as a teacher. I believe that the department succeeded in maintaining an excellent balance in this respect.

A factor in this success may have been that a continuing effort was made to keep in touch with student opinion and desires. With small classes and a less numerous student body than today's, this was easier then than it is now; but the department relied also on an annual report prepared by the Mathematics Club. The club was made up of both graduates and undergraduates who had a special interest in mathematics and enjoyed its program of papers read by local mathematicians and members. The students were encouraged to criticize teaching and course offerings. Each year the students suggested that Professor George Birkhoff be restricted to graduate teaching; each year the department ignored the suggestion, preferring to maintain its cardinal principal that every faculty member should teach an undergraduate class. As a matter of fact, Professor

Birkhoff enjoyed undergraduate teaching and was very successful with many of the students. He was very stimulating for the more gifted students. The inspiration he was able to give such students did not rest upon having complete, beautiful command of what he said in the classroom.

I have an anecdote and a comparison to illustrate this observation. In a course on functions of a complex variable of which I was a member, Professor Birkhoff started to demonstrate Cauchy's integral theorem. He failed, but promised to complete the proof at our next meeting. At that meeting, he failed again. This time he turned to a member of the class, not the brightest among us, and said, "Well, Mr. X, would you prepare a demonstration and present it to us at our next meeting?" No doubt Professor Birkhoff was engaged deeply in other matters, possibly in some research he had underway, and thought he could work out the proof in class with little or no preparation. We all fall into this trap occasionally, I suppose.

In Professor Birkhoff's case, it was precisely the opportunity of watching a first-class mathematician work out proofs and solutions that appealed to able students and led them to rate him as a fascinating and inspiring teacher. His classroom ways were in sharp contrast to those employed by Professor William C. Graustein. The latter always came into class perfectly prepared, wrote theorems and proofs on the blackboard in copperplate script, and at the end of the hour left his class with a beautiful set of notes of what he had communicated to them. Professor Graustein's lectures were very popular with students, especially the less gifted; but Professor Birkhoff's were more of a challenge to some of the others and were preferred by them. It was often necessary for Birkhoff's students to work harder if they wished to complete good sets of notes for his courses. This sort of joint effort always seemed to me an excellent experience for a good student.

This was borne in on me again at the University of Chicago. During the years when Professor André Weil lectured there, he always gave very fine, very well-organized lectures on an impressive variety of subjects. He was very sparing with the details of proofs. His students used to tape his lectures, then play them back for discussion and amplification until good notes could be worked out. This sort of collaboration, similar to that undertaken by some of Birkhoff's students, was an equally valuable experience for young Chicago mathematicians. As these examples show clearly and convincingly, different styles of teaching appeal to students and influence their learning in different ways.

At Harvard there was a deep concern for individual styles or methods of teaching to exist. This concern extended to the guidance of assistants who were beginning their careers as teachers. Each autumn Professor Osgood met with them to review some of the simple rules that needed to be kept in mind by the lecturer and teacher; throughout the year, established members of the department visited their classes and offered helpful comments. The department had many fine and popular teachers in its ranks and succeeded in performing its teaching functions in an effective and highly creditable manner.

In 1946, I moved to the University of Chicago. An important reason for this move was the opportunity to participate in the rehabilitation of the mathematics department that had once had a brilliant role in American mathematics but had suffered a decline accelerated by World War II. During the war, the activity of the department fell to a low level, and its ranks were depleted by retirements and resignations. The administration may have welcomed some of these changes because they removed persons who had opposed some of its policies. However, the University resolved at the close of the war to rebuild the department. The decision may have been influenced by plans to create new institutes of physics, metallurgy, and biology on foundations laid by the University's role in the Manhattan Project. President Hutchins had seized the opportunity of retaining many of the atomic scientists brought to Chicago by this project; he succeeded in making a series of brilliant appointments in physics, chemistry, and related fields. Clearly, something similar needed to be done when the university started filling the vacancies that had accumulated in the department of mathematics. Professors Dickson, Bliss, and Logsdon had retired recently, and Professors W. T. Reid and Sanger had resigned to take positions elsewhere. The five vacancies that had resulted thus offered a splendid challenge to anyone mindful of Chicago's great contribution in the past and desirous of ensuring its continuation.

When the University of Chicago was founded under the presidency of William Rainey Harper at the end of the nineteenth century, mathematics was encouraged and supported vigorously. Under the leadership of E. H. Moore, Bolza, and Maschke, it quickly became a brilliant center of mathematical study and research. Among its early students were such mathematicians as L. E. Dickson, Oswald Veblen, George Birkhoff, and R. L. Moore, who were destined to future positions of leadership in research and teaching. Some of these students remained at Chicago as members of the faculty. L. E. Dickson, G. A. Bliss, E. P. Lane, W. T. Reid, and Magnus Hestenes were among them. Algebra, functional analysis, calculus of variations, and projective differential geometry were fields in which Chicago obtained special distinction. With the passage of time, retirements and new appointments brought an increased emphasis on the calculus of variations, there was also a certain tendency toward inbreeding. When such outstanding mathematicians as E. H. Moore or Wilczynski, a brilliant pioneer in projective differential geometry, retired from the department, replacements of comparable ability were not found. Thus, in 1945 the situation was ripe for a revival.

A second, and perhaps even more important reason for the move to Chicago was my conviction that the time also was ripe for a fundamental revision of graduate and undergraduate mathematical education. I was eager for an opportunity to stimulate and institute such a revision, one that would bring to fruition the informal and somewhat tentative efforts undertaken at Harvard by Hassler Whitney, Saunders Mac Lane, Garrett Birkhoff, and me. The great work being done by Bourbaki, with which I had become familiar through my friendship with André Weil, was an inspiration to me in my thinking about the problems of education. As I mentioned previously, the accumulated manuscripts of the group had been given me in duplicate for safekeeping before the fall of France. I had thus enjoyed the opportunity of acquainting myself in some detail with the progress made by Bourbaki and was strengthened in my conviction that a modernized curriculum was essential for advancement of mathematical science. The invitation to Chicago confronted me with a very difficult question: "Could the elaboration of such a curriculum be carried out more successfully at Harvard or at Chicago?"

President Hutchins invited me to visit the University in the summer of 1945 to interview as a possible candidate for the deanship of the Division of Physical Sciences. After two or three days of conferences with department heads, I was called to Mr. Hutchins's residence, where he announced that he would offer me not the deanship but a distinguished service professorship in the Department of Mathematics. The negotiations over this offer occupied nearly a year. During this time, it became clear that Harvard was not ready for the kind of change to which I hoped to dedicate my energies in the decade following the war. However, it was by no means clear that circumstances would be any more propitious at Chicago than they seemed to be at Harvard.

In consulting some of my friends and colleagues, I was advised by the more astute among them to come to a clear understanding with the Chicago administration concerning its intentions. There are those who believe that I went to Chicago to execute plans that the administration already had in mind. Nothing could be farther from the truth. In fact, my negotiations were directed toward developing detailed plans for reviving the Chicago Department of Mathematics and obtaining some kind of commitment from the administration to implement them. Some of the best advice given to me confirmed my own instinct that I should not join the University of Chicago unless I were made chairman of the department and thus given some measure of authority over its development. Earlier experience had taught me that administrative promises of whole-hearted interest in academic improvements were too often untrustworthy. I therefore asked the University of Chicago to commit itself to the development program that was under discussion, at least to the extent of offering me the chairmanship. This created a problem for the University because the department had to be consulted about the matter and responded by voting unanimously that Professor Lane should be retained in the office. Because I was unwilling to move merely on the basis of a promise to appoint me to the chairmanship at some later time, the administration ultimately arranged the appointment and I accepted it. Mr. Lane, a very fine gentleman in every sense of the word, never showed any resentment. Neither of us ever referred to the matter, and he served as an active and very loyal member of the department until he retired several years later. I was very grateful to him for the grace and selflessness he displayed in circumstances that might have justified a quite different attitude. Even though the University made no specific detailed commitments to establish the program I had proposed during these year-long negotiations, I was ready to accept the chairmanship as an earnest of forthcoming support. I felt confident that with some show of firmness on my part, the program could be established. In this optimistic spirit, I decided to go to Chicago, despite the very generous terms on which Harvard wished to retain me.

Regardless of what many seem to believe, rebuilding the Chicago Department of Mathematics was an uphill fight. The University was not about to implement the plans I had proposed in our negotiations without resisting and raising objections to every point. The department's loyalty to Mr. Lane had the fortunate consequence for me that I felt released from any formal obligation to submit my recommendations to the department for approval. Although I consulted my colleagues on occasion, I became an autocrat in making my recommendations. I took a strong line in the beginning in order to make the department a truly great one.

The first recommendation sent to the administration was to offer an appointment to Hassler Whitney. The suggestion was rejected promptly by Mr. Hutchins's second in command. It took some time to persuade the administration to reverse this action and to make an offer to Professor Whitney. When the offer was made, he declined it and remained at Harvard for a short time before moving to the Institute for Advanced Study. The next offer I had in mind was one to André Weil. He was a somewhat controversial personality; I found a good deal of hesitation, if not reluctance, on the part of the administration to accept my recommendation. In fact, although the recommendation eventually received favorable treatment in principle, the administration made its offer with a substantial reduction in the salary that had been proposed. I was forced to advise Professor Weil, who was then in Brazil, that the offer was not acceptable. When he declined the offer, I was in a position to discuss the matter at the highest level. Mr. Hutchins was willing to renew the offer on the terms I had proposed originally. Professor Weil's acceptance of the improved offer was an important event in the history of the University of Chicago and in the history of American mathematics. My conversation with Mr. Hutchins brought me an unexpected bonus. At its conclusion he turned to me and asked "When shall we invite Mr. Mac Lane?" I was happy to be able to reply, "Mr. Hutchins, I have been discussing the possibility with Saunders and believe that he would give favorable consideration to a good offer whenever you are ready to make it." That offer was made soon afterwards and was accepted.

There were other appointments, such as that of Professor Zygmund, that also went smoothly, but what would happen in any particular case was always unpredictable. One explanation doubtless was to be found in the University's hand-to-mouth practices in budgeting. This would appear to have been the reason why I was given indirect assurances from Mr. Hutchins that S. S. Chern would be offered a professorship, only to be informed by Vice President Harrison the next morning that the offer would not be made. Such casual, not to say arbitrary, treatment of a crucial recommendation naturally evoked a strong protest. In the presence of the Dean of the Division of Physical Sciences, I told Mr. Harrison that if the appointment were not made, I would not be a candidate for reappointment as chairman when my three-year term expired. Some of my colleagues who were informed of the situation called on the Dean a few hours later to associate themselves with this protest. Happily, the protest was successful; the offer was made to Professor Chern, and he accepted it. This was the stormiest incident in a stormy period. Fortunately the period was a fairly short one, and at the roughest times, Mr. Hutchins always backed me unreservedly.

As soon as the department had been brought up to strength by this series of new appointments, being enabled thereby to cope with the large enrollments characteristic of the years immediately following World War II, we could turn our attention to a thorough study of the curriculum and the requirements for higher degrees in mathematics. During the thirties, the department had established an elaborate scheme of course and credit requirements for both the master's and the doctor's degree. A thesis was still required for the master's degree. The courses themselves had not been changed to the extent needed to keep abreast of recent advances in mathematics. The group that was about to undertake the task of redesigning the department's work was equipped magnificently for what it had to do. It included, in alphabetical order, Adrian Albert, R. W. Barnard, Lawrence Graves, Paul Halmos, Magnus Hestenes, Irving Kaplansky, J. L. Kelley, E. P. Lane, Saunders Mac Lane, Otto Schilling, Irving Segal, M. H. Stone, Andrè Weil, and Antoni Zygmund. Among them were great mathematicians, great teachers, and leading specialists in almost every branch of pure mathematics. Some were new to the University, others were familiar with its history and traditions. We were all resolved to make Chicago the leading center in mathematical research and education that it always had aspired to be. We had to bring great patience and open minds to the time-consuming discussions that ranged from general principles to detailed mathematical questions.

The presence of a separate and quite independent college mathematics staff did not relieve us of the obligation to establish a new undergraduate curriculum in addition to the new graduate program. Indeed the college mathematics staff offered only two courses — a general introductory course required of all college students, and an experimental calculus course open only to honor students. The department was therefore responsible for a complete undergraduate program, including elective courses for college students and service courses for graduate students who might need some elementary mathematics in order to meet various departmental or divisional requirements. This program also had to be coordinated with the graduate work for the sake of those college students desirous of going on to higher studies in the department.

Two aims on which we came to early agreement were to make course requirements more flexible and to limit examinations and other required tasks to those having some educational value. We decided to establish a nucleus of courses covering the elements of real and complex function theory, point-set topology, algebra, projective geometry, and differential geometry, which were required for the master's degree. We did not include algebraic topology here, although we did plan an introductory course at the same level that commonly was taken by candidates for the master's degree. In view of the large number of candidates, we eliminated the master's thesis and instituted a general written and oral examination covering the nucleus of required courses. Our intent was that this examination should serve as a first qualifying examination for doctoral candidates, whether or not they desired to be awarded the master's degree. For students entering with graduate credits from other institutions, it was sometimes necessary to make special modifications of these requirements; but normally they were expected to take the general examinations or some portion of them. We hoped that students would exercise an option to replace some of the core courses by independent reading, but there were few who did so. For the student who had obtained a general or liberal introduction to mathematics as represented in our nucleus of master's courses, we provided a layer of more advanced courses intended to lead into deeper or more specialized aspects of the various branches of mathematics. It was expected that at this level future candidates for the doctorate would begin to discover their preferences for thesis work. We felt that admission to candidacy should be conditioned at this point upon passing a suitable test, but one different from the usual written or oral qualifying examinations. Recalling the Harvard practice of requiring a minor thesis from doctoral candidates, we decided on an oral variation. At Harvard, the minor thesis topic was assigned and the student was given one month to complete his essay. The topic usually was chosen from a field with which the candidate was relatively unfamiliar. Our modification at Chicago was to require the candidate to prepare a lecture instead of a paper and to answer questions put to him by an examining committee. Originally we asked for two such topics from each candidate, one at least in an area unfamiliar to him. Later, however, we asked for only one topic and tried to assign it in an area relevant, but generally not related closely, to the area of his major interest. A student who was successful in passing this test and in obtaining a sponsor was then accepted as a doctoral candidate and would work thereafter on advanced topics of his choice and on his dissertation. As the last hurdle on the track to the Ph.D., we abandoned the usual oral examination in favor of a defense of the thesis, familiar to European mathematicians. Once the candidate's thesis had been completed and accepted, he was required to give a public lecture on it and to answer questions bearing on it.

The streamlined program of studies, the unusual distinction of the mathematics faculty, and a rich offering of courses and seminars have attracted many promising young mathematicians to the University of Chicago ever since the late forties. Successful coordination of these factors was reinforced by the concentration of all departmental activities in Eckhart Hall with its offices (for faculty and graduate students), classrooms, and library. Because most members of the department lived near the University and generally spent their days in Eckhart Hall, close contact between faculty and students was established and maintained easily. This had been foreseen and planned for by Professor G. A. Bliss when he counseled the architect engaged to build Eckhart Hall. It was one of the reasons why the mathematical life at Chicago became so spontaneous and intense. By helping to create conditions favorable for such mathematical activity, Professor Bliss earned the eternal gratitude of his university and his department.

Anyone who reads the roster of Chicago doctors since the later forties cannot but be impressed by the prominence and influence many of them have enjoyed in American — indeed in world mathematics. It is probably fair to credit the

Chicago program with an important role in stimulating and guiding the development of these mathematicians during a crucial phase of their careers. Thus, the program may be rated highly effective. The stability of the program, which has undergone little modification since it was initiated some 25 years ago, is evidence of its adequacy and suitability for the mathematical education of promising students. Further evidence is provided by the similarity of other programs that were developed and introduced later by other universities.

The Chicago program made one conspicuous omission — it provided no place for applied mathematics. During my correspondence of 1945-46 with the Chicago administration, I had insisted that applied mathematics should be a concern of the department, and I had outlined plans for expanding the department by adding four positions for professors of applied subjects. I also had hoped to bring about closer cooperation than had existed in the past between the departments of mathematics and physics. Circumstances were unfavorable. The University felt little pressure for increasing its offerings in applied mathematics. It had no engineering school and rather recently even had rejected a bequest that would have endowed one. Several of its scientific departments offered courses in the applications of mathematics to specific fields such as biology, chemistry, and meteorology. The Department of Physics and the Fermi Institute already had worked out an entirely new program in physics and were in no mood to modify it in light of subsequent changes that might occur in the mathematics department. However, many students of physics elected advanced mathematics courses of potential interest for them, for example, those dealing with Hilbert space or operator theory, subjects prominently represented among the specialties cultivated in the mathematics department.

On the other hand, there was pressure to create a Department of Statistics, particularly from the economists of the Cowles Foundation. A committee was appointed to make recommendations to the administration for the future of statistics, with Professors Allen Wallis, Tjalling Koopmans, and me as members. Its report led to the creation of a committee on statistics, Mr. Hutchins being firmly opposed to the proliferation of departments. The committee enjoyed powers of appointment and eventually of recommendation for higher degrees. It was housed in Eckhart Hall and developed informal ties with the Department of Mathematics. Later a similar committee was established to bring into focus the instruction in applied mathematics by coordinating the courses offered in several departments and eventually recommending higher degrees. Long before that, however, the Department of Mathematics had sounded out the Dean of the Division, a physicist, about the possibility of a joint appointment for Freeman Dyson, a young English physicist visiting the United States on a research grant. We had invited him to Chicago for lectures on some brilliant work in number theory that had marked him as a mathematician of unusual talent. We were impressed by his lectures and realized that he was well qualified to establish a much needed link between the two departments. However, Dean Zachariasen quickly stifled our initiative with a simple question, "Who is Dyson?" (He was soon to become a permanent member of the Institute of

Advanced Study.) Many years later when I was no longer chairman of the mathematics department, it was possible to make several appointments in applied mathematics and thus to cooperate fully with other science departments in the work of the Committee on Applied Mathematics.

By 1952, I realized that it was time for the Department of Mathematics to be led by someone whose moves the administration had not learned to predict. It was also time for the department to increase its material support by entering into research contracts with the government. Fortunately, there were several colleagues who were qualified to take over. The two most conspicuous were Saunders Mac Lane and Adrian Albert. The choice fell first on Professor Mac Lane, who served for the next six years, and then upon Professor Albert, who followed him for another six years. Under the strong leadership of these two gifted mathematicians and their younger successors, the department experienced many changes, but flourished mightily and was able to maintain its acknowledged position at the top of American mathematics. I shall not attempt to recount in detail the events of those years, as I have only an incomplete knowledge of them and cannot do them justice.

By the same token, I know less of what took place at Harvard after 1946 and still less of Princeton, and therefore can contribute nothing of value to the historian who may be interested in their role in American mathematics after World War II. Thus, this paper has reached its close, were it not for my inclination to recall some remarks with which I opened it.

Hope was expressed that lessons of some value or significance might be drawn from history. Perhaps I may be permitted at the end of these recollections to suggest some of the lessons I would find in them. The most important observation of all is that the continuity and the increase of mathematics in our country rest with its departments of mathematics. It is in the department that youth is guided to the sources of mathematical knowledge and inspired to search and discover mathematical truths beyond the present limits of what is known. If the department is to perform this task well, it must be active in research and alert to the directions in which mathematics and its applications are developing. Measures have to be taken to guard against losing contact with the advancing frontiers. Perhaps the best way is to recruit promising young mathematicians and place confidence in their enthusiasm for striking out into the unknown. Our experience in America demonstrates clearly how responsive a good department can be to the challenge of the future and how effectively it can apply even the most modest support to the accomplishment of its mission of teaching and research. Indeed, recent events have taught us the same lesson in reverse — a good department is extremely sensitive to the withdrawal of support. This in itself might lead us to conclude that we should be more cautious in becoming dependent upon a single source of support, even if we believe that government sponsorships of mathematical education and research will become a permanent necessity. It may be that a sufficient degree of independence will be unattainable by the isolated efforts of single departments. In that case, a concerted effort with the participation of the professional societies, in particular of the American

Mathematical Society, could be decisive for the future of mathematics in our country. The Society, as we have seen in our brief allusions to its history, has been influential in advancing mathematics and surely can be influential in circumventing the obstacles that may detain further advances. Through its publications, its symposia, and its colloquia, the Society has performed a signal service in keeping our mathematicians abreast of progress in pure and applied mathematics. It may be called upon to make an even greater contribution along these lines as the traditional means for disseminating mathematical knowledge become inadequate. As we mention these matters, it becomes apparent, I think, that whatever may be the lessons taught by history, they cannot be applied simply in a literal way but have to be read with imagination and courage.

INTERNATIONAL RELATIONS IN MATHEMATICS

MARSHALL H. STONE

The history of mathematics in the United States would be woefully incomplete if the treatment of its relations to mathematics in other parts of the world were confined to mere passing remarks. In my first paper, I made only casual comments on international relations in mathematics, but in this paper I shall make them the central theme. I shall be concerned above all with America's participation in international organizations carrying on mathematical activities of various kinds. This means that attention will be focused primarily on the International Mathematical Union and the International Commission on Mathematical Instruction.

The story of the intellectual contacts and influences in mathematics is certainly more interesting and more fundamental than any account of mathematical organizations ever could be. However, as contacts become more frequent and influences more complex and subtle, they call for organization. Thus a time comes when institutions have to be created for the cultivation of activities that originated spontaneously. All this becomes a part of history that eventually has to be traced in some detail.

After the European discovery of the Americas, the mathematics that had been developed over the centuries in Asia and Europe was transplanted to the New World. Mathematicians came from the Old World to the New as teachers; students traveled to the sources in Europe to complete their mathematical education. Sylvester, for example, came from England to spend several years as the professor of mathematics at the Johns Hopkins University, and he was followed by others. When Byerly took his doctorate at Harvard (in the seventies, I believe), he was something of an exception. The majority of American students desirous of studying advanced mathematics went abroad for graduate work in European universities, especially to Germany, but also to Great Britain, France, and Italy. This practice continued into the 1920's, when the stream of mathematics students going abroad became a mere trickle. For at least 40 years now, it has been rare for a young American mathematician to go abroad until he has earned his doctorate and feels ready for postgraduate research in an active, foreign mathematical center. The growth of mathematics in this country and increased numbers of fellowships for graduate and postgraduate study were factors in this shift. Occasional appointments of foreign mathematicians to posts in the United States during the nineteenth and early twentieth centuries were but the precursor of that extraordinary period in the 1920's and 1930's when large numbers of our mathematical colleagues found refuge among us from persecution in their homelands. This influx of mathematical talent had a profound and beneficial effect upon the development of mathematics in the United States.

It was only natural that working mathematicians should feel a deep need for discussions and correspondence with colleagues who shared their own particular interests. On the international level, this led quickly to the organization of international meetings at which papers were read by the participants. The first of these was held in Zürich in 1896; the second, in Paris in 1900. At the latter, Hilbert spoke on his famous list of problems that has been so influential on mathematics throughout the present century. Some of Hilbert's problems have been solved; others have been shown to be unsolvable, but the list still fascinates mathematicians and influences them in many ways. Those earliest congresses were followed by others at regular intervals, except for interruptions caused by World Wars I and II. Whereas these congresses were international in scope, in the sense that their participants were mathematicians from many nations, they generally were organized on a strictly local basis — that is to say, the host country exercised complete control over the program and invitations to speakers. These early congresses were very small compared with those that have been held in recent years. It is very interesting to review the lists of papers read and to see how mathematics has shifted over the years. Recently I had occasion to look at the *Proceedings of the 1908 International Congress of Mathematics,* held in Rome, and was struck by the observation that a very large fraction of the papers presented there were in what we now would call applied mathematics. The proceedings of the Nice Congress in 1970 offered a striking contrast, being overwhelmingly concerned with pure or abstract mathematics of the kind that has been developed so extensively over this century. There was very little in the *Proceedings* that properly can be called applied mathematics or regarded as a specific application.

Another activity that was organized with more international cooperation than the congresses was an association of mathematicians interested in education. Some of the most prominent figures of the time, such as Felix Klein, Jacques Hadamard, and H. Fehr (Geneva), were the leaders. In connection with the international congresses, this group arranged discussions of mathematical education that became a regular feature and are maintained to this day.

Nevertheless, the international contacts among mathematicians were more informal than organized until the 1920's. The international congresses were not tied to any permanent organization before the International Mathematical Union was founded in or about 1925. Even then the congresses were almost completely independent of the Union for a rather long time. Each congress in plenary session accepted a national invitation for the next one, to be held after an interval of four years. The International Mathematical Union, after its formation, gradually came to serve as a link, although its organizing role remained a very weak one until the 1950's. The congresses were interrupted by the two world wars, and the Union was disbanded in 1936 and not revived until 1952. Thus it is only quite recently that a more secure basis for international cooperation in mathematics has been laid. Now the Union is able to foster and promote mathematics at all levels in both research and education.

The International Mathematical Union, like similar unions for physics, chemistry, and certain other branches of science, was founded as an outcome of the First World War. During the war, there was much collaboration in scientific research for war purposes among the Allied Powers, particularly England, France, and the United States. At the conclusion of the war, international cooperation continued for peacetime purposes. The Congress of the United States specifically designated funds for the support of the Union, although it eventually dropped the practice in favor of less direct financial contributions. One consequence of the historical origin of the Union was that the Central Powers, defeated in World War I, were excluded from the Union. Thus the congresses held in Strasbourg (1920) and Toronto (1924) did not invite representatives of the Central Powers. It was not until 1928 that a German delegation, led by David Hilbert, attended a postwar congress—the one invited by Italy to meet in Bologna and Florence. This led to important consequences because, despite the applause with which Hilbert and his delegation were welcomed there, the International Mathematical Union did not join in the spirit shown on that occasion. Powerful members of the governing body, particularly some of the French members, were adamant in their stand for exclusion of the old Central Powers. Quite naturally, this led to the gradual disintegration of the Union.

It is hard to say when the Union came to end, whether at the Zurich Congress in 1932 or the subsequent one at Oslo in 1936. There is no question but that, if still alive at the Oslo Congress, it was given the coup de grace there by the American delegation. Because the congresses were activities independent of the Union, they could continue to flourish after the Union had expired. Apart from the congresses, the Union really had not found very much to do. Indeed, there were some who had come to think that the Union existed for the sole purpose of perpetuating a schism. In any case, the Union died at Oslo; and it was not until after the Second World War that the mathematicians' desire for an international organization in which they could meet freely and equally led to the revival of the old International Mathematical Union.

Immediately after the Second World War it became apparent that this desire was strong enough for a considerable number of mathematicians in various countries to propose the reconstitution of the Union. The French mathematicians showed great interest in this possibility, but, justifiably or not, there was sentiment in other countries that perhaps they would again insist on a union excluding the more recent enemy powers. In 1945, this would have meant excluding Italy and Japan as well as Germany. Thus, at the very outset, the problem of deciding between a universal union open to all countries or regions interested in mathematics or a union intended to preserve political differences arose. There was potential opposition to the creation of a universal union following the brutalities of a war conducted against civilians as well as armed forces and perverted into a genocidal attack on the Jewish people. It was by no means clear to anyone that the terrible bitterness left by the war would permit the establishment of a union open to all nations. Fortunately, there were mathematicians like Mandelbrojt in France and the United States, and Kuratowski in Poland who might have expressed bitter opposition but who instead took the lead in publicly favoring the admission of Germany. Thus, difficulties that had resulted in the dissolution of the first International Mathematical Union were avoided.

The brunt of organizing was borne initially by the United States. American mathematicians in positions of leadership were extremely active in rallying sentiment for the revival of the Union and in taking the necessary steps to achieve it. Marston Morse, John Kline (then Secretary of the American Mathematical Society), and I were eventually the principal American representatives who carried out the detailed negotiations involved in convening a constituent assembly. I was to draft a new constitution that avoided some of the underlying weaknesses of the first Union. We all had to exert ourselves in renewing ties with colleagues all over the world and in convincing them of the importance of creating a new union based on principles of universality and equality.

One of the interesting aspects of our negotiations emerged when we sought to renew contacts with mathematicians in the occupied countries so that they could participate in founding the new Union. Because I was invited to lecture in India in 1949-50, I had the opportunity to pass through Tokyo and meet influential Japanese mathematicians. However, I had to secure permission from the Supreme Command to visit the country and to confer with colleagues. From the difficulties that had to be overcome in arranging details of a tour that was readily approved, I inferred that I was perhaps the first nonofficial scientific visitor to Japan, even though four years had passed since the end of the Pacific war. It had been possible for me to arrange a personal interview with General MacArthur at the beginning of my visit to Japan in order to discuss the purpose of organizing a new union. I have the official letter he wrote in his unmistakable style to authorize my conversations about the Union. In it he welcomed the proposal that Japan should be returned as quickly as possible to the world community of mathematicians. I had been an admirer of General MacArthur's military greatness as revealed in the Pacific war, but this experience was for me evidence of true statesmanship as well. In this case it led directly to the subsequent entry of Japan into the Union when it was organized in 1952.

Another important and difficult question was posed by the partition of Germany. In what manner could both East and West Germany be admitted to the new union? What could be done in other situations where countries were divided? In preparing a draft constitution, I devised a formula answering such questions by allowing the representation of any region — not necessarily a nation or a country — in which there was some significant mathematical activity. This formula seems to have worked fairly well. For quite a long time, East and West Germany were represented in the Union as a single region. This was also the case for Singapore and Malaya.

An innovation proposed in the draft constitution was an article giving the Union power to consider the problems of mathematical education. I believe that our Union was the first to realize that the international study of educational issues in a particular science is a proper and fruitful activity. Although the international congresses always had accepted papers on mathematical education and had provided sectional meetings where they might be read, no union, so far as I could ascertain at the time, had made any formal provision for working in the field of science education. The consequence of this constitutional innovation has been the creation of the International Commission on Mathematical Instruction (ICMI) and the development of a worldwide program of activities under its sponsorship. When the Union first was organized, Professor Fehr of Geneva requested that international work in mathematical education be assumed by the Union and entrusted to such a committee. Because interest in teaching mathematics has been widespread during the past 20 years, it is no small wonder that ICMI has been busy and has reached the point where it has organized two successful world congresses, one in Lyon in 1968 and the second in Exeter in 1972. These congresses bring together mathematicians and large numbers of others whose interest is primarily in various aspects of teaching rather than in mathematical research. It is safe to say that ICMI has been very influential in the efforts to reform primary and secondary education.

When the new Union was formed, it had little relation to the international congresses, but it gradually has acquired an influential role in organizing them. Thus the Amsterdam Congress of 1954 reflected quite faithfully the mathematical tastes and interest of the Netherlands, as there was virtually no outside influence on the planning of the program. The feeling that programs would have to become more diversified in order to keep pace with the rapid development of mathematics grew stronger and led to involvement of the Union in planning and organizing the congresses and their programs. The large size of recent congresses has made them more difficult and more expensive to organize so that the backing of a permanent institution like the Union has become quite essential for their success. Many mathematicians believe that the Union's influence has been beneficial and hope that future programs will be improved further.

The International Mathematical Union has developed a number of additional activities and interests, some of which arise from its contacts with the International Council of Scientific Unions and UNESCO. A small but useful task initiated by the Union was the publication of a *World Directory of Mathematicians,* which appears every four years, coinciding with the World Congress. The Union has organized a few small conferences or symposia on specialized subjects. Union sponsorship is usually quite helpful in obtaining financial support in addition to its own contributions. The Union has sponsored from time to time some visiting lectureships, usually in cooperation with a scientific academy or mathematical society. It clearly has done a great deal for international cooperation and undoubtedly will find other opportunities as time goes on.

When the Union was organized early in 1952 on the basis laid down in 1950 by the constituent assembly, the acceptance by most of the mathematically active countries of the world was gratifying. There were and still are many countries where mathematical research is of little interest or concern. Although no one counted on the adherence of such countries, there was disappointment that many East European countries followed the lead of the Soviet Union in taking a cautious attitude toward the new Union and hesitating to join it before assessing the implications of adherence. Eventually the USSR entered the Union and so did several other socialist countries that had waited on its example.

An amusing incident in which I played a part bore on the Soviet adhesion. In 1956 there was an annual meeting of the Indian Science Congress in Agra, which was attended by various official delegations from overseas. There was a large and very distinguished Russian delegation, including the science attaché of the Soviet Embassy in New Delhi. As I was having tea on the hotel terrace, a man at a nearby table (he turned out to be the Soviet science attaché) rose and approached me. He insisted that I must be a Russian and treated me in a very hearty, cordial manner. Although I had to assure him that I was an American without any Slavic ancestry known to me, I also had to admit that more than once before I had been taken for a Russian. Our conversation gave me an opportunity to raise the question of Soviet adherence to the Internation Mathematical Union. The attaché became enthusiastic and jovial over the prospect that his country might enter the Union, so I arranged a meeting with Professor Enrico Bompiani, the Secretary of the Union at that time, and an Italian delegate to the Indian Science Congress. That morning meeting with Soviet delegates seems to have had a catalytic effect, because very soon afterwards the USSR adhered to the Union, and the Union took a giant stride toward its goal of universality. Ever since, the USSR and the other communist nations of Eastern Europe have been very active, useful, and cooperative members of the Union. I never have seen nor heard of any of those incidents where communist delegates have tried to make a point by some sort of boorish behavior. Perhaps this is because the affairs of mathematicians are relatively free from political problems, but I believe that it also has been because of the wisdom and devotion to mathematical science that have characterized the Soviet representatives.

The question of Chinese participation in the International Mathematical Union is presently a delicate one. Participation of the Republic of China (Taiwan) never was recognized or tolerated by the People's Republic of China, which therefore refused invitations to adhere. This and other difficulties have stood in the way until now. Presumably the matter is one to which a solution at once pragmatic and diplomatic will eventually be worked out.

Although something has been said about international activities in the field of mathematical education, a good deal needs to be added if an adequate picture is to be drawn. These activities have expanded far beyond what anyone anticipated when the Union was given the authority to participate in them. Although some of them have been the work of the Union's Commission on Mathematical Instruction, others have been connected only loosely with ICMI, or have been even entirely independent of the Commission.

ICMI itself organized a number of small conferences devoted to special themes, such as teaching geometry, and planned special sessions on mathematical education at the successive International Congresses of Mathematicians. It also stimulated other conferences and activities with which it retained only very loose ties, if any. Thus on the motion of Professor Howard Fehr of Columbia Teachers College when he was a U.S. representative in the Commission, ICMI decided to organize a regional conference in the Americas. With the assistance of UNESCO, the Organization of American States, the U.S. National Science Foundation, the Ford Foundation, and the Rockefeller Foundation, the conference met in Bogota, Colombia, in 1961 on invitation from the Colombian Ministry of Education, which offered gracious hospitality and important local support. As a result of this conference, there was formed an Inter-American Committee on Mathematical Education (IACME). It was my privilege to serve on this committee as its president until 1972, when Professor Luis A. Santaló of the University of Buenos Aires succeeded me. IACME organized similar conferences in Lima (1965) and Bahia Blanca (1972). These three conferences seem to have stimulated a very general interest in modern views of mathematical education throughout Latin America.

At first it was not clear whether IACME should retain a connection with ICMI or seek an association with the Pan-American Union in the Organization of American States. It soon became apparent that such an association would be difficult to establish on mutually satisfactory terms. Formal but very loose ties then were established with ICMI in accordance with the latter's terms of reference. There are prospects of a more substantial and effective association between the two committees now that the Commission, led by Sir James Lighthill, current president of ICMI, is taking a greater interest in regional conferences. In fact, at the Exeter Congress, ICMI decided to organize such conferences in Africa, Japan, and India. The conference in Japan is scheduled for the autumn of 1974 and will bring together representatives from the entire Western Pacific region, from the Soviet Union to Australia and New Zealand. Thus, ICMI is becoming active on a worldwide scale, and logically should sponsor IACME's conferences as regional activities in which it has a natural historical interest.

Important conferences and organizations without links to ICMI have been numerous in recent years. Many of them have been regional in character, reflecting the desire for permanent cooperation in studying the problems of mathematical education. The Organization for Economic Cooperation and Development (originally the Organization for European Economic Cooperation) found that the economic growth of the European member-states depended in part upon reforms in science and mathematics education. The Organization therefore convened three successive conferences to deal with questions of mathematical education. The first, at Royaumont, prepared general recommendations; the second, at Dubrovnik, proposed suggestions for a modernized mathematical curriculum; and the third, at Athens, reviewed implementation of the earlier proposals in the member-countries. The Scandinavian nations formed a smaller group for mutual exchanges concerning the teaching of mathematics. From their regular conferences, they drew guidance for their own national programs of school mathematics. Of course these same countries also have made it a practice to hold frequent congresses of research mathematicians. In South America, also, recent years have seen the formation of new regional groups and the organization of regional congresses dealing with both teaching and research in mathematics.

The two most active of these groups, the Bolivarian and the Andean countries, respectively, overlap to a large extent though Chile, which is certainly Andean, but is hardly Bolivarian, and Panama, which might claim to be Bolivarian, but is certainly not Andean. At two year intervals, the Bolivarian countries have been organizing week-long congresses with elaborate programs covering all kinds of mathematical interests. There is room for other groupings of this type in both South and Central America. As communications and economic conditions improve, such regional groups should flourish and should find unity in establishing appropriate relations with IACME. Some of the Bolivarian congresses have established relations with Europe by inviting European speakers, and these contacts — indeed, worldwide contacts as well — could be improved only by working with IACME.

In Southeast Asia, a formal, highly official organization has been established at the level of the Ministries of Education in the participating countries from the area: Indonesia, Singapore, Malaysia, Thailand, South Vietnam, and the Philippines. This treaty organization has promoted projects for curriculum reform and teacher training in the field of school science and mathematics education. It has an international advisory board on which many countries outside Southeast Asia, such as Ceylon, Great Britain, Japan, and the United States, are represented. I have been honored to serve as a member of the board, without having had much opportunity to work with it. The association has afforded me chances for knowing colleagues in both Ceylon and Japan whom I might not have met otherwise.

It is evident that the interest in seeking better forms of mathematical education has spurred a great deal of international activity all around the world. This interest has affected areas I have not mentioned here, such as parts of Africa. At the present moment, the attitude of the United States government toward these activities could be the occasion of some concern to mathematicians who believe in the mutual benefits derived from international cooperation in matters involving teaching, research, and the applications of their science. It is my impression that for some time our country has been reducing its interest in the International Mathematical Union and in other international organizations such as those described here. I have had no close contacts with affairs of the Union for a long time, but until last year I was deeply involved in the affairs of IACME and have noted changes as they have occurred. So far as mathematics is concerned, the international activities of NSF and USAID have been dwindling. It is now very difficult to secure support or even convincing expressions of interest from Washington for activities for which it was once very enthusiastic. The Third Inter-American Congress on Mathematical Education had no official support from the U.S. government, which did not send even a single delegate; Great Britain, France, the Netherlands, Switzerland, and Spain all sent representatives. The presence of U.S. representatives was made possible by IACME, UNESCO, IBM, and Control Data Corporation. This was due partly to the general policy of retrenchment, but went beyond that at the operating level. There are many ways in which government can encourage and help worthwhile international activities in mathematics besides contributing financial support. If there seems to be disinterest in official circles, I believe that interest in international relations has declined in mathematical circles as well since the days when the United States took a leading part in reestablishing the International Mathematical Union.

If American mathematicians are convinced that the Union and other international mathematical organizations are worthwhile, there is a good deal they could do to revive interest at the governmental level. This would involve working more closely with the Foreign Secretary of the National Academy of Sciences and his office. The Academy also has an influential role to play in the International Council of Scientific Unions, in which the Mathematical Union participates. In fact, constitutionally the various national academies of science have considerable power in UNESCO and in the International Council. The organ through which the mathematicians of America can work with the Academy is the U.S. National Committee for Mathematics and its subcommittee on mathematical instruction, created to deal with the International Union and ICMI, respectively. It is my feeling that these channels need to be exploited more vigorously if American mathematicians are to be as active in international mathematical and scientific affairs as they should be.

In closing these remarks, I would be wrong to say nothing about the Institute for Advanced Study, which has done so much for international relations in mathematics at the highest intellectual level. It would be easy to argue that the Institute has been the most important point of international contact that Americans have had. From the time of its foundation, both the professors and the temporary members have been drawn from many parts of the world. The Institute has become a place where mathematics can be pursued in a truly international atmosphere. At first this was not clearly the case, because the Institute was to some extent a place of refuge from the effects of the American depression and the Nazi evil. It was important that the Institute did provide shelter to mathematicians who needed it at the time, but it was more important that it should become the great international center into which it soon developed. This was certainly Oswald Veblen's dream, his great contribution on the human side of mathematics. It was a great achievement, brought about by American funds, American goodwill, and American inspiration. I hope that this will not be forgotten at this time when the Institute is experiencing serious internal difficulties. The great work done there must continue at full strength.

Anyone who has faith in mathematics has to believe that the next decades are going to be extremely creative ones. We mathematicians need to participate in a common effort, both national and international, to accomplish more in our cherished science. The more united we are, the more we can make our subject useful and relevant for our fellowman. This is the true significance of our international activities.

THE RISE OF MODERN ALGEBRA TO 1936

GARRETT BIRKHOFF

Abstract, or "modern" algebra as it has been called for over four decades, has profoundly influenced the thinking of four academic generations of American mathematicians. It strongly influenced my own generation, the generation of Ph.D.'s produced just after World War II, the lucky mathematicians educated in the expansive 1950's and early 1960's, and the annual flood of 1500 Ph.D.'s produced recently from the "baby bulge" following World War II. Initially sweeping over the mathematical world like a tidal wave, and later christened the "new" mathematics by our journalists, all signs point to its now having been absorbed into the mainstream of mathematics (Bell, 1938). Therefore, it seems timely to review its rise — though premature to try to review its accomplishments.

This tidal wave was generated in Germany and assumed a coherent shape in Göttingen during the 1920's under the leadership of Emmy Noether (1882-1935). This, in his careful review of *Fifty Years of Algebra in America, 1888-1938 (1938:32)*, E. T. Bell wrote: "this latest phase of algebra is . . . practically all German, if the ideas of Galois be excluded as too remote historically. Its roots are in Dedekind's work . . ., Steinitz' paper of 1910 on fields, and Emmy Noether's abstract school, trained by her either personally or through her writings from about 1922 to her death in 1935."

Its most essential feature is its reliance on the axiomatic approach. Thus, van der Waerden's now classic *Moderne Algebra* (1930), the book that gave "modern" algebra its name and won for it worldwide recognition, describes the goal of the book as follows. "The *abstract, formal* or *axiomatic* direction, to which Algebra owes its renewed upswing in recent years, has above all led to a series of novel concepts giving new connections and far-reaching results in field theory, *ideal* theory, *group* theory, and the theory of *hypercomplex numbers*." Likewise Hermann Weyl, no great admirer of axiomatics, stated: "In her (Emmy Noether's) hands, the axiomatic method had opened new, concrete, profound problems and pointed the way to their solution."

A second essential feature is its emancipation of algebra from primary concern with the real and complex number systems, and of proofs that depend on analysis, for example, of the classical "fundamental theorem of algebra." Thus is treats algebra as a self-contained discipline "whose formulations and methods are diametrically opposite" to those of analysis.[1] It tries to replace analytical methods by rational and set-theoretic methods, including finite chain conditions and transfinite induction.

In particular, it considers the fundamental theorem of algebra as providing just one example of an algebraically complete (or closed) field, and pretty much ignores partial fractions, continued fractions, Sturm sequences, and the like. This

1. H. Hasse, 1930, Die moderne algebraische Methoden, Jahresb. der Deutsche Math. Ver., 39:23-34.

is because its primary emphasis is on wide classes of structures, whose elements can be numbers but are more likely to be transformations, sets, or just symbols.

Among the pioneer abstract algebraists of the Emmy Noether school, perhaps most outstanding were Emil Artin (1892-1962), Richard Brauer, Helmut Hasse, and van der Waerden. Deuring, Fitting, Krull, and E. Witt were her students, whereas Saunders Mac Lane and Oystein Ore (1899-1968) studied at Göttingen. In retrospect, it is easy to recognize the enormous importance of Emmy Noether's work and influence.

This influence was so great that it is easy to overlook the equally fundamental influence of English and French-speaking mathematicians on the development of "modern" algebra. I shall try to make clear this influence, emphasizing especially (partly for sentimental reasons) the contributions of American mathematicians.

Roughly speaking, the rise of modern algebra divides into two main periods: 1921 to 1935, ending with the death of Emmy Noether, when it was centered in Germany; and 1936 to 1950, when it found a new home in our country and was expanded by Bourbaki into a new global view of mathematics. By 1950, its rise had been completed. I shall discuss these two periods in succession.

EMMY NOETHER

In reviewing the rise of modern algebra, it seems fitting to begin with a glimpse into the background and contributions of Emmy Noether. I shall not try to be systematic; more extensive accounts were given shortly after her death by van der Waerden and Hermann Weyl.[2] I have drawn freely from these sources and from Kimberling (1972) and Constance Reid (1970), whose index will guide you to many fascinating anecdotes.

The daughter of the notable algebraist Max Noether, she wrote her thesis on invariant theory under Gordan, and was his only Ph.D. student. It was only gradually that she fell under the spell of Hilbert's methods and formulation of problems. According to van der Waerden, the essence of her scientific credo became contained in the following maxim: "All relations between numbers, functions and operations first become perspicuous, capable of generalization, and truly fruitful after being detached from specific examples (besonderen Objekten), and traced back to general conceptual connections."

Emmy Noether developed slowly. One wonders what her academic status would have become without the male vacuum created by World War I. Even so, it was only Hilbert's support and influence that got her a position in Göttingen, over the opposition of his colleagues (Reid, 1970: 143, 168). And she never became an "ordentliche Professor."

She was 39 before she published her first major paper, in 1921. It dealt with ideal theory, and its announced purpose was "to generalize the factorization theorems for natural integers and algebraic number fields to arbitrary integral domains and rings." Five years later, she published a second paper (1926),

2. H. Weyl, 1935, Emmy Noether, Scripta Math., 201-220; B. L. van der Waerden, 1935, Nachruf auf Emmy Noether, Math. Annalen, 111:469-476.

giving "an abstract characterization of all (commutative) rings whose ideal theory coincides with that of all integers of an algebraic number field.[3] The best known examples of these rings are provided by the ring of all integers in an algebraic function field in one indeterminate — or more generally, in several indeterminates, provided that one considers, with Kronecker, only ideals of maximum dimension, thus having suitable quotient rings."

The main thrust of these two papers is clear: to develop an axiomatic factorization theory that is valid in any commutative ring with unity having no zero divisors. She shows that, in an integral domain J,[4] it is sufficient to assume two chain conditions and one axiom of "integral closure," namely:

I. *Chain condition on divisors*. Every nested sequence $A_1 \leqslant A_2 \leqslant A_3 \leqslant \ldots$ of ideals A_j has only a finite number of distinct terms.

II. *Chain condition on multiples*. Given a proper ideal A in J, R/A has the property that every descending chain of ideals A_j/A has only a finite number of distinct terms.[5]

V. *Integral closure*. The field $Q(R)$ of quotients of R is *integrally closed:* in $Q(R)$, every element "integral" over R belongs to R.

The "ascending chain condition" of Axiom I above is abstractly equivalent to Hilbert's Basis Theorem (van der Waerden, 1931), originally proved for polynomial ideals by Hilbert. Hilbert himself seems not to have attached very great importance to the result; his proof of it was so abstract that Gordan said of it: "This is no proof, this is theology." Emmy used the "theology" described by her mentor so effectively that rings satisfying it have come to be known as "noetherian" rings.

Emmy Noether used this condition also in her theory of hypercomplex number systems, alias "linear associative algebras." In another major paper (1929), she showed that the theory of group representations developed by Frobenius, I. Schur, and A. Young could be derived as a corollary of the general theory of semisimple algebras.[6] (Note also her important work on "crossed algebras" with Brauer and Hasse, published in Crelle 167(1932:399-404.)

Other Germanic Antecedents

Emmy Noether was the greatest single source of inspiration for van der Waerden's *Moderne Algebra*. However, Artin's influence was almost as great; actually, volume 1 was based largely on Artin's lectures, and some of its brilliance surely reflects Artin's supreme mathematical artistry.

However, van der Waerden also drew on many other contemporary sources, which he has kindly summarized in a long, thoughtful letter (to appear in *Historia Mathematica)*. One notes that his sources are almost exclusively Germanic. Three reasons can be given for this.

3. Emmy Noether obviously had Dedekind's theory in mind. On p. 29, she alludes to the relevance of E. Prüfer, Neue Begründung der algebraischen Zahlentheorie, Math. Annalen, 94(1924):198-243.

4. In an integral domain, E. Noether's Axioms III and IV hold automatically.

5. In van der Waerden (1931, vol. 2), this is replaced by the condition that proper prime ideals are divisorless.

6. This idea stems from Molien's thesis, 1891.

First and most important, *Moderne Algebra* was intended to be a self-contained exposition of recent research; familiarity with classical algebra was tacitly assumed. And most of the recent algebraic research done in the style of Emmy Noether had been done in Germany. Moreover it stemmed from earlier ideas about number theory and algebra also originating in largely German-speaking areas (Germany, the former Austria-Hungary, German Switzerland, and the like).

Thus, much of Artin's work was concerned with the class field theory and the theory of Abelian field extensions created by Hilbert in his 1897 *Zahlbericht*. The Artin-Schreier theory of "formally real" fields was also invented to solve one of Hilbert's famous problems and prove that every real polynomial or rational function whose values are all positive is a sum of squares. (The converse is, of course, obvious.)

From an abstract standpoint, this beautiful theorem can be summarized as follows. Using transfinite induction freely, Steinitz (1910) has proved that every field K has a unique algebraically complete extension Ω. Artin and Schreier showed that if K is "formally real," in the sense that $\sum x_k{}^2 = 0$ implies that every $x_k = 0$, then there is a *maximal* field P between K and Ω such that $\Omega = P(i)$ (van der Waerden, 1930-31, §68).

Second, Emmy Noether herself attributed her inspiration mainly to Germanic sources. "Alles steht schon bei Dedekind" was one of her favorite statements; and in particular, his celebrated unique factorization theorem for integral ideals in an arbitrary number field inspired her work on ideals. Her axiomatic approach had been used by Dedekind in his later work on lattices in 1897 and 1907[7], by Hilbert (1899) to rigorize the foundations of geometry and (less successfully) physics, and by Steinitz (1910) to develop a general theory of fields. She was inspired not only by Hilbert's Basis Theorem, but also intrigued by the general concept of a (commutative) ring; Kronecker, Molien, and Frobenius were other obvious sources of inspiration for her and her colleagues. Moreover Germany was largely cut off from the allied countries during 1914 to 1928, German mathematicians not having been invited to the International Congresses of 1920 or (primarily because of French objections) 1924.[8]

Finally, German mathematicians in the 1920's believed strongly that Hilbert was the world's greatest mathematician and Göttingen the center of world mathematics — a belief strongly reinforced by the birth of quantum mechanics there.

In view of these facts, it is not surprising that, as of 1930, the Emmy Noether school attributed "modern" algebra most exclusively to Germanic sources. However, the subject was destined to change in character and to move elsewhere very soon. Its next phase of development would owe much more to the influence of British and American ideas. There already existed equally able algebraists in the United States. To quote Hermann Weyl's notice on Emmy Noether:

7. See Emmy Noether's commentary in Dedekind's Ges. Math. Werke, 2:147.

8. It was for this reason that American mathematicians did not want to participate in an International Mathematical Union, under the auspices of the League of Nations, in the 1920's.

"Her methods need not, however, be considered the only means of salvation. In addition to Artin and Hasse, who in some respects are akin to her, there are algebraists of a still more different stamp, such as I. Schur in Germany, Dickson and Wedderburn in America, whose achievements are certainly not behind hers in depth and significance. Perhaps her followers, with pardonable enthusiasm, have not always recognized this fact."[9]

Moreover, the importance of modern algebra was not recognized widely in Germany at the time. Constance Reid wrote (1970:166-167): "She and her work were not . . . much admired in her native land. She was never even elected to the Göttingen Scientific Society . . . her classes usually numbered no more than five or ten."

Algebraic Geometry To 1935

In the introduction to his *Moderne Algebra,* van der Waerden said he omitted "algebraic functions and continuous groups, because transcendental methods are needed to treat them properly." Actually, he and Artin already had begun to free algebraic geometry from the need for transcendental methods (that is, "modernize" it). In the rest of this section, I shall try to give you some idea of what was involved, and how much progress had been made by 1935.

Ever since the publication in 1637 of Descartes' "La Géometrie," as an appendix to his *"Discours sur la Méthode,"* algebra and geometry have been inextricably intertwined. At first, attention was confined largely to real, affine algebraic curves, or loci defined to consist of the points in the real (x, y)-plane satisfying some polynomial equation $p(x, y) = 0$, that is, as the graph of a real algebraic function. For p quadratic, there are three nondegenerate cases: the ellipse, hyperbola, and parabola studied by Apollonius of Perga in the third century BC. For p cubic, Newton found in 1673 that there are 72 kinds.

The classification of algebraic curves becomes much simpler if one replaces the real field by the complex field C, and the affine plane by the *projective* plane. Thus there are only three projective distinct complex cubic curves, that is, sets of projective points $\lambda z = (\lambda z_0, \lambda z_1, \lambda z_2)$ satisfying an equation of the form $p(z_0, z_1, z_2) = 0$, p a homogeneous cubic polynomial. The group of birational transformations introduces still more uniformity.

The situation in higher dimensions is similar. Over any field F, one can define an affine algebraic variety as the locus of points satisfying some finite set of polynomial equations

$$p_1(x_1, \ldots, x_n) = p_2(x_1, \ldots, x_n) = \ldots = p_r(x_1, \ldots, x_n) = 0,$$

and a projective algebraic variety as the set of all projective points λx satisfying $p_j(x_0, x_1, \ldots, x_n)$ for $j = 1, \ldots, r$. Ordinarily, this variety will be an $(n-r)$-dimensional manifold with exceptional singularities.

Associated with each complex algebraic "curve," defined by $p(z,w) = 0$, is an algebraic function $w = f(z)$. In 1850, Puiseux sharpened parts of Newton's analysis of the singularities of algebraic functions by considering them in the

9. See footnote 2.

complex domain.[10] From 1951 on, Riemann showed how the behavior of complex algebraic functions could be understood much more clearly if one considered them as single-valued on a suitable "Riemann surface." This led to the notion of genus and to the all-important Riemann-Roch theorem (1857, 1864). Special interest was taken also, during the nineteenth century, in integrals of algebraic functions (so-called Abelian integrals), including hyperelliptic integrals of the form $\int dz/\sqrt{Q(z)}$, Q a polynomial (quartic Q giving, of course, elliptic integrals).

The above ideas, along with the invariant theory of Cayley and Sylvester, were developed extensively in Germany by Clebsch, Gordan, Brill, and M. Noether (1844-1921), whose daughter Emmy was trained in this tradition. Contemporary with it was the nonrigorous "Schubert calculus," for calculating intersection numbers of pairs of curves on an algebraic surface, among other things.[11] This theory was partially straightened out for surfaces by F. Severi, in a famous paper written in 1912.

Over the complex field, Lefschetz[12] gave in 1921 to 1924 a rigorous topological definition of the intersection multiplicity for two "surfaces," considering them as four-dimensional cycles in the complex projective space P^4, an eight-dimensional orientable manifold. In 1930, van der Waerden had shown how to justify Schubert's calculus in this case, on the basis of Lefschetz' definition.[13] Two years earlier, he had generalized Bézout's theorem to an arbitrary ground field. Thus by 1930, van der Waerden had made substantial progress toward the two main goals of "modern" algebraic geometers: 1) making the theory rigorous, and 2) giving a unified theory over an arbitrary ground field. Moreover he knew that the tools he wanted were available in the general theories of fields, ideals, and elimination developed by Dedekind, Kronecker, Hilbert, Steinitz, and their followers.

Zariski's influential monograph *Algebraic Surfaces,* written in 1934, was influenced much more by Lefschetz than by "modern" algebraic ideas. It refers exclusively to complex projective algebraic varieties (which Zariski calls "algebraic varieties" for brevity). Its stated aim is to emphasize the "interrelations between algebro-geometric, topological, and transcendental [aspects]." Though he refers to ideals in his first section, he never mentions them because, as he has told me, "he still had to learn modern algebra."

The fact that "higher congruences" in number theory could be interpreted as polynomial equations over finite fields, and hence as "algebraic varieties" in a generalized sense, had been noticed by Artin in his Thesis written in 1923. This led Artin to define the zeta function of any finite extension K of the field $F_q(x)$, F_q

10. See J. L. Coolidge, 1940, A history of geometrical methods, Clarendon Press, Oxford, 196-197.

11. Coolidge, Chap. 4; B. L. van der Waerden, 1971, Archive for history of exact sciences, 7:171-180.

12. S. Lefschetz, 1971, Selected papers, Chelsea, 41-198, 283-442; Bull. AMS, 29(1923):242-258. Lefschetz wrote (*ibid.,* 13) that his lot was "to plant the harpoon of algebraic topology into the body of the whale of algebraic geometry."

13. B. L. van der Waerden, 1930, Topologische Begründung des Kalküls der abzählenden Geometrie, Math Annalen, 102:337-362; *op. cit.,* 176.

a Galois field. Artin's definition was simplified by F. K. Schmidt, who also observed that the functional equation

$$z(1/qu) = q^{1-g}u^{2-2g}z(u)$$

simply expressed the Riemann-Roch theorem.[14]

From 1928 on, A. Weil also studied connections between algebraic geometry and number theory. In 1935, he even wrote a monograph on the subject.[15] In particular, he was very interested in Hasse's proof (1934) of the "Riemann hypothesis for curves over finite fields," when $g = 1$, and in applications by Skolem of the technique of p-adic completion to diophantine equations. So one can say that the germs of the takeover of algebraic geometry by the abstract algebraic approach were already in existence in 1935.

British Sources

Although technically extremely deep and skillful, German algebraists of the 1920's had a somewhat limited view of algebra, being primarily concerned with problems arising in algebraic number theory and algebraic geometry. Whereas their axiomatic and set-theoretic style was modern, their problems were classical, often traceable to Gauss, through Hilbert, Dedekind, and Dirichlet.

A much broader concept of algebra was held by British algebraists of the mid-nineteenth century (and, indeed, by Leibniz before 1700). Thus Boole (1854) wrote before 1854: "It is not the essence of mathematics to be concerned with the idea of number and quantity." Likewise, the ultimate forms and processes (of Logic) are mathematical."[16] It was with this philosophy in mind that Boole developed Boolean algebra as the appropriate algebra for logic; it is now considered even more appropriate for sets.

In much the same spirit, Cayley introduced the concept of an abstract group around 1850. Somewhat later, he introduced the algebra of matrices, as distinguished from determinants. This and Hamilton's quaternions led to vector analysis, a subject that owes as much to Green, Stokes, and Maxwell as to Gauss and Grassmann.

Likewise, Cayley "first proposed in [Cambridge Math. J., 4(1845):193-209] the general problem on invariants (that is, functions of the coefficients invariant [under] linear transformation of the facients)."[17] Moreover, he and Sylvester had developed this theory quite far before Clebsch and Gordan proved their first major theorem on the subject in 1868. Emmy Noether (like Hilbert 22 years earlier) wrote her doctoral thesis on invariant theory in 1907, and continued to work on this subject until 1919; the whole story is told ably in (Kimberling, 1972:927-932).

14. See J. Dieudonné, 1972, The historical development of algebraic geometry, Amer. Math. Monthly, 79:851-853.

15. A. Weil, 1935, Arithmetique et geometrie sur les varietes algebriques, Act. Sci. et Ind. #206, Hermann, Paris. For Hasse's work, see Crelle, 172(1935):37-54, and, with H. Davenport as coauthor, 151-182.

16. In Boole's own copy, he added in ink: mathematical "in form." Leibnez had also taken a general view of the symbolic method, and recognized the relevance of some Boolean formulas.

17. Artin, 1965:584. Cayley says he was stimulated by an even earlier paper of Boole, 3(1843):1-20.

These remarks are not intended to belittle the enormous influence on the development of modern algebra of Hilbert's Basis Theorem and other German contributions to invariant theory, but only to show that British contributions were fundamental, too. Even in the limited areas of algebra with which they were primarily concerned, Emmy Noether and her students owed more to British algebraists than they recognized.

And, except for problems arising ultimately from algebraic number theory, algebraic topology, and algebraic geometry, the techniques developed by the Emmy Noether school have not exerted a decisive influence. Thus, current activity in pure group theory has been influenced more by Burnside's (1897) meandering classic on the subject and the deep researches of Philip Hall (which were nearly contemporary with the publication of *Moderne Algebra),* than by anything produced in Germany in the 1920's (for example, by the theory of group extensions). This is not to denigrate the beautiful book of Speiser (1922), of which I shall say more later.

Some difference between British and German ideas about algebra can be seen by looking at Whitehead's *Universal Algebra* (1898). Consider the Table of Contents of the first two "Books":

Whitehead's treatise continues with a discussion of different kinds of algebraic systems, including Boolean algebras. However, it has almost no overlap with the work of the Emmy Noether school. Thus, the relation between the contents of Whitehead (1898) and van der Waerden (1930-31) is expressed most simply through the equation

$$\text{A. N. Whitehead} \cap \text{Emmy Noether} = \emptyset$$

or, (van der Waerden, 1930-31) $\cap$ (Whitehead, 1898) $= \emptyset$.

FORMAL LOGIC AND FOUNDATIONS

Generally speaking mid-nineteenth century British algebraists were philosophically inclined, like Leibniz and Grassmann, whereas the most important contributions of the Emmy Noether school were technical and intended for fellow-specialists.

This broader philosophical view of algebra also was shared by Bourbaki (1951), who wrote: "To do algebra is essentially to calculate, that is, to perform algebraic operations on elements of a set . . . the best known example is furnished by [the four operations and rules of] elementary arithmetic." The same idea is expressed by computer scientists when they describe algebra as symbol manipulation, very rightly I think.

This description also brings out the substance of modern algebra as we know it today, as distinguished from classical algebra. In substance, it is concerned with all kinds of systems to which the symbolic method is applicable, whereas classical algebra was primarily concerned with the real and complex fields and polynomials over them. Modern algebra also differs from classical algebra in its axiomatic style, as exemplified by van der Waerden's book, but it is primarily to the substance of modern algebra that British mathematicians have contributed.

Perhaps the most ambitious such contribution was Whitehead and Russell's *Principia Mathematica* (1908-12). This treatise attempts to axiomatize (rather discursively, not at all in the style of van der Waerden) and develop all of mathematics from an extended "algebra of logic" invented largely by Peano and Frege. This book in turn exercised an indirect influence on the work of the Göttingen school through Hilbert's interest in formalizing logic. Thus, Hilbert wrote in 1918:[18] "Russell's Axiomatization of Logic is the crowning achievement of axiomatics."

Indeed, all through the 1920's, Hilbert's main concern was to formalize all mathematics.[19] Thus, he refused to take seriously the paradoxes associated with Cantor's set theory ("From the Paradise made for us by Cantor, no one shall cast us forth"); the brilliant successes of the Emmy Noether school may have contributed to his faith in formalization.

Van der Waerden wrote his book before Hilbert's optimism (and that of Whitehead and Russell) had been shattered by Gödel. After Gödel's work came out, van der Waerden deleted his Chapter VII on the well-ordering theorem. Although his ostensible reason was for expository simplicity, he may also have been influenced subconsciously by his earlier training in Brouwer's intuitionism (see the sources for van der Waerden's *Moderne Algebra*).

In general, the influence on modern algebra of the ideas about logic of Whitehead, Russell, and Hilbert seems to have been limited to encouraging emphasis on the axiomatic approach. (The main thrust of Whitehead and Russell's three volumes was to construct an axiomatic model for formal logic that was adequate for discussing the real field **R**, and to demonstrate its adequacy.)

In contrast, van der Waerden (1930-31) simply reviewed in six pages (his §64) the construction of R from the rational field Q; after two pages on p-adic numbers in his §66, he proved the solvability of quadratic equations and of polynomial equations of odd degree $C = R(i)$, incidentally presenting the theory of Sturm sequences as an application of Euclid's algorithm, and freely using Zermelo's theorem (van der Waerden, 1930-31:231) to derive the Artin-Schreier theory of formally real fields. From this, in turn, he derived the classical Fundamental Theorem of Algebra as a Corollary (van der Waerden, 1930-31:228, Satz 3a). No need for classical analysis or for "visualizing" the complex plane is evident anywhere.

By maintaining a consistently axiomatic approach and avoiding classical analyses, van der Waerden gave to his book great integrity and unity. It carried on the tradition of Dirichlet (1880), Dedekind, and Steinitz (1910). But of course

18. Hilbert, 1932:153; Math. Annalen, 78(1918):405-415.

19. See his address at the Bolongna International Congress, 1(1928):135-142; the skepticism expressed by Lusin on pp. 295-300 of the same volume presents an interesting contrast.

it also ignored analytic number theory, and one may wonder today whether the more eclectic approach of Hardy and Wright (1938) may not be more stimulating to students.

American Traditions

Although seldom mentioned in the publications of the Emmy Noether school in the 1920's, American mathematicians already had a strong tradition in several areas relating to "modern" algebra, and especially in hypercomplex algebra, postulate theory, and Boolean algebra. Thus, Wedderburn (1907) was the first to present the theory of linear associative algebras over an arbitrary field (its "modern" form), avoiding its previous restriction (by Molien, Cartan, and others) to algebras over the real and complex fields; see (Birkhoff, 1973). He may have been inspired to develop this abstract viewpoint by E. H. Moore in Chicago, where Wedderburn went as Carnegie Fellow in 1905. And in 1923, Dickson had sharpened and extended Wedderburn's arguments in his *Algebras and Their Arithmetics* (Dickson, 1923), several years before Emmy Noether wrote (1929) her "Hypercomplexe Grössen . . . ."

The pre-1920 "modernity" of American thinking about hypercomplex numbers has been epitomized by Artin[20] as follows:

> From the very beginning the abstract point of view is dominant in American publications, whereas for European mathematicians a system of hypercomplex numbers was by nature an extension of either the real or the complex field. While the Europeans obtained very advanced results in the classification of their special cases with methods that were not well adapted to generalization, the Americans achieved an abstract forumlation of the problem, developed a very suitable terminology, and discovered the germs of the modern methods.
>
> On the American side, one has first of all to consider the very early paper by B. Peirce, "Linear associative algebras" (1870). In it he states explicitly that mathematics should be an abstract logical scheme, the absence of a special interpretation of its symbols making it more useful in that the same logical scheme will in general reflect many diverse physical situations.

One wonders why Emmy Noether made so little of the important and relevant work of Peirce, Wedderburn, and Dickson in her papers. In retrospect, her extensions of Wedderburns's structure theorems to "rings with double chain condition" seems of minor importance, and are not even mentioned in Wedderburn's *Lectures on Matrices* (1934). It is, of course quite otherwise with her applications of these structure theorems to obtain representation theorems for groups and rings. These representation theorems do indeed stem primarily from Molien (1892), Frobenius (1896-1910). amd Schur (1905-1911), although A. Young and Burnside made important contributions independently (Speiser, 1922:143-144).

Like many other of our most eminent mathematicians, Wedderburn was born and trained in Europe. He made other notable contributions to algebra, being the first to prove that every finite division ring is a field, and to argue (nonrigorously, I fear) that the unique factorization theorem was true for finite groups.

20. E. Artin, 1950, The influence of J. H. M. Wedderburn on the development of modern algebra, Bull. AMS, 65-72; Artin, 1965:526-533.

Another distinguished British import was J. J. Sylvester, who taught at Johns Hopkins from 1876 to 1883. However, I think that E. T. Bell (1938) was correct in saying that, although Sylvester's own contributions (1904-12) to algebra, especially invariant theory, were brilliant and varied, he did not exercise a lasting influence on his students.

Far more influential on American mathematics than Wedderburn or Sylvester was E. H. Moore. To quote E. T. Bell:

> In the late 1890's and early 1900's the history of mathematics in this country is largely an echo of Moore's success and enthusiasm at the University of Chicago. Directly through his own work and indirectly through that of the men he trained, Moore put new life in the theory of groups and the foundations of [21] and of mathematics in general, finite algebra and certain branches of analysis as they were cultivated in America. All this work had one feature in common: he strove unceasingly towards the utmost abstractness and generality available.

In other words, the modern spirit was in E. H. Moore's work around 1900.

Among Moore's many students, L. E. Dickson did the most for algebra and number theory. He and his student Adrian Albert secured for Chicago the leading position in the study of hypercomplex number systems, which it held until Albert's untimely death. The leadership has now passed to Yale with Nathan Jacobson, who studied with Wedderburn and Albert.

Bell has reviewed Dickson's publications (Bell, 1938:28-32) so thoroughly and well that I shall not try to supplement Bell's review in any way.

Postulate Theory

Contributions to postulate theory by E. H. Moore, E. V. Huntington, and other American mathematicians were especially relevant to modern algebra since, as van der Waerden pointed out in his Introduction (see Introductory Remarks), the axiomatic approach is the essence of modern algebra's style.

There had been considerably earlier European work on postulate theory, for which I refer you to Birkhoff (1973). After giving an amusing survey of this earlier European work (Bell, 1938:15-16), E. T. Bell gives a detailed account (1938:17-19) of the more important American contributions. Hence, I shall just touch on a few highlights. The earliest American contribution to postulate theory was Huntington's definition of a group in 1902, in which Hilbert's concepts of an independent and of a categorical set of postulates were applied to algebra, apparently for the first time. Related contributions were published in the *Transactions* during the next few years by E. H. Moore, Dickson, and Veblen, whereas Huntington continued his elegant and basic studies of postulate systems for all kinds of "algebras" for at least three more decades.

These studies of postulate systems were strongly influenced by Hilbert's celebrated *Grundlagen der Geometrie* (1899), which systematically avoided both Peano's use of symbolic logic and Euclid's reliance on visual demonstrations. I mention the first of these features because E. H. Moore's *Introduction to a Form of General Analysis* represented an ambitious and creative attempt to

21. E. T. Bell had a genius for overstatement!

develop analysis in the symbolic style of Peano, which failed to gain wide adoption.[22] And I mention the second because it contributed so much to the modern "algebraization of geometry" (compare the section on Lattice Theory and Algebraic Geometry in my second paper).

In particular, E. V. Huntington wrote several papers on postulate systems for Boolean algebra. Boolean algebra is a subject untouched by the Emmy Noether school, but used extensively by mathematical logicians such as A. N. Whitehead, who came from "the other Cambridge" to Harvard around 1920. The most definitive papers on the foundations of Boolean algebra are those of Marshall Stone,[23] who clarified for the first time the precise relation between Boolean algebra and rings, demonstrated conclusively that Boolean algebra was precisely the algebra of sets, and established its connections with general topology.

Group Theory

Bell (1938:8) observed: "The intensive activity of American algebraists in the theory of groups during [1888-1938] was due to a few active men who induced numerous proselytes to contribute at least one paper apiece relating to groups," and added "without the inspiration of the work of C. Jordan in France, G. Fronbenius and O. Hölder in Germany, and W. Burnside in England, . . . the history of the theory of groups in American would be much shorter."

Perhaps the first American group theorists of any note was Frank Nelson Cole, for whom the Cole Prize in algebra was named. He inspired the early work of G. A. Miller, who lived at Cole's home in Ann Arbor during 1893-95 (Bell, 1938:9). About the same time, E. H. Moore lectured and did research on groups and fields, inspiring Dickson to think about groups in the process.

Other active group theorists were Blichfeldt, who emigrated here from Denmark, and H. B. Manning, whose daughter married the algebraist Malcolm Smiley. But the most active was G. A. Miller, who published a prodigious number of short papers. In particular, Miller collaborated with Blichfeldt and Dickson in a three-part book *Finite Groups,* published in 1916.

Miller was remarkable in another respect: he amassed a fortune of $2,000,000, which he gave to the University of Illinois. As a by-product of this philanthropy, his own collected papers were published; I think it fair to say that the publication was not in response to irresistible public demand.

American Philanthropy

Miller's unexpected bequest was but one of many great contributions made to the support of scholars by American philanthropists. Much earlier Carnegie had set up a pension to help prevent professors from ending their years in poverty. I

22. Whitehead and Russell, (1908-12) were (temporarily) somewhat more successful in getting others to adopt their Peanesque symbolism.

23. See Stone, 1936, and Applications of the theory of Boolean rings to general topology, Trans. Amer. Math. Soc., 41(1937):375-481. Stone made preliminary announcements of his ideas in 1934-35.

believe that the current TIAA and CREF pension plans, on which many of us rely for reasonable comfort during retirement years, stem from Carnegie's gift.

Before World War I, the United States was a debtor nation, and Europe — especially Western Europe — was the great center of wealth and culture. But World War I changed all that. I remember having the comfortable feeling in the 1920's that all our friends were getting more affluent. This affluence, and the philanthropy that was based on it, led not only to a great expansion of our university system, but also to some notable gifts to European mathematical centers. Especially, as head of the Rockefeller Foundation, Max Mason provided Rockefeller money to set up both the Institut Henri Poincaré in Paris and the Mathematical Institut in Göttingen. In Constance Reid's book (1970), Hilbert is quoted as attributing the second gift to Courant's "matchless administrative skill." She may be right: Warren Weaver later donated $2,000,000 of Sloan money to the Courant Institute at N.Y.U.

However I think that Max Mason's appreciation, and the generosity of the Rockefeller family and Sloan, who made his money in our much maligned automobile industry, deserve much more of the credit. In this connection, I recall also the munificence of the Ford Foundation, large gifts from which raised many academic salaries appreciably in the early 1950's.

HARVARD: 1928 TO 1932

The support given to Göttingen by the Rockefeller Foundation foreshadowed a much greater dependence of science on American institutions in the next two decades. When van der Waerden's book was published, German algebraists could have had no idea of the changes "modern" algebra was to undergo in that time, nor of the influence on it of mathematicians having very different ideas and backgrounds.

Marshall Stone has given you an idea of his training; I will now give you some glimpses of mine. I hope that, in due course, Saunders Mac Lane, André Weil, and other *dramatis personae* will give you authentic views of theirs. My account will be personal, and will center around my own experiences.

It will begin with the International Mathematical Congress at Bologna in 1928. This took place just two years after my father's first trip to Europe, and was the first International Congress since 1912 that included Germans or Austrians. Of the 16 general addresses, six were given by Italian speakers and only two by Americans, Veblen and my father. My father's was especially dramatic; it was entitled "Mathematical Elements in Art," and was opened by heralds with trumpets in the beautiful Palazzo Vecchio in Florence.

Indeed, most of the general addresses were philosophical, historical, or applied in nature: Hilbert on foundations, Hadamard on the development and scientific role of functional analysis, Puppini on public works in Italy, Borel on probability and the exact sciences. Veblen and Castelnuovo gave survey talks on geometry, W. H. Young talked on the mathematical method and its limitations, Volterra on hereditary phenomena, Tonelli on Italian contributions to real variable theory, Amoroso on mathematical economics, von Kármán on mathematical problems of modern aerodynamics, Fréchet and Lusin on set theory and general topology, Marcolongo on Leonardo da Vinci, and my father on mathematics and art. The only really technical talk was by Hermann Weyl on continuous groups and their representations. What a contrast with recent International Congresses.

At 17, I attended only Hilbert's and my father's talks, and felt that Hilbert's objectives were obscure and unconvincing. (I had learned some calculus that summer and German within the year.)

A month later I entered Harvard; it may be interesting to compare my undergraduate education there with Marshall Stone's graduate education of 5 to 10 years earlier. I can confirm his statement that we were taught analysis and mechanics almost exclusively. I took complex variable as a sophomore with Walsh, and I learned my rigor from him; it was a great experience. Those who do not know Walsh should know about his technique, which I recommend. Some people raise their voice when they get to important things; Walsh did the opposite. When he was coming to a point of exceptional importance, his voice got lower and lower, so that to catch what he was saying one really had to hold his breath. He almost whispered when he reached the crucial point. I got a great sense of the nature and importance of rigor in analysis from Walsh. I had the calculus of variations and analysis situs with Morse, and he taught me about integral bases in Minkowski's lattices. Although I had Heinrich Brinkmann as a tutor, I learned less from him; it was my fault.

I wrote a thesis on one-dimensional measure in n-space, which I took terribly seriously and worked very hard at. I submitted it hopefully (all 80 typewritten pages) to the *Transactions* and was quite hurt when Tamarkin wrote me in a very kindly personal letter that, although it showed promise, I mustn't think that things like this got published. But a tiny piece of it, on topological axioms for metrizable spaces, was published a year later in the *Bulletin,* nevertheless.

My frequent and stimulating talks with Morse were made natural by Harvard's new tutorial system and House Plan. The latter was made possible by the munificence of Harkness; it gave students a good social environment.

As a Harvard undergraduate, I had less contact with my father, although I took a most interesting half-course on ordinary differential equations from him. However, I didn't believe everything he said — including his assertion that a general linear second order differential equation couldn't be integrated by quadratures. I had no concept of things such as Galois theory, let alone Picard-Vessiot theory, and spent some time trying to discover an ingenious construction that would show that he was wrong. Needless to say, I did not succeed.

Indeed, I would not have even heard of groups had not casual browsing in our departmental library made me aware of Miller, Blichfeldt, and Dickson's *Finite Groups*. After skimming through the first two or three chapters, I decided it would be interesting to enumerate all possible finite groups (up to isomorphism, then a new concept to me). At the time, I had not studied algebra since I was 14, except for a few days spent thinking about Fermat's problem ($x^4 + y^4 = z^4$ and all that) and about solving cubic equations by radicals, both at my father's suggestion, and a little reading about determinants in connection with analytic geometry. Had I not browsed in finite groups by accident, I doubt if I would have ever become an algebraist.

In spite of this immaturity and lack of mathematical sophistication, I got a string of A's and a Henry Fellowship to Cambridge University. My intention was to become a mathematical physicist, having taken basic courses in a lot of analysis and a lot of physics, including thermodynamics, electromagnetism, and quantum mechanics. Fowler was to be my adviser, and I was to attend Dirac's lectures. Thus I imagined myself as on the road to a career that I thought my family would approve of. My father's parting advice was to read Stone's book on *Linear Transformations of Hilbert Space*, and Wiener's papers on generalized harmonic analysis. With this advice still ringing in my ears, then, I left for a year in Europe.

My Year Abroad

The year began with a month in Munich, where I pursued in lonely solitude the group theory that I had discovered in Miller, Blichfeldt, and Dickson. I loved the axioms for a group; I thought that I had never seen anything so perfect. So in the Munich library that summer, I conjectured and patiently proved by myself Kronecker's fundamental theorem on Abelian groups, thus achieving the first step towards my goal of determining all finite groups.

However, I luckily called on my father's friend, Constantin Carathéodory, who was very kind to me. I was impressed by his library, which was utterly beyond anything that I had ever seen. His bookcases filled it (not just its walls); they must have contained most of the best mathematical books written since 1900. He said: "You know, people around here talk quite a bit about Speiser's book on group theory; and if you would like to go on to algebra in general, here are two volumes by van der Waerden which many people say are worth reading." Thus, in 1932, as a Harvard undergraduate, I had never heard of van der Waerden, or Emmy Noether, or Artin or Hasse, the apostles of the faith I was soon to espouse.

So when I got to Cambridge, I bought and studied both books, but did not neglect Stone or Wiener. In the meantime, I attended the International Mathematical Congress in Zurich, with the ambition of understanding at least the general addresses. This time, they were technical enough for anyone. One of them was by Emmy Noether on "Hypercomplex Systems in Relation to Commutative Algebra and Number Theory." It begins: "The theory of hypercomplex systems, of Algebras, has had a strong upswing in recent years, but only very lately has its significance for commutative questions become clear." Reading it today, one sees clearly how completely German algebraists had taken over: no reference to Wedderburn, passing references to Dickson and Chevalley, and the rest is Germanic![24] On the other hand, one also sees no thought of the impact of "modern algebra" on elementary courses; this was to come later.

24. Three years earlier, she was more generous (Noether, 1929).

When I arrived that September in Trinity College, Cambridge, I immediately called on G. H. Hardy. He greeted me and asked "What's your father doing these days? How about that esthetic measure of his?" I replied that my father's book[25] was out. He said "Good; now he can get back to real mathematics."

I was naturally shocked by Hardy's offhand dismissal of years of patient effort devoted to extending the range of applicability of mathematics. In retrospect, I think it shows Hardy's limitation: he had zero appreciation for anything that was not sharp and difficult as *pure* mathematics. Nevertheless, he was a brilliant lecturer, who deepened my knowledge of analysis and gave me a good introduction to number theory.[26] Moreover he gave Marshall Hall and me a friendly welcome and stimulated us intellectually.

In particular, he invited us to a dinner in honor of Hobson, whose "Functions of a Real Variable" made the Lebesgue integral known in England. As an after dinner speaker, he observed that Hobson had done his best work after 50 and expressed the hope that he, Hardy, would have the same fate. He didn't.

Abstract Algebra

Neither Steinitz nor van der Waerden had any visible influence on Hardy. His lectures on algebraic numbers never referred to rings or fields as such, let alone groups. The only person in Cambridge who took any interest in abstract algebra was Philip Hall, who opened my eyes to the deeper problems of group theory.

However, my interest in groups was more wide-ranging and less deep than his, and I soon decided that I could make a more fundamental contribution to a different class of algebraic systems, which I christened lattices, than to groups. Hence, during my twenty-second year, I concentrated my attention on these, still laboring as a self-taught "gifted amateur" in virtual solitude.

Among the mathematicians near Cambridge was A. Young, who had invented group representation theory 30 years previously,[27] independently of Frobenius and Schur. He was a shy cleric, who still came to Cambridge once a week to give a seminar on group representation theory. Marshall Hall and I both went to his first lecture. I discreetly avoided the second; Marshall Hall was there (one of two). Being a Yale man, he was too courteous to abandon the nice old man to an empty room, so he continued to attend for the rest of the term.

By my twenty-second birthday, I had virtually given up on mathematical physics. I was totally unprepared for Dirac's lectures on quantum mechanics. Whereas I expected him to consider the Schroedinger equation as a partial differential equation, I think in retrospect that he must have been explaining his ideas about elementary particles. I had not heard of these at Harvard, and first learned of them when Anderson lectured on the positron in the spring of 1933 at Cambridge, England.

Although I found Stone's book and Wiener's "Generalized Harmonic Analysis" most interesting and challenging, I was also well aware that I had not

25. George David Birkhoff, 1932, Aesthetic Measure, Harvard University Press, 3-225.
26. His lectures were later published in his classic "Theory of Numbers" (Hardy and Wright, 1938).
27. The "Young tableaux" in particular were his creation.

really mastered either in any sense. Hence, by June 1933, I had essentially given up on my mission as conceived when I left Harvard. Instead of becoming any kind of mathematical physicist or even analyst, I turned my back on all that I had learned in my Harvard courses and became an abstract algebraist.

Because I have published elsewhere (Abbott, 1970:1-40) an anecdotal and personal account of my early work on lattices, I shall not go into it here. Suffice to say that I was convinced from the beginning of their intrinsic importance, and indeed devoted nearly half of my research efforts during the eight-year period 1933 to 1941 to their study.

While I was becoming an abstract (or "modern") algebraist in England, adopting the style if not the content of van der Waerden, the world at large was racked by the Great Depression of 1930 to 1935, and Hitler seized power in Germany. I remember vividly the acute recognition by the (London) *Times* of the significance of his coup d'état, and the bovine placidity with which it seemed to be accepted by the American press at the time.

HARVARD'S SOCIETY OF FELLOWS

I have benefited personally many times from American private philanthropy. The Harvard House plan, endowed by Edward Harkness, came into being my junior year; I was one of the first four Henry Fellows to go to England, thanks to an endowment by Lady Julia Henry; Abbott Lawrence Lowell established the Society of Fellows at Harvard with his personal fortune just in time for me to be part of it in 1933 to 1936.

This was a matchless opportunity: to be a full-time researcher at age 22. Other, slightly later, Junior Fellows included Nobel Prize winners Bardeen (twice), Bloembergen, Samuelson, and Woodward. We were encouraged to carry through ambitious independent projects on our own, freed from normal Ph.D. requirements. We dined weekly by candlelight with Lowell, Whitehead, and five other deep and brilliant thinkers and conversationalists,[28] all eager and ambitous to see us make our mark.

During these years I concentrated on developing lattice theory to a subject with stature. I had dreamed up lattices while in England the year before, plucking them out of semithin air as a generalization of the Boolean algebra, which I knew Huntington wrote about. They were not mentioned in van der Waerden; moreover while Philip Hall knew only of Fritz Klein's essentially postulational studies (Klein called them "Verbände"), and was not aware of Dedekind's deeper earlier work, I did not know of Marshall Stone's very deep results on Boolean algebras, which were announced nearly a year after I completed my first paper on lattices.[29] And so I came home, having written a paper on the subject that had been accepted by the Cambridge Philosophical Society for publication in its *Proceedings*. On my return, my father asked me pointedly what one could

28. These were the Senior Fellows; we were the Junior Fellows; the generation gap was bridged in a most congenial and inspiring way.

29. M. H. Stone, 1934, Boolean algebras and their application to topology, Proc. Nat. Acad. Sci., 20:197-202, and 21(1935):103-105. My paper had been submitted on 15 May 1933.

do with lattices that couldn't be done without them. Although I thought that this was not really a very polite question, it honestly compelled me to admit that he was fundamentally right. My admiration for abstract algebra has been moderated by his question ever since.

Ore also informed me through my father about Dedekind's earlier work on lattices (which Dedekind called "Dualgruppen"); Ore had helped Emmy Noether edit Dedekind's *Werke*. It came as a mild shock to learn that many of "my" theorems were just rediscoveries of results published decades earlier by Dedekind. One such result was the fact that the free modular lattice with three generators had just 28 elements.

But in retrospect, I think that I was lucky not to have known about Dedekind's results when I first began thinking about lattices; it would have been discouraging to find nothing new for months. As it was, by the time I learned about Dedekind's work, I had advanced beyond him in several respects (semimodular lattices and the intrinsic topology in lattices, to name two), and this encouraged me to persevere.

I used the free modular lattice with three generators to attract von Neumann's interest. At a mathematics meeting (everybody who could, went to all the mathematics meetings in those days; there weren't nearly so many), I saw him standing alone. I introduced myself to him and said: "I understand you're interested in Hilbert space," and he agreed. So I continued: "Well, suppose you take three subspaces of Hilbert space and form repeated intersections and linear spans, do you know how many you'll get?" He said he didn't, and I said: "You'll get 28." That impressed him, and I think it was what stimulated him to study the lattice of invariant subspaces of Hilbert space (under a given operator). This led to his continuous geometries.

During 1934 to 1940 I talked and corresponded with him frequently, especially about lattices but also about mathematics and mathematicians, often staying at his house. He was a major influence on my scientific thinking and philosophy, second only to my father. In the spring of 1940, we gave a joint seminar on lattices at the Institute for Advanced Study.

Ore also decided that lattices provided a powerful tool for studying the structure of "groups with operators." He demonstrated this in his great 1935 paper on the strucutre of algebras, in which he emphasized that groups with operators were the unifying theme of van der Waerden's book, and that lattices (or "structures," as he called them) were applicable much more generally.

In a less scholarly way, I had formulated the same idea, in a much more general context: that of *universal algebra*. I had borrowed the name from Whitehead's book (1898), trying to reformulate the basic ideas of *Moderne Algebra* rigorously in this more general philosophical context, giving simple examples, such as the (geometric) lattice of all equivalence relations on a finite set and proving a few fairly deep and extremely general theorems.[30] I had emphasized that every algebraic system had associated with it two lattices: the lattice of its subalgebras and the "structure" lattice of its congruence relations.

30. G. Birkhoff, 1935, On the structure of abstract algebras, Proc. Camb. Phil. Soc., 31(1935):433-454.

Topological Groups

During the 1930's, the theory of topological groups began to come into its own, strongly reinforcing the trend away from classical analysis towards more abstract and general concepts. At first, this theory was limited to rather obvious combinations of general group and set-theoretic concepts; the early papers of O. Schreier and D. van Dantzig,[31] which influenced my own thinking considerably, were prime examples. One of Schreier's most impressive achievements was to prove that the covering group of any connected, locally Euclidean, topological group was Abelian.

However, underlying these gropings for sound basic principles were much deeper ideas. Some of these stemmed from theories of "insolvability." Not only the insolvability of the quintic by radicals (classical Galois theory), but the analogous Picard-Drach-Vessiot theory of the insolvability of a general second-order linear homogeneous differential equation by quadratures,[32] as well as that of "monodromy" groups, had shown the power and depth of group-theoretic concepts. A recognition of the relevant groups is also important for geometry, as had been made abundantly clear by Felix Klein in his celebrated Erlanger-Programm, and in physics where the crystallographic groups are important, and where relativistic mechanics differs from classical mechanics most essentially in being governed by the Lorentz group of electromagnetic theory rather than the Galilei-Newton group. In quantum mechanics, which dominated physical research in the 1930's, the theory of group representations had been shown by Weyl and Wigner to play an important role likewise.

An even greater stimulus for research into the theory of topological groups was provided by Hilbert's Fifth Problem: to determine whether or not every locally Euclidean topological group is a Lie group, that is, a group whose "function of composition" is an analytic function relative to suitable coordinates. A great deal was known about Lie groups. Thus, sharpening earlier ideas of Killing (1888), Élie Cartan had in his Thesis written in 1898 determined all simple and even semisimple real and complex Lie groups by very difficult arguments.

However, as of 1935, many basic facts about Lie groups were still unknown. Thus, Lie's own theory was strictly *local*, and it had not yet been proved that every local "Lie group nucleus" could be extended to a *global* Lie group. Neither was it known whether or not every Lie group was locally *or* globally isomorphic with a *linear* group (group of matrices).

Research workers in the 1930's, including myself, were attracted greatly to all these fundamental problems of pure mathematics, whose main thrust combined with that of "modern" algebra to push classical analysis and the theory of

31. O. Schreier, Abstrachte kontinuierliche Gruppen, 1925, Abh. aus dem Math., 15-22, 5(1926):233-244; D. van Dantzig, Zur topologischen Algebra, Math. Annalen, 107(1933):608-626 and later volumes; G. Birkhoff, A note on topological groups, Compositio Math., 3(1936):427-430; Deane Montgomery, What is a topological group?, Amer, Math. Monthly, 52(1945):302-307; R. Arens, A topology for spaces of transformations, Annals of Math., 47(1946):480-495.

32. The "modern" algebraic theory is due to Ritt and Kolchin. See E. R. Kolchin, On certain concepts in the theory of Algebraic Matric Group, Annals of Math., 49(1948):1-42, where some classical references can be found; others are in G. H. Hardy, "The integration of functions . . . ," Cambridge Tracts in Math. and Math. Phys., 1928 No. 2, 2nd ed. For Kolchin's final, very abstract exposition, see his Differential algebra and algebraic groups, Academic Press, 1973.

differential equations (both of which stemmed from the calculus of Newton, Leibniz, and Euler) off center stage.

A major impulse supporting this trend was provided by Haar's 1933 paper,[33] proving that every locally compact topological group could support an essentially unique "right-invariant" Borel measure, invariant under right-translations. Von Neumann applied this result to show that every compact, connected locally Euclidean topological group was isomorphic to a Lie group.

A year later, Pontrjagin wrote a notable paper on the duality between any locally compact Abelian group G and the dual group G^* of its "characters." In particular, he showed that the dual of any compact group was discrete, and conversely. An easy corollary of his results was the fact that any Abelian connected, locally Euclidean topological group was necessarily a Lie group.

From E. Cartan's Thesis written in 1897, on the other hand, it followed that every real or complex Lie algebra was the semidirect sum of a semisimple Lie algebra and a (quasi-Abelian) solvable "radical." Mathematicians therefore began to hope in the latter 1930's that, by combining such structural theorems with the results of Haar, von Neumann, and Pontrjagin, it might be possible to give a general affirmative solution to Hilbert's Fifth Problem; in any case, many of the hitherto unresolved questions concerning Lie groups were resolved quickly by Élie Cartan (now nearly 75) and others. Even I made some modest contributions to their solution.

Moscow, 1935

Toward the end of my term as a Junior Fellow, I attended three memorable meetings: a Topological Congress in Moscow in 1935, the International Mathematical Congress in Oslo in July 1936, and Harvard's Tercentenary in August-September 1936. I shall conclude by relating a few impressions of them.

Because of my father, I was welcomed by Harald Bohr in Copenhagen and Rolf Nevanlinna in Helsinki on my way to Moscow. As in the case of Carathéodory and of Erhard Schmidt, on whom I called during my return, I had the impression of keen, worldly, deeply cultured minds and most gracious manners. (My personal impressions of Emmy Noether and Hilbert were very different.)

With Marshall Stone and David Widder, I also met L. V. Kantorovich in Leningrad. This was two years before he published his pioneer paper on semi-ordered linear spaces, although only a year before he published his classic book with Krylov on the numerical solution of partial differential equations.[34] I reviewed his paper when it came out, and immediately became excited; it and a paper by Freudenthal (whom I met in Moscow) led me to the study of vector lattices and their applications to functional analysis and ergodic theory. But it was not until the 1960's that I became interested in a later edition of his book.

33. A Haar, 1933, Der Massbegriff in der Theorie der kontinuierlichen Gruppen, Annals of Math., 34:147-169; J. von Neumann, 170-190.

34. L. V. Kantorovich, 1937, Lineare halbgeordnete Raüme, Mat. Sbornik, 44:121-128; L. V. Kantorovich and V. I. Krylov, Approximate Methods of Higher Analysis, translated from 1941 edition by Curtis D. Benster, Interscience Noordhoff, 1958.

The Topological Congress in Moscow was a small gathering of many of the most notable topologists of the world; needless to say, it thrilled me, at age 24, to meet them all personally. The Moscow contingent included P. Alexandroff, Pontrjagin, Kolmogoroff, Kurosh, and Gelfand. Gelfand impressed me as a very young man with enormous drive. Others present included Kuratowski, Jakob Nielsen of free group fame (he brought trousers for Alexandroff; Russia was exceedingly poor at the time), and Freudenthal. There was also a strong American contingent (Lefschetz, Alexander, von Neumann, and Stone) of men whom I knew already. The connections of topology with the rest of mathematics were emphasized strongly; moreover, Dirac passed through, which gave the Congress an even broader flavor.

We chatted informally in small groups between lectures, eating bread mounded with fresh caviar and drinking tea. The faith of some younger attendees in the abstract "modern" approach was apparent. Thus Kurosh assured me several times with great emphasis that "Die Strukturn sind sehr wichtig, nicht Wahr?"

On the return trip, I met Saks, Ulam, Eilenberg, and many others in Warsaw. Saks rowed with me in a "double" shell on the Vistula, and assured me that he had American cold cereals for breakfast every morning. (G. H. Hardy was a devotee of American apple pie and coffee — as well as of baseball, though he preferred cricket.)

I returned via Berlin and Hamburg. In Berlin, I met Richard and Alfred Brauer, and J. Schröder; we all walked silently past the anti-semitic racial institute at the University of Berlin, run by the famous complex analyst Bieberbach. In Hamburg, I met Emil Artin, and it was clear that the morale of the German scientific community already had been destroyed by Hitler's persecution of those having Jewish grandparent ancestry.

The Oslo Congress

The following year, I attended the last International Mathematical Congress to take place before World War II. On the way there, I again saw Artin in Hamburg and visited Copenhagen. Artin was now most anxious to leave Germany, as he told me while military planes droned ceaselessly overhead and German youth marched, marched, marched. By this time, Springer had transferred the headquarters of the new *Zentralblatt für Mathematik* to Copenhagen, where Neugebauer was running it with superb charm and efficiency. In Copenhagen, Harald Bohr also showed me around the new Matematisk Institute, with a couch in every study; his brother Niels lived in the mansion of the founder of the Tuborg brewery filled with beautiful sculptures by a famous Danish sculptor.

In spite of the destruction by Hitler of German academic morale, the Oslo Congress itself was a gay and brilliant occasion. World War II was still three years off, and the fall of France nearly four. I think most mathematicians whose lives were not personally affected by Nazi persecution hoped that things would get straightened out somehow.

In any case, a wide panorama of mathematical ideas was presented to nearly 500 participants. My father's lecture on the foundation of quantum mechanics was attended by the Crown Prince, and we all met King Haakon. Størmer, Oseen, and Bjerknes gave other invited addresses on various aspects of mathematical physics. Neugebauer spoke on the relation of Greek mathematics to its precursors, and classical analysis was represented by Fueter, Wiener, and Ahlfors. The first two Fields Medals went to Ahlfors and Jesse Douglas, for work in classical analysis.

In this connection, I recall my father being accosted by Archibald in a trolley the afternoon before these Medals were awarded, who asked what he thought of the choice of Fields medallists? My father replied: "The choice is still confidential!" "Nonsense," said Archibald, "Everyone knows that they will be Douglas and Ahlfors."

There were many invited addresses on algebra and number theory. Thus C. L. Siegel lectured on quadratic forms, Hasse on the Riemann Hypothesis in function fields, and Hecke related the theory of elliptic modular functions to Dirichlet L-series, which relate to the Weil conjecture recently proved by de Ligne. Likewise, van der Corput spoke on Diophantine approximation, Mordell on the geometry of numbers, and Élie Cartan about the role of Lie groups in the evolution of "modern" (primarily differential) geometry.

The only invited address concerned with axiomatic or "modern" algebra was by Ore. He described the then very new applications of lattices (which he still called "structures") to the decomposition theorems of algebra.

However, there were also two addresses on functional analysis that were sympathetic to the "modern" abstract viewpoint. Banach discussed the antecedents of the theory of operators on function spaces in the work of Volterra, Hadamard, Hilbert, and Fréchet, and the (G. D.) Birkhoff-Kellogg-Schauder-Leray fixpoint theorem and its applications. Fréchet's talk on abstract spaces was more meandering; while paying homage to some of the great names of the past, he stressed the great activity of many younger mathematicians (including myself) in generalizing old ideas.

The Harvard Tercentenary

Later that summer, Harvard celebrated its three hundredth birthday. It was another gala occasion in which Franklin Delano Roosevelt, Harvard's most famous alumnus, participated. Hardy, Élie Cartan, Levi-Civita, Dickson, and the statistician R. A. Fisher were among the honored foreign invitees; the American Mathematical Society held its summer meeting in conjunction with this celebration in late August.

In his after-dinner speech at the Society banquet in the Copley Plaza Hotel, Hardy stated that the United States had become number one in mathematics, ahead of Germany, France, or England. I think everyone present was thrilled by his statement; only 30 years before, most aspiring American mathematicians had felt the need to study in Europe to acquire the requisite sophistication and depth to do research.

Actually, our country's preeminence, which was to continue for the next quarter-century, was due in large part to political and economic factors. But whatever the reason, it caused modern algebra to enter into a very new and different phase during 1936 to 1960. It is this phase that will be the main concern of my second lecture.

THE RISE OF MODERN ALGEBRA, 1936 TO 1950

Garrett Birkhoff

By 1936, the ideas of the Emmy Noether school of "modern" algebraists had permeated the thinking of quite a few of the most active younger mathematicians in the United States, France, and Russia. In each of these countries, these ideas had been influenced and modified by national traditions. Meanwhile, Emmy Noether had died; Hitler's persecutions had demoralized German universities; and increasing numbers of continental mathematicians were seeking asylum in the British Empire and the United States. For all these reasons, "modern" algebra had become international. But what happened between 1936 and 1950 involved much more than a mere dissemination of German ideas; in the hands of American and French mathematicians, these ideas became transformed into a *new approach to mathematics*. In this paper I shall try to describe some aspects of this transformation of modern algebra that I saw at close range, *as I saw them*. This will give to my account a somewhat personal flavor; I hope you will enjoy it.

You must remember that in 1936, I was only 25 years old; that after four years of undergraduate study of classical analysis and mathematical physics, I had spent four more doing research (mostly on lattices and groups); and that I had not yet done any teaching. In 1936, I fondly expected to continue to do research and to teach "pure" mathematics, hopefully at Harvard, for another 40 years or so. I had met personally, or at least listened to over half of the leading mathematicians of the time, and I was well aware of my good fortune in being attached to a relatively affluent university in a politically stable country. I was firmly resolved to do my best to help both to achieve as high a level of mathematical proficiency as possible.

At the same time, I realized that I was a neophyte, and that I could accomplish little alone. Therefore, I was most grateful for the presence at Harvard of Marshall Stone, of Hassler Whitney, and (from 1934 to 1936 and from 1938 on) of Saunders Mac Lane. Together, I felt, we could make modern algebra, topology, and functional analysis vital components of mathematics at Harvard, alongside of the classical analysis and geometry that I had studied there as an undergraduate.

But I did not realize that within four years political events would change the situation dramatically. The mathematical career that I had imagined as lasting 40 more years in fact lasted only four. After 1940, my own dedication to modern algebra was to become much more diluted and qualified. I became increasingly absorbed in war work (during the years 1940 to 1945) and applied mathematics (from 1945 on). Hopefully, this broader range of interests has given me a better perspective on modern algebra. In any case, I shall try to explain how I view today its evolution from 1936 on, with special emphasis on the years from 1936 to 1940 when I knew it best.

THE AMS SEMICENTENNIAL

The publications of the Semicentennial Anniversary of the founding of the American Mathematical Society in 1938 provide excellent documentation for the transition in mathematical leadership, which was about to sweep the world. In the first place, they described the somewhat provincial (if very original) nature of American mathematics prior to that time. Thus, as I emphasized in my first paper, most mathematicians still regarded modern algebra as a primarily Germanic creation.

A broader view of American mathematics was presented by my father in his lecture "Fifty Years of American Mathematics." My father expressed in this talk some concern at the inundation of our shores by research-oriented European refugees, who might demote young native mathematicians to mere "hewers of wood and drawers of water." And indeed, at the University of Pennsylvania a few years later, German refugees held research professorships, while young American Ph.D.'s had to teach 20 or more hours a week.

The issues involved were very complex. The Institute for Advanced Study, run by Abraham Flexner, became the chief advocate of the preeminence of continental European mathematics and mathematicians. This position was strongly supported, quite naturally, by Oswald Veblen.

My father and many other American mathematicians, on the other hand, were not anxious to see rapid takeover of the best American jobs by European mathematicians, however eminent. As a very junior person, I was not involved in any decision making. However, it was my conviction that the American college and university system, in which there were few "high priests" and research was considered more as a privilege than an obligation, was socially if not intellectually preferable to the continental European system. In particular, I felt that persecutions of college teachers such as occurred under Hitler would have aroused a storm of protest in our country, because our colleges were more human and personal. Conditions are, of course, very different today, and our educational system has become far more dominated by federal and state tax support.

In any case, the natural conflict of interest between young American mathematicians and European refugees never led to much jealousy or hostility that I could see, presumably because a sense of fair play and mutual sympathy dominated most decisions. Another overriding consideration was the well-founded conviction that the United States offered the best place in the world to do scientific work — a state of affairs that was to last for a quarter century at least, but which was altogether unprecedented then. So who could complain?

I shall conclude my comparative discussion of the social milieu for scientific work in Europe and the United States by a few personal reminiscences that may be relevant. I had spent two years as Senior Tutor at Lowell House, in daily contact with undergraduates with whom I usually dined. Another resident there was Heinrich Bruening, the exiled former Chancellor of Germany, with whom I became good friends. A much closer friend was Stanislas Ulam, who lived in

Adams House as a Junior fellow. The Master of Lowell House was Julian Lowell Coolidge, the well-known geometer.

That June I married, going to Europe on a two-month wedding trip. My parents joined us for a week in Ireland; we were invited to lunch by Eamon de Valera, Prime Minister of Ireland, a friend of the mathematician E. T. Whittaker and a former high school teacher of mathematics. Aesthetic measure was discussed at lunch.

My wife and I timed a visit to Cambridge University to coincide with a mathematical meeting at which Fréchet was an invited speaker; he spoke about "structures" (lattices). We visited King's College as the guests of Philip Hall, with whom I also had some stimulating if superficial conversations about algebra.

Although we spent 10 days in Paris, I did not call on any mathematicians there; unfortunately, I did not yet know Henri Cartan, Chevalley, Dieudonné, or André Weil. Neither did we think it safe to take advantage of Stanislas Ulam's invitation to visit a castle in the Carpathians belonging to his uncle. This was the summer of the Czechoslovakian crisis and Munich ("peace in our time"); England was feverishly building airplane factories near the Bristol Channel, as far as possible from German bomber bases.

The summer's most poignant moment was a visit to our hotel one evening by Heinrich Bruening, who brought with him a bunch of carnations for my wife. He had just returned from Holland, where he had gone to the border of Hanover for a secret (and dangerous) rendezvous with his sister, whom he had not seen for many years. The personal tragedy of this kind, world-famous German leader made a deep impression on both of us.

When we returned to the AMS Semicentennial Celebration in New York, where we sat at the head table, it was with a great sense of returning to the security of home and friends. My wife sat next to Lefschetz, in whose coffee (he had clumsy artificial arms) she deposited four lumps of sugar. Dunham Jackson was one of the featured speakers. As Jackson rose to speak, Lefschetz turned to my wife and said: "He's going to tell four funny stories and sit down," which is exactly what Jackson did.

MATHEMATICS 6

Between the Harvard Tercentenary in 1936 and the AMS Semicentennial in 1938, my teaching career had begun in earnest.

The first year was pure apprenticeship. I taught a section of first year calculus and an advanced half course ambitiously entitled "Foundations of Abstract Algebra and Topology." The first was very conventional and moderately successful; it convinced a number of freshmen to concentrate in mathematics. The second was taken by three luckless graduate students, who were exposed to a breathless survey of a great variety of topics, from metric spaces to Lie groups of transformations. Hassler Whitney and I had given an informal seminar on continuous groups the year before, and the subject fascinated me. I had also given a seminar on finite groups as a Junior Fellow, but it was not until 1937-38 that I first taught modern algebra to undergraduates as a full year course numbered Mathematics 6.

This was a major undertaking, intended from the start to change the Harvard curriculum permanently, and to serve as an example to other American colleges. It seemed to me unfortunate that Harvard students, after three years of algebra and geometry in high school, should be faced with three solid years of the calculus in college (Math. 1, 2, 5), the only variety being geometry (Math. 3), mechanics (Math. 4) or possibly probability (Math. 9), before being exposed to modern ideas about algebra.

On the other hand, it was obvious that anything along the lines of van der Waerden would be totally inappropriate for unsophisticated Harvard juniors, let alone even less sophisticated undergraduates elsewhere. Therefore, I carefully prepared about 150 pages of mimeographed notes, which began with sets, combinatorics, and Boolean algebra, and ended with finite groups. I was even so uncivilized as to work on these notes during several half-days in the summer of 1937, while visiting my future wife, not yet even a fiancée, at her family's camp in northern Maine!

The next year Saunders Mac Lane returned to Harvard from Cornell, and taught a quite different Math. 6 in 1938-39. Being an independent and strong-minded character, he began with groups, ended with Boolean algebra, and issued his own notes. I thought it essential to reach some agreement on a stable course content, and we both agreed that it would be desirable to write a joint text that could be used by others, thereby freeing us from the necessity of teaching the course in alternate years in perpetuity. Since the resulting book (1967) has been widely used in many countries and differs essentially from van der Waerden's book, I thought it might be interesting to comment on its basic design and organization as we conceived it at the time.

The first five chapters attempted, in the main, to cover the material in Dickson's *First Course in the Theory of Equations,* but in axiomatic form and modern terminology. Fine's *College Algebra* had been an earlier, also popular text covering much the same material. After studying these chapters, students became reasonably familiar with integral domains and fields in general, and with the real and complex fields, the modular fields Z_p, and polynomial domains in particular. Most important, they became familiar with the technique and style of formal proofs of unique factorization theorems and the like, a side of mathematics almost completely ignored in calculus courses.

We next introduced students to the group concept, bringing out the idea that algebra was *not* exclusively concerned with "numbers"; the axiomatic approach and symbolic method were universal. We continued by showing the power of the axiomatic approach for treating "vector spaces" over arbitrary fields, including an elegant deductive treatment of linear independence and dimension due in part to Hassler Whitney.

There followed three very substantial chapters on matrices with their eigenvectors, canonical forms, determinants, and characteristic polynomials, giving in detail the applications of real symmetric matrices to n-dimensional geometry.

These chapters were drafted primarily by Saunders Mac Lane and polished by me; we both agreed to keep them basis-free (''intrinsic'') and determinant-free as long as possible.

These were succeeded by two rather skimpy chapters, drafted by me and polished by Saunders, on Boolean algebra and transfinite numbers. They were intended to introduce the student to Cantorian set theory, including the all-important distinction between countable and uncountable sets.

With these chapters behind us, it was easy to prove (following Cantor) that almost all real numbers were transcendental, and to acquaint students with some of the beauties of algebraic number theory, including ideal theory and Steinitz' theory of fields. The book concluded with a chapter on Galois theory, in which we proved that the method for solving quartic equations by radicals presented in our fifth chapter had no analog for quintics, because the group of permutations on five letters was ''insolvable.''

I have reviewed the design of Birkhoff-Mac Lane in detail, partly to emphasize how completely it deviated from van der Waerden in spirit and content. Its axiomatic approach was similar,[35] and its discussion of Galois theory comparable. But it was diametrically opposite in its emphasis on the primacy of the real and complex fields, on geometric applications, and in its inclusion of Boolean algebra.

The somewhat earlier *Modern Higher Algebra* by A. A. Albert, which I had reviewed, and Mac Duffee's *Introduction to Abstract Algebra* were equally indigenous. Transplanted onto American soil, ''modern'' algebra had taken on a quite different character.

Lattice Theory

During the years 1937 to 1941 that an Americanized ''modern algebra'' was becoming part of the standard undergraduate curriculum at Harvard, and the first edition of ''Birkhoff-Mac Lane'' was being written and published, many other developments were taking place. One of these was the acceptance in our country of lattice theory as a significant new branch of this same ''modern algebra.''

I have already described my initial efforts (1933 to 1935) to establish and popularize the lattice concept; the extent to which many of my ideas had been anticipated by Dedekind around 1900; and my success in stimulating work by von Neumann and Ore in the subject and its applications. Although van der Waerden's book ignored lattices, it was clear to me that it should not have, because associated with every algebraic system or ''algebra'' were *two* lattices: its subalgebra lattice and the ''structure lattice'' of its congruence relations.

Although Ore and I communicated only rarely, von Neumann and I talked and corresponded extensively, even writing a joint paper in 1936-37 *On the Logic of Quantum Mechanics*, which related complemented modular lattices to physics. This paper was related to von Neumann's ingenious and difficult papers on continuous-dimensional projective geometries published during 1936 to 1938,

35. Even this was based as much on the work of our colleague E. V. Huntington as on any other source.

and to his papers with F. J. Murray on operator theory. In these papers he showed how the (often modular and orthocomplemented) lattice of subspaces of a Hilbert space H that were invariant under a given linear operator $T: H \rightarrow H$ helped to classify the "type" of T.

Quite independently, Marshall Stone had become interested in 1933 in Boolean algebras (which are essentially just complemented distributive lattices) and their applications to topology. He had shown in a long paper published in 1937 that Boolean algebras also can be regarded as a special class of "Boolean" rings having an idempotent multiplication — which implies commutativity. He showed in a second long paper that they can be regarded as defining zero-dimensional compact spaces — a very novel and original viewpoint giving great insight into the difficult theory of infinite Boolean algebras.

Independently also, Karl Menger had considered affine and projective geometries from a lattice-theoretic standpoint already in 1927, although not using the word "lattice" or any synonym for it. He published a long paper developing these ideas further in 1937; in the meantime, I had proved (knowing of Veblen's but not of Menger's related work) the crucial result that every complemented modular lattice was a direct product of projective geometries and a Boolean algebra.

The American Mathematical Society took cognizance of all this work and recognized its common thrust by holding a symposium on lattice theory in conjunction with the spring 1938 meeting in Charlottesville, Virginia. The participants were Ore, Stone, von Neumann, Baer, Menger, and myself.[36]

But by this time, my own ambitions for lattice theory had developed much further, and I had begun writing the first edition of *Lattice Theory,* a Colloquium volume that was published in 1940. In it, I gave considerable prominence to the concept of a *vector lattice,* which I defined as a vector space that was also a lattice under an order relation invariant under (additive) group translation. This concept, implicit in writings of Friedrich Riesz, had been axiomatized in 1936-37 by H. Freudenthal and L. Kantorovich, the latter of whom had shown various interesting interconnections with properties of Banach spaces and other topological vector spaces. I showed the relevance of E. H. Moore's basic concept of "relative uniform convergence" to this complex of ideas, which I fitted into the general framework of lattice theory.

Von Neumann and Stone also were interested in this development, and in the spring of 1940, von Neumann and I gave a seminar on lattices and their applications at the Institute for Advanced Study. That same year, Friedrich Riesz published in the *Annals of Mathematics* a French translation of a very original paper on vector lattices (still often called Riesz spaces, to honor his work), first published in Hungarian in 1939. This stimulated me to develop an even more general theory of "lattice-ordered groups," which was published in 1942.

But I am getting well beyond the period 1936 to 1940, to which this part of my paper was intended to be devoted.

36. See Bull. Amer. Math. Soc., 44(1938):793-827. Note that four of the six participants were European emigrés.

ALGEBRAIC GEOMETRY, 1936 TO 1950

"Modern" algebraic geometry utilizes the theories of (commutative) rings, fields, valuations, and ideals, to give it rigorous foundations over a general ground field, independent of analysis, that is, of the theories of real and complex functions. In the preceding paper, I recalled that van der Waerden had already begun the task of modernizing algebraic geometry by 1925, but that he had decided to omit it from his *Moderne Algebra*. However in 1933, he resumed his campaign of freeing algebraic geometry from geometric intuition and analytical arguments, in a series of papers published in the *Mathematische Annalen*. Finally, in 1938, he succeeded in rigorizing Severi's 1912 interpretation of Schubert calculus, which dealt with generalizations to higher dimensions of Bézout's theorem on the *intersection multiplicitis* of plane curves. If one remembers that Schubert proposed his calculus in 1879, and that to rigorize it was one of Hilbert's famous unsolved problems, one will appreciate the magnitude of van der Waerden's achievement.

Van der Waerden's final success depended on precise definitions (over an arbitrary field) of the concepts of "specialization" and "algebraic family" of cycles that he and Chow had developed in 1937. Moreover, in 1938 Krull invented the concept of a "local ring,"[37] which was also to become essential for "modern" algebraic geometry. Thus as late as 1938, German algebraists were still leaders in "modernizing" algebraic geometry.

But by this time, Zariski had mastered modern algebra with a vengeance, and was beginning[38] to generalize to varieties over general fields most of the major theorem of classical algebraic geometry, including especially those on the resolution of singularities by birational transformation and on local uniformization. He also developed the important notion of a "normal" algebraic variety, that is, one having only a finite number of singular points.

After 1940, our country became the greatest center of activity. This was partly because Weil and Chevalley were here from 1940 to 1946, joining Zariski (and getting moral support from Lefschetz) in a development that was virtually uninterrupted by the war.

During these years, Weil sharpened further the notions of "generic point" and "intersection multiplicity," obtaining rigorous *local* definitions over general fields. Since he had number-theoretic applications in mind, he needed to cover the case of inseparable extensions. This work, published in book form in 1946, extended further the rigorization of Severi's 1912 ideas in the context of arbitrary ground fields, thus making them technically independent of classical analysis. (See André Weil, 1946, *Foundations of Algebraic Geometry,* Am. Math. Soc.)

Weil also continued to develop connections between algebraic geometry and number theory, taking full advantage of observations of Hasse and F. K.

37. W. Krull, 1938, Dimensions theorie in Stellenringen, J. Reiene Ange W. Math., 179:204-226.

38. His first publication along these lines was: Some results in the arithmetic theory of algebraic functions of several variables, Proc. Nat. Acad. Sci., 23(1937):410-441; in the preceding five years, he had been mainly using topological methods, following Lefschetz.

Schmidt about the relevance of the generalized Riemann hypothesis and zeta function.

One gets a vivid impression of the completeness of the conquest of algebraic geometry by "modern" algebra in the period 1936 to 1950 by reading the review of Weil's 1946 book by O. F. G. Schilling (another refugee from Europe), and I. S. Cohen's review of J. G. Semple and L. Roth's 1949 *Introduction to Algebraic Geometry* in Math. Revs. 9(1948):303, and 11(1950):535. For more thorough surveys, one can consult the invited hour addresses by Zariski and Weil at the International Congress of 1950, which are published in 2:77-89, 90-103 of its Proceedings. One should also read the addresses by W. V. D. Hodge and B. Segre 1:182-192, 490-493; Hodge still uses complex projective space in his. For a broad historical overview, see the authoritative accounts by Dieudonné (1972) and van der Waerden (1971).

Algebraic Topology

Under the influence of Emmy Noether, by 1928 Heinz Hopf already was using group-theoretic ideas in topology (see Gött. Nachr., Math.-Phys. Kl., 1928). However, if one reads his treatise with Paul Alexandroff on *Topologie* (1935), one sees how small the influence of modern algebra on combinatorial topology was before 1936. Its preface pays tribute to Emmy Noether's influence, but even more to that of the Princeton topologists Veblen, Alexander, and especially Lefschetz. It claims the distinction of being the first book to contain a unified treatment of general ("point-set") topology and combinatorial topology ("analysis situs").

Its most relevant chapter (chapter 5, pp. 205-239) is on "Betti groups." On pages 168-170, it defines chains over any polyhedral complex with coefficients α_i in any Abelian group A or ring R with unity. It uses the latter to define an algebraic complex. After stating that the book would only use as coefficient domains the rings Z and Z_n, the field Q, and the additive group $[Q;-]/(1)$ of rational numbers mod 1, it states that the authors postulate general coefficient domains, because "the most recent [work] shows that other coefficient-domains are also important."

Using the classic formula $\partial\partial E = 0$, it defines (p. 180) r-dimensional *cycles* as chains $\sum \alpha_j c_j^r$ with boundary $\partial(\sum \alpha_j c_j^r) = 0$. It defines the r-th Betti *homology* group as the (Abelian) group of r-cycles modulo the *subgroup* of cycles that are themselves *bounding* cycles $\sum \alpha_j c_j^r = \partial \sum \beta_k c_k^{r+1}$), boundaries of chains of one higher dimension. However, its approach is far from abstract.

At the Topological Conference in Moscow in 1935, A. W. Tucker described a generalized homology theory over arbitrary *partially ordered sets*, which he renamed "cell spaces."[39] I was impressed by the fact that he was led to Hausdorff's postulates for a "Tielweise geordnete Menge" by a totally different route. I have been unable to relate this early work of Tucker to recent developments of homology theory over lattices due to Rota, Mather, and others.

39. See A. W. Tucker, Cell spaces, Annals of Math., 37(1936):92-100; An abstract approach to manifolds, 34(1933):191-243; also Birkhoff, 1948, 14-15.

Although I have always been attracted by this generalization, the main thrust of generalization of homology theory has been in the quite different direction of homological algebra. An excellent analysis of the origins of this new branch of algebra has been given by P. J. Hilton and U. Stammbach on pages 184-186 of *A Course in Homological Algebra* (1970). It stemmed from attempts to define the homology groups of a connected "aspherical" space from its fundamental group.

Its basic ideas were initiated by H. Hopf, Eilenberg, and Mac Lane in the years 1945 to 1947. In particular, Eilenberg, Mac Lane, and Chevalley developed cohomology theories for group extensions and for Lie groups and algebras; a vivid idea of contemporary thinking can be gleaned from the Proceedings of the 1950 Congress (2:8-24, 344-362). An important by-product was the abstract theory of *categories*, or classes of "objects" and "morphisms" having the general properties of algebraic systems $A, B, C, \cdots$ and homomorphisms f: $A > B$, and so forth between them under composition.

World War II

My own dedication to lattices, groups (both finite and continuous), "general" topology, and "general" analysis (to use E. H. Moore's apt terminology), and indeed to abstract mathematics generally, was rudely interrupted by World War II. During 1936 to 1938 and even during the "phoney" war that preceded the fall of France, I had hoped that the European powers could keep Hitler in check. But by the spring of 1940, it became clear that this would not be the case and indeed, that Hitler might soon rule the world.

At the same time, I also saw with devastating clarity how useless were the beautiful abstractions with which I had become enamored for defeating Hitler; the classical analysis and mathematical physics that I had studied as an undergraduate could be far more helpful. Accordingly, I began to reorient my scientific perspectives, spending some time browsing in applied mathematics, including exterior ballistics and hydrodynamics.

Hitler's invasion of Russia in July 1941 began to make the lineup for the final struggle clear, whereas Pearl Harbor brought an immediate sense of urgency and dedication five months later. From then on, I concentrated increasingly on teaching the calculus, mechanics, potential theory, and hydrodynamics; I also began looking seriously for solutions to mathematical problems that might contribute to the war effort.

At first, it was difficult to find such problems. Our National Defense Research Council (NDRC) had taken no official cognizance of mathematics as such; President Conant and his associates could not see how it would contribute to victory. But within a year, Warren Weaver had obtained sponsorship for a committee including Marston Morse, John von Neumann, and me to analyze the quantitative improvement in the effectiveness of antiaircraft shells that was likely to be achieved by installing in them the then secret "proximity" fuses, activated by radar, that had been developed by Merle Tuve.

Not long after, I collaborated with my father in a study sponsored by the Bureau of Ordnance of the Navy, of the impact on water of bombs and torpedoes released by airplanes. After another year, the scope of the study was enlarged to include Norman Levinson and Lynn Loomis. This began an association with the Navy on problems of naval research that has continued ever since.

My closest contact with wartime military research was as a consultant to the Ballistics Research Laboratory at the Aberdeen proving ground in Maryland. Here I had the privilege of occupying a desk in the office of Robert H. Kent, chief scientist and a very remarkable person. I think it fair to say that he was our most outstanding ordinance scientist in World War II. My first assignment concerned the evaluation of controlled fragmentation characteristics of antiaircraft shells; it was similar to the work I had done for Warren Weaver's committee, and very pedestrian.

My most useful contribution was to help explain the effectiveness of "shaped charge" conical liners in penetrating tank armor when placed in high explosive shells launched even at low speeds (for example, by bazookas). The instant I saw X-ray shadowgraphs of exploding shaped charges with conical liners being shown to "Bob" Kent by "Jack" (John C.) Clark, who had just taken them, I had a sense of *dêjá vu.* I had been discussing very similar schematic drawing of "impinging jet" phenomena with my class in hydrodynamics a few months before, and had observed that the most important relationships could be deduced by high school algebra and trigonometry from the laws of conservation of mass, momentum, and energy. (I was then teaching three terms a year, and could only come to Aberdeen during Harvard's reading periods, examination periods, and brief between-semester interludes.)

The same observation had been made by G. I. Taylor in England shortly before, and we published a joint paper on the subject several years later (G. Birkhoff, D. P. MacDougall, E. M. Pugh, and G. I. Taylor, J. Appl. Phys., 19(1948):563-582).

My desk in "Bob" Kent's office made me privy to other fascinating interchanges. One of these occurred toward the end of the war in Germany. Von Neumann by this time had begun making pioneer numerical experiments on the leading digital computers of the time. He had observed that the kinetic theory of gases predicts essentially the same statistical behavior of a gas from a wide variety of force laws, and was trying to simulate plane shock wave propagation with a "gas" consisting of about 100 molecules in this spirit. He was presenting informally his ideas and results to a small but select group, which included Kent and von Kármán, the famous aeronautical engineer.

When von Neumann had finished his talk, with its dizzying prospects of glamorous future developments, von Kármán said with a mischievous smile: "Of course you realize that Lagrange used just the same model in his *Mécanique Analytique*, nearly two centuries ago." I imagine that von Neumann must have felt somewhat deflated at that moment.

Postwar Reconstruction

With the surrender of Germany in the spring of 1945, wartime tensions began to relax, and I accepted a summer professorship at the University of Mexico. Although it was a wonderfully refreshing change in many respects (I lectured on lattice theory in a totally new and very friendly environment), it was also sad because my father had died the year before, and many of the leading people were working on his theory of gravitation and other problems he had proposed.

By the time I returned to Cambridge with my family, the first atomic bomb had burst over Hiroshima, Japan had surrendered, and the era of postwar reconstruction had begun. Its mathematical aspects were quite different in various countries.

Great Britain, which had staunchly borne the brunt of the war the longest (after Germany) and had been preparing for it since 1937 at least, seemed devitalized except in areas (such as fluid mechanics) where strenuous wartime efforts had been called for. In such areas, she was preeminent.

Russia, which had suffered the greatest human and material losses, nevertheless retained enormous mathematical vitality. Somehow, her scientists had been kept busy at their profession when circumstances permitted it.

German science was badly fragmented, and those mathematicians who had stayed in Germany must have felt the opprobrium with which Germany, as of 1945, was generally regarded. Any remaining vestiges of arrogance quickly disappeared below the surface. The stark reality of an East Germany ruthlessly dominated by Moscow and the devastations of Hitler's last years made almost everyone want a return to normalcy. This normalcy included, incidentally, generous salaries and pensions for a number of exiled mathematicians who had been unjustly dismissed.

The United States

In our country, the period of postwar reconstruction initiated a quarter-century of incredible scientific expansion and affluence, with a sustained "growth rate" of over ten per cent yearly. This period was inaugurated by a great expansion in higher education: thanks to the G. I. Bill of Rights, great numbers of veterans returned to our colleges and universities. Those of us equipped to supervise Ph.D. theses had many able students who were especially interested in areas of mathematics that had been active during the preceding two decades. This resulted in an enormous increase in activity in modern algebra, topology, and functional analysis at the expense of more traditional fields. In particular, the sales of "Birkhoff-Mac Lane" skyrocketed from about 800 to about 2500 copies per year, and classical analysis became dethroned from its position of primacy, even at Harvard.

Indeed, I had an uneasy feeling that with the death of my father, Kellogg, and others, the Harvard Mathematics Department had perhaps become too modern: the revolution that Stone, Whitney, Mac Lane, and I had been striving for in the prewar years might have been too successful. Therefore, after the war, I regularly taught ordinary differential equations and often other courses in applied analysis and mechanics.

Partly for this reason, unlike my colleagues Mac Lane and Whitney, I never resumed full-time activity in the abstract mathematics that had dominated my thinking during 1933 to 1940. But the main reason was my judgment that the "ivory tower" of prewar academe was not likely to return for many years, if ever during my lifetime, and that therefore I should try to contribute to both pure and applied mathematics, and to use ideas from each to stimulate the other — in much the same spirit that this had been done by Poincaré, my father, and H. Weyl. I realized that this was an ambitious plan, and that it would be easy to achieve nothing significant in either area by trying to achieve too much.

I picked fluid mechanics, with which much of my wartime "applied" research had been concerned, as a worthy and challenging area of application. And I made arrangements to spend the spring terms of 1947 and 1948 at the California Institute of Technology and Cambridge University, respectively, two of the world's greatest centers for research in fluid mechanics. The fact the Caltech was a leading center of lattice theory was an added attraction, and I incidentally gave the Walker-Ames lectures at the University of Washington on lattice theory that same spring. I used the opportunity to polish a second edition of my book *Lattice Theory*, which was duly published by the American Mathematical Society in 1948.

I also gave lectures on "universal algebra," concerning which my ideas had developed and matured considerably since 1935, both at the First Canadian Mathematical Congress in Montreal in 1945 and at the Princeton Bicentennial in 1946. The first of these turned out to be anticlimactic for a very peculiar reason. I had prepared what I thought was a fascinating talk, but I unfortunately followed von Neumann. Von Neumann gave a brilliantly optimistic sketch of the potentialities of high-speed, large-scale digital computers, both as substitutes for expensive physical experiments (for example in wind tunnels or shock tubes), and as means for solving nonlinear partial differential equations with which classical analysis could not begin to cope. When he sat down there was a thunderous ovation; one of the audience rose to say that this was the most fascinating talk he had ever heard in his whole life. After this, any topic from pure algebra would have seemed like an anticlimax. The recollection of von Neumann's deflation by von Kármán gave only meager consolation.

Groups and "Hydrodynamics"

As has often been the case with me, my postwar activities were influenced considerably by a surprise invitation to give the Taft lectures at the University of Cincinnati, and to publish them in book form. I decided to give them on hydrodynamics, basing them in part on wartime impressions of the superficiality of many academic rationalizations and partly on some connections with the group-theoretic notion of "dynamic similarity" with which my studies of model experiments had made me familiar. I used my visit to Caltech in 1947 and my Guggenheim Fellowship in 1948 to deepen my knowledge; by the time I completed my manuscript, I had visited a large fraction of the greatest hydraulic and aeronautical laboratories of the Western world.

The last three chapters of the book, which dealt with dimensional analysis, the notion of a self-similar solution and other applications of symmetry (that is, group) concepts, and connected virtual mass (Kelvin and Kirchhoff) with the geometry of the Euclidean group manifold (suitably metrized), represented a sustained attempt to demonstrate the relevance of abstract mathematics for natural philosophy. These chapters attempted to do for fluid mechanics, in some small way, what Felix Klein had done for geometry in his Erlanger Programm, and Weyl and Wigner had done for quantum mechanics.

Actually, I found that the notion of "self-similar" solution had been anticipated by the Russian mathematicians L. I. Sedov (Doklady URSS, 47(1945):91-93, 52(1946):17-20), and K. P. Staniukovich (Doklady URSS, 48(1945):310-312). For their ideas, see their books, which have been translated into English: L. I. Sedov, 1959, *Similarity and Dimensional Methods in Mechanics;* K. P. Staniukovich, 1959, *Unsteady Motion of Continuous Media*. Notable further extensions of these ideas have since been made by L. V. Ovsjannikov, who has published a brief synopsis of the new results of his book, *Group Properties of Differential Equations*, in the *Atti del Convegno Lagrangians* (1964).

In the United States, although my book as a whole was popular, only recently have pure or applied research mathematicians begun to develop further the ideas presented in these chapters.

In 1949, stimulated by J. Kampé de Fériet, I made a second valiant attempt to relate modern algebra to hydrodynamics by applying algebraic concepts to an identity $T(fTg) = (Tf)\ (Tg)$ on "averaging" operators first postulated by Osborne Reynolds in his classical studies of turbulence. Its implications were studied further by Mme. Dubreil-Jacotin, Gian-Carlo Rota, and others.

Bourbaki

No story of the rise of modern algebra would be complete which ignored the role of Bourbaki. As everyone knows, this was a pseudonym adopted in the 1930's by a brilliant group of young French mathematicians that initially included Henri Cartan, Claude Chevalley, Jean Dieudonné, and Andre Weil. Although I knew of Bourbaki, of course, he seemed quite remote until after the fall of France.

By this time, he had already begun publishing his celebrated treatise on mathematics, which began with three books on set theory, algebra, and general topology. Its "abstract and axiomatic" style of exposition was very like that of van der Waerden. However, its announced aim was much more comprehensive: "to give a solid foundation . . . to all of modern mathematics," based firmly on a rigorous treatment of "the fundamental structures of analysis." Curiously, the definition of 'structure,' promised in 1939, seems not to have been forthcoming until 1957 (Act. Sci. Ind. #1258, Book 1, ch. 4).

Though Bourbaki has never written his autobiography, rumor has it that under his banner a revolt was instigated in the 1930's against the firm control by aging mathematicians, born in 1870 or before of French mathematical journals, professorships, and the French Academy. This feeling that a revolution was needed must have been exacerbated by mossback refereeing that, rumor also had it, had forced Chevalley to publish his famous 1935 paper on idèles in Japan.

Bourbaki published the first four chapters of Book 3 in 1940 to 1942; characteristically, the description of it of the real field was deferred until after the reader had been taught H. Cartan's theory of "filters," A. Weil's theory of "uniform spaces," and the elementary theory of topological groups!

After the fall of France, Chevalley and André Weil came to this country as refugees. I even had the honor of going bond for Chevalley, guaranteeing to our government that he would not become a public charge. Weil taught at Lehigh, but without much liking for our American system of education. With difficulty, J. R. Kline (Secretary of AMS) persuaded Weil not to thank the Guggenheim Foundation in the preface of his Colloquium volume on algebraic geometry for rescuing him "from the indignity of teaching the American undergraduate."

Among major European countries, only France emerged from World War II with renewed mathematical vitality. Perhaps this was because after the German half-occupation and the establishment of the Vichy government, her able younger mathematicians had not much they could do except to think about their favorite subject. Whatever the reason, Bourbaki's admirable treatise underwent great expansion during the wartime and postwar years.

Whereas Mac Lane and I had tried to temper the purism of van der Waerden's "modern" algebra in our book, Bourbaki was ultramodern. For instance, his book on *Linear Algebra* discusses vector spaces after modules, tensor products, and projective and inductive limits of modules. And his first theorem about vector spaces states: "Every vector space (over a field K) is a free K-module." Matrices come much later.

Bourbaki's abstractionist philosophy, when originally presented in articles by J. Dieudonné, Revue Sci., 77(1939):224-232, and H. Cartan, Revue Sci., 81(1943):3-11, had evoked little enthusiasm — probably because everyone was worrying about Hitler. In essence, it was that mathematics is the study of axiomatically defined structures, among which a few "mother structures" such as groups are especially fertile. However, the thesis seemed perfect to many pure mathematicians in America in the complacent postwar years. He and André Weil were invited to write feature articles for the *American Mathematical Monthly* in 1950.

Bourbaki reiterated his philosophy. His peroration was the clearly elitist statement: "If he [the mathematician] be reproached with the haughtiness of his attitude, if he be summoned to do his part, if he be asked why he perches on the high glaciers whither no one but his own kind can follow him, he will answer, with Jacobi: For the honor of the human spirit."

In the next issue, in a similar vein of condescension, Weil mentioned van der Pol's equation (really Rayleigh's)[40] as "one of the few interesting problems which contemporary physics has suggested to mathematicians; for the study of

40. See G. Birkhoff and G. C. Rota, 1962, Ordinary differential equations, 2d ed. Ginn, Boston, 143.

nature, which was formerly one of the main sources of great mathematical problems, seems in recent years to have borrowed from us more than it has given us."

Bourbaki's lofty philosophy captured the imagination of a large fraction of the younger mathematicians of those times. And it foreshadowed the axiomatic purism of the "New Mathematics" that swept our country in the 1960's. If mere *users* of mathematics object to the logical order of exposition and great generality, let them realize their intellectual inferiority and bow down.

Weil's article ends with a sharp criticism of American mass production in education. One wonders what he would have written had he known that, by 1972, the United States would be "producing" 1500 mathematical Ph.D.'s annually, all supposedly dedicated to research.

THE INTERNATIONAL CONGRESS OF 1950

The most definitive landmark of postwar reconstruction in mathematics was the International Mathematical Congress that took place in Cambridge in September 1950.[41] This was the first really large-scale international gathering of mathematicians since 1936. It had originally been planned for 1940, with my father as President and David Widder as Chairman of the Organizing Committee. Funds had been raised, a number of loyal Harvard alumni making individual gifts of $1000.

After my father's death, Veblen was named to take his place, and Widder asked me to take his. I realized that much work would be involved, but decided to be a good sport and accept anyway, partly impelled by curiosity. In fact, the work took about an hour a day for two to three years, but I have no regrets. The cooperation of Ted Martin from M.I.T. made the job much less onerous than it would have been otherwise.

One of the major tasks was to secure additional money, especially for travel grants for distinguished mathematicians who otherwise could not come. I only worked on one part of this, getting substantial funds from UNESCO. There was no official channel for doing this; the logical channel would have been an International Mathematical Union (IMU), but none had been formed in the 1920's because the major mathematical countries had disagreed on including the defeated Central Powers.

Upon reflection, I decided that $25,000 was a fair contribution, in view of the importance of mathematics, the fact that it would receive no other funds from UNESCO, and that it was the most promising occasion for forming an IMU to round out ICSU, UNESCO's International Council of Scientific Unions. "Bill" (now Sir William) Hodge, the most active British mathematician on such matters, agreed completely.

We called on the two British UNESCO representatives at that time (1948). The first was the distinguished Chinese scholar Joseph Needham, who told us that UNESCO was very poor, but that it might scrape up $6000 to $8000 from the

41. See Proc. Int. Math. Congress, 1950, Am. Math. Soc., 1(1952):6.

bottom of the barrel. We proceeded through other channels. Fortunately we did get $25,000.

However, this had to be divided with Marshall Stone, who was playing a major role in planning the IMU. After a brief discussion, we agreed to split it fifty-fifty.

Part of the negotiations with UNESCO took place in Paris; my wife and I were staying at the same hotel as J. M. Burgers, the distinguished applied mathematician and fluid dynamicist. He said he was greatly relieved that the IMU would not get organized until 1950. He feared that otherwise it would continue to include theoretical mechanics as mathematics had done in the past.[42] As it was, he would have time to organize IUTAM (the International Union of Theoretical and Applied Mechanics) as a separate adherent to ICSU, which he did.

Foreign Participation

Securing broad foreign participation in the Congress was a major concern. The cold war had begun, and Poland and Yugoslavia were the only Iron Curtain countries to send delegates.

For the same reason, it was hard to get visas for known communists, even Trotskyites like Laurent Schwartz. We thought that if Schwartz did not get a visa, the French would probably boycott the Congress, and under nascent McCarthyism, it seemed doubtful if he could. However J. R. Kline, devoted AMS secretary, found out that a certain Washington legal firm with connections in our State Department (under Truman) could get one or two visas through for us *sub rosa,* at a cost of $1000 each. We paid for two visas.

Italy was also feeling touchy, perhaps because Zariski and Weil rather than an Italian had been invited to give hour addresses on algebraic geometry. To placate the Italians, I agreed that we would pay first-class fare for Severi to come to the Congress — nobody else got more than tourist fare, and most only half (the other half being supplied by their own country).

Lighter Moments

The Congress had its lighter side. Bourbaki tried to register, and I strongly favored including and listing him as a dues-paying member, but I was overruled by J. R. Kline.

Again, the Rumanian financier Metaxas, who had earlier founded a prize won by Leray and Schauder, half-offered to set up a $50,000 prize in mathematics, along the lines of the Nobel prizes. However, his reputation was unsavory (he had been denied an American visa), and he did not really put any money "on the line." Partly to avoid the danger of fishing in muddy waters, and partly because Veblen expressed himself as unenthusiastic about prizes, the matter was dropped.

42. Prandtl first introduced "boundary layer theory" at the International Mathematical Congress in Heidelberg in 1904.

Invited Addresses

An analysis of the subjects of the 20 invited addresses (leaving out history and philosophy) will show how much the abstract point of view had come to dominate mathematics since 1936. Gödel spoke on "rotating universes in general relativity theory," but he was surely invited as a logician. Of the remaining 19 papers, five were on algebra and number theory (Albert, Davenport, Ritt on differential algebra; Weil and Zariski on modern algebraic geometry). Hodge spoke on algebraic geometry, in the tradition of Lefschetz.

There were three addresses on topology as such (S. S. Chern, H. Hopf, W. Hurewicz), two on global analysis (H. Cartan, M. Morse), and two on functional analysis (L. Schwartz, S. Kakutani). Moreover H. Whitney talked on "r-dimensional integration in n-space, "developing ideas first presented under the title "algebraic topology and integration theory" — and incidentally ignoring the earlier work of Carathéodory, Hausdorff, and Besicovich on the subject. Thus about 75 per cent of the 20 "core" talks were centered on ideas from algebra and topology.

Of the remaining five talks, two were on probability and statistics (A. Wald, N. Wiener); von Neumann's was on shock intersections (thus, five years after his talk in Montreal; he should have been asked to talk on computing). Thus only two invited addresses were on classical analysis; moreover Beurling never provided a manuscript (a weakness of his). Thus Solomon Bochner, whose paper is included in our symposium, gave the only published invited address dealing with classical analysis.

The Nascent Influence of Computers

If Emmy Noether could have been at the 1950 Congress, she would have felt very proud. Her concept of algebra had become central in contemporary mathematics. And it has continued to inspire algebraists ever since.

However, a new revolution in algebra already was brewing then. Much as the axiomatic approach revolutionized *pure* algebra in the period 1890 to 1930 rose to supremacy in 1930 to 1950 and has dominated it the 25 years since then, the digital computer has revolutionized *applied* algebra in the years 1945 to 1975, and it may well reign supreme over all algebra by 2000 — although this remains to be seen. I thought it might be interesting to conclude by describing, at least sketchily, how this revolution already had begun by 1950.

Today, so-called computer science deals in depth with at least three areas of algebra: the theory of automata, algebraic coding theory, and numerical linear algebra. The origins of all three can be traced to before 1950. Although very few pure algebraists are cognizant of these areas yet, the first two build on ideas of modern algebra, and so it is especially appropriate to discuss them here.

These areas are in addition to the use of Boolean algebra to design the *logic networks* of computers. Such uses of Boolean algebra were foreshadowed in a 1938 paper by Claude Shannon on switching circuits for relay networks; the relevance for logic networks of Boolean algebra, which used to be called the "algebra of logic," is hardly surprising.

The *theory of automata* had its origins in a famous 1936 paper by the logician and mathematician Turing. This paper reformulated the notion of "definability" in terms of computability by a class of automata now called *Turing machines*. Remarkably, theoretical models of (sequential) digital computers still resemble Turing machines in outline.

Algebraic coding theory is another fascinating new area of applied algebra. It is a by-product of the "information theory" initiated by Shannon in 1948 (see Bell System Tech. J., 47(1948):379-423). Shannon proved a number of basic existence theorems describing the maximum rate at which information could be reliably transmitted through unreliable channels. These solved theoretically the problem of "synthesizing reliable organisms from unreliable components," a problem that strongly influenced von Neumann's work on computer design. (For this, see vol. 5 of von Neumann's *Collected Works*.)

Unfortunately, Shannon's proofs do not lend themselves to simple electronic implementation, and good coding and decoding schemes are essential for transmitting information efficiently and reliably through imperfect channels. The best such schemes known today are based on properties of (binary) finite groups and fields, hence on modern algebra in the sense of van der Waerden.

Besides inspiring new areas in modern algebra, high-speed computers have revolutionized linear algebra. I cannot resist smiling at the following passage from Artin's review[43] of Bourbaki's book on *Algèbre Linéaire:* "the computational aspect is not neglected: §60 is a complete discussion of matrices." This was not true even in 1950; Bourbaki ignored many ideas familiar to Gauss.[44]

Actually, if solving simultaneous linear equations were as easy as Bourbaki makes it sound, solving most linear partial differential equations would be trivial. For example, a fairly good approximation to the Laplace equation is obtained by constructing a fine square mesh, and making the value at each point the arithmetic mean of the four neighboring values. In principle, this reduces solving the Dirichlet problem, to any desired accuracy, to solving a (very large) system of simultaneous linear equations.

It was precisely this problem that I proposed to David Young in 1948, having in mind solution by computer. In 1950, after two years of patient work and thought, David Young finally had discovered and rationalized his automatic "Successive Overrelaxation" (point SOR) algorithm, which converges an order of magnitude more rapidly than the methods of Gauss. The essence of the Dirichlet problem is contained in the repeated use of the "residual"

$$\sigma_{i,j}^{(r)} = u_{i,j}^{(r)} - \frac{1}{4}\left[u_{i-1,j}^{(r+1)} + u_{i,j-1}^{(r+1)} + u_{i+1,j}^{(r)} + u_{i,j+1}^{(r)}\right]$$

43. E. Artin, Book review of Eléments de mathématique by N. Bourbaki, 1953, Bull. Amer. Math. Soc., 59:474-479. The parts reviewed had appeared in 1942, 1948, 1959, and 1952.

44. For what Gauss knew, see A. M. Ostrowski, Determinanten mit überwiegender Hauptdiogonale und die absolute Konvergenz von linearen Interationsprozessen, Comm. Math. Helv., 30(1956):175-210.

and a well-chosen ''overrelaxation factor'' ω, to define a new approximate value

$$u_{i,j}^{(r+1)} = u_{i,j}^{(r)} - \omega\sigma_{i,j}^{(r)}$$

Using modifications of this ''point SOR'' algorithm, one can solve 10,000 equations in as many unknowns in minutes on a high-speed computer.

In work published before 1950, von Neumann and Goldstine[45] had posed a much more general basic problem: given a *random* nonsingular $n \times n$ matrix A, and a computer in which real numbers are stored with r significant digits, when and how can the vector equation $Ax = b$ be solved with s-digit accuracy? This problem has many variants: given s and r, how large can s be made? Given r and s, how large can n be made? The answers, naturally, depend on the matrix A considered.

In this generality, the problem posed by von Neumann and Goldstine remains unresolved to this day. However, they introduced and resurrected a wealth of algebraic concepts: the notion of a random matrix A and the probable error of solving $Ax = b$; the ''condition number'' of a matrix; the importance of preserving symmetry (destroyed by Gaussian elimination) in computations relating to symmetric matrices; and the fact that ''iterative'' methods may be more efficient than ''direct'' methods.

Like successive overrelaxation, these ideas play a central role in ''modern'' linear algebra. They initiated a revolution that began around 1945, stimulated by new vistas in computing. For more adequate descriptions of how ideas have changed, I refer you to two great classics on modern matrix computation: R. S. Varga, 1962, *Matrix Iterative Analysis*, Prentice-Hall; and J. H. Wilkinson, 1965, *The Algebraic Eigenvalue Problem*, Clarendon Press, Oxford, These books, many ideas of which were known before 1950, describe the revolution in linear algebra that has occurred in the last 25 years; they are unrelated to anything in van der Waerden or Bourbaki.

Together with the new vistas in abstract algebra opened up by the theories of automata, algebraic coding, and other computer-oriented ideas, they made axiomatic algebra cease to be truly ''modern'' around 1950, primarily because of the advent of high-speed computers.[46]

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45. See J. von Neumann, 1963, Collected works, vol. 5, Pergamon Press, 411-610; Numerical inverting of matrices of high order, with H. H. Goldstein, Bull. Amer. Math. Soc., 53(1947):1021-1099.

46. In this connection, see also Birkhoff, 1973; Proc. IV, SIAM-AMS Symp. Appl. Math., Computers in algebra and number theory, 1972:1-48, where I have presented my views in more detail.

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MATHEMATICAL AMERICANA

Salomon Bochner

My first awareness of American mathematics came soon after enrolling at the University of Berlin in the late fall of 1918. In the early part of 1919, Leonard Dickson's three-volume work, *History of the Theory of Numbers* (1919 to 1923), appeared, the scholarship of which made a deep impression. For instance, it was a rumor among students that the proofreading of the galleys took Dickson two whole years.

It is a curious feature of this work, which I discovered only much later, that it apparently nowhere states, certainly not with due emphasis, the "fundamental theorem" on prime numbers, namely, that every natural number is a product of primes, and uniquely so. The work does have statements that correlate properties of a natural number to properties of its prime number factors, but it does not state that every natural number is a product of prime numbers, and uniquely so. This reluctance to present the fundamental theorem, in an otherwise very conscientious historical account, may be due to the fact that, although this theorem is sometimes vaguely attributed to Euclid, it apparently had not been stated expressly before 1801, when Gauss featured it in his *Disquisitiones arithmeticae* rather early in the work. The nearest that Euclid himself came to this theorem was his proposition (vii, 24): "if two numbers be prime to any number, their products also will be prime to the same."

G. H. Hardy and E. M. Wright, in their *An Introduction to the Theory of Numbers* (1938), made the following statement, which was triggered by remarks I made to G. H. Hardy in Trinity College, Cambridge, England, sometime in 1933.

> It might seem strange at first that Euclid, having gone so far, could not prove the fundamental theorem itself; but this view would rest on a misconception. Euclid had no formal calculus of multiplication and exponentiation, and it would have been most difficult for him even to state the theorem. He had not even had a term for the product of more than three factors. The omission of the fundamental theorem is in no way casual or accidental; Euclid knew very well that the theory of numbers turned upon his algorithm, and drew from it all the return he could.

This is a twentieth century insight, whereas Dickson's book was rooted in Victorianism; and the difference is a telling one. Altogether, the work of Hardy and Wright is a rich source book for the history of mathematical ideas, however technical it is.

Of course, what had discomfited Dickson was not that the fundamental theorem does not occur in Euclid, but, much more so, that none of the great number theorists in the seventeenth and eighteenth centuries, such as Fermat, Wallis, Euler, Lagrange, and others, had chanced upon it either. This may have been a general malaise among Victorian number theorists from which Dickson was unable to free himself. However, some textbooks in the nineteenth century did present and analyze the fundamental theorem in a forthright and satisfactory manner. But even when they expressly credited the fundamental theorem to Gauss, they did so in a very circumspect manner, as if to leave for themselves an escape tunnel in case somebody should suddenly discover that the theorem had been known explicitly in the eighteenth century after all.

But their circumspection was not called for. It was a very natural development of mathematical ideas that the fundamental theorem reached the stage of explicit articulation only in the nineteenth century. In my book, *The Role of Mathematics in the Rise of Science* (1966:16), I put it thus:

> Arithmeticians have been showing embarrassment over the fact that the express formulation of the [fundamental] theorem came so late, and they have been trying to 'pre-date' it. But there is no substance to assertions that the fundamental theorem had been consciously known to mathematicians before Gauss, but that they had neglected to make the fact known. We think that the 17th, and even the 18th century was not yet ready for the peculiar kind of mathematical abstraction which the 'fundamental theorem' involves; just as only the 19th century was comfortably prepared to conceptualize satisfactorily the notions of real number, limit, derivative, convergence, *etc.*

I am stating this because, although Leonard Dickson was not yet receptive for this kind of explanation, Charles Sanders Peirce, who was almost a generation older than Dickson, had been. Offhand, I could not quote a reference in C. S. Peirce to this kind of comprehension because Peirce never wrote any kind of comprehensive treatise and his purely mathematical writings have not been published yet. But they are being edited by Carolyn Eisele in four or five volumes, and some day I hope to trace this sort of thinking to Peirce, who was very advanced in many things of this kind.

Something about Dickson's *History* makes it a very old-fashioned number theory, and if one wants to have a concise summary of this old-fashioned number theory, then one may take to hand an article, from around 1928, by E. Bessel-Hagen, in the so called Pascal's *Repertorium der höheren Mathematik* (1928). But if one wants a good whiff of modernity, of genuine modernity, not just of surface radicalism, then one may consider the 1923 book on number theory by E. Hecke, *Vorlesungen über die Theorie der algebraischen Zahlen*, which, I think, never has been translated. One of the first statements in the book is a formulation and proof of the then abstract theorem that a finite abelian group is a product of cyclic ones. Before Hecke's book, this beginner's theorem was presented only in the midst of applications, usually in the theory of the cyclotomic equation for primitive roots of unity. Also around 1923, there was still something ancillary about abstract theorems of this kind. Thus among my fellow students in Berlin, there was some brow knitting over the fact that Heinz Prüfer dared to initiate his academic career in mathematics with a doctoral thesis in 1921 entitled "Unendliche Abelsche Gruppen von Elementen endlicher Ordnung," on nothing other than the structure of abelian groups, albeit his groups were infinite ones. Some of us students even took it for granted — I don't know why — that the great algebraist Issai Schur, who was Prüfer's thesis advisor, was rather lukewarm towards this work.

Returning to Dickson, it must be said that he was also quite advanced in some specialized directions. Thus, Issai Schur, in a middling advanced classroom course, referred emphatically to Dickson's book, *Linear Groups with an Exposition of the Galois Field Theory* (1901), which appeared in English in the prestigious Teubner Collection in 1901 as the source book for Galois Fields. Around that time, very few books of this collection were in any language other than German. Several years after my student days, van der Waerden's *Algebra* appeared, in which the Galois Field theory occupied several pages; the book of Dickson was soon eclipsed and forgotten.

Another book by an American of which I became aware very early was *Elementary Principles in Statistical Mechanics* by Josiah Willard Gibbs (1902); it was translated immediately into German by Ernst Zermelo, creator of the axiom of choice. Zermelo performed the translation very admirably even though Gibbs did not quote Zermelo even for an important theoretical objection, the so-called Wiederkehreinwand, to the assertion that the entropy is steadily increasing. This objection, raised by Zermelo in 1896, argues that the usual kinetic model of a completely and permanently isolated gas behaves quasiperiodically in time, and thus should not be endowed with a quantity like the entropy whose growth in time is in no way periodic. An earlier objection, the so called Umkehreinwand raised by Joseph Loschmidt in 1876, is the crude argument that a mechanical system, and thus also any physical process described by a mechanical system, is reversible with regard to time, whereas the growth of the entropy is not. For references and details, see Paul and Tatiana Ehrenfest, *The Conceptual Foundations of the Statistical Approach in Mechanics* (1912).

I do not remember whether I heard the name of Gibbs in a lecture course or not, but I do remember most distinctly that it was in a lecture course of Issai Schur that I was introduced to the name of Joseph Henry Maclagan Wedderburn as that of the true expert on "Schiefkörper," that is "noncommutative fields." Schur also mentioned that Wedderburn was at Princeton, in the United States; this was the first time that I was introduced to the name of the place with which I would become associated for so long a time.

I do not think that Schur at that time introduced "hypercomplex system," or some such expression, and I am most certain that he did not speak of an algebra as a mathematical object with certain properties. Algebra was a part of mathematics, namely, the part dealing with symbols, and an algebra was, around 1920, still totally unfamiliar to a student on the Continent, like myself. In fact, an algebra, as a term and object, is, in my present-day view, the most outstanding nineteenth century native American contribution to present day mathematics, and I must briefly report on how I found out about it. I heard the term for the first time in 1925 at Oxford at a small seminar taught by G. H. Hardy and attended by, among others, Besicovitch, Oppenheim, perhaps Titchmarsh, Echo Dolores Pepper (a young American girl on a fellowship), and myself. She came from Chicago, and she gave at this seminar a lecture on algebras. She spoke in a well-prepared, well-articulated, self-confident manner, but none of us were remotely aware of the terms she used, and we were lost.

To my astonishment and education, ten years later in 1935, there appeared a German book by a young algebraist, no more than 28 years of age, Max Deuring, entitled *Algebren* (*Algebras*). The word algebren in German sounded harsh and unappealing, and yet when I turned the pages in the book, the references were to mathematicians all over the world, with American mathematicians perhaps not even dominating.

This did not still in me the quest for knowing when the concept of an algebra had really come into being. However, only about 10 years ago did I make an earnest attempt to answer the quest. I had to go back surprisingly far, until I discovered Benjamin Peirce's famous essay, *Linear Associative Algebra*, first published, quite informally, in 1870-71, and based on lectures given in 1866 to 1870 to the National Academy of Sciences, and then published formally in volume 4 of the *American Journal of Mathematics* in 1882.

I now wish to make some external observations to this article. It not only speaks of an algebra, but it also uses the plural "algebras" probably for the very first time anywhere in the English language. It also created the terms idempotent and nilpotent in our present day sense. It tried to introduce a nilfactor, which was used sporadically, but this is presently a zero divisor. Correspondingly, it also tried to introduce an idemfactor to indicate a solution of $ab = a$ for $b \neq 1$, but this apparently has not caught on at all.

Furthermore, and very importantly, the edition of the essay in the *American Journal* is annotated by Charles Sanders Peirce, the son of Benjamin Peirce, by far the greater intellect of the two. He created American pragmatism, which I do not understand at all and which is, objectively speaking, not easy to define or characterize. In a different direction, C. S. Peirce created algebra of relations, or algebra of "relatives," as he called it. This is probably his most specific achievement of a mathematical turn and probably his most durable.

I want to discuss three Americana that I have chanced upon, one from the twentieth century, and two from the nineteenth century. The one from the twentieth century is very peculiar, one that is hardly realized. Carathéodory published three major works: first, his large book on real variables (1918); then a book on the calculus of variations (1935), which appeared in the early 1930's; and finally, a two-volume work on functions of one complex variable, *Funktionenetheorie* (1950), which was published posthumously. Actually, Carathéodory was planning the book on complex variables ahead of the book on the calculus of variations, but what was delaying the book on complex variables was a widespread presumption among analysts in the 1920's that any such book had to contain a complete and rigorous proof, without any prerequisites, for C. Jordan's theorem that a simple closed curve in the plane decomposes the plane into two disjoint parts, an interior and an exterior. The analysts in the 1920's knew that this was a "topological" theorem, which had been extended by L. E. J. Brouwer in 1911 from a simple closed curve in two-dimensions to an $(n - 1)$-dimensional topological sphere in n dimensions. Also, B. V. Kerékjártó (1923) published an extensive work on two-dimensional topology in which the Jordan curve theorem and all other two dimensional prerequisites of complex function theory as then needed were treated in great rigorous detail. But, for all that, the complex variable analysts of the time still were vying with each other in the quest for producing a proof to end all such proofs for Jordan's theorem; Carathéodory experimented for many years with a proof of his own.

What finally did persuade Carathéodory, however reluctantly, to dispense with such a proof of his own, was a grand tour of the United States that he made in 1930. He was received almost regally in all the great places on the East Coast, the West Coast, and in the Midwest. At Princeton, he met James Alexander, who had recently not only given a very elegant topological proof of Jordan's theorem for $(n - 1)$-dimensional spheres in E_n, but had even succeeded in giving a meaningful extension to lower dimensional spheres in Euclidean E_n, something European mathematicians did not have in their vision at all. This finally gave Carathéodory the excuse for abandoning efforts of his own, and the published version of Carathéodory's work accepts two-dimensional topology as known.

Another American mathematical event in the twentieth century that had a sobering effect on Carathéodory was the great work of Marston Morse around 1928 (*cf*. Morse, 1934). Until then, Carathéodory had mastered everything and anything ever produced in the calculus of variations, but the Morse theory was something I think he did not understand. I remember an evening, sometime in 1931 or 1932, in Carathéodory's home with S. Lefschetz at which a student of Carathéodory tried to present Morse's main paper. It was obvious to me that nobody present understood much. I looked into the face of Lefschetz trying to gauge how much he understood, but he was very good at not giving himself away.

Now I would like to report on the two very separate Americana from the nineteenth century. For the first, I must return to my earliest student days. One of the first mathematics books I ever bought, perhaps literally the first one, was Hermann Weyl's *Die Idee der Riemannschen Fläche* (1913) or, as the translation has it, *The Concept of a Riemann Suface*. I studied this book for years on end, and many a piece of work of mine originated from its influence. But I never have been able to assimilate the Rieman-Roch theorem. On page 137 it states as follows:

On a surface of genus p, if d is a divisor of order m ($m = m_1 + \cdots + m_r$; $d = p_1^{m_1} \cdots p_r^{m_r}$, $m_q \neq 0$), if B is the number of linearly independent (in the complex sense) differentials which are multiples of d; and if A is the number of linearly independent meromorphic functions which are multiples of $1/d$, then

$$A + (p - 1) = B + m.$$

This is undoubtedly an aspect of a very comprehensive duality theorem, which lies athwart all of mathematics. Schematically, as I see it, the most important typical aspects of the comprehensive duality theorem are the following ones. If both sides of the duality are continuous sums, then the duality is a functional equation of the Riemann-Hecke type for zeta functions in number theory, and the functional equation is somehow a key to the Riemann hypothesis. If in the duality, one side is a continuous sum and the other is discrete, then in the simplest classical case this is the relation between a periodic function and its Fourier series, or, almost equivalently, it is the Cauchy residue formula; and in a very modern recondite version it seems to be the general Selberg trace formula.

(The inversion formula for Fourier integrals, in which the sums on both sides are continuous, is a limiting case of this, and does not belong to the Riemann-Hecke functional equation.) I think the role of this formula is clear. It is, in classical physics, the duality between an electromagnetic field on the one hand and electric and magnetic "discrete" charges on the other hand. And in quantum physics, it is the much more trenchant Janus-like de Broglie duality between the undulatory and corpuscular aspects of the same elementary particle valid simultaneously.

But in the Riemann-Roch theorem, both sides are discrete sums, and both finite; and the relation to the other aspects of the duality theorem is not easy to state. (The reciprocity formula for Gaussian sums, in which both sums are finite, does not belong here, but is a degenerate case of a Jacobi modular relation for theta functions that underlie the Riemann-Hecke functional equation; see my paper: "Remarks on Gaussian Sums and Tauberian Theorems," 1951.) Undoubtedly the Riemann-Roch duality is linked to the duality between gradience and cogradience, and homology and cohomology, both of which have been stated first by Poncelet in projective geometry and by Grassmann in his nascent tensor calculus; but these links are somehow too obvious and not too enlightening to me. In my search to understand the Riemann-Roch theorem, I have looked at the references in the footnotes of Weyl's book (1913). In the English edition (pp. 136-137), there is a reference to E. Ritter, who, in 1894, was one of the first to give a proof of the Riemann-Roch theorem even for fractional divisors. In Weyl's book in a footnote (p. 147), E. Ritter even is credited with formulating an extension of the entire Riemann-Roch theorem from system of differentials and functions that are univalent on the closed Riemann surfaces to such differentials and functions that are defined on the covering space of the Riemann surface, but are reproduced by the elements of the Poincaré monodrony group by a fixed multiplicative character of the group. Weyl added that in a later paper (*Mathem. Annalen,* 1896), E. Ritter even further generalized this to solutions of certain types of differential equations on the Riemann surface.

This sounded to me like the work of a very good mathematician, and I wondered why I did not hear from Ritter otherwise. So, one day I decided to look up the last quoted paper, and I had a surprise. The very last sentence of the paper announced a continuation, but a footnote by the editor stated that there would be no such continuation. The author, Dr. Ernst Ritter, had accepted a call to Cornell University at Ithaca, but on the way to Ithaca on 23 September 1895, he succumbed to a typhus attack. Also, the footnote announced a detailed obituary in the *Jahresbericht der Deutschen Mathematicker Vereinigung* that was written by F. Klein, and was, oddly enough, dated 25 September 1895. It made me unhappy that Cornell and the United States missed out on such a gifted mathematician.

Ritter was only 28½ years old when he died. He had become a *Privatdozent* at Göttingen only two terms before receiving the call from Cornell. His appointment was the means by which Cornell had hoped to become aligned with other mathematically forward-striding American universities.

The last item of Americana is about a mathematician at the other end of the nineteenth century, at the beginning of it. He did arrive in this country and was very active here. In fact, he was even hyperactive in a sense. He did manage to carve out for himself a niche, an extremely tiny one, but a niche in the hall of immortal mathematical fame, so that he was something of a pioneer of American mathematics, beyond question. Yet it is extremely difficult to assess what it is that American mathematics really owes to him, durably, that is.

His name was Robert Adrain; the best and most accessible source of information about him is an article by the somewhat redoubtable Harvard mathematician Julian Lowell Coolidge, entitled "Robert Adrain, and the Beginning of American Mathematics," in *American Mathematical Monthly* (1926) in which earlier biographical information is listed. Of interest is also a subsequent, brief appraisal in the *Dictionary of American Biography* (1:109-110) by the able historian of mathematicians, David Eugene Smith, who taught at Columbia University.

Adrain was born 30 September 1775 at Carrickfergus, Ireland. He came to the United States around 1800, and lived on his farm in New Brunswick, New Jersey, until his death on 10 August 1843. He must have been a tempestuous person, because he was frequently changing jobs, even though they were not always better ones. He was a master in the academy of Princeton, New Jersey; a principal in a similar school in York, Pennsylvania, and later in Reading, Pennsylvania; a professor at Rutger's College (then called Queen's College) in New Brunswick, New Jersey; a professor at Columbia College, New York City; then again at Rutgers; then at the University of Pennsylvania where he even became vice provost; a teacher at the Columbia College grammar school, yes grammar school, following which, he retired, or was retired, to his farm at New Brunswick. For the most part, he published only middling-interesting mathematics, mainly through books and in a short-lived journal of his own, called the *Analyst*, or *Mathematical Companion* (1808); but even this middling-interesting activity produced, in a journal called the *Mathematical Correspondent* (founded in 1804 by George Baron), a curve called by Adrain the catenaria volvens, which half a century later was rediscovered, quite independently and very systematically, by the leading algebraic geometer Alfred Clebsch.

But Adrain's actual claim to fame was the presentation of two versions of what the nineteenth century called "the exponential law of (accidental) errors (of observation)," one of which even appeared in print a year before the publication of the version of Gauss, by whom the law was subsequently known and usually named. The nineteenth century produced many versions of the exponential law of errors (see Emanuel Czuber, *Theorie der Beobachtungsfehler*, Leipzig, 1891); Adrain's versions are perhaps among the least satisfactory ones (see the article of Coolidge). But Adrain did achieve a measure of firstness in the matter. Although adumbrations of the law reach back into the eighteenth century to the work of De Moivre (see also I. Todhunter, *A History of the Mathematical Theory of Probability from the Time of Pascal to that of Laplace,* 1865), D. E. Smith may be forgiven for the extravagant statement: "If, instead of being a self-made mathematician, he had come under the influence of men like Laplace, Legendre, and Gauss, (Smith might have added the name of Poisson), he might have been a great leader. As chance had it, he was in a mathematical desert."

It is rash to assert, as D. E. Smith seems to do, that Adrian received no mathematical stimuli from the ecological setting in this country. The fact is that throughout the nineteenth and into the first decades of the twentieth century, there was in this country a broad interest in and cultivation of the kind of mathematical probability and statistics into which the nineteenth century law of errors naturally falls. The best evidence for this is the fact that even the above-named gentleman-mathematician, J. L. Coolidge, wrote a book on this subject. By mathematical avocation, Coolidge was a geometer, as evidenced by a string of textbooks; all were published by Clarendon Press, Oxford, and they were as follows: 1) *The Elements of Non-Euclidean Geometry,* 1909; 2) *A Treatise on the Circle and the Sphere,* 1916; 3) *The Geometry of the Complex Domain,* 1924; 4) *Algebraic Plane Curves,* 1931; 5) *A History of Geometrical Methods,* 1940; and 6) *A History of the Conic Sections and Quadratic Surfaces,* 1945; and 7) *The Mathematics of Great Amateurs,* 1949. Amidst all such books of a rather uniform trend, he also wrote a very different book in 1925, likewise at the Clarendon Press, entitled *An Introduction to Mathematical Probability,* which also had chapters on errors of observation, errors in many variables, indirect observations, the statistical theory of gases, and, note, on the principles of life insurance. In the introduction the author stated: "The present work is based upon the lectures which I have delivered, usually in alternate years, at Harvard University." This book, however hodge-podgey it may seem to a mathematician today, was still very much in the style of 1925, and, in fact, the book of Coolidge was quickly translated into German, and I myself so read it.

But it is a curious fact that only a few years afterwards, this kind of book simply went out of style; and it is not the fault of J. L. Coolidge that very soon after the appearance of his book, it began to collect dust on the shelf. Suddenly, different conceptions of the cognitive nature, the role of probability, and the role of statistics and their mutual relations came into being around 1930, and a different kind of textbook became necessary. All of a sudden, "exponential law of errors," which in nineteenth century statistical activity was a household word, a state of mind, as it were, almost disappeared from textbooks and their subject indexes; what was left of it was simply some versions of the law of large numbers, more or less, and of some other asymptotic laws, perhaps. It should be added that very soon after the changeover, entirely different American crews joined in the "new directions," both in probability and in statistics, and have been on top of developments since.

In some peculiar manner, the American folk genius must have had some kind of awareness in the late 1920's that an important phase of mathematical statistics had come to an end, because an American professional statistician even wrote a valedictory summary on this entire period. It was called *Studies in the History of Statistical Method, with Special Reference to Certain Educational Problems;* the author was Helen M. Walker, then Assistant Professor of Education, Teachers

College, Columbia University. It is also a remarkable feature of this book, and a comment on the still very nineteen-centuryish American outlook of it on life and education, that the educational aspect in the title of the book is taken very seriously indeed in the body of the book. Thus Chapter VII has the title, ''Statistics as a Subject of Instruction in American Universities, and the sections of the chapter are: 1) Purpose and method of the chapter, 2) Instruction in statistics in 1890, 3) First college courses in statistics, 4) Pioneer courses in various departments, 5) Outline of the development of the teaching of educational statistics in America, 6) The present situation, and, note, 7) Remarks by Florence Nightingale concerning instruction in statistics.

Returning to Adrain, I would like to repeat that it is not at all certain that by being in this country he was lost in a mathematical desert. It is even more hazardous to speculate that by staying in Europe he might have come under the influence of men like Laplace, Legendre, and Gauss, and thus become a great leader. Adrain's true activity fell into the period between 1790 and 1830, an Age of Upheaval in the terminology of the *New Cambridge Modern History* (I myself called it, for my purposes, The Age of Eclosion, and placed it from 1776 to 1825). This era is one of the most difficult and recalcitrant to analyze and comprehend with regard to intellectual developments and spontaneities, and with regard to the influence of intellectuals upon each other. By what is known but not comprehended about this era, it is equally valid to speculate that it was precisely this relative isolation in the United States that brought out whatever originality there was in Adrain; if he had stayed in Europe, this originality he was endowed with might have been nipped in the bud altogether or smothered and choked off entirely. If it is true that in the United States this era was in some respects a desert, then in Europe it was, in the same respect, a suffocating jungle; it would be rash to say that Adrain would have fared better in a jungle than in an alleged desert.

What was reminiscent of a jungle in Europe at that time was an overwhelming outburst and uncontrollable proliferation of all kinds of knowledge on all levels. Leaving at first (and I mean at first) aside mathematics and mechanics in all its parts (mechanics of point systems, and of continuous and of hydrodynamic and elastic media), a whole spectrum of sciences, natural, social, and even humanistic, then came into being; or else one may state that they did organize themselves in such a way as if this were their true beginning. Finally, during that period there was the true beginning of physics proper (thermodynamics, electricity, magnetism, even of optics, ancient as this discipline was); chemistry (beginning with Dalton and Avogadro); geology; biology (Lamarck); psychology (Herbart); systematic sociology (Auguste Comte); even economics, certainly mathematical economics, or, at least economics built on models; linguistics; the so-called ''higher criticism'' in all kinds of ancient literature; the Old Testament and Homer among others; and various specialized histories, like history of astronomy, of medicine, of Roman law; and so forth. (To all this, compare my book *Eclosion and Synthesis*, 1970.)

Even in mathematics the developments during that period were much more radical, more difficult to account for than commonly realized even if, externally, the developments did not appear to be as tumultuous as in other areas of knowledge, scientific or other. During this era, mathematics and mechanics separated from each other; the conception of an existence theorem, a uniqueness theorem, and a necessary and sufficient condition finally was born. One need only compare the spirit of and approach to analysis in Lagrange and Cauchy, who were in a common French tradition, to see how radically analysis was reoriented. Lagrange and Cauchy simply did not speak the same mathematical language, whatever the similarity of vocabulary and grammar. For instance, no matter how much Lagrange may have asserted and insisted that a function was for him an "abstract" mathematical object, in his thought patterns it somehow was residually a mechanical orbit or perhaps a physical function of state, whereas for Cauchy, orbits, forces, and pressures were functions, as for us today. The history of this transition in mathematics must of course be pursued to the limit of possibility as a problem of the genetic unfolding of mathematics as mathematics. But it is also a fact that this transition in mathematics has parallels to transitions in many other areas of academic pursuits; and in order to round out the comprehension of the transition in mathematics, such other transitions must be taken into account as well.

The leading transition mathematicians were Laplace, Gauss, and Poisson. The provenance of Laplace is easy to see; he was in a continuing French tradition, from Lagrange to Cauchy. Poisson, although also a French mathematician, is much harder to understand and to place. And it is a curious fact that he is the only French mathematician of consequence whose *Oeuvres Complètes* have never been edited, and about whom there is no decent scientific biography. Gauss, finally, is totally inexplicable as to his "ethnic" origin. His only mathematical "forbear" would be Leibniz; it is simply impossible to say where Gauss came from, mathematically, that is. He was suddenly there, just as some of the greatest prophets of the Old Testament were suddenly there.

The assertion that Gauss, for all his greatness, was the figure of a transition period is borne out by the fact that although he already felt the mathematical need for giving rigorous proofs, he was not yet in an intellectual frame of mind to define satisfactorily the convergence of a sequence or of a series of numbers. This was done only by Cauchy, who had the unusual distinction of being the first to give finally a reasonably satisfactory definition of continuity as it presently occurs in mathematics.

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MATHEMATICS IN COLONIAL AND EARLY REPUBLICAN AMERICA

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The history of mathematics can be presented in different ways. One is the ''skyline'' route, in which one concentrates on the high points: the great mathematicians and the great discoveries. This is the way most books on the subject are organized. Another way is the development of mathematics as a social phenomenon, as an aid to physics, astronomy, and other sciences, or as the subject of education; here one can study also its influence on the general world outlook of a generation, a class, or a special group of men.

The ''skyline'' approach to this period does not lead us very far. The histories of mathematics do not deal with it. The searcher for a path to the skyline can find some satisfaction in Franklin's magic squares, Adrain's derivation of the normal error law, and Bowditch's discovery of the figures commonly named after Lissajous, but little more. The first time the skyline is reached is with Benjamin Peirce's *Linear Associative Algebras* (1870) or perhaps with some of the astronomical mathematics of G. W. Hill.

More profitable is the other approach. In this case, it leads mainly to mathematics of use in astronomy, surveying, and hydrography. We shall confine our attention to North America, inclusive of Mexico.

European mathematics came to the New World with Columbus in the form of computations with the decimal position system expressed in symbols not very different from the ones presently used. More advanced mathematics came in the wake of the Conquest, as can be seen in the booklet written by Juan Diez, probably one of Cortes' chaplains; this booklet of 1556 (reprinted in 1921) deals with assaying, but has some arithmetic and algebra suggestive of knowledge of Diez's contemporary, Cardan. Among the scholars in and around the University of Mexico, which opened in 1553, one finds men with knowledge of surveying and navigation equal to that of the best contemporary Europeans. Outstanding is Enrico Martinez, a German, who, around 1600, excelled as an engineer, astronomer, linguist, and polymath in general; a kind of Mexican Stevin. He is remembered primarily for his labors on the drainage system of Mexico City, where there is a monument of him on the main plaza. Passing salute goes to Thomas Hariot, visitor to the present North Carolina in the 1580's and author of a famous description and map of his discoveries in Virginia. He was a young man at the time; his fame as a mathematician came later, but had nothing to do with America.

When, after 1600, the Atlantic coast of North America was being colonized, there were many settlers with a university education, something often meaning no more, mathematically speaking, than some knowledge of the rule of three. Few of these men ever crossed the *pons asinorum*. (Euclid, Elements 15. The angles at the base of an isosceles triangle are equal.) This also may have been true for the teachers at the two newly founded colleges, the Puritan one at Cambridge, and the Jesuit one at Quebec. Some Jesuits probably knew more mathematics, or at any rate did appreciate it. In 1665 French born Martin Boutet, Sieur de St. Martin, was appointed professor matheseos at the Quebec college. Here was taught a course in hydrography as well as the mathematics deemed necessary for navigation, surveying, and cartography. The Jesuits were a little ahead of the Puritans in mathematics, but the Puritans were more receptive to new theories, willing to listen to Copernicus and Descartes and later to Locke, whereas Quebec preferred Aristotle. Both parties paid attention to formal logic, Quebec again adhering to Aristotle, Harvard more to Ramus (the Protestant educator of Paris); but at that time logic was not thought of as a mathematical discipline.

Although I know of no published mathematical work of his, Carlos de Siguënza y Gongara (1645-1700) of Mexico, the poet-cartographer-astronomer-historian-polemicist, should be mentioned.

The first at Harvard to show a deeper interest in mathematics was Thomas Brattle (1658-1713), merchant-astronomer, working "here alone by myself, without a meet help in respect to my studies," as he wrote to Flamsteed in 1703. He had been able to cross the asses' bridge, and that done, he wrote that the rest of geometry came easy, and trigonometry followed. He used the telescope, a gift to Harvard in 1672 by John Winthrop II (son of the first governor of Massachusetts and later himself governor of Connecticut); his observations on the famous comet of 1680 were appreciated by Newton, and he may well have been the first to determine astronomically not only the latitude (already roughly known), but also the longitude of Boston by observing a lunar eclipse. In the same period, the position of Quebec was established astronomically in 1685 by the visiting French cartographer Jean Deshaies. The correct position of Mexico City was known at least from the middle of the seventeenth century, but it was not published; thus, until late into the eighteenth century, Mexico City was placed west of Acapulco in the Pacific. The Spanish were not eager to inform other nations about their empire.

During the intellectual stagnation of the Spanish empire and the later revival stimulated by Carlos III, there was little interest in mathematics in eighteenth century Mexico. Quebec, and especially Harvard, however, were improving their appreciation of mathematics; the leading figures were Isaac Greenwood and John Winthrop IV. Father J. P. DeBonnecamps taught hydrography at Quebec between 1743 and 1758. There was also an intellectual group at Philadelphia, its Quaker atmosphere tolerant to new ideas; here, during the first half of the century, was the impressive James Logan, magistrate, botanist, physicist, author, and aristocrat; his correspondence showed critical understanding of Huygens' dioptrics and of Newton's algebra and fluxions. A Pennsylvanian Maecenas, he encouraged the young mechanic Thomas Godfrey (of Godfrey's quadrant, predecessor of the sextant) and Benjamin Franklin. As a result of talking to Logan, Franklin set up his magic squares, first the 8 by 8, then the 16 by 16 one, although he thought them *difficiles nugae* (difficult trifles). Most people would not disagree with Franklin on this.

Greenwood was the first Hollis professor of mathematics and natural philosophy (1728), a combination that lasted until the nineteenth century. He was the first teacher in America of Newtonian philosophy, giving lectures with demonstrations of the "discoveries of the incomparable Sir Isaac Newton"; these lectures may well have included some algebra and fluxions. He wrote *Arithmetic, Vulgar and Decimal* (1729), the first separate treatise on arithmetic written by a native British-American. (The first still existing American arithmetic in English is part 2 of *The Young Man's Companion,* published as a second edition in 1710 by William and Andrew Bradford, New York. The first edition was in 1705.) He lost his Harvard job in 1737 for "gross intemperance," and spent his later years as a traveling lecturer.

John Winthrop IV, Greenwood's successor to the Hollis chair, was, luckily for Harvard and Newton's prestige, full of the social graces, and continued to teach in the Newtonian tradition until his death in 1779. His was primarily applied mathematics, especially to astronomy, but he also taught pure mathematics, including fluxions. From a letter he wrote in 1764, it is known that, apart from hydrostatics, mechanics, optice, astronomy, he taught: "the elements of Geometry, together with the doctrine of Proportion, the principles of Algebra, Conic Sections, Plane and Spherical Trigonometry, with general principles of Mensuration of Planes and Solids, the use of globes, the calculations of the motions and phenomena of the heavenly bodies according to the different hypotheses of Ptolemy, Tycho Brahe and Copernicus . . ." as well as cartography, surveying, and navigation. Specialization was not an eighteenth century weakness. Winthrop's library survives and reflects a good acquaintance with the mathematics of his time, except the continental mathematics of Leibniz and Euler. They had to wait at Harvard until Farrar introduced them in the early nineteenth century. Winthrop was still a typical British scholar. He also was, according to Count Rumford, who as a young man listened to some of his lectures, "an excellent and happy teacher." But his influence on the development of mathematics in America seems to have been minimal.

Another astronomer with mathematical interest was the self-taught clockmaker David Rittenhouse of Philadelphia, also an able surveyor. In later life he published some mathematical papers; the most interesting one (1793) deals with what he called "the sums of the several powers of the sines," that is,

$$\int_0^{\pi/2} \sin^n \psi d\psi,$$

all in Newton's style of writing. This was new to Rittenhouse, and Newton apparently never published these sums, but the result is at least as old as Pascal. In another paper Rittenhouse (1799) used series for a solution to Kepler's equation $M = E - e \sin E$.

De Bonnecamps' mathematics also was applied. At Quebec College he taught hydrography and made astronomical observations with instruments less satisfactory than those available to Winthrop and Rittenhouse. One of his pupils was Michel Chartier de Lotbinière, known as the architect of Fort Carillon, now

Ticonderoga, built in the mathematical tradition of Vauban. Another fortress built in this school was Louisbourg.

This brings one to the military engineers and that other group of men applying mathematics in their trade: the scientifically trained surveyors and cartographers in French and British service. Well known in their days were Joseph Frédéric Wallet des Barres, Samuel Holland, Bernard Romans, and William Gerard De Brahm, all active in the last decades of the eighteenth century, the first two and especially des Barres, author of that famous cartographic work, *The Atlantic Neptune* (1780). Some of these men came from Europe and stayed, others returned after some years.[1] Joseph Bernard Chabert took accurate observations with the then modern method of lunar distances and eclipses of the Jupiter satellites along the North Atlantic Coast during 1750-51, with Louisbourg as base. He later returned during the American Revolution and, with the geodesic surveyor Jean-Charles Borda, tested chronometers. From England came the most famous surveyors of America's eighteenth century, Charles Mason and Jeremiah Dixon; they were active in the determination of the Pennsylvania-Delaware-Maryland line. The influence of these men on the growth of interest in applied mathematics is difficult to estimate; it has been studied only in special cases, as in that of Mason and Dixon, and Pennsylvanian surveyors. The same holds for the foreign observers of the transit of Venus in 1769, although it is known that the participation of the Abbe Jean-Baptiste Chappe in the California expedition had some influence on Mexican science.

The new republic had several colleges (Harvard, Yale, William and Mary, Pennsylvania, Princeton, Rutgers, Bowdoin) and many academies. The college in Quebec had disappeared with the Conquest and the expulsion of the Jesuits, the latter event also was harmful to the instruction of science in the Spanish Empire. (However, only the mathematics of the young United States will concern us here.) The colleges, however, showed little interest in mathematics. Harvard, in 1803, required for entrance the mere rudiments of arithmetic; in 1816, the whole of elementary arithmetic; and in 1819, a light knowledge of algebra. Not until 1837 was arithmetic dropped from the freshman course. The situation in other colleges was not much better. However, after 1800 some good teachers appeared and gallantly engaged to raise the level of mathematical knowledge. Irish born Robert Adrain (1775-1843), who taught at Columbia, Philadelphia, Princeton, and Rutgers; New England born John Farrar (1779-1856), who modernized mathematical instruction at Harvard; Theodore Strong, who taught at Rutgers, and some French teachers such as Claude Crozet at West Point represented a new element in American mathematics: the influence of France and its Revolution.

All through colonial days, the only European influence had been that of Great Britain with its strict Newtonian tradition. With the American Revolution came

1. An aide-de-camp to Montcalm in the Canadian campaign of 1758-59 was young Louis-Antoine de Bougainville, a protégé of D'Alembert and already the author of a two-volume text on the integral calculus, at that time the best exposition of the continental approach. He returned to France and became famous as an explorer. Although he had no influence on American mathematics, he probably was the first one to set foot on North American soil with a thorough knowledge of continental mathematics. Think of him when you see Bougainvilleas.

the admiration of, or at any rate the interest in, France and its advanced mathematical schools. When, under Jefferson's influence West Point was established, the Paris Ecole Polytechnique, with its emphasis on mathematics, served as example. Strong, Farrar, and Crozet brought French mathematical texts to the attention of their students. It was Farrar, since 1807 the occupant of the Hollis chair at Harvard, who introduced into his instruction French material through English versions or translations. Between 1818 and 1829, he introduced presentations of material by Lacroix, Legendre, Bězout, Biot, and Euler, beginning with Lacroix's *Elements of Algebra*. At West Point, Crozet taught descriptive geometry, Monge's brain child.

Legendre's *Elements de geometrie,* first published in 1794 as a then modern approach to Euclid, was one of the most influential textbooks of the period. It was translated several times into English, first by Farrar in 1819; another translation, brought out anonymously by no other than Thomas Carlyle during his early years (*ca.* 1820), was very successful in a revision of 1828 by the West Point professor Charles Davies (1798-1876). Davies wrote many other textbooks, among them *Analytical Geometry,* an English version of a book written by Pierre Bourdon. In 1843-44 Harvard first made geometry a requirement for admission, this through the influence of Benjamin Peirce (1809-1880), professor since 1833 and a prolific writer of textbooks, beginning in 1835 with a *Treatise on Plane Trigonometry.*

But it was Laplace, in particular through his *Mecanique Celeste* (5 vols, 1799 to 1825), who stimulated the awakening creativity of American mathematicians, as shown in the works of Adrain and Nathaniel Bowditch (1773-1838). Adrain's best known paper, with the derivation of the normal law of errors (1808), was inspired by Laplace, and so were his articles on the shape of the earth. Bowditch, a Salem merchant-skipper and after 1823 a well-to-do Boston insurance executive, showed his dedication to Laplace by commenting on and translating the first four volumes of *Mecanique Celeste* into English (1829 to 1839), a labor of love paid for from his own pocket.

Among Bowditch's original work can be mentioned a paper suggested by the apparent motion of the earth as seen from the moon. Here he found the figures now known as those of Lissajous. It was published in 1815 in the "Memoirs" of the *American Academy of Arts and Sciences* founded in 1780 in Boston. This shows that the time had come when mathematical papers could be published in an American periodical. The first such periodical was the *Transactions of the American Philosophical Society,* established at Philadelphia and first published in 1771. Yet all though the first half of the nineteenth century and even later there were few such periodicals. Several attempts to publish such journals, even on a modest scale, had little success, from Adrain's *The Analyst* of 1808 (with his paper on errors) to *The Cambridge Miscellany of Mathematics, Physics and Astronomy,* started in 1842 by the Harvard men Benjamin Peirce and Joseph Lovering. The time had not yet come for a deeper interest in mathematics in the many academies and the growing number of colleges.

A few other names of some importance in these first decades of the nineteenth century should be mentioned. Benjamin Banneker (1731-1806), a Maryland astronomer, and a friend of the Ellicott family of merchants and surveyors, was the first black man in America to achieve distinction in science. Charles Gill, a Yorkshire man who came to America in 1830 at the age of 25, was a teacher and an actuary; he edited a periodical *Mathematical Miscellany* (Flushing, New York, 1836-39) and made several contributions to number theory. Of great importance for the U.S. Coast Survey after 1816 was Swiss-born Frederic Hassler, and for the mathematics instruction at the Naval Academy after 1845, William Chauvenet. Other names can be found in *A History of Mathematics in America Before 1900* by Smith and Ginsburg (1944). Significantly, their chapter on the introduction of modern mathematics into the United States deals with the period of 1875 to 1900, long after our period has come to an end. Our period deals with the British influence, typical of the colonial era, and the French, typical of the mercantile and early industrial period of the Republic. The period 1875 to 1900 is that of the German influence, and of the developed industrialism after the Civil War.

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SOME EARLY AMERICAN MATHEMATICIANS

PHILLIP S. JONES

While completing studies of the teaching of mathematics in the early American colleges, I met three intriguing persons who seemed to typify themes in American collegiate education. These were the Scot, Walter Minto (1753-1796), who was the third professor of mathematics (1787 to 1796) at the New Jersey College at Princeton; Claudius Crozet (1789-1864), a Frenchman who was brought to the United States to be professor of engineering (1816 to 1823) at the Military Academy at West Point; and Truman Henry Safford (1836-1901), a native American who was professor of mathematics and of astronomy (1876 to 1901) at Williams College. Minto represents the early dependence of the colonies upon Europe, Great Britain in particular. Crozet represents both the shift from British to French sources and influences typical of the early nineteenth century, and also the rapid growth at that time of technical schools associated with the development of railroads, canals, and mining. Safford, born and educated in this country, represents both the influence of astronomy and geodesy in early American mathematics and the beginnings of a modern concern for research and publication.

Safford, born in Royalton, Vermont, was a calculating prodigy, mentally multiplying four digit numbers and finding the square and cube roots of ten digit numbers at the age of six, and at 14 calculating the elliptic orbit of a comet. He graduated from Harvard at the age of 18 and stayed on as an observer and later became director of the Cambridge Observatory. From there he went to Chicago as professor of astronomy and director of the Dearborn Observatory until it was destroyed by fire in 1871. After joining Captain Wheeler's survey of the Far West (1874 to 1876), he became professor of astronomy and mathematics, and later (1879) librarian at Williams College where he built an observatory and established a Ph.D. program. He, himself, was awarded a Ph.D. by Williams in 1878.

Most of Safford's rather lengthy list of research publications are about astronomy. They were printed in English, German, and American journals; in fact he had articles in each of the first three volumes of the *Bulletin of the New York Mathematical Society,* which became the *Bulletin of the American Mathematical Society.* Even his article on combinations of Pythagorean triangles was inspired by a previous note by Sir George B. Airy, the Astronomer Royal.

Mathematically, Safford's greatest contributions were probably in the realm of the teaching of mathematics. His *Mathematical Teaching and Its Modern Methods* (1887) was the first "methods" book published in this country if we discount Charles Davies' *The Logic and Utility of Mathematics, with the Best Methods of Instruction Explained and Illustrated* (1851). Safford's book was the text for a senior elective course at Williams. The alternate to it was a course including exercises in Gauss' *Theoria Motus Corporum Coelestium.*

Safford regarded mathematics as a part of physics, and, according to Ernest B. Skinner's memorial address, he was "not wholly in sympathy with the new mathematical school, chiefly in certain branches of abstract higher algebra recently established at the Johns Hopkins University." His views had more than a local effect, because he, together with Simon Newcomb, William Byerly, Florian Cajori, and H. B. Fine, was a collegiate member of the Conference on Mathematics; they were appointed in 1892 as a subcommittee of the important and influential Committee of Ten on Secondary School Studies. The composition of this first national committee on mathematics in the schools is interesting. Newcomb was noted as an astronomer. Byerly's 1873 Harvard thesis on the heat of the sun was the first American doctoral thesis in mathematics. Cajori wrote extensively on the history and teaching of mathematics, and Fine laid the foundations, as teacher, department chairman, and dean, for advanced work in mathematics at Princeton.

The United States Military Academy, founded in 1802, also was regarded as a center for engineering and mathematics at that time. Claudius (or Claude) Crozet, a graduate of L'École Polytechnique, was brought over to teach there. There was no text in descriptive geometry, a subject invented by Gaspard Monge to solve the problem of defilement in the design of fortifications and kept as a military secret in its earliest years. Crozet wrote the first English text on this subject, published in New York in 1821.

Crozet lived an interesting and varied life as railroad and tunnel designer and builder, state engineer, teacher, and college president, but had little further connection with mathematics.

Walter Minto, the first of my triumvirate, published only one work in this country, *An Inaugural Oration on the Progress and Importance of the Mathematical Sciences* (1788). Before coming to this country he had written *Researches into Some Parts of the Theory of Planets, in which is Solved the Problem, to Determine the Circular Orbit of a Planet by Two Observations* (1783), and, with the Earl of Buchan, *An Account of the Life, Writings, and Inventions of John Napier of Merchiston* (1787).

Throughout all of these, Minto showed an extensive familiarity with a wide range of mathematical works of his time and earlier. His first book, *Theory of Planets,* is dedicated to Joseph Slop de Cadenberg, professor of astronomy at Pisa with whom he had studied and shared observations of the new planet (Uranus) recently discovered by William Herschel. The book also contained some of Slop's observations, theory, and computation.

Minto was enlisted to share in the writing of the second book by David Steuwart Erskine, Earl of Buchan, whose interest in Scottish history and genealogy led him to propose the publication of a series of definitive biographies of Scots. There is no explicit demarcation between the work of Buchan and Minto in this book. It seems probable that Buchan wrote the initial biography and the first two sections on arithmetic and Napier's bones. Sections III through VIII and the five appendices deal with logarithms and trigonometry. Although Napier's approach to logarithms is sketched, there is more space given to "a species of calculus called the exponential . . . invented by John Bernoulli . . . founded on two principles: 1. . . . $xLa = La^x$ . . . and . . . 2. The fluxion of the logarithm of a quantity is proportional to the quotient of that quantity or $La = \dot{a}/a$."

It seems likely that these materials were written by the young and impoverished Minto, who was employed by Buchan for this purpose. Buchan undoubtedly knew some mathematics. His early education had been conducted by his mother, who had studied under Colin Maclaurin. His further education had been by private tutors and attendance at the University of Glasgow. Neither his education nor his other interests and activities would seem likely to have given him the background in knowledge and the familiarity with the fairly extensive literature cited by the writer in the latter part of the book.

Recent investigations of materials in Edinburgh, Princeton, and Ann Arbor, Michigan, have added to our knowledge of Minto's life, interests, personality, and relationship to his time, but show very little more of his mathematical accomplishments. He was born in County Merse, Scotland, on 6 December 1753 to a poor but respectable family possibly of Spanish background. He attended the University of Edinburgh 1768 to 1771 but did not take a degree. This was quite common, according to Charles P. Finlayson, Keeper of Manuscripts, because at that time professors' personal certificates were thought better than a degree.

He and an impecunious friend had decided to travel abroad in the guise of pilgrims when Minto was offered a position as tutor to the sons of George Johnstone, who were being sent to Italy while the father went to America on official business.

His period in Italy (1776 to 1782) terminated with some ill will on the part of the boys' father, who, foreseeing war with France, sent for them upon his return from America. They left tardily and were captured by the French. Minto returned to Scotland, leaving the boys with friends in that country.

The Earl of Buchan wrote in a manuscript, now in the University of Edinburgh Library, that he met Minto at the Orange Society. The William L. Clements Library of the University of Michigan in its Minto papers has Minto's Orange Society card dated 20 May 1786, which states "The bearer is a friend to Liberty and the Protestant Religion." Buchan also wrote that when he sought out Minto at his lodgings, he found him in a room about the size of "the tub of Diogenes the Cynic, smoking a segar and reading Newton's *Principia*."

Minto's correspondence with Buchan both before and after leaving Scotland for a hoped-for job in America shows an interested and observing traveler. He wrote of new cotton mills, balloon ascensions, and the beauties of Loch Lomond in Scotland. Once, while reading proof for their book, he objected to a title page that attributed the M.A. degree to him inasmuch as he had no degree. He suggested that Buchan pay for a degree if he thought it necessary; Buchan did arrange for Minto's L.L.D. from the University of King James the Fourth in Aberdeen.

After his arrival in America in 1786, Minto wrote of travels to Albany and Philadelphia; of Indian names for the constellations; of his marginal existence on the salary earned as principal of Erasmus Hall, Flatbush, Long Island; and, finally, of his illness on a trip to Maryland, which preceded his receipt of an offer of a professorship at the College of New Jersey for which Buchan had recommended him.

He complained of the slowness of English astronomers in publishing a paper that he had submitted, and wrote for copies of his book as soon as it came out. He sent them to Washington, Franklin, and Benjamin Rush.

His certificate of membership in the American Philosophical Society is signed by Benjamin Franklin and David Rittenhouse, among others. He also belonged to the Fire Company (he had married and bought a house), the Presbyterian Church in Princeton, and the St. Andrew's Society of New York.

One of his first acts at Princeton was to ask that five pounds be deducted from his salary and set up as a prize for an essay on "The Impolicy of Capital Punishment or Slavery."

There are a few manuscript lecture notes in the library at Princeton that give some view of the range of his teaching; his inaugural oration further elaborates the nature and breadth of his view of mathematics.

The major concerns of a historian of mathematics should be to depict the growth and development of mathematical ideas, the genetics of mathematical concepts, the prerequisites for a new advance or creation, the motives that stimulate mathematicians, the methods by which mathematics is created and expanded, and the changing views that mathematicians have of their own subject. This brief review of three early American mathematicians contributes only slightly to these goals. However, students and citizens need to be shown the humanness of mathematicians and the lively humanism of this changing and growing subject. Perhaps these tales of the early days of American mathematics will advance those goals.

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THE NEW ELEMENTS OF MATHEMATICS BY CHARLES S. PEIRCE

CAROLYN EISELE

The draft of a letter from Charles S. Peirce to Georg Cantor in the Charles S. Peirce Manuscript Collection at the Houghton Library of Harvard University is but the first link in a chain of evidence that makes Peirce an important nineteenth-century man of mathematics in the United States. The letter, dated 23 December 1900, states in part:

> I yesterday succeeded in borrowing Volumes XLVI and XLIX of the *Mathematische Annalen* containing your wonderfully beautiful and masterly memoir, which I have read but have not had time to digest. I am nothing but a farmer living in the wildest part of the Eastern States; although our National Academy of Sciences has most indulgently honored me with one of its chairs. So my isolation accounts for my not having read that great work before. I had already read the translations in Vol. II of the *Acta Mathematica*; but could not understand that from Vol. XXI of the *Annalen*, because I failed to grasp the idea of an ordinal number. Even before I knew any of your papers, I had been led, in 1881, from the study of the Logic of Relatives, to a few ideas about numbers, and had particularly seen that finite classes differ from infinite ones in that a certain form of reasoning is valid for the former, that is not valid of the latter. It is De Morgan's Syllogism of Transposed Quantity of which this is an example:
>
> Every Hottentot kills a Hottentot
> No Hottentot is killed by more than one Hottentot
> ∴ Every Hottentot is killed by a Hottentot.
>
> Every property which distinguishes a finite from an infinite class can be deduced from that. I also saw that what you call a class of *Mächtigkeit* aleph null is distinguished from other infinite classes in that the *Fermatian Inference* (very improperly called *vollständige Induktion*, in German) is applicable to the former and not to the latter; and that generally, *to any smaller class some mode of reasoning is applicable which is not applicable to any greater one*. For greater classes allow greater possibilities, — a very significant fact. Later, after reading your papers in the *Acta Mathematica*, except the one I could not understand, I was led to some further considerations, which I shall venture to submit to your judgment. They have been printed in part only. One of these is a proof (without resort to ordinal numbers which were a closed book to me) that two classes could not be each greater than the other. I will mail you a number of the *Monist* containing this proof; and since I there use my two algebras for the logic of relatives, I will mail also some papers about that and a book where in "Note B" the notation is briefly explained.
>
> I have also reached another result which may very likely be well-known to you but which I do not find that you state and which leads directly to ideas which seem quite opposed to yours about continuity, and which appear to me extremely important not only for the theory of logic, in general, but also as leading to a strictly rigid demonstrative method in Topical Geometry (*Topologie*) and I hope may lead to some practical way of dealing with that puzzling subject, — and show, for example, how to treat the problem of coloring a map.

The evidence at Harvard University fails to produce Cantor's direct response, if there ever was one. But Max Fisch found at Houghton Library in Peirce's personal library a volume of offprints bound together and entitled *Peano and Cantor*, a copy of Cantor's *Zur Lehre vom Transfiniten* with the following inscription in Cantor's hand: "Herrn C. S. Peirce in Milford, Pa., U.S.A.; hochachtungsvollst, d.V., Halle, 9ten *Juli*, 1901." Additional evidence of some kind of communication between the two men was uncovered with the discovery by Dr. Ivor Grattan-Guinness (1971) of a letter in the Philip Jourdain papers in which Cantor speaks of a letter from Peirce "den ich noch nicht beantwortet habe. Ich möchte zuerst wissen ob dies derselbe Peirce ist, welcher von Herrn Schröder in Karlsruhe in dessen Werken oft citirt wird." Grattan-Guinness added that Jourdain replied that this was the same Peirce. However, this reassurance came in mid-July 1901 apparently after Cantor had sent the offprint to Peirce.

To those who associate Peirce with logical and philosophical research only, this peep-hole view of him in mathematics must seem like a contrived setting. It is true that by 1856 when he was but 17 years of age he was already systematically studying logic, especially Kant's *Critique of Pure Reason;* by 1862 he was engaged in original research; and by 1865 he was publishing in that field. But his scientific activities in the employ of the United States Coast and Geodetic Survey from 1859 to 1891 as well as his association from 1879 to 1884 with the brilliant group of mathematicians at the Johns Hopkins University served to mathematicize his thought to such an extent that his approach to logical and metaphysical problems thereafter reflected a totally mathematical stance.

A brief review of his Johns Hopkins experience is illuminating. Although Peirce had been appointed lecturer in logic by President Gilman, his close association with members of the mathematics department in general and Professor Sylvester in particular made him seem to be one of their number. The reports of meetings and course offerings as found in the *Johns Hopkins University Circulars* imply that he was one of the active staff members. Peirce's diverse interests included: the linear associative algebra of his father to which he made a significant contribution that associates him with Frobenius, Tabor, and Cayley; ongoing attempts to handle the geographical problem of the four colors as provoked by a communication (with that title) to the Johns Hopkins group from Kempe; the non-Euclidean geometry as presented by Story; the ever-renewed interest in the extension and application of probability theory and his teaching of a course therein; the many geometric and analytic innovations of Cayley and Sylvester; and metaphysical problems discussed at the Metaphysical Club of the University. An indication of Peirce's professional status is found in the report of a meeting of the mathematics seminar in January 1882 when Sylvester, Cayley, and Peirce were together on a program, Peirce speaking on "Relative Forms of Quaternions."

Peirce's connection with the Johns Hopkins University was abruptly terminated in 1884. But his interest persisted in the then current mathematical developments. He became an Associate Editor of the *Century Dictionary* published in 1889 and wrote for it the terms in mathematics as well as in logic and metaphysics, astronomy, weights and measures, and mechanics. Further effort went into writing mathematics textbooks during the 1890's. By the turn of the century, he wrote that mathematics was a great aid to logic, an instructive subject for logical analysis, and that "methodeutically," mathematics was its own logic. Had he been fortunate enough to continue with a group of inspired mathematicians, he might well have turned to the heuristic aspect of mathematical research. But the Johns Hopkins contact had brought him far into the field at that, especially into foundations, number theory, topology, projective geometry, existential graphs, the logic of relatives, and, above all, the nature of the continuum.

With his particular metaphysical and logical interests, Peirce, like many another before him, had been plagued by the need for a definitive mathematical statement on the infinite and on continuity. Recalling the Hottentots in the letter to Cantor, it is not surprising to have him define a finite class of elements in the following terms: "Suppose a lot of things, say the A's, is such that whatever class of ordered pairs λ may signify the following conclusion shall hold. Namely, if every A is a λ of an A, and if no A is λ'd by more than one A, then every A is λ'd by an A. If that necessarily follows, I term the collection of A's a *finite* class" (Hartshorne and Weiss, 1933, 4. 187). Peirce advocated that the proposition that "finite and infinite collections are distinguished by the applicability to the former of the syllogism of transposed quantity ought to be regarded as the basal one of scientific arithmetic."

If the collection is not found to be finite after application of the above criterion, it is called an infinite collection. But what is the grade or order? The letter to Cantor mentioned the criterion applicable to the lowest grade. Peirce called it the *Fermatian Inference* to honor the mathematician whose reasoning prowess filled him with the greatest admiration. At one time Peirce characterized Fermat's skill in the use of the inference as "the greatest feat of pure intellect ever performed." In his Grand Logic, Peirce referred to Fermat's general method of "infinite descent" as consisting in proving a proposition to be true of such a series, because otherwise it must be false of an enumerable collection, such falsity by reasoning on the principle of the part being less than the whole, being shown to be impossible. In a "logical transformation" of this process, Peirce exhibits what is recognized as the familiar pattern of induction.

> The *first* of a certain series has a certain
> character;
> But if any member of the series has that character,
> so has the next following
> member.
> Hence, every member of the series has that
> character.

In a paper of 1898 in the *Educational Review*, Peirce postulated that arithmetic begins with the fundamental theorem: "*Whatever character belongs to zero, and belongs to the number following hard after any number to which it belongs, belongs to all numbers*" (Hartshorne and Weiss, 1933, 3.562g).

It is seen, then, that Peirce divided multitudes (Mächtigkeiten) of collections (Mengen) into three categories according to the applicability to each in turn of the syllogism of transposed quantity and the inference of Fermat, and this he had already done in his 1881 paper on "Logic of Number" in the *American Journal of Mathematics*. His thought, in that case, was crystallized before he became familiar with the work of Dedekind. In manuscript notes for a lecture he was to deliver to philosophy students at Harvard University (15 May 1903), he wrote, "My work has been, I believe, completely independent of Cantor. I never knew anything definite about him until 1884. I have seen it stated in some book that I modified the statement of Dedekind. But the truth is that Dedekind's *Was sind und was sollen die Zahlen* first appeared in [1888]. It contains not a single idea which was not in my paper of [1881]." Excerpts from a passage in one of Peirce's notebooks reveal his conviction that his priority in the distinction between the finite and infinite had been pointed out in Germany. He probably referred to Schröder's study of both approaches to the problem, those of Peirce and of Dedekind.

Peirce indeed had been involved deeply in the implications of logical continuity or generality — continuity of time, space, thought, experience, and so forth. The inductive process of his logic of science required a passage from the finite to the infinite, every inductive reasoning passing from "the observation of the finite and discrete to belief in the infinite or continuous." Moreover, "every understanding of experience of the like of which all our useful knowledge is composed relates to an *endless series of possibilities*. Whatever is endless is composed of what partakes more or less of the nature of *possibility*, of *idea*" (Peirce, n.d., Lowell Institution Lecture, ms. 458).

For the further elucidation of thought that brought him ever closer to the age-old problem of the continuum, Peirce needed the logic of substantive possibility. He noted that the variety of qualities exceeds not only all number but all multitude, finite or infinite. Qualities in themselves agree or differ in *general* respects. The idea of human knowledge is realized imperfectly as long as it is confined to existent individuals so that the nature of a science, for example, is not altered radically until it becomes a study not of existent collections but of classes of possibilities. One finds in the same Lowell lecture that Peirce claimed to have proved that there is an infinite series of infinite multitudes, apparently the same as Cantor's *alephs*. Peirce called the first of them the denumerable multitude, the others the abnumerable multitudes. He maintained that the aggregate of all abnumerable collections finally leads to so crowded a field of possibility that the units of the aggregate lose their individual identity. The aggregate ceases to be a collection and becomes a continuum. This is a supermultitudinous collection beyond all the *alephs* in which the elements are no longer discrete but are welded together. This is the basis of his conception of the continuum, and accounts for his disparagement of the "pseudo-continuity" of the analysts in which the elements remain discrete. It also accounts for his retention of the infinitesimal basis of the calculus.

Because time offers an example of primitive and simple continuity, Peirce's analyses are often in terms of time series reflecting, perhaps, a Hamiltonian influence. He associated in the usual way distances of points on a line segment with instants in an interval of time to demonstrate that a multitude of instants between two limits of analytic time is the same as the multitude of all possible *collections* of whole numbers. He claimed priority for the proof that the multitude of ways of distributing the singulars of any collection whatsoever under two headings is always greater than the multitude of those singulars themselves, that is, $2^n > n$. Similarly, two to the aleph-null is greater than aleph-null; and two raised to two to the aleph-null is greater than two to the aleph-null, and so forth. Peirce thus reaches abnumerable multitudes of higher and higher order. He concludes that there is room for any multitude of instants whatever, of any transfinite order whatever. The instants are welded together so as to lose their distinctness as instants just as the aggregate of all abnumerable collections leads to so crowded a field of possibility that the individuals lose their identity. "I cannot see," he wrote in 1902, "that Cantor has ever got the conception of a true continuum, such that in any lapse of time there is room for any multitude of instants however great" (Peirce, n.d., Carnegie Institution Correspondence, L75).

Peirce once said that everything in reality is welded together. His evolutionary philosophy in which the principle of growth is a "primordial element of the universe," in which instinct and reasoning shade into one another by imperceptible gradations, in which one finds the conservation of energy, the connection of ideas, probabilities and likelihoods, the intensity of color or of feeling, the duration of pitch, depends upon a serial variation in those areas which is not discrete but continuous. The qualities as *possibilities* have no existence and no individuality. According to Peirce the continuum is concrete developed possibility. The whole universe of true and real possibilities forms a continuum upon which this Universe of Actual Existence is but an element of discontinuity.

The continuum "becomes the true universal." Peirce's inductive methodology depends upon this, and for him the transition to continuity is of supreme importance to the theory of scientific method. If science is to advance at all, it must do so by passing from the characters of single things to the study of classes of things. He wrote that "the connection of the doctrine of multitude with the logical maxim called pragmatism is interesting. All things that exist ought by pragmatism (as a regulative principle) to form an enumerable collection. But what may be *in futuro* forms a denumerable collection. Now, according to tychism, law determines some things (excludes some future contingencies) and leaves others indeterminate. In that case the possible different courses of the future have a first abnumerable multitude. The possibilities of such possibilities will be of the second abnumerable multitude and when we reach the infinitieth exponential, which is thoroughly potential with no relic of the arbitrary existential left, we have a *true* continuity, such that on a line there are not only points at every value of the analytical variable (all such values forming a first abnumerable collection) but there is room for any multitude of points whatsoever" (Peirce, Letter to E. H. Moore, L299).

A related question concerns Peirce's thought on the infinitely small. Unlike Cantor, who postulated the infinitely large but not the small, the continuity concept in Peirce's system presupposed the infinitesimal such as Fermat and Leibniz and subsequent textbook writers of the eighteenth and nineteenth centuries had utilized in the foundations of the calculus. Peirce (unpublished manuscript 718) wrote: "When the scale of numbers, rational and irrational, is applied to a line, the numbers are insufficient for exactitude; and it [is] intrinsically doubtful precisely where each number is placed. But the environs of each number is called a point. Thus, a point is the hastily outlined part of the line whereon is placed a single number. When we say placed, we mean would be placed, could the placing of numbers be made as precise as the nature of numbers permits.

"When we say that the lengths on the line are equal, we mean that the numbers which measure those lengths are equal. Lengths immeasurably shorter than measureable lengths are equal to zero. Yet they are lengths just the same. Numbers are equally applicable to these also; and then they are algebraically treated as infinitesimals. Again, there will be lengths not measurable by such numbers, nor by limits of series of them. These, when numbers are applied to them, become infinitesimals of the second order."

On the interleaf of the page proof of the *Century Dictionary* that Peirce received as an editorial contributor before its appearance in 1889, he attacked the problem from a related point of view, stating that continuity consists in *Kanticity* and *Aristotelicity*, the Kanticity meaning that there is a point between any two points, whereas the Aristotelicity means the inclusion of every point that is a limit of an infinite series of points belonging to the system. Peirce clung to his belief in the infinitesimal concept in this context in which points clustered about limits and piled up in density so as to merge at last, making of the limit a general. As *generals*, the principle of excluded middle does not apply to them. The line continuum is no longer a collection. It no longer contains points. Peirce had no objection to the method of limits in the foundations of the calculus, but to him the method of infinitesimals offered a simpler procedure. In a letter to William James dated 26 February 1909, he wrote: "I have long felt that it is a serious defect in existing logic that it takes no heed of the *limit* between two realms. I do not say that the Principle of Excluded Middle is downright *false*; but I do say that in every field of thought whatsoever there is an intermediate ground between *positive assertion* and *positive negation* which is just as Real as they. Mathematicians always recognize this, and seek for that limit as the presumable lair of powerful concepts; whereas metaphysicians and old-fashioned logicians — the sheep and goat separators — never recognize this. The recognition does not involve any denial of existing logic, but involves a great addition to it. To recognize such a mode of composition of concepts as that of the barycentric calculus would be one way of recognizing the idea of the limit of lower dimensionality between any two mutually exclusive fields."

Peirce's concept of the principle of excluded middle not holding of points on a line carries a greater overtone than first meets the eye. Just as the points on the line continuum and the instants in the time continuum do not preserve their distinct identities, Peirce claimed that it follows in the same way that were a proposition to be false up to a certain instant and thereafter to be true, at that instant "it would be both true and false." A proposition once true does not necessarily always remain true. "It only follows," he said, "that it remains true through a denumerable series of instants, which is a lapse of time inexpressibly less than any sensible or assignable time, if it can properly be called a lapse of time at all, wanting as it does most of the characteristics of duration."

Peirce fusses with boundary problems throughout his topological studies. They bring to light a third kind of existence in which an element on the boundary is not quite this nor that, but a bit related to both. This may account in part for his interest in the nonmetrical aspect of topology, for he had criticized Cantor for making his work depend upon "metrical considerations."

When one once lets this light of trivalency shine on Peirce's writings, some rather astounding insights are revealed and his total thought takes on new overtones. For example, in a preliminary statement for a Lowell lecture in 1903, he spoke of existential graphs he had invented as representing the simplest kind of mathematics. Such a graph has one of two grades of value, a false graph or a true graph. He said, "This is the very simplest kind of mathematics, this system with only two different values." Then he made the amazing observation: "*There would be an interesting system with three values*, which I have slightly examined." He returned to the problem in another draft, concluding this time that "in like manner there would be a *Mathematics of a System of Three Values* which would not be without utility and which has been in some measure developed."

Professor Max Fisch, who is writing the definitive biography of Peirce, discovered in Peirce's unpublished "Logic Notebook" evidence of Peirce's formalization in his own thinking by 23 February 1909 of a three-value logic with a matrix treatment similar to that of the well-known two-value system. Peirce scribbled next to the first appearance of the matrix: "All this is mighty close to nonsense." But three pages beyond that he confidently proclaimed that "Triadic Logic is that logic, which, though not rejecting entirely the Principle of Excluded Middle, nevertheless recognizes that every proposition, S is P, is either true, or false, or else has a lower mode of being such that it can neither be determinately P, nor determinately not P, but is at the limit between P and not P." It is probable that, as a result of his analysis of the nature of the continuum, Peirce developed the need for a third value in logic that led him to the elements of a firm triadic logic by 1909.

In the letter to Cantor, Peirce referred to the problem of coloring a map. With his interest in the topology of Listing, it was natural for him to try his hand at the fascinating topological problems of the late nineteenth century. His first attempt to solve such a problem probably was made after Story presented the communication from Kempe at a meeting of the Scientific Association at the Johns Hopkins University on 5 November 1879. In the *Circulars*, Peirce is recorded as "showing by methods of logical argumentation that a better demonstration of the problem than the one offered by Mr. Kempe is possible." Today there is no manuscript extant that might be identified as the summary of Peirce's remarks at the meeting. There also is no manuscript of the paper he read on the subject at a meeting of the National Academy of Sciences 15 November 1899. But his writings over the years reflect his preoccupation with the problem. In 1902 Peirce made an unsuccessful attempt to gain subsidization from the

Carnegie Foundation for his great work on logic. In his application, he mentioned his efforts to solve the map color problem and finally revealed that he had been thereby testing the progress of his own work in heuretic mathematical thought by using the advancement of his skill in handling the four-color problem as a "landmark." He confessed that he had not proved it up to that time, although the last time he tried, he thought he had a proof, which "close examination proved to contain a flaw." Despite this failure, he claimed to have applied his logical theory to the demonstration of several other propositions that had "resisted mathematicians," that he had greatly improved on Listing's theory, and that the same method "has only to be pushed a little further to solve the map problem."

Peirce was interested in maps in an even more direct way. As the assistant in the United States Coast and Geodetic Survey in charge of the investigation of gravity and the shape of the earth, he was in Paris on a pendulum-swinging mission in the late 1870's. Peirce's report to the Superintendent of the Survey, dated 24 January 1877, told of his discovery when in Paris that the best of Ptolemy's catalogue of stars "had never been properly transcribed." Peirce had now accomplished this, had set them down on a modern atlas, and had "the materials for new and improved identification of them." Peirce intended "to make a new edition of Ptolemy's catalogue with identification and notes. Also with a planisphere showing the stars and the figures of the ancient constellations . . . ." He continued with the description of new projections of the sphere that he had invented, both methods involving the use of "Elliptic Integrals." From this research, Peirce finally produced the Quincuncial Map Projection that attracted attention in a number of high places. The *Report of the Superintendent of the Coast and Geodetic Survey* showing progress of work during the fiscal year ending June 1879 described Peirce's work as follows: "Among several forms of projection devised by Assistant Peirce, there is one by which the whole sphere is represented upon repeating the squares. This projection, as showing the connection of all parts of the surface, is convenient for meteorological, magnetological, and other purposes. The angular relation of meridians and parallels is exactly preserved; and the distortion of areas is much short of the distortion incident to any other projection for the entire sphere."

Peirce's map was published not only in Appendix 15 of the annual report of the *Coast Survey* for 1880, but also in Volume 2 of the *American Journal of Mathematics* in 1879. Thomas Craig of the Coast Survey published *A Treatise on Projection* in 1880 in which he referred to Peirce as one of the greatest map-makers of all times. Another volume published in 1925 by Oscar S. Adams, also of the Coast Survey, stated that in the account that Peirce published in 1879 of a conformal projection of the sphere within a square, one finds the first application of elliptic functions to conformal mapping for geographical purposes. Adams refers to the work of H. A. Schwarz of Halle in 1864 and to Schwarz's proof that a circle can be conformally mapped within a regular polygon of n sides by use of the function

$$\omega = \int_0^z \frac{dz}{(1 - z^n)^{2/n}} .$$

The tale of Peirce's ingenuity in this area can best be ended here with an account of a paper by Albert A. Stanley, the then Special Assistant to the Director of the Coast and Geodetic Survey. It is entitled "Quincuncial Projection" and appeared in *Surveying and Mapping* in 1946. The article explains that "the U.S. Coast and Geodetic Survey recently published Chart #3092, showing major International Air Routes on a chart constructed on a Quincuncial Projection of the sphere . . . . The resulting configuration of land areas is conformal with the whole sphere being represented on repeating squares; also the major air routes, which for the most part follow approximate great circles, are in areas of least distortion, and in most cases are shown as straight lines. Angles of intersection are preserved exactly, and the maximum exaggeration of scale is less on the quincuncial projection than on either the Mercator or the stereographic projection." Stanley attributed the invention of the chart to Peirce, and its revival to its showing the major air routes as approximately straight lines on a world outline, preserving satisfactory shapes. Moreover, it provided "peoples residing in either the Eastern or Western Hemispheres with a world pattern in accordance with their inherent geographical conception." A citizen of the United States or Asia "is able to observe the relationship of world land areas from his point of view as occupying a central geographical position."

Belated recognition of the fruitfulness of Peirce's ideas has been showing on numerous other fronts as well. For example, a simple presentation by Eisele in the *Proceedings of the American Philosophical Society* (1957) of correspondence between Peirce and Simon Newcomb included a letter dated 17 December 1871 in which Peirce discussed a matter in political economy from a mathematical standpoint. The article was noticed by Professors Baumol and Goldfeld of Princeton University, and the letter became the key evidence to justify their inclusion of Peirce as a precursor in mathematical economics in their book of the same title published by the London School of Economics and Political Science (1968:182-186). They pointed to the fact of Peirce's knowledge of the mathematical approach of Cournot as early 1871; that Jevons' rediscovery of Cournot's *Recherches* was not published until 1879; and that Jevons did not even possess a copy of Cournot until 1872. Peirce's program for the economy of research permeates his thought in his pragmatic approach to the analysis of problems in general whether they be in philosophy, logic, or the history of science. Peirce's use of mathematical techniques warrants further review and study. His paper entitled "Note on the Theory of the Economy of Research," first published in the *Report of the Superintendent of the Coast Survey* (1879:197-201) was reprinted recently in an article in *Operations Research* (1967). It was written by W. E. Cushen and entitled "C. S. Peirce on Benefit-Cost Analysis of Scientific Activity."

Much more remains to be discovered and celebrated in the mathematical thought of the American Charles S. Peirce. A four-volume edition of *The New*

Elements of Mathematics by Charles S. Peirce, as edited by Eisele, is being printed by Mouton Publishers at the Hague. It is a collection of hitherto unpublished mathematical papers mostly from the Peirce Manuscript Collection in the Houghton Library at Harvard University. As an educator in mathematics, Peirce foresaw 75 years ago what has come to pass in curriculum revision in recent years. Manuscript for two textbooks in geometry and one in arithmetic are among the many items included, and one finds therein first attempts anywhere to include in lower school level books topics such as the cyclical arithmetic of Gauss; the topology of Listing; the knots of Tait; elements of the non-Euclidean geometry of Bolyai, Lobachevsky, and Riemann; the geometric absolute and the matrices of Cayley; the barycentric calculus of Möbius; the unified geometric outlook of Klein; and, of course, the inevitable algebra of Boole to which Peirce made so significant a contribution. But how could any publisher in the late 1890's have dared to handle so costly a job when teachers were not even aware of the new mathematical developments, and no National Science Foundation existed to subsidize their further education? Yet he was appreciated by the then small circle of members of the American Mathematical Society, especially by Professors Fiske and Moore. He addressed the Society twice, one on the nature of geometry and again on an *Arithmetic* of 1424 written by one Rollandus.

In 1902, Peirce wrote that his logical studies already had enabled him to prove some propositions that had arrested mathematicians of power. He further stated: "Yet I distinctly disclaim for the present, all pretension to having been remarkably successful in dealing with the heuretic department of mathematics. My attention has been directed to the study of its procedure in demonstration, not upon its procedure in discovering demonstrations. This must come later; and it may very well be that I am not so near to a thorough understanding of it as I may come. I am quite sure that the value of what I have ascertained will be acknowledged by mathematicians" (Peirce, n.d., Carnegie Institution Correspondence L75).

In a review of the development of mathematics in America (Eisele, 1957, 1959, 1963, 1964, 1971, 1974), the mathematical genius of Charles S. Peirce (1839-1914) may be celebrated with considerable pride.

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GEORGE BRUCE HALSTED AND THE DEVELOPMENT OF AMERICAN MATHEMATICS

ALBERT C. LEWIS

George Bruce Halsted, if he is cited at all in histories of mathematics, is only in the footnotes. And rightfully so, as an original contributor to the development of pure mathematics, I cannot claim for him any place among the distinguished mathematicians of the nineteenth century. But one whose name appears as often as does Halsted's in the footnotes of the history of American mathematics deserves to be looked at in his own right. Such a look at Halsted will, I believe, show that he played an essential role in the development of American mathematics, in the sense that without Halsted the development of American mathematics would have been substantially slower and narrower. First, I would like to present a brief account of Halsted's life, and then show that Halsted had a conception of a distinctively American contribution to mathematics of which he considered himself a part.

To illustrate the latter, perhaps I may insert here a sample of Halsted's typical purple prose that begins an article in *Science* (1905) on the Bolyai Prize:

> America will rejoice that at last Hungary is honoring herself in honoring her wonderchild, John Bolyai. His marvel diamond, the most extraordinary two dozen pages in the history of human thought, appeared in America in English before it appeared in Hungary in Magyar, proud as they are of their language; and more, the American was reproduced entire in Japan before even the original was reproduced in Hungary.
>
> An American, not a European, was the first from outside Hungary to make the journey to Máros-Vásarhely for John Bolyai's sake and to see there the letter in Magyar which constitutes his preemptive claim and title-deed to the new universe, and to publish for the first time that letter making the date 1823 ever memorable.

Halsted is referring to his own translation of Bolyai, but he at least subsumes himself under a nationalistic emphasis here; in general, however, he managed to achieve a reputation for putting himself forward first.

George Bruce (1853-1922) was the fourth generation of Halsteds to attend Princeton University (A.B. 1875, A.M. 1878); he obtained his doctoral degree at Johns Hopkins in 1879. Halsted's first two footnotes in American mathematics occurred at Johns Hopkins where he was J. J. Sylvester's first graduate student, his "first class" as Halsted put it. He also studied in Berlin where he arrived with a flattering letter from Sylvester introducing him to Carl Borchardt, the editor of *Crelle's Journal*. E. T. Bell (1937:395) quotes Sylvester's account of how he resumed work in invariant theory:

> But for the persistence of a student of this University in urging upon me his desire to study with me the modern Algebra, I should never have been led into this investigation . . . . He stuck with perfect respectfulness, but with invincible pertinacity, to his point. He would have the New Algebra (Heaven knows where he had heard about it, for it is almost unknown on this continent), that or nothing. I was obliged to yield, and what was the consequence? In trying to throw light on an obscure explanation in our textbook, my brain took fire, I plunged with requickened zeal into a subject which I had for years abandoned, and found food for thoughts

which have engaged my attention for a considerable time past, and will probably occupy all my powers of contemplation advantageously for several months to come.

Halsted was the persistent student; in a published letter (1916:180) Sylvester tells Halsted: "Nor can I ever be oblivious of the advantage which I derived from your well-grounded persistence in inducing me to lecture on the Modern Algebra, which had the effect of bringing my mind back to this subject from which it had for some time previously been withdrawn, and in which I have been labouring, with a success which has considerably exceeded my anticipations, ever since."

In one of Halsted's several biographies of Sylvester, (1916:184) he tells how the idea for the first journal devoted to mathematics in this country came about:

> I was learning that Sylvester was a dependent variable, a function of an independent variable, his environment . . . . I realized what a stimulus and help to him would be a mathematical journal, nominally under his editorship. He stipulated that it should be in quarto form, to give scope for big formulas, and to it, the *American Journal of Mathematics*, he contributed while at Baltimore thirty memoirs, some of tremendous volume. Despite scruples of mine, he insisted that my "Bibliography of Hyper-Space and Non-Euclidean Geometry" should appear in the newborn *Journal*, but even his inspired prevision could not have foreseen that this was subsequently to find place in Russian and Latin works published at the birth-places of Lobachevski and Bolyai and finally make part of Sommerville's astonishing Bibliography (1911).

From 1879 to 1884 Halsted taught at Princeton, two years as tutor in mathematics and three years as instructor in postgraduate mathematics; from there he was invited to come to the new University of Texas in Austin as professor of mathematics. Halsted's background was not exclusively eastern; his mother came from Charleston, South Carolina, and he seemed to have an affinity for the South. Although what attracted Halsted to Austin in particular is not known, money was at least a factor, as he implied in an exuberant letter from Austin (Class Secretary, 1885) to a Princeton classmate: "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."

Halsted's most productive period occurred at the University of Texas where he was a professor from 1884 to 1903. During these 19 years, he published about 10 books, some in several editions, and nearly 200 articles. His books included geometry textbooks such as *Elements of Geometry* (first edition 1885, sixth edition 1895) about which Cajori (1890:237) said: "Halsted is the first writer in this country to preface a geometry by a preliminary chapter on logic." In the 1890's he wrote the English translations of Bolyai's and Lobachevskii's principal works on non-Euclidean geometry, which are still in print. After the turn of the century he had the personal cooperation of Henri Poincaré in translating his most important philosophical works, *Science and Hypothesis, The Value of Science, Science and Method*, some of which are still in print. Among his lesser known translations are: from the Latin, a seventeenth-century geometry by Giovanni Saccheri, *Euclides Vindicatus;* from the Italian, Gino Loria's *History of Geometric Methods*; and, from the Russian, Tolstoi's novel *Master and Man*. He had among his pupils at Austin L. E. Dickson and R. L. Moore. He helped to found the Texas Academy of Sciences, was a member of numerous other learned societies, and found time to travel to Hungary, Russia (the homelands of Bolyai and Lobachevskii, respectively), France, Mexico, and Japan.

A fellow faculty member, T. U. Taylor, looking back in *Fifty Years on Forty Acres*, (1938:290-291) wrote about Halsted:

> Of all the rare and odd professors that have been on the Faculty of the University of Texas, I think George Bruce Halsted will rank number one.
>
> He came to the University in the fall of 1884 during the University's second year and for about sixteen years his sayings and doings in the classroom and in public lectures were the talk of the campus and the town.
>
> He was rather caustic and made personal remarks to the students and commented on their replies. Some took offense and one day a personal encounter was prevented by the chairman of the faculty.

There are numerous amusing stories about Halsted that I refrain from giving here. Although well known as a campus figure, Halsted was not well liked; a strained relationship with the Regents was brought to a head over Halsted's insistence on hiring his former student, R. L. Moore (B.A. 1901, M.A. 1901), as a tutor in mathematics. In an article in *Science* (1902), Halsted gave his reaction to the Regents' refusal to hire Moore.

> And finally among the gifted few who have the divine gift and the divine appreciation of their gift, the exquisite bud in its incipiency may be cruelly frosted.
>
> Of the great Hilbert's "betweenness" assumptions one was this year proved redundant by a young man under twenty working with me here, and by a demonstration so extraordinarily elegant and unexpected that letters from high authorities came congratulating the university on the achievement. Professor E. H. Moore, of the University of Chicago, has published his congratulatory letter spontaneously written [Amer. Math. Monthly (June-July):152,153].
>
> This young man of marvelous genius, of richest promise, I recommend 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 politican.

A few months later, in December 1902, Halsted was fired by the Regents. Afterwards he taught successively at St. Johns College in Annapolis, Kenyon College in Gambier, Ohio, and Colorado State College of Education in Greeley. After leaving Austin, his scholarly productivity virtually ceased. During his retirement in Greeley he wrote a former Princeton classmate (Harvey, 1915:58): "Trying to get work in Oklahoma, I was taken to a prayer meeting for rain. I am working as an electrician, as there is nothing (for me) in cultivating vacant lots."

To turn now to Halsted's work, in keeping with the subject of this conference, I have selected three examples that, I believe, also serve to illustrate a broader theme. It has been maintained that the distinctive trait of American civilization of Halsted's time was the influence of the frontier; certainly the forces of civilization were brought to bear on newly settled land, but such forces were, in the process, altered in unique ways. There were many like Halsted who were conscious not only of bringing their essentially European learning to the West but also of transforming it into something useful for the "progress" of their country. The idea of progress itself has been described as a characteristic of American civilization, and Halsted applied the idea to mathematics, contrasting it with the Greek ideal of perfection, in the Introduction to his translation of Lobachevskii's *Geometrical Researches on the Theory of Parallels* (1891:6): "The American ideal is success. In twenty years the American maker expects to be improved upon, superseded. The Greek ideal was perfection. The Greek Epic and Lyric poets, the Greek sculptors, remain unmatched. The axioms of the Greek geometer remained unquestioned for twenty centuries."

One popular area of progress in applied mathematics and in which Halsted participated was that of refining prismoidal formulas — formulas used by engineers for obtaining certain volumes from a few easily made measurements. For example, T. U. Taylor published a number of books and articles on the subject, including a 100-page book entitled *Prismoidal Formulas and Earthworks* (1898) in which the principal application is in the earthworks involved in constructing railroads. The work is as concerned with mathematical proofs of formulas and priorities of discovery as with applications. Everyone is aware of the special importance of the railroad in the West; when it is pointed out that half the world's railroad track in 1900 was in the United States, one can appreciate that an American could gain some satisfaction from contributing to the subject of prismoidal formulas. Halsted, as far as I can determine, never mentioned railroads, earthworks, or any other application in his writings on the "two-term prismoidal formula," perhaps thereby maintaining a status of "pure" mathematician. Besides Taylor, Halsted knew W. H. Echols, who had published one or two papers on prismoidal formulas. Echols was professor of applied mathematics at the Missouri School of Mines, and he remarked in a paper in 1890 (1:20) that "No small subject connected with the profession [of engineering] has probably received so much labor and attention as this, in the direction of facilitating the computation of the volumes of these earthwork solids."

In a paper entitled "The Criterion for Two-term Prismoidal Formulas," (1897:19-20) Halsted outlined the history of prismoidal formulas from Newton, 1711, as far as Steiner, 1842, and wrote: "But, in seeming ignorance of all this, American engineers began and continue to give their time to doing over again what had been already done." But when in this paper Halsted continued to give more recent developments in the subject, it turned out that he had himself been "doing over again" in his earlier work "what had been already done" due to a lack of a European journal. In 1881 in his *Elementary Treatise on Mensuration,* Halsted presented a two-term prismoidal formula; in 1934 a historian of mathematics, R. C. Archibald, accused Halsted (1934), among others, of appropriating this formula "without acknowledgment" from Hermann Kinkelin of Basel (1832-1913), who published it in the *Archiv der Mathematik,* 1862. But Archibald overlooked Halsted's (1897) paper in which he stated: "The discovery of the most important two-term prismoidal formula, occurs in a paper so little known (I read it for the first time March 7, 1896, in a copy lent me by Prof. T. U. Taylor) that a translation of it is here given.'' Thus Halsted admitted the priority of Kinkelin albeit after 14 years, although it was presumably no fault of his that the library at the University of Texas did not have the *Archiv der Mathematik*.

The second example, a brief one, is on the subject of linkage. Halsted wrote a 14-page monograph while at Johns Hopkins entitled ''Extract from a Lecture on Linkage, Showing Connexion of Modern Mathematics with Important Machines. Addressed Particularly to Mechanicians, Inventors, and Persons Interested in the Application of Scientific Principles to the Industrial Arts, Delivered at the Johns Hopkins University, Baltimore, March, 1878.'' Sylvester, influenced by Chebyshev, did work on linkage and gave a lecture on the subject at the Royal Institution in 1874 before coming to Johns Hopkins in 1877. In the historical introduction to his monograph, Halsted wrote: ''Dr. Tchebicheff happened to visit England, and there Dr. Sylvester asked him about the progress of his proof of the impossibility of the exact conversion of circular into rectilinear motion. Tchebicheff answered that, far from being impossible, it had actually been accomplished. . . . He than made a rough diagram of the instrument, which consists of seven links.'' Presumably steam engines, and, in particular, locomotives, were one example of an application of such a ''scientific principle to the industrial arts.'' Halsted gave no details along this line and only made the general remark: ''This should be of especial interest in America, the land of practical applications; and so we have attempted to bring here into connection the new achievements with some of the old ones.''

The third example concerns what must be Halsted's favorite topic, non-Euclidean geometry, or, more precisely, the relationship between non-Euclidean and Euclidean geometry. Halsted helped introduce the subject of non-Euclidean geometry into this country by his translations of Lobachevskii and Bolyai, and his ''Bibliography of Hyper-Space and Non-Euclidean Geometry.'' Along with these writings might be mentioned an exposition for the nonmathematician published as a letter to the *Popular Science Monthly* (1877) under the title ''The New Ideas About Space,'' in which Halsted stated: ''The great attention now given to this subject in Europe seems to render appropriate a short communication to bring it more directly before Americans . . . . the great Gauss whom Germany is now celebrating, and a Russian named Lobatchewsky . . . both said that the space with which we are familiar is only one kind of space out of a number of possible spaces, each logically self-consistent.'' This is probably the first printed mention of Lobachevskii in America. Halsted saw his textbooks in Euclidean geometry as laying a suitable foundation consistent with the more advanced fields of geometry then being developed. The textbook *Rational Geometry* (first edition 1904) was an attempt to present Hilbert's *Foundations of Geometry* in elementary form, which, however, did not receive Hilbert's blessing as Halsted had hoped and thus it was revised in later editions, omitting reference to Hilbert.

But Halsted did more than introduce the new geometry — he watched over its development, giving lectures to teachers of mathematics and lecturing any fellow mathematicians who seemed to fail to grasp what was going on. From 1894 to 1904 articles on non-Euclidean geometry published in the *American Mathematical Monthly* were subject to the sometimes acerbic criticism of the Texas professor: for example, "Mr. J. N. Lyle is as usual hopelessly muddled." "QUERY: Is a man who writes . . . [quotation from Lyle] properly to be considered sane?" One might add that apart from usually being right in matters of fact, Halsted was also one of the original financial contributors to the *Monthly*.

From letters written during the last years of his life, it is evident that Halsted, as a historian, was proudest of the new life he was able to give to Bolyai and Lobachevskii. Especially Lobachevskii, whose situation as a mathematical revolutionary leading an obscure life in Kazan, a virtual frontier city, must have appealed to Halsted. One may recall that Lobachevskii never achieved recognition for his work in non-Euclidean geometry during his life. He died in 1856 having gained only local fame as the rector of the University of Kazan, where he spent almost all his life. Lobachevskii entered the Gymnasium of Kazan in 1802 and then attended the new University in 1807. As a student, Lobachevskii had a rebellious nature. He was once accused by university authorities of atheism, but rebelliousness was more than offset by success in physical sciences and mathematics.

At the University he came under the influence of a German, J. M. Bartels, who had been a teacher of Gauss. He was well qualified to teach the recent major developments in European mathematics. Bartels was one of a number of German professors; a historian of Russian science, A. Vicinich, wrote (1962): "These scholars were responsible for the conversion of Kazan University from an isolated intellectual oasis at the threshold of Siberia into a cultural center alert to modern scientific developments. Under the influence of these men, Lobachevskii found himself deeply involved in mathematics, which became his lifelong pursuit."

Such a description of the University of Kazan invites comparison with the University of Texas in Halsted's day. Halsted well could have had such a comparison in mind when he wrote of Lobachevskii (1898): "This outspoken and passionate youth in a young university just opened in a half-wild country, in the *ultima Musarum Thule*, as the first professors, Germans, called it, was typical of the there prevailing ardent desire for knowledge, enthusiasm for study, for progress. With this fire of spirit there reigned among the pupils, as says S. T. Aksakov in his *Family Chronicle*, 'complete contempt for everything bad and low, and deep veneration for everything honest and noble, even if it were unreasonable.'"

The principal figure in Russia behind the moves to gain the recognition due Lobachevskii was A. V. Vasiliev, professor of mathematics at Kazan while Halsted was at Austin. Halsted and Vasiliev had met and kept in touch with each other regarding their common interest. Some of Halsted's papers on non-Euclidean geometry were published in Kazan on the occasion of the unveiling of a monument to Lobachevskii, and a speech by Vasiliev in 1893 for the centenary of Lobachevskii's birth was translated into English and published by Halsted the following year. In his speech at Kazan (1893:34), Vasiliev noted that the work that Lobachevskii had started was being carried on in his fatherland and in "all the civilized countries of Europe, in France, Germany, Italy, in Spain, just now awaking from its intellectual sleep, and in the midst of the virgin forests of Texas."

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A BRIEF HISTORY OF GRAPHICAL ENUMERATION

Robert W. Robinson

Most of the modern methods in graphical enumeration can be traced to one of four major lines of development. Indicated below are the origins of each.

1. 1758, Euler found the number of ways of triangulating a convex polyhedron.
2. 1847, Kirckhoff found the number of spanning subtrees of a labeled graph.
3. 1857, Cayley counted rooted trees, then unrooted trees some years later.
4. 1927, Redfield counted unlabeled graphs.

Euler, Kirckhoff, and Cayley were well known in their own times, so their contributions to graphical enumeration were noticed at once. J. Howard Redfield was not well known as a mathematician, and apparently published just one mathematical article. Parts of Redfield's brilliant work have been rediscovered over the years by Pólya, Harary, Read, de Bruijn, and many others.

The only readily available source of biographical information about Redfield is a letter from Cletus Oakley to Frank Harary, which is reprinted on pages 81 and 82 of *Graphical Enumeration* by Harary and Palmer. The letter seems to imply that Redfield earned a Ph.D. in mathematics at Harvard. Edgar M. Palmer ordered a copy of Redfield's Ph.D. thesis in hopes that it contained results not included in the published paper. When it arrived, he found himself with a 140-page thesis by Redfield dated 1914 and entitled "The Earlier Latin-Romance Loan Words in Basque, and Their Bearing on the History of Basque and the Neighboring Romance Languages." Cletus Oakley's letter did indicate that Redfield studied Romance philology, and taught Romance languages at Swarthmore and Princeton for several years. For most of his life, however, Redfield was a civil engineer.

HISTORY OF AMERICAN MATHEMATICS
TEXAS TECH UNIVERSITY
MONDAY, 28 MAY 1973

9:00 AM	Welcoming Address .... *Dean J. Knox Jones, Jr.*
	Opening Remarks .... *J. Dalton Tarwater*
9:30 AM	"Men & Institutions in American Mathematics" .... *Marshall H. Stone*
	Discussion
1:00 PM	"Historiography of Mathematics in America" .... *Kenneth O. May*
2:00 PM	"Recent Development of Mathematics" .... *Robert M. Thrall*
3:00 PM	"Men and Institutions in American Mathematics. International Scene" .... *Marshall H. Stone*
	Discussion
8:00 PM	Reception
	TUESDAY, 29 MAY 1973
9:00 AM	"Mathematical Reminiscences and Mathematical *Americana*" .... *Salomon Bochner* (Read by *R. O. Wells*)
	Discussion
1:00 PM	"Philosophical Conception of Continuity in the Thinking of C. S. Peirce" .... *Salomon Bochner* (Read by *R. O. Wells*)
2:30 PM	"New Elements of Mathematics" by C. S. Peirce .... *Carolyn Eisele*
3:30 PM	Tour of Ranch Headquarters
7:30 PM	"Some Early American Mathematicians" .... *Phillip S. Jones*
8:30 PM	"Mathematics in Colonial and Early Republican America" .... *Dirk J. Struik*
	WEDNESDAY, 30 MAY 1973
9:00 AM	"The Rise of Modern Algebra" .... *Garrett Birkhoff*
1:30 PM	"George Bruce Halsted" .... *Albert C. Lewis*
2:30 PM	"History of Graphical Enumeration" .... *Robert W. Robinson*

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