Saunders Mac Lane studied at Yale, Chicago, and at the University of Göttingen, where he received his doctorate in 1934 under the supervision of Paul Bernays and Hermann Weyl. After early positions at Harvard, he returned to the University of Chicago in 1947. His research has ranged through algebra, logic, algebraic topology, and category theory. Among his books are Homology, Categories for the Working Mathematician, *and (with Garrett Birkhoff) the influential text* A Survey of Modern Algebra. *His numerous honors include a Chauvenet Prize and a Distinguished Service Award from the MAA, and a Steele Prize from the AMS. He served as president of the MAA in 1951–1953, as president of the AMS in 1973–1974, and as vice-president of the National Academy of Sciences in 1973–1981.*

Mathematics at the University of Chicago
A Brief History

SAUNDERS MAC LANE

Since its inception, the University of Chicago has had an active department of mathematics. Indeed, in two periods, 1892–1909 and 1947–1959, this department has been perhaps the dominant one in the United States. This essay will sketch the development of the department.

THE FIRST MOORE DEPARTMENT

The University of Chicago opened its doors in 1892. William Rainey Harper, the first president of the university, immediately started out to emphasize active graduate study, bringing in a number of university presidents to be his department heads. For acting head of mathematics, he found Eliakim H. Moore (1862–1932), a lively young mathematician (Ph.D. Yale 1885, student at Berlin, Germany 1885–1886), then an associate professor at Northwestern. Moore came in 1892 and immediately found and appointed two excellent German mathematicians, then at Clark University: Oskar Bolza (1857–1936), a student of Weierstrass in the calculus of variations, and Heinrich Maschke (1853–1908), a geometer — both had been students at Berlin and at Göttingen. They constituted the core of the first department at

E. H. Moore

(University of Chicago Archives)

Chicago. G. A. Bliss [1] (who studied at Chicago then) has written of them: "Moore was brilliant and aggressive in his scholarship, Bolza rapid and thorough, and Maschke more brilliant, sagacious and without doubt one of the most delightful lecturers on geometry of all times". This team almost immediately made Chicago the leading department of mathematics in the United States.

In the period 1892–1910, Chicago awarded 39 doctorates in mathematics (far surpassing the next institutions: Cornell, Harvard, and Johns Hopkins). Even more striking is the quality of the first doctorates. The first (1896) was Leonard Eugene Dickson, who subsequently did decisive research on algebra [5] and in number theory [4]. He and five others in this group of 39 Ph.D.s subsequently held the office of president of the American Mathematical Society (AMS). These five (with their subsequent institutions) were, with year of Ph.D.:

1900	Gilbert Ames Bliss	University of Chicago
1903	Oswald Veblen	Princeton
1905	Robert Lee Moore	Texas
1907	George David Birkhoff	Harvard
1910	Theophil Henry Hildebrandt	University of Michigan

All except Bliss were doctoral students of E. H. Moore although it appears that Veblen actually directed most of R. L. Moore's thesis work! In the next generation, these mathematicians were probably the dominant figures at their institutions (Dickson was also dominant at Chicago). Note that this includes the three institutions generally regarded as the leading ones in mathematics from 1910–1940: Chicago, Harvard, and Princeton. At Texas, R. L. Moore was a great individualist, while Hildebrandt, as a long-time chairman at Michigan, set the style for a major state university. Birkhoff was interested in differential equations and dynamics. In 1912, Henri Poincaré, the leading French mathematician, had formulated and left unproven his "last geometric problem". Birkhoff provided the proof in 1913 and was in consequence soon regarded as the leading American mathematician. Veblen also had a major role in the development of topology and mathematical logic at Princeton University and later (from 1932) administered the mathematical group at the Institute for Advanced Study.

Clearly these results indicate that remarkable and aggressive advanced mathematical education took place at Chicago. G. A. Bliss [1] writes, "Those of us who were students in those early years remember well the intensely alert interest of these three men (Bolza, Maschke, and Moore) in the papers which they themselves and others read before the club ... Mathematics ... came first in the minds of these leaders". In the files of the mathematical club, which met biweekly, I have also found a rapturous account of a visit by Dickson, who in 1897 returned for a brief visit after his year of study in Paris and Leipzig, to report on the current mathematical developments in Europe. Some echoes of this sense of excitement were still present when I was a graduate student (1930–1931) in Chicago. I took a seminar on the "Hellinger integral", conducted by E. H. Moore, with assistance from R. W. Barnard. Moore, realizing that I knew little about Hellinger's integral, asked me to present E. Zermelo's [13] famous second proof that the axiom of choice implies that every set can be well-ordered. I gave what I thought was a very clear lecture, but after Barnard and the two Chinese students had left, Moore took me aside and spent an hour explaining to me what was really involved and what I should have said in my lecture. It was a thrilling experience — one which reflects in brief the excitement of the early days at Chicago.

What sorts of mathematics were studied then?

The archives at the University of Chicago library contain lecture notes taken by Benjamin L. Remick (1894–1900) on the following subjects:

By Bolza, with academic quarter indicated:

Functions of a Complex Variable	Autumn, 1894
Notes on Quaternions	Autumn, 1894
Hyperelliptic Functions	Autumn, 1897
Invariants I and II	Winter, 1897, 1898
Theory of Abstract Groups	Summer, 1899

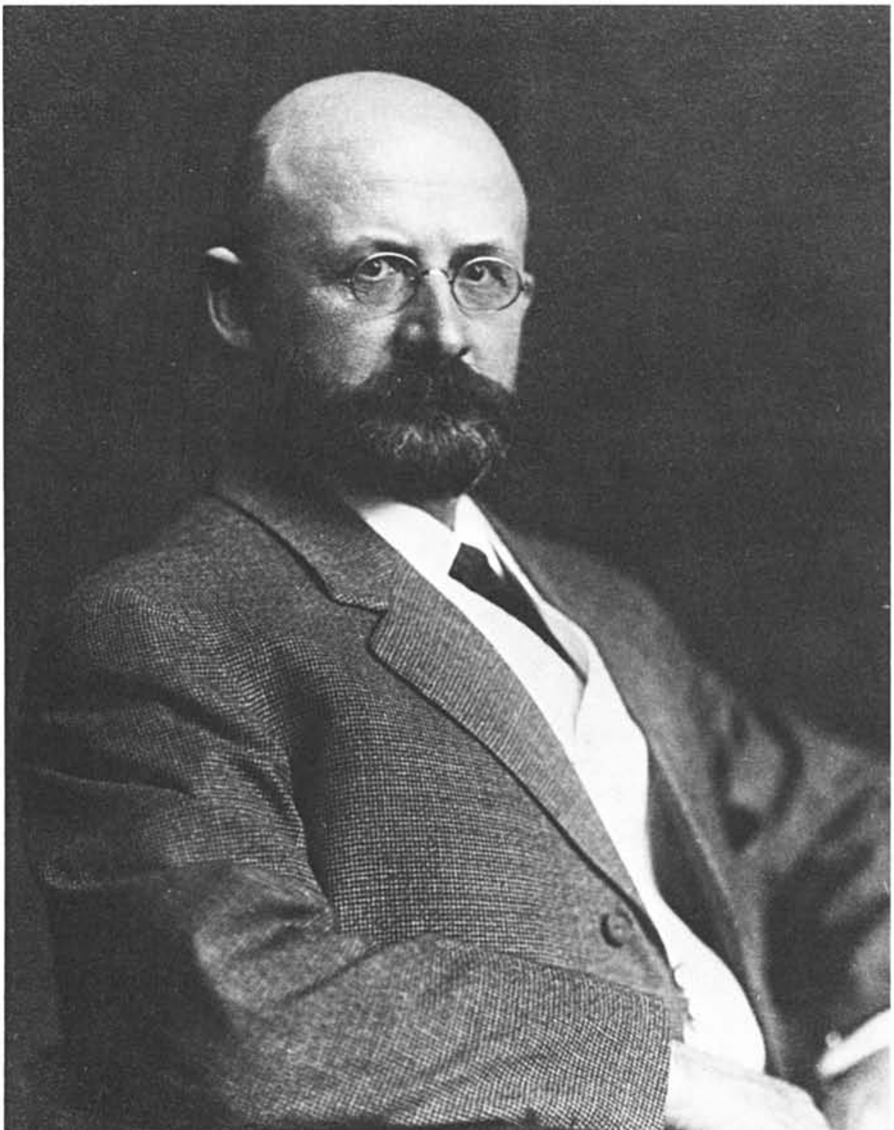
By Maschke:

Higher Plane Curves	Autumn, 1894
Analytical Mechanics	Spring, 1895
Algebraic Surfaces	Spring, 1895
Weierstrass on Elliptic Functions	Winter, 1895
Higher Plane Curves	Winter, 1897
Algebraic Surfaces	Spring, 1897
Linear Differential Equations	Spring, 1897

By E. H. Moore:

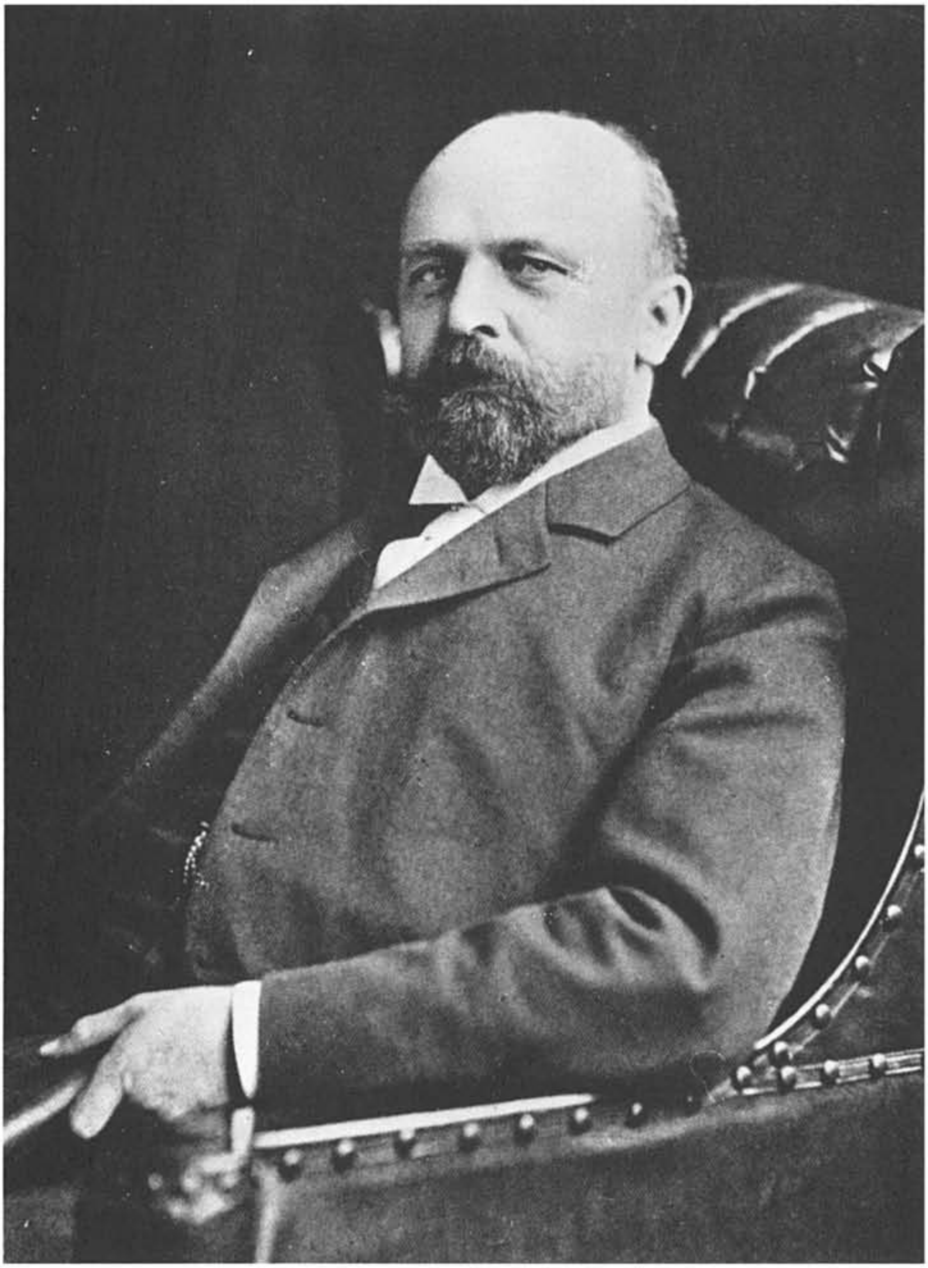
Projective Geometry	Autumn, 1896
Theory of Numbers	Autumn, 1897
General Arithmetic I and II	Winter, Spring, 1898

Also Harris Hancock (later at the University of Cincinnati) lectured on the calculus of variations (Spring, 1895), while George A. Miller (later at the University of Illinois) lectured on permutation groups (Summer, 1898). There was a closely related astronomy department, which emphasized mathematical astronomy. F. R. Moulton lectured there on general astronomy (Autumn,



Óskar Bólza

(University of Chicago Archives)



Heinrich Maschke

(University of Chicago Archives)

1896), and Kurt Laves on "the three body problem" (Spring, 1897). In 1902, the full list of graduate courses given reads as follows:

Autumn:	Theory of Equations	Bolza
	Projective Geometry	Moore
	Modern Geometry	Maschke
	Theory of Functions	Bolza
	Finite Groups	Dickson
Winter:	Theory of Equations II	Bolza
	History of Mathematics	Epsteen
	Higher Plane Curves	Maschke
	Theory of Functions II	Bolza
	Linear Substitution Groups	Maschke
Spring:	History of Mathematics	Epsteen
	Teaching Laboratory	Moore
	Vector Analysis	Lunn
	Linear Differential Equations	Maschke

These courses cover most of the topics of mathematics then of current interest. In the mathematics club, Moore spoke of finite fields (presumably his proof classifying all such), on Peano's space-filling curve, and on his elegant system of generators for the symmetry group S_n. Current concerns in finite group theory started then: J. W. A. Young spoke "On Hölder's enumeration of all simple groups of orders at most 200." In 1904, J. H. M. Wedderburn, a visitor from Scotland, proved his famous theorem that every finite division ring must be a commutative field. Algebra was there with a vengeance.

The original faculty at Chicago included some junior members: Jacob William Albert Young (Ph.D. from Clark University, presumably a student of Bolza); he retired as associate professor in 1926 and Harris Hancock (1892–1900). Then several of Chicago's own Ph.D.s were appointed to the faculty:

> Herbert Ellsworth Slaught (1861–1937); Ph.D. 1898. From assistant professor (1894) to professor (1913–1931). Slaught was primarily concerned with mathematical education and with assisting students. He was (with lively support from E. H. Moore) one of the principal founders of the Mathematical Association of America in 1916.

> Leonard Eugene Dickson (1874–1950); Ph.D., 1896. From assistant professor (1900) to professor (1910–1939). His massive and scholarly *History of the Theory of Numbers* was a landmark **[4]**, while his monograph *Algebras and Their Arithmetics* was translated into German and had a major influence on the German school of abstract algebra **[5]**. He was a powerful and assertive mathematician who directed at least 64 doctoral theses. It is rumored that he consciously had two classes of doctoral students: the regular ones and the really

promising ones (such as C. C. MacDuffee, 1921, who later went to Wisconsin; C. G. Latimer 1924, to Kentucky; Burton W. Jones 1928, to Cornell and Colorado; A. A. Albert 1928, to Chicago; Gordon Pall, 1929, to IIT; Alexander Oppenheim 1930, to Singapore; Arnold E. Ross 1931, to Notre Dame and Ohio State; R. D. James, 1928, to Berkeley and British Columbia; and Ralph Hull 1932, to Purdue). One can contemplate with amazement the wide influence exerted by Dickson. I can also recall his course (1930) in number theory, taught from a book of that title which he had written with sparse precision: He expected his students (Hull, James, Mac Lane, et al.) to have understood every argument and every shift in notation.

Arthur Constant Lunn (1877–1949); Ph.D. 1904, rose from associate professor in applied mathematics (1902) to professor (1923–1942). He had accumulated a massive knowledge of all of classical mathematical physics and lectured on this in enthusiastic but rambling ways that did not end with the formal end of the class hour. It was rumored among the students that a 1926 paper of his containing some of the new ideas of quantum mechanics had been rejected by some uncomprehending editor. Whether or not this was true, Professor Lunn was discouraged but very knowledgeable when I listened to him in 1930–1931.

2. THE SECOND MOORE DEPARTMENT

In the period 1908–1910, the verve and dynamism of the original Chicago department appears to have been gradually lost. Professor Maschke died in 1908; in 1910 Professor Bolza returned to Germany (Freiburg in Baden) but kept up an essentially nominal "nonresident" professorship at Chicago. He was still alert when I visited him in Freiburg in 1933. E. H. Moore developed his interest in postulational generality to a form of "general analysis" which tracked properties of integral equations in terms of functions "on a general range". The first form of his general analysis was presented in his colloquium lectures of the AMS at Yale University in 1906, and published (in considerably altered form) in 1910. At that time, it was very much in order to find the ideas underlying the existence theorems for solutions of integral equations — and indeed this objective led David Hilbert to his study of what are now called Hilbert spaces. Moore's formulation of these ideas did not succeed, in part because of his delay in publishing. He continued to work on it for 20 or more years, developing a second form of general analysis which was written in a logistic notation derived from Peano — a notation which was precise but hard to read, and which did not include formal logical rules of inference. In 1930–1931, his general analysis was presented in a six-quarter sequence of courses at Chicago; there were not many students. This version has been written up by Moore's student and associate R. W. Barnard; it is a monument to a timely but failed initiative [**11**].

This then is the sense in which the initial department at Chicago (Moore, Bolza, and Maschke) came to its effective end in 1907–1910. A new team appeared: Dickson, as already noted, plus Bliss and Wilczynski:

> Gilbert A. Bliss (1876–1951), Ph.D., 1900, Chicago, wrote his thesis with Bolza. After teaching at Minnesota, Chicago, Missouri, and Princeton, he became an associate professor at Chicago in 1908, Professor in 1913, and chairman in 1927 until his retirement in 1941. His interests covered many fields of analysis — algebraic functions, implicit function theorems, and the theory of exterior ballistics. He was an enthusiast for the calculus of variations.

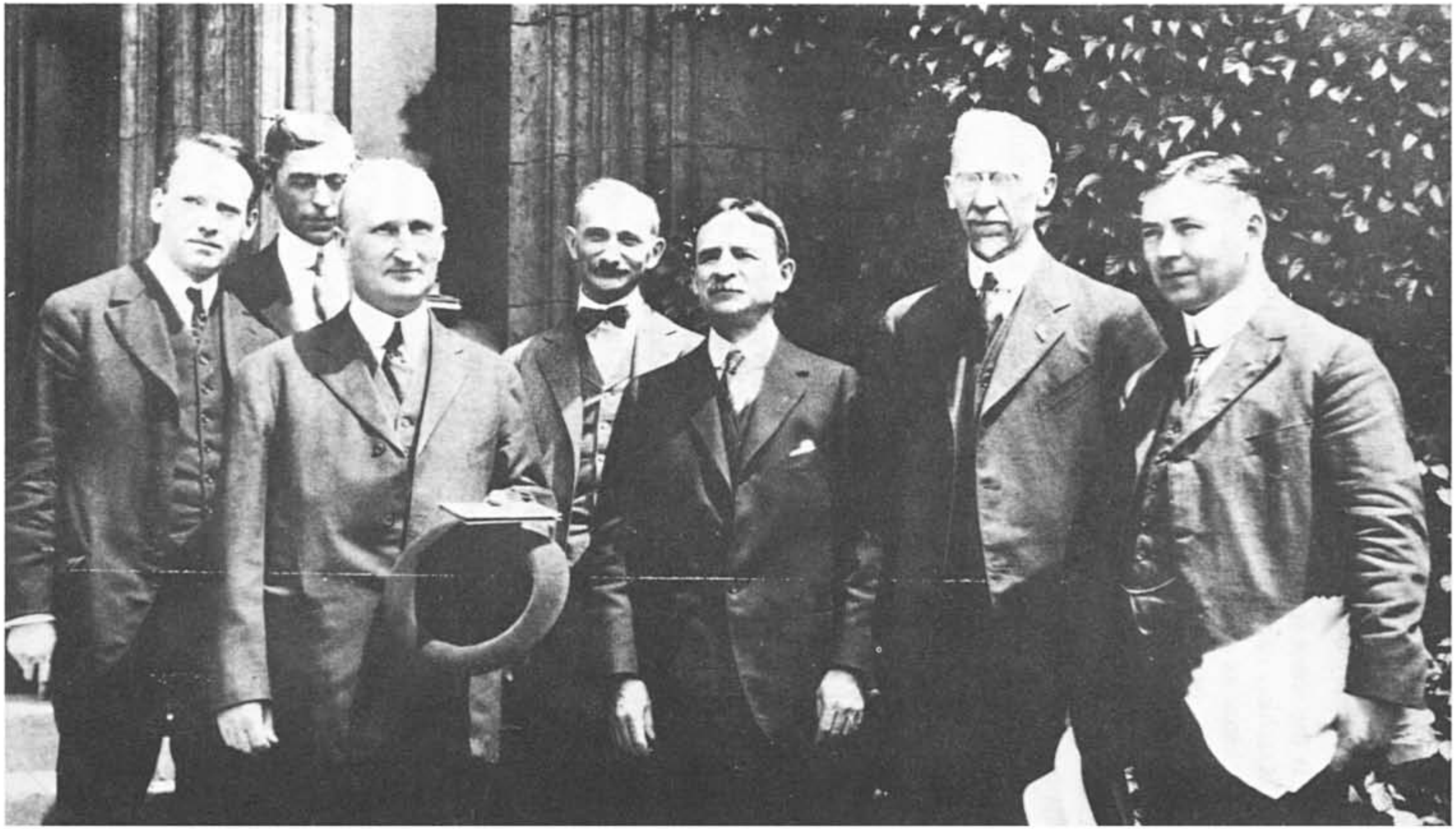
> Ernst Julius Wilczynski (1876–1932) received his Ph.D. at Berlin in 1897. After teaching at the University of California (1898–1907) and at the University of Illinois (1907–1910), he became an associate professor at Chicago in 1910, and full professor (1914–1926). He published voluminously and enthusiastically, especially in his favorite subject of projective differential geometry, where the local properties of curves and surfaces were analyzed in terms of canonical power series expansions. Clearly, Wilczynski was appointed at Chicago as a successor to the previous geometer, Maschke.

There were also two more junior appointments in this period:

> Mayme Irwin Logsdon (1881–1967) received her Ph.D. at Chicago in 1921, with a thesis on equivalence of pairs of hermitian forms, directed by Dickson. She was an instructor at Chicago from 1921 and rose to be an associate professor (1930–1946). It is my own observation that one of her duties was that of advising and helping many women who were graduate students at Chicago; moreover, she taught a survey course required of all undergraduates. After her retirement from Chicago, she taught for many years at the University of Miami, in Coral Gables, Florida.

> Ernest P. Lane (1886–1969) received his Ph.D. at Chicago in 1918 with a thesis in geometry, and returned to Chicago as assistant professor in 1923. He was a meticulous man, and an enthusiast for projective differential geometry.

Thus, in the period 1910–1927, the team at Chicago, headed by E. H. Moore, consisted primarily of Bliss, Dickson, and Wilczynski. There were



Left to right: Dickson, Wilczynski, Lunn, MacMillan, Moore, Slaught and Moulton
(Courtesy of Saunders Mac Lane)

many Ph.D.s in this period — 115 of them. Some of the more memorable, classified by subject, were the following:

E. H. Moore directed theses in general analysis:

- 1910 Anna Pell (later Anna Pell-Wheeler), subsequently a professor at Bryn Mawr College. In 1927 she delivered colloquium lectures for the AMS on the "Theory of quadratic forms in infinitely many variables and applications."
- 1912 E. W. Chittenden, who became a leader in point-set topology at the University of Iowa.
- 1916 W. L. Hart, later of Minnesota, a prolific author of textbooks.
- 1924 Mark H. Ingraham, later chairman and dean at Wisconsin.
- 1926 H. L. Smith, later a leader at Louisiana State University; he worked on the Moore–Smith limits well known in topology.
- 1926 R. W. Barnard, later Moore's amanuensis at Chicago.

G. A. Bliss directed theses in analysis:

- 1914 W. V. Lovitt, who taught at Colorado College and wrote a book on integral equations.
- 1924 L. M. Graves, subsequently professor at Chicago.

L. E. Dickson directed theses in algebra; for example:

- 1921 M. I. Logsdon
- 1921 C. C. MacDuffee
- 1924 C. G. Latimer

C. J. Wilczynski directed theses in geometry:

- 1915 Archibald Henderson, who became influential at North Carolina.
- 1918 E. P. Lane, later professor at Chicago.
- 1921 Edwin R. Carus, who later founded the Carus monographs (MAA).
- 1925 V. G. Grove, subsequently chairman at Michigan State.

Applied Mathematics: 1913, E. J. Moulton, to Northwestern.

Clearly, Moore was still a dominant influence. However, none of the Ph.D.s of this period achieved the profundity in mathematical research of the best five earlier Ph.D.s. Many, however, did rise to influential positions in important universities, as indicated below:

Bryn Mawr: Anna Pell-Wheller

Cornell: Burton W. Jones (Ph.D., 1928)

Louisiana State University: H. L. Smith

Michigan State: V. G. Grove

Northwestern: E. J. Moulton

University of Iowa: E. W. Chittenden

Colorado College: W. V. Lovitt

Wisconsin: M. H. Ingraham, C. C. MacDuffee

In 1924, E. H. Moore reported proudly that the department had by then trained 116 Ph.D.s, plus 15 more in mathematical astronomy, and that 52 of this total of 131 were already full professors at their respective institutions. In 1928 (according to the lists in the *Bulletin of the American Mathematical Society*), 45 Ph.D.s were granted in mathematics in the United States, of whom 12 (according to the Bulletin) or 14 (according to department records) were at Chicago. The nearest competing institutions were Minnesota (four Ph.D.s) and Cornell and Johns Hopkins, with three Ph.D.s each.

My conclusion is this: Chicago had become in part a Ph.D. mill in mathematics.

3. The Bliss Department

In 1927 G. A. Bliss became chairman at Chicago, while E. H. Moore continued as head — by then largely a titular formality. This ushered in a new period which lasted all during Bliss' terms as chairman (1927–1941). At about this time there were a number of new appointments to the faculty:

E. P. Lane was promoted to an associate professorship in 1927.
R. W. Barnard was appointed assistant professor in 1926.
L. M. Graves was appointed assistant professor in 1926.
Walter Bartkey (Ph.D. Chicago 1926) became an assistant professor of applied mathematics and statistics in 1927; he was subsequently dean of the division of physical sciences at Chicago (1945–1955).
Ralph G. Sanger (1905–1960), Ph.D. Chicago 1931, became instructor in 1930 and assistant professor in 1940–1946; he later moved to Kansas State University.

I note explicitly that every one of these appointees had received his Ph.D. at Chicago.

In the period 1927–1941, Bliss (who retired in 1941) and Dickson (who retired in 1939) were the dominant figures in the department. In the eleven year period from 1927–1937, there were 117 Ph.D.s awarded. Of these theses, Bliss, with the occasional cooperation of Graves, directed 35, of which 34 were in the calculus of variations. (In the prior 21-year period 1906–1926, there had been 17 theses devoted to this calculus.) Dickson directed 32 theses. In this period, Dickson's interests shifted from his earlier enthusiasms for quadratic forms and division algebras to an extensive (and somewhat numerical) study of aspects of Waring's problem: for each exponent n, find a number k such that every (or every sufficiently large) integer is a sum of at most k n-th powers. At that time, the then newer methods of analytic number theory could prove the "sufficiently large" part; Dickson was concerned with a corresponding explicit bound and with the calculation of what happened below that bound. The topics of Dickson's 32 theses projects were distributed as follows: thirteen on quadratic forms, twelve on Waring's problem, six on division algebras, and five on general topics in number theory. There were four additional algebraic theses directed by A. A. Albert. In geometry, Lane directed 20 theses and Logsdon two. Moore and Barnard together directed six theses. There were seven in aspects of applied mathematics and ten others on assorted topics. All told, this period represents an intense concentration on the calculus of variations and on number theory.

In this period there were some outstanding results. Two Chicago Ph.D.s went on to become president of the AMS: A. Adrian Albert (Ph.D. 1928, with Dickson) and E. J. McShane (Ph.D. 1930, with Bliss). In the calculus of variations, I note four: W. L. Duren, Jr. (Ph.D. 1929), who soon played an important leadership role at Tulane and later at Virginia, M. R. Hestenes (Ph.D. 1932) was later influential at UCLA, Alston S. Housholder (Ph.D. 1937) who shifted his interests and became a leader in numerical analysis at Oak Ridge, while Herman Goldstine (Ph.D. 1936) was associated with von Neumann in the development of the stored program computer. As already mentioned, some half dozen of the Dickson Ph.D.s did effective work in number theory. Mina Rees (a Dickson Ph.D., 1931) subsequently was the first program officer for mathematics at the Office of Naval Research. Her leadership there set the style for the subsequent mathematics program at the NSF; subsequently, Dr. Rees became founding president of the Graduate School and University Center of the newly established City University of New York. In 1983, she was awarded the Public Welfare Medal of the National Academy of Sciences.

In functional analysis Leon Alaoglu (Ph.D. 1938, with L. M. Graves) became famous for his theorem that the closed unit ball in the dual space of a Banach space is compact in the weak-star topology. After teaching at Harvard and Purdue and doing more research, he became a senior scientist at the Lockheed Aircraft Corporation. Malcolm Smiley took his Ph.D. in the calculus of variations in 1937, but then switched to active research in algebra. Ivan Niven, a Ph.D. of Dickson's in 1938, studied then with Hans Rademacher at the University of Pennsylvania. After teaching at Illinois and Purdue, he went to the University of Oregon and did decisive research on uniform distribution of sequences modulo m. Frederick Valentine (Ph.D. 1937 in the calculus of variations) was subsequently at UCLA where he published an important book on convex sets.

To summarize: In this period the department at Chicago trained a few outstanding research mathematicians and a number of effective members of this community — plus produced a large number of essentially routine theses. Was this because there was an undue concentration on a few special fields, or because the presence of so many graduate students meant that the faculty was forced into finding routine topics? In some cases they may have failed to appreciate students' potential. I do not know. I do clearly recall my own experience as a graduate student at Chicago (1930–1931). Since the calculus of variations was evidently a major issue there, I signed up for Professor Bliss' two-quarter course in this subject. Sometime well into the first quarter I had trouble putting the (to my mind necessary) ε's and δ's into his rather sketchy proof of the properties of fields of extremals. So I ventured to ask Professor Bliss how to do this. At once he produced all the needed epsilons, with great skill — but he also made it very clear to me that I did not need to concern myself with such details; graduate students were expected to get chiefly an overall impression of the shape of the subject. Some years later, I had occasion to study Bliss' book on algebraic functions; I observed then that this book correctly reproduced the suitable German sources but did not press on to get a real understanding of why things worked out and what the Riemann–Roch theorem really meant.

There were lighter moments. Professor Bliss liked to kid his students. One day in his lectures on the calculus of variations, he recounted his own earlier experiences in Paris. After he sat down in the large lecture amphitheatre there, an impressive and formally dressed man entered and went to the front. Bliss thought it was the professor himself, but no, it was just his assistant who cleaned the blackboards and set the lights. When the professor finally arrived, all the students stood up. At this point in his story, Bliss observed that American students do not pay proper respect to their professors. So the class agreed on suitable steps; I was the only member owning a tuxedo. The next day, arrayed in that tuxedo, I knocked on the door for Professor Bliss to report that his class awaited him. When he came in they all rose in his honor.

Of the six students of Moore and Barnard during this period, only Y. K. Wong (Ph.D. 1931) continued substantial activity. With Moore, he had studied matrices and their reciprocals; in his later research (at the University of North Carolina) he was concerned with the use of Minkowski–Leontief matrices in economics (Wong, **[12]**).

There remains the fascinating question: In the early days, Moore had been dynamic and remarkably effective in training graduate students. What changed? As I have already noted, he was still an alert critic when I knew him in 1930, and he had continued to work diligently on his form of general analysis. But he did not publish. According to an obituary by Bliss, he published only two substantial research papers after 1915, both in 1922, and one of them with H. L. Smith on the important concept of the Moore–Smith limit. At the start, Moore had been in lively contact with many current developments in mathematics. I conjecture that he had gradually lost that contact, in part because of a heavy preoccupation with his own ideas in general anlaysis, and in part because he may have depended on the exchange of ideas with his contemporaries Bolza and Maschke, while the newer and younger appointments at Chicago did not provide an effective such exchange.

4. Appointments by Bliss

In this period (1927–1941), there were a number of other appointments to the faculty, as follows:

A. Adrian Albert (1905–1972), Ph.D. Chicago 1928; assistant professor (1931) to professor (1941).

M. R. Hestenes (1906–), Ph.D. Chicago 1932; assistant professor (1937), associate professor (1942–1947) later influential in numerical analysis and combinatorics at UCLA.

W. T. Reid (1907–1977), Ph.D. Texas 1927; instructor, Chicago (1931), associate professor (1942–1944); later a professor at Northwestern, Iowa, and Oklahoma.

With these appointments, note the emphasis on the two fields of algebra and the calculus of variations — and on Ph.D.s from Chicago. (Reid came from Texas, but had spent the years 1929–1931 as a postdoctoral fellow at Chicago.)

Later on, the department made real attempts to appoint mathematicians not from Chicago and in new fields; two of them, as follows, did not last:

Saunders Mac Lane (Ph.D. Göttingen 1934); instructor, Chicago (1937–1938), then to Harvard. I believe that my appointment in 1937 at Chicago was due to the intervention of President Hutchins. At any rate, I had met Hutchins in 1929, and he had personally arranged to get me a graduate fellowship at Chicago for 1930–.

Norman Earl Steenrod (1910–1971), Ph.D. Princeton 1936; assistant professor Chicago (1939–1942); then to the University of Michigan as assistant professor (1942), in 1945 to Princeton.

Otto F. G. Schilling (1911–1973), Ph.D. Marburg 1934; instructor Chicago, (1939) to professor (1958); in 1961 to Purdue.

There are, to be sure, rumors of appointments which were *not* made. Thus, the famous German analyst and number theorist Carl Ludwig Siegel left Göttingen in the spring of 1940 and escaped via Norway to the United States. It then became clear that he needed a suitable position in this country; rumor has it that G. A. Bliss knew this but did not act on this possibility; soon Siegel became a professor at the Institute for Advanced Study in Princeton.

The appointment of Steenrod, who soon became a noted topologist, may well have been stimulated by the use of topology and the related theory of critical points (Marston Morse) in the calculus of variations. Up until this point the appointment policy at Chicago seems to have been based on what I might call the "inheritance principle": If X has been an outstanding professor in field F, appoint as his successor the best person in F, if possible the best student of X. Let me re-examine the appointments at Chicago in this light.

Bolza was outstanding in analysis and had written an authoritative book on the calculus of variations. Shortly after he left in 1908, his best student, G. A. Bliss, was appointed. Subsequently, three students of Bliss were appointed: Graves, Sanger, and Hestenes, as well as W. T. Reid from Texas. There resulted a great concentration on such topics as variants of the problem of Bolza in the calculus of variations, but the school at Chicago missed out on the major development of the subject in the early 1930s, as represented by the work of Marston Morse on the calculus of variations in the large. Chicago was of course aware of this work, but did nothing much about it. Specifically, in the spring of 1931, Bliss conducted a seminar on this topic, and assigned Mac Lane to report on Betti numbers and their meaning. Mac Lane thereupon studied the (then unique) text by Veblen, and reported on the Betti numbers but not on their meaning (which he did not really understand).

In geometry, the death of that notable geometer Maschke in 1908 was soon followed by the appointment of another geometer, Wilczynski, in 1910, and then, upon his retirement in 1926, by the promotion in 1927 of his best student E. P. Lane. The special emphasis on the subfield of projective differential geometry (as in Lane's subsequent book) gradually lost its importance, both in Chicago and in Shanghai (where the senior professor Buchin Su worked in this field). In 1939, George Whitehead, one of the graduate students, asked Professor Lane for a thesis topic in projective differential geometry. Instead of giving him a topic, Lane gave Whitehead the good advice to work in the newer field of topology with Steenrod; Whitehead later (at MIT) became a leader in this field.

In general analysis, E. H. Moore had considerable influence on Lawrence Graves; then in 1928, Moore's student Barnard was appointed to the faculty. However, Moore did not work out the possible connection between his general analysis and the study (at other centers) of Hilbert spaces and of functional analysis. Moore was a great enthusiast for infinite matrices, postulational methods, and Peano. In early work, Peano had the axioms for a (two-dimensional, real) vector space. I never learned about these axioms from Moore — and had to learn them in 1932 from Herman Weyl in Göttingen (who had clearly formulated them in his 1917 book on relativity). This is another small piece of evidence that Moore had lost contact.

In mathematical astronomy and applied mathematics, Kurt Laves (first appointed about 1894) was the first faculty member. Then various Chicago Ph.D.s were appointed in astronomy or in mathematics. The most outstanding was perhaps F. R. Moulton; others were W. D. Macmillan, A. C. Lunn, and then Walter Bartky in 1926. Perhaps because of his activity in military research during WW II, Bartky's interests shifted to administrative matters; he became a dean and finally a vice president for research at the university from 1956 to 1958. In this sense, the line of inheritance in applied mathematics died out, not to be renewed until the appointment in 1963 of two former students (at Chicago) of S. Chandrasekhar.

E. H. Moore provided the initial impetus in algebra, group theory, and number theory; the appointment of his first Ph.D. student Dickson in 1900 was a strong step. Then in 1931, Dickson's best student, Adrian Albert, was appointed. In 1945, Albert's recommendation brought the appointment of Irving Kaplansky (Ph.D. from Harvard; Mac Lane's first student). Albert kept the interest in algebra generally and in group theory in particular alive, and in 1961 organized a "special year" on group theory at Chicago. It was during this year that Walter Feit (M.S. Chicago, 1951) and John Thompson (Ph.D. Chicago 1959) worked out their "odd order" paper with the remarkable proof that every finite simple group is either cyclic or of even order. This was a vital step toward the subsequent classification of all finite simple groups.

Thus, in algebra the inheritance theory of appointments worked splendidly, while in other fields, as noted, it was not successful in the long run.

5. The Lane Department

When G. A. Bliss retired in 1941, E. P. Lane became chairman of the department of mathematics. He made several attempts to revive and strengthen the department, but the times were not propitious, largely because of the onset of WW II. When President Robert M. Hutchins (with considerable administrative courage) brought the Manhattan project on atomic energy to Chicago, it was soon housed in the department's treasured building, Eckhart Hall, and the mathematicians were moved out to one of the towers of Harper Library. There were no new appointments till the postwar appointment of Kaplansky (1945). There were 21 Ph.D.s (1941–1946), including Whitehead (1941), the algebraists R. D. Schafer (1942) and Daniel Zelinsky (1946), and in the calculus of variations the very young mathematician J. Ernest Wilkins (1942) who later did notable research in applied mathematics.

Ph.D.s to Women at Chicago

In the 39 years 1908–1946, the department awarded 51 Ph.D.s to women out of a total of 270 Ph.D.s in mathematics in that period. It is likely that more Ph.D.s were awarded to women at Chicago than at any other American university in this period. Chicago had been coeducational from the start, but 1908 was the year when the first Ph.D. was awarded to a woman — Mary Emily Sinclair, who subsequently became professor and chairman at Oberlin College. Also, 1946 is the year when Marshall Stone, as a new chairman, came to drastically change the direction of the department; this determines the period 1908–1946 which I chose for this list.

Thanks to Marlene Tuttle of the alumni relations office of the University of Chicago I have been able to collect definitive information on almost all of these 51 women mathematicians. In particular, I could locate the college or university where they subsequently taught mathematics. After classifying these institutions as women's colleges, coeducational colleges (e.g., Oberlin), universities, or research universities, I get the following table based on one chosen institution for each Ph.D.; in a few cases there was a change of institution:

Subsequent Academic Employment of Women Ph.D.s

Ph.D.	Date: 1908–1931	1932–1946
Women's college	8	12
College	7	6
University	5	5
Research university	6	1
Total	26	24

In one case (in the second period) I was unable to locate any subsequent teaching employment; I believe that the individual was married and did not take up teaching. But note that of the 51 listed, 50 did engage in teaching, most of them at just one institution and for a considerable period. The Ph.D.s from Chicago provided an effective source of faculty — especially at women's colleges. Note that teaching loads at such colleges were then quite heavy.

I have ventured to classify seven of the institutions as "research universities", although that term was not then in use. The word appeared only later as a label for those universities which seek to acquire substantial research funds from the government. At any rate the seven research universities listed above were (in chronological order of the degrees) Wisconsin, Berkeley, Minnesota, Chicago, Illinois, Northwestern, and Illinois again, the last in 1932. It will make my classification clear if I list the five universities (1932–1946) as Kent State, University of Utah, University of Oklahoma, University of Alabama, and Temple University. All told, this tabulation indicates clearly that in all this period very few of the women went or were sent to major research universities. (In 1916, the University of California at Berkeley did not then have its present standing in mathematics.)

In reporting this situation, I deliberately said "were sent," because in those days positions for new doctorates in mathematics were managed by what is now called the "old-boy network". At present this is a term of opprobrium; at that time it referred to a placement system for a small number of graduates that in fact worked much more efficiently than the present system, which inevitably is applied to much larger numbers and involves massive employment interviews at the January AMS meetings, plus pious declarations of equal opportunity in advertisements which (especially today) make it clear that opportunity beckons at X university only if your research lies in a field already X-represented.

The old-boy network functioned as follows: all the active mathematicians such as Veblen at Princeton, Bliss at Chicago, or Birkhoff at Harvard (plus many others, such as Hildebrandt at Michigan) had pretty shrewd ideas as to the level of mathematical activity at many schools, and they also had quite detailed (but perhaps mistaken) knowledge of the qualities of their own current products. So when they heard that Oberlin College or the women's college of North Erehwon or the University of W had a vacancy, they knew which of their graduates would be an appropriate candidate there, and they acted accordingly. (Of course, the candidate's professor was also an actor in this network.) The system did make mistakes. For example, in 1957, Michigan sent to Chicago letters of recommendation for a new Ph.D., one Steven Smale. The letters were not especially enthusiastic. At that time, the department had few vacancies for an instructor, so Smale was appointed at Chicago in the college mathematics staff, then separate from the department and intended primarily for undergraduate teaching. This goes to show that there can be misjudgements about research potential.

At any rate, the table above makes it clear that Chicago did not normally send its women Ph.D.s to universities anxious to acquire research hot-shots.

I also tabulated published research papers in mathematics (1931–1960) for the 25 women Ph.D.s in the second period. In ten cases, I found 14 publications all told, in most cases the publication of the thesis, but I note that one woman had three publications. I have not tabulated research publications for men Ph.D.s from the period (1932–1946); some were prolific, while others hardly published.

On the evidence, I summarize thus: In this period, women were encouraged to study for the Ph.D. degree at Chicago, and there was a role model on the staff to help and support them (Mayme I. Logsdon). But these women students were not really expected to do any substantial research after graduation; the doctorate was it, and in many cases the thesis topic was chosen to suit. This last sentence agrees with my own recollection of the situation and atmosphere at Chicago during my graduate study there in 1930–1931. I might add that for some of the men students there was the same low level of research expectations — but not for all.

For completeness, I add that in the following period (1946–1960) at Chicago there were exactly four Ph.D.s granted to women; among those, one to Mary Weiss (Ph.D. with Zygmund 1957) who made an impressive research career. I note that the women's liberation movement was yet to come, and that there apparently were very few women graduate students present. My own course records (in basic graduate courses in algebra and topology in this period) show 38 women out of 267 students all told in my courses — about 14 percent. It appears that women students began graduate work, but that few went on to the Ph.D.

6. The Stone Department

Robert Maynard Hutchins, president of the University of Chicago (1929–1951), had brought the Manhattan project to the university during WW II, and with it many notable scientists including Enrico Fermi, James Franck, and Harold Urey. As the war drew to a close, he and his advisors decided to try to hold these men and their associates at Chicago. For this purpose, he established two research institutes, now known as the Fermi and the Franck Institutes. He and his advisors realized that there should be a much-needed strengthening of the department of mathematics. With the advice of John von Neumann (who had been associated with the Manhattan project), they approached Marshall H. Stone, then a professor at Harvard, suggesting (after some talk of a deanship) that he come to Chicago as chairman of mathematics. Stone had thought deeply about the conditions which would support a great department of mathematics at a level well above that then present at Harvard or Princeton. After receiving suitable assurances from President Hutchins, Stone came to Chicago in 1946. He thereupon brought together what was in effect a whole new department. In each such new case, I specify the dates of their activity on the Chicago faculty:

As professors:

> André Weil (1947–1958), a notable (and contentious) French mathematician, one of the leading members of the Bourbaki group. He had just published his fundamental book on the foundations of algebraic geometry, containing his proof of the Riemann hypothesis for function fields.
>
> Antoni Zygmund (1947–1980), a Polish analyst, interested in Fourier Analysis and harmonic analysis. Zygmund had been in this country at Mount Holyoke and then at the University of Pennsylvania.
>
> Saunders Mac Lane (1947–1982), from Harvard; he was at that time active in studying the cohomology of groups and the related cohomology of Eilenberg–Mac Lane spaces in topology.
>
> Shiing–Shen Chern (1949–1959), an outstanding Chinese mathematician with interests in differential geometry and topology (for example, his characteristic classes).

As assistant professors, Stone brought to Chicago:

Paul R. Halmos (1946–1961), working in measure theory and Hilbert space. He had just published his elegant exposition "Finite Dimensional Vector Spaces", with a presentation influenced by his contacts with John von Neumann.

Irving E. Segal (1948–1960), an enthusiast for rings of operators in Hilbert space and their application to quantum mechanics.

Edwin H. Spanier (1948–1959), a young and knowledgeable algebraic topologist from Princeton; he had recently finished his Ph.D. under the direction of Norman Steenrod.

Of the previous department, Albert and Kaplansky were immediately enthusiastic members of this new team which then read:

Professors: Albert, Chern, Mac Lane, Stone, Weil, and Zygmund.

Assistant Professors: Halmos, Kaplansky, Segal, and Spanier.

Of the other previous members, Hestenes soon left for UCLA and J. L. Kelley, who had briefly been an assistant professor, left for Berkeley. Professors Barnard, Graves, Lane, and Schilling stayed on (in most cases until retirement); they cooperated but were not really full members of the new dispensation.

The new group covered quite a variety of fields. There were exciting graduate courses, and some clashes of opinion (for example, between Weil and Segal). Weil, in continuing the tradition of Hadamard's seminar in Paris, taught a course called "Mathematics 400" in which the students were required to report on a paper of current research interest *not* in their own field; a few students were discouraged by his severe criticisms but many others were encouraged to broaden their interests. Under Stone's encouragement, a whole new graduate program was laid out, with three-quarter sequences in algebra, analysis, and geometry (see Mac Lane [**8**]).

This was the immediate postwar period, when many ex-soldiers could take up advanced study under the G. I. Bill. Thus, there were many lively students at Chicago; in the period 1948–1960 there were 114 Ph.D.s granted by the department. Among the recipients were a number of subsequently active people, including:

1950: A. P. Calderón, R. V. Kadison, and I. M. Singer

1951: Murray Gerstenhaber, E. A. Michael, and Alex Rosenberg

1952: Arlen Brown and I. B. Fleischer

1953: Katsumi Nomizu

1954: Louis Auslander and Bert Kostant

1955: Errett Bishop, Edward Nelson, Eli Stein, and Harold Widom
1956: R. E. Block, W. A. Howard, Anil Nerode, and Guido Weiss
1957: B. Abrahamson, Donald Ornstein, Ray Kunze, and Mary Weiss
1958: Paul Cohen, Moe Hirsch, and E. L. Lima
1959: Hyman Bass, John G. Thompson, and Joseph Wolf
1960: Steve Chase, A. L. Liulevicius, and R. H. Szczarba.

The qualities of this group of graduates, in my view, match the qualities of the best graduates of the first group of the Moore department. For example, by 1988, eight of those listed just above had been elected to membership in the section of mathematics of the National Academy of Sciences; by that date in the whole country, about 30 had been elected from those with Ph.D.s from these years 1948–1960; Princeton, with six, contributed the next largest contingent.

In this period at Chicago, there was a ferment of ideas, stimulated by the newly assembled faculty and reflected in the development of the remarkable group of students who came to Chicago to study. Reports of this excitement came to other universities; often students came after hearing such reports (I can name several such cases). This serves to emphasize the observation that a great department develops in some part because of the presence of outstanding students there. (This is true also of Göttingen in 1930–1933 and Harvard in 1934–1948 in my own experience.)

By 1952, Marshall Stone had grown weary of the continued struggle with the administration for new resources; Mac Lane succeeded him as chairman (1952–1958). The department continued in similar activity until about 1959, when it suddenly came apart. In 1958, Weil left to go to the Institute for Advanced Study, Chern and Spanier left to go to Berkeley in 1959, Segal left for MIT in 1960, and Halmos left for Michigan in 1961. Those departures essentially brought to a close the Stone Age. The department was soon rebuilt under A. A. Albert (chairman, 1958–1962, and dean, 1962–1974) and Irving Kaplansky (chairman 1962–1967). (This later period will not be described in this essay.) But there had been just this one period 1945–1960 when Chicago, in its new style, was without doubt the leading department of mathematics in the country.

7. Why the Change?

One may wonder why the Stone Age came to such an abrupt end. In some part, this may just be the inevitability of changes in human situations; people grow older and shift their interests. Nationally, Sputnik in 1957 stimulated much more extended government support for mathematics in the period 1958–1960; one result was that there soon were more mathematics departments of major standing — for example, Berkeley and MIT. There are also explanations "internal" to the University of Chicago. After 1950, Marshall Stone traveled frequently, and clearly the loss of his presence and leadership made a difference. Mac Lane may have made mistakes as chairman; Albert (unpublished) and Halmos (published) evidently thought so. A major observation is this: In the period 1949–1957, except for temporary instructors, there were no new appointments to the faculty; there was one appointment in 1958. This suggests that there was not a sufficient inflow of ideas.

The top administration of the university had changed. Robert Maynard Hutchins resigned as president in 1951; the new president or "chancellor" (1951–1960) was Lawrence A. Kimpton. It seems clear that the trustees instructed Kimpton to pay attention to the neighborhood and to achieve a balanced budget for the university. This he did, but there were intellectual costs. For example, about 1954 André Weil noted an important paper of a young man, Felix E. Browder, on partial differential equations; Browder came to visit and gave a talk. The department proposed his appointment as assistant professor, but the administration declined to act: they had observed that Browder's father, Earl Browder, had been head of the communist party in the United States. In fact, Felix had been born in Moscow. A decision on appointments on such shaky grounds would never have happened while Hutchins was president (and indeed Browder was subsequently appointed to the faculty). There are other examples of the intellectual ineptitude of the Kimpton administration. Perhaps universities cannot maintain great departments without outstanding academic leadership at the top — leadership which was subsequently restored at Chicago.

8. Requirements for Good Departments

On the basis of this and other examples, it is tempting to speculate: What does it take to make a great department of mathematics?

(1) Outstanding faculty, preferably younger; in particular, including some not on tenure.

(2) Numerous lively students, helping to prod the faculty.

(3) Exciting fields of study, preferably some new thrusts, and certainly several different fields — perhaps even a clash of interests between fields.

(4) Several instructors (e.g., postdoctorals or temporary instructors), again bringing in new ideas.

(5) Active contacts between people, e.g., colloquiums, mathematics clubs, seminars, and (important) meetings at tea.

(6) Understanding support by the university administration.

(7) An active sense of common purpose.

These conditions seem to me to have been met in the examples of great departments which I personally know: Göttingen (1930–1933), Harvard (1930–1960), Princeton, and Chicago (1897–1908, Moore; 1947–1960, Stone). When two or more of these conditions fail, a department can lose momentum. When they are present, real advance is possible.

9. The Bliss Department Reviewed

Was this department just a "diploma mill," as asserted above, or are there other aspects? This will now be reconsidered in the light of Bill Duren's recollections [6] and the autobiographical notes of Bliss [3] himself, all as cited below. I have also profited from a considerable discussion with Herman Goldstine, who served as a research assistant to Bliss in the mid thirties, when Bliss was preparing his book on the calculus of variations.

In the period 1920–1935 there were many graduate students at Chicago, and hence quite large advanced classes; this is very different from the present case when at Chicago there may be 15 or 20 advanced (post master's) courses offered in a given quarter, with no requirement for a minimum number of attendees. It is reported that in the twenties the department of education at Chicago arranged for special trains from Texas to bring the students for the summer quarter. In some departments, it became the custom for teachers elsewhere to come to Chicago summer after summer so as to finally arrive at a Ph.D., and indeed this happened then in some cases in mathematics. It would be unthinkable now, only in part because the summer quarter has shrunk. Mathematics Ph.D.s from Chicago were stationed in influential positions at universities throughout the Midwest and the South, and of course they sent their best undergraduate students to Chicago for graduate study. The activity of the department must be judged in the light of this massive input of students. According to Goldstine, Bliss felt that there was in the United States a great need for well-trained teachers of mathematics, and that Chicago was ideally placed to fill that need. In his autobiographical note, Bliss says that the merit of a department of mathematics should not be rated by an index such as the average number of research papers per Ph.D.; at Chicago there were just too many students to expect them all to do research. He implies that what really matters is the research done by a few outstanding students, while in the faculty itself what matters most is the research done by a few outstanding professors, such as Dickson (whom he names). All this took place long before the present widespread conviction that every department member is expected to do research to get promotions and government grants. In department meetings, Bliss often depended for advice on H. S. Everett, whose formal position was that of extension professor, and who was not interested in research. Everett was indeed effective in helpfully correcting student's papers in correspondence courses, and this activity did indeed bring students to Chicago — for example, I. M. Singer, post WW II.

Bliss said: "The real purpose of graduate work in mathematics, or in any other subject, is to train the student to recognize what men call the truth and to give him what is usually his first experience in working out the truth in some specific field".

If graduate work at Chicago in this period is judged on this basis, it must be accounted a rousing success — as for example with the Ph.D.s to women noted above.

The autobiographical note [**3**] by Bliss also exhibits the development of his interest in the calculus of variations. After studying mathematical astronomy with F. R. Moulton, he switched to mathematics and Bolza, and soon came across a copy of the 1879 lecture notes by Weierstrass on the calculus of variations. They were fascinating, as might well be, because it was there that rigorous proof was finally brought to fruition in this centuries-old subject; the dissemination of such Weierstrass notes had a wide effect. It may be that this initial enthusiasm was the leading principle of all of his career — there he found additional problems in a more general setting in which the truth could be teased out, and which students could handle. All these truths were brought together in his treatise [2], published at the end of his life, which can be viewed as a systematic extension of the Weierstrass method to all the variants of the "problem of Bolza". Moreover, the ideas there were then ready to hand, so that when Pontrjagin and others much later saw that the calculus of variations was adapted to the study of optimal control, Bliss's student Hestenes brought it all together in his 1966 book [**1**].

In a recent issue of the *Mathematical Intelligencer*, I have argued that many mathematicians today may specialize so narrowly on their first research field that they miss important connections. This may not be new.

As noted above, Bliss became chairman in 1927; I have argued that there might then have been more widely spread appointments to the faculty, with less emphasis on inheritance (and indeed, Sanger may have been regarded as the successor to Slaught). But in 1927 there may have been a different objective: a new mathematics building. Up until that time, the department had been housed on the upper floors of Ryerson, the physics lab. Bliss laid the plans for constructing Eckhart Hall next door as a building for mathematics, with a fine common room, central offices for mathematics and mathematical astronomy, ample faculty offices, and even space for graduate students. (In 1930, as a beginning graduate student, I occupied a fourth floor office which 40 years later served as the office for a full professor.) Eckhart Hall may well have set a pattern for mathematics buildings; at any rate it is reported that Oswald Veblen in Princeton kept track of Eckhart as he planned for the construction of Fine Hall for the Princeton Mathematics department.

These important things said, I return to my harsh judgment that by 1930 the department at Chicago had ceased to be really first class. This conclusion is not so much based on the various items of evidence assembled above, but on my own direct experience.

In the fall of 1929, as a senior at Yale, I chanced to meet Robert Maynard Hutchins, recently law dean at Yale and newly president at Chicago. He knew of my academic interests; finding that I intended to study mathematics, he told me that Chicago had an outstanding department, and that I should come there. Some weeks later, he wrote me to offer me a fellowship in the (then handsome) amount of $1,000. I accepted.

When I came in the fall of 1930, I attended Moore's seminar, as above, and signed up for courses with the leading members of the department:

Dickson's course on number theory presented a good treatment of the representation of integers by quadratic forms, but there was no indication of the connections of this with algebraic number fields, a subject with which I had a passing acquaintance. Dickson's own current interests were in the computations for the Waring problem, but when Landau came to give a visiting lecture, I could see that the center of interest was with the new ideas of analytic number theory (Hardy–Littlewood, Vinogradoff, and Landau). I learned something about approximations on major and minor arcs of the unit circle, but that was not a Chicago subject.

Lane lectured on projective differential geometry. I had never studied differential geometry, nor did Lane teach it; this left a serious gap in my background, not even adequately filled when in 1933 Herman Weyl's warning before my oral exam led me to bone up on the first and second quadratic forms of a surface. Despite this lack of background, I took Lane's course. I soon noticed an older student up in the front row with an open notebook in which he made only occasional careful entries; eventually, I learned that he had taken the course once before, and was now bringing his notes up to date with the latest refinements. At the time I was deeply offended by this display of pedantry.

The calculus of variations with Bliss (two quarters) taught me all about the brachistrochrone (I did not care) and about fields of extremals (I did), but I did not really learn anything about the connections with geometrical optics (I found this out in Göttingen) or about the connections with Hamiltonian mechanics, which I had to tease out later on my own. Bliss knew that there was Morse theory, but it was not taught at Chicago.

When I signed up for a course in the philosophy department with Mortimer Adler, Bliss disapproved.

Barnard supervised my M.A. thesis, which was an unsuccessful attempt to discover universal algebra. Barnard was then much taken up with Moore's use of functions on a general range, meaning functions $X \to F$, where X is an arbitrary set and F is a field — reals, complexes, or quaternions. Goldstine and I both think that Moore's emphasis on this "general" idea may have blinded him to other axiomatic approaches to functional analysis; I did not learn about Banach spaces until 1934 at Harvard.

Moore himself was in poor health.

At that time, President Hutchins was beginning to press for his new college devoted to general education; Bliss and other senior faculty members strongly opposed his ideas. This did not help the department.

My conclusions were not clearly formulated at that time, but they really came to this: The department of mathematics at Chicago in 1930–1931 was no longer outstanding in attention to current research. With Moore ill, there was no one on the faculty under whose direction I would have liked to write a Ph.D. thesis. I did not say this but simply put it that I had the the wanderlust and that I wanted to study logic — so I took off for Göttingen. There I did indeed find a great mathematics department. I can still recall the excitement at the start of each new semester with many new courses at hand: Lie groups (Herglotz) or group representations (Weyl) or Dirichlet series (Landau) or PDE (Lewy) or representation of algebras (Noether) or logic (Bernays). And there were many lively fellow students (many more, and on a level hardly present at Chicago): Gerhard Gentzen, Fritz John, Hans Schwerdtfeger, Kurt Schütte, Peter Sherk, Oswald Teichmüller, and Ernst Witt, for examples [10].

The conclusion seems to be that there are times when certain developments achieve a vibrancy and excitement with ample contacts with current departments which serve to stimulate faculty and students alike. May this analysis perhaps help to encourage more such cases.

Acknowledgments

Though the opinions voiced in this article are my own, I have been much helped by incisive comments from Bill Duren, Herman Goldstine, E. J. Mc Shane, Ivan Niven, Mina Rees, Arnold Ross, Alice Turner Schafer, and George Whitehead. All of them had direct experience with the mathematics department at Chicago.

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Eliakim Hastings Moore and the Founding of a Mathematical Community in America, 1892–1902

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Summary

In 1892, Eliakim Hastings Moore accepted the task of building a mathematics department at the University of Chicago. Working in close conjuction with the other original department members, Oskar Bolza and Heinrich Maschke, Moore established a stimulating mathematical environment not only at the University of Chicago, but also in the Midwest region and in the United States in general. In 1893, he helped organize an international congress of mathematicians. He followed this in 1896 with the organization of the Midwest Section of the New York City-based American Mathematical Society. He became the first editor-in-chief of the Society's *Transactions* in 1899, and rose to the presidency of the Society in 1901.

Contents

1. Introduction

The final decade of the nineteenth century and the first decade of the twentieth marked an incredible period of growth in American mathematics. During this time advanced mathematics, including mathematics at the research level, firmly took hold in many American universities. No longer did college mathematics mean merely arithmetic, trigonometry, the rudiments of algebra and geometry, and a smattering of calculus. No longer were American students essentially forced to travel to the great universities of Europe if they wished to study the modern advances in mathematics seriously. Whereas in the 1870s they would have been limited to working under Benjamin Peirce (1809–80) at Harvard, Hubert A. Newton (1830–96) at Yale,[1] or James Joseph Sylvester (1814–97) at Johns Hopkins, by the 1890s a dozen or more American universities could boast able research mathematicians. Furthermore, by 1910 several of these schools had native-son professors who enjoyed, or would soon enjoy, international reputations, a situation which was unprecedented in the history of American

[1] Hubert Anson Newton became a tutor at Yale in 1853 after having earned his undergraduate degree there in 1850. At the age of twenty-five he was appointed full professor in mathematics, a position he held until his death in 1896. Although his primary scientific interest lay in the studies of meteorites and comets, he was the mathematical mentor both of Josiah Willard Gibbs (1839–1903) and E. H. Moore. For a review of Newton's life and work, see J. Willard Gibbs, "Hubert Anson Newton", *The American Journal of Science*, 4th series, 3 (1897), 359–78.

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mathematics up to this time. At the University of Chicago, Eliakim Hastings Moore (1862–1932) mastered and extended the foundational work in geometry of David Hilbert (1862–1943), in addition to making important contributions to group theory, while his student, Leonard Eugene Dickson (1874–1954), pursued group theory and the study of linear associative algebras. At Harvard, William Osgood (1864–1943) worked in complex variables as well as in other branches of analysis, while Maxime Bôcher (1867–1918) advanced the theory of integral equations. At Princeton, another of E. H. Moore's students, Oswald Veblen (1880–1960), did seminal work in geometry, while perhaps Moore's most famous student, George D. Birkhoff (1884–1944), concentrated primarily on dynamics and mathematical physics before moving on to Harvard in 1912. With this list as evidence, it comes as no real surprise that many consider E. H. Moore to have been the prime driving force which finally turned the United States from a mathematical wasteland into a leader in the field.[2]

Moore's expertise in parenting new mathematicians, although important to the subsequent development of mathematics in America, proved to be only one of his lesser talents. First and foremost, he was a mathematical activist bursting with new ideas and full of the vigour needed to implement them successfully. During the decade from 1892–1902 his energy produced not only a major centre of mathematics at the newly-formed University of Chicago, but also a firm and growing national commitment to serious mathematical activity. In this study we shall follow the young Moore to his appointment as professor at Chicago in 1892 and through his term as president of the American Mathematical Society in 1902. In tracing these ten years of his life, we shall see American mathematics develop from early puberty to young adulthood.

2. The Mathematics Faculty at the University of Chicago, 1892

E. H. Moore was born on 26 January, 1862, in Marietta, Ohio, a small town on the Ohio–West Virginia border.[3] The son of of a scholarly Methodist minister and the grandson of a well-to-do United States Congressman, the young Eliakim had perhaps more opportunities than most children growing up in rural Ohio during those immediately *postbellum* days. While his grandfather was in Congress, for example, Moore spent a summer in Washington employed as a congressional messenger. More important for our story though, he worked one summer as a research assistant to Ormond Stone (1847–1933), then the head of the Cincinnati Observatory. This

[2] See, for example, Judith V. Grabiner, "Mathematics in America: The First Hundred Years," and Garrett Birkoff, "Some Leaders in American Mathematics: 1891–1941", both in *The Bicentennial Tribute to American Mathematics*, edited by Dalton Tarwater (n.p.: Mathematical Association of America, 1977), pp. 9–24 and 25–78, respectively. The semicentennial addresses of Eric Temple Bell and George D. Birkhoff also reflect this opinion. See Eric T. Bell, "Fifty Years of Algebra in America", and George D. Birkhoff, "Fifty Years of American Mathematics", both in *Semicentennial Addresses of the American Mathematical Society*, 2 vols, edited by Raymond C. Archibald (New York: American Mathematical Society, 1938), II, 1–34 and 270–315 respectively.

Many of E. H. Moore's correspondents also hold this view. Moore's papers are housed in the Department of Special Collections at the University of Chicago in nineteen boxes. The first four boxes contain his correspondence, but unfortunately the number of letters dating from the period of our study are exceedingly few and unilluminating. The bulk of the preserved correspondence dates from 1918 to 1932.

[3] The majority of the biographical details presented here come from Gilbert A. Bliss's article "Eliakim Hastings Moore", *Bulletin of the American Mathematical Society*, 2nd series, 39 (1933), 831–8. Bliss followed this up with a careful look at Moore's mathematical contributions in "The Scientific Work of Eliakim Hastings Moore", *Bulletin of the American Mathematical Society*, 2nd series, 40 (1934), 501–14. For other biographical sketches of Moore, see Archibald, ed. (footnote 2) I, 144–50; and Leonard E. Dickson, "Eliakim Hastings Moore", *Science*, new series, 77(1933), 79–80. Articles on Moore and the majority of the other people discussed in this paper also appear in *Dictionary of Scientific Biography*, edited by C. C. Gillispie, 16 vols (New York:Charles Scribner's Sons, 1970–1980).

association, in addition to providing Moore with a firsthand look at scientific research, gave him his first real taste of mathematics. As a researcher Stone leaned much more toward the mathematical than toward the observational side of astronomy. Also, as a teacher, 'he had keen insight in the choosing of able students, and, once they came under his influence, the ability of turning their interests permanently to scientific careers'.[4] Whether Stone was a primary or only a secondary influence on the later career of the teenaged E. H. Moore, it is impossible to say, but Moore did proceed to Yale where he came under the influence of another mathematician–astronomer, Hubert A. Newton.

As an undergraduate at Yale, Moore excelled in astronomy, English, and Latin, in addition to mathematics, taking prizes in all of these subjects at one time or another during his academic career. In 1883 he received the A.B. degree as valedictorian of his class. Two years later he earned a Ph.D. in mathematics under Newton for his thesis entitled 'Extensions of Certain Theorems of Clifford and Cayley in the Geometry of n Dimensions'.[5] Realizing that Moore had advanced as far as an American education at the time allowed, Newton encouraged his mathematical son to continue his studies in Germany. There, as Newton knew from personal experience,[6] he could immerse himself in the mainstream of mathematics rather than remaining in the American backwater.

With money lent to him by his advisor, Moore travelled to Germany in the summer of 1885. His first stop was Göttingen, where he studied German as well as mathematics before moving on to Berlin for the winter semester. At that time Karl Weierstrass (1815–97) and Leopold Kronecker (1823–91) dominated the Berlin mathematical scene, and Kronecker's abstract thinking dominated Moore's Berlin experience. As with so many other young American mathematicians who studied abroad, the ideas which Moore encountered in Germany dominated his mathematical thinking for the remainder of his career.

His *Wanderjahr* completed, Moore returned to the United States to begin his professional life. His first position took him to the Academy at Northwestern University in 1886, a rather remote outpost of American academe. After serving there as an instructor for one year, he moved back East to Yale where he assumed the temporary post of tutor. Finally, in 1886 he obtained his first permanent university post at Northwestern, and two years later he was promoted to associate professor. With his livelihood thus secured, Moore could have happily remained at Northwestern for the rest of his career, but another opportunity, the chance to forge his own Department of Mathematics, came his way.

[4] Charles P. Oliver, "Ormond Stone", *Popular Astronomy*, 41 (1933), 295–8 (p. 296).

[5] E. H. Moore, "Extensions of Certain Theorems of Clifford and Cayley in the Geometry of *n* Dimensions", *Transactions of the Connecticut Academy of Arts and Sciences*, 7 (1885), 9–26. J. Willard Gibbs also published his dissertation as well as much of his subsequent work in this journal.

[6] When Newton was appointed to the professorship in mathematics in 1855, he was immediately granted a year's leave of absence so that he could better prepare himself for the job. He spent his year in Paris listening to Michel Chasles's (1793–1880) lectures on geometry at the Sorbonne. These lectures left a permanent impression on Newton even though the majority of his subsequent work focused on astronomical problems. See Gibbs (footnote 1) p. 360 and p. 373. In fact, this undying interest manifested itself almost thirty years later in the geometrical topic which Newton and Moore worked out for the latter's dissertation research. In urging Moore to study in Europe, Newton most likely wanted to assure his student of an equally lasting and rewarding experience.

Whether because of the original and promising research which Moore had done after earning his degree or because of 'his agressive genius as a rising young scholar'[7], William Rainey Harper (1856–1906), the president-elect of the new University of Chicago, stole Moore from Northwestern with an offer of a full professorship and an acting chairmanship of the department of mathematics.[8] This selection proved fortunate not only for the University of Chicago, but also for American mathematics as a whole. The academic and administrative superstructure which Harper built, guided by his own attitudes and beliefs and from Rockefeller's money, served as the foundation upon which Moore built not only a department but also an American mathematical community.

On 13 May, 1889, after several years of pressure from education-minded Baptists, John D. Rockefeller (1839–1937) agreed to put up $600,000 toward the endowment of a college to be located in Chicago, provided an additional $400,000 could be raised by 1 June, 1891. In September 1890, Rockefeller supplemented his already generous contribution by pledging another $1,000,000 'for the support of theological and graduate studies'.[9] In keeping with his wishes, $800,000 of this sum went toward graduate work. Thus, from the very beginning a precedent was set at Chicago for the liberal promotion of graduate education and research. Rockefeller, however, had no real interest in personally supervising the running and policy-making of the university. Since his attitude was essentially one of *laissez-faire*, someone had to take charge and lay both the philosophical and the physical foundations of the school. Harper energetically and enthusiastically accepted these tasks.

A true scholar himself, Harper deeply believed in both inspired teaching and first-rate research. He felt that a strong University of Chicago depended on this twofold foundation. As a consequence he sought to draw scholars who excelled in both domains

[7] Bliss, "E. H. Moore" (footnote 3) p. 833. In 1892 seven years after receiving his Ph.D., Moore had only six publications to his credit, and one of those was a research announcement. These six papers, however, ranged widely in subject matter from geometry to group theory to the theory of elliptic functions. Although breadth and depth do not always go hand in hand, E. H. Moore made important contributions to these and other fields during the period from 1892 to 1902.

Moore's interest in geometry dated to the time of his dissertation, 1885. At first his interest centred on so-called algebraic geometry, geometry studied by algebraic means in the spirit of Felix Klein's *Erlanger Programm* of 1872. Moore's early work concentrated on algebraic properties of surfaces and of curves on these surfaces. In 1899 with the publication of David Hilbert's *Grundlagen der Geometrie*, however, Moore became totally involved in a very different aspect of geometry, its foundations. Among his contributions here was his proof that two of Hilbert's axioms were redundant.

As a natural outgrowth of his approach to geometry prior to 1899, Moore also studied the theory of groups. In 1893 he proved the important fact that every finite field was a Galois field, that is, every finite field had a primitive element over a field with p elements, p finite. Later while under Moore's influence, Joseph H. M. Wedderburn proved his famous theorem which stated that Galois fields were in fact exactly the finite division algebras.

Finally, the theory of functions, which first captured Moore's attention in 1890, became his all-consuming passion in later life, for he utilized his interest in both foundations and analysis to approach the theory of functions with the proper degree of abstraction. Rather than studying individual functions, the basic tenet of his 'general analysis' was to study classes of functions and to move from one such class to another by means of functional transformations. Modern functional analysis shares this same basic tenet.

For further information on Moore's mathematics, see Bliss, "The Scientific Work" (footnote 3) pp. 501–14.

[8] Harper's raid on other colleges and universities earned him and the University of Chicago quite a bit of notoriety in 1892. The most striking exodus occurred at Clark University, where almost half of the faculty left their positions to go to Chicago in the wake of disagreements with the administration. As we shall soon see, one of the three original members of the mathematics department, Oskar Bolza, was one such fugitive. For a detailed description of Harper's efforts to assemble a faculty, see Richard J. Storr, *A History of the University of Chicago: Harper's University The Beginnings* (Chicago:, 1966), pp. 65–85.

[9] Ibid., p. 47.

to the new university, and he did everything within his power to plan for and to provide a congenial and stimulating environment for them. As part of his plan for making the University a haven for the research-oriented members of the faculty, Harper put forth, among others, the following two suggestions in his decennial report:

> 4. There should be established Research Professorships, the occupants of which might lecture or not according to the best interests of the work in which they are engaged. This is practically the character of the Professorships in the [Yerkes] Observatory. There should be chairs in other Departments, perhaps a chair in every Department, to which there might be made a permanent appointment, or which might be occupied for a longer or shorter period by the various members of the Department capable of doing research work...
>
> 6. Arrangements should be made to encourage a larger number of men to devote six months of the twelve to research and investigation, their lecture work and teaching being confined to the other six months. This plan has already been adopted in several individual cases. It is very desirable to place the advantages of this arrangement at the command of the others. With the privilege thus secured of living a year abroad and a year at home, the highest results may be achieved.[10]

With policies such as these, Chicago could not fail to attract the best people in the various fields.[11]

Harper also made a special commitment to the pure sciences among which he included mathematics. When pressure was applied early on to establish an engineering school at Chicago, Harper did not take money away from the infant departments of science to finance the venture. Although he supported the idea and agreed that such an addition would benefit the University, he refused to sacrifice the progress that had already been made in the sciences. He tried to circumvent the problem of capital in a variety of ways. In 1896 he sought to annex the Chicago Manual Training School, thereby bringing to the University existing facilities and a substantial endowment. During the first few years of the new century, he aimed to join forces with the Armour Institute of Technology. Both of these attempts failed,[12] yet Harper's original conviction did not change. In defence of his unwillingness to divert funds from the sciences he wrote:

> First, it seemed upon the whole wise to devote the entire energy of the institution in scientific lines to departments of pure science, with the purpose of establishing these upon a strong foundation. This work being finished, there would be ample opportunity for the other work, and the other work would be all the stronger when it came, because of the earlier and more stable foundation of pure science. Second, it was also thought wise not to lay too much emphasis on the practical

[10] *The President's Report: Administration—The Decennial Publications*, 1st series, vol. I (Chicago: University of Chicago Press, 1903), p. xxv.

[11] That Chicago did attract good people, at least in mathematics, is reflected in Maxime Bôcher, David R. Curtiss, Percey F. Smith, and Edward B. Van Vleck, "Graduate Work in Universities and in Other Institutions of Like Grade in the United States", *Bulletin of the American Mathematical Society*, (1911), 122–37. As they put it, '... the opening of the University of Chicago in 1892 may almost be said to mark an epoch in the development of graduate [mathematical] instruction in the West and Middle West, for, though that university had from the start an undergraduate department, it stood out, through the character of its faculty and the emphasis laid on research work, as a strong exponent of the graduate idea'. (p. 125).

[12] Storr (footnote 8), pp. 134–5.

> side of education at the start... The greater danger was that pure science might be left without provision.[13]

In Harper's view the University's strength depended as much on its excellence in pure science as on its excellence in the humanities. If these areas were firmly established and well-represented, the rest would follow suit.

To insure that his commitments became reality, Harper drew up a meticulous blueprint for the organization of the University. In this master plan, he made provisions for the appointments of so-called Head Professors, or department chairmen, to whom he entrusted the task of building the various departments. Rather than bringing in a 'few younger instructors and allowing the work to grow more gradually under the domination of a single spirit',[14] Harper's aim was to 'bring together the largest possible number of men who had already shown their strength in their several departments, each one of whom, representing a different training and a different set of ideas, would contribute much to the ultimate constitution of the University'.[15] He wanted to create a lively atmosphere where many new and fresh ideas met, clashed, and ultimately meshed into the best of all possible university situations. In this way the president defined the overall philosophy of the institution, and the Head Professors set the working guidelines which best suited their respective departments. The University was thus tailored to the constituent parts. The fit was optimal—not too loose, not too tight.

Consistent with his policy of providing the superstructure on which to build, Harper compiled a list of duties and powers of the Head Professor.

> He was to supervise the entire work of his department in general, prepare all entrance examination papers and approve all course examinations prepared by other instructors, arrange course offerings from quarter to quarter, examine all theses offered in the department, determine the textbooks to be used, edit any appropriate papers or journals, conduct a club or seminar, consult with the librarian about needed books and periodicals, consult with the President on appointments of instructors, and countersign the course certificates in the department.[16]

Most certainly, E. H. Moore, Harper's choice for acting Head Professor of the Department of Mathematics, fulfilled these duties very well.[17]

In the autumn of 1892, the University of Chicago opened its doors for business, and E. H. Moore and his colleagues set about the job of organizing their department. During its first year of operation, the University counted three mathematicians among its ranks: Moore as professor, Oskar Bolza (1857–1942) as associate professor, and Heinrich Maschke (1853–1908) as assistant professor. We have already seen how painlessly Harper snared Moore. He had a much more difficult time securing the rest of the department. Even when he had finally succeeded, there was no guarantee that the men he had assembled would fit together to form a unified whole.

[13] *The President's Report* (footnote 10), p. xviii.
[14] Ibid.
[15] Ibid.
[16] Storr (footnote 8), p. 62.
[17] Because of his relative youth and inexperience, Harper made Moore acting department chairman in case he proved unsuited for the job. With a long list of successes already behind him and many more to come, Moore became permanent head in 1896.

After hiring Moore, Harper, in his quest for talent, approached Oskar Bolza with an offer to leave his position as associate[18] at Clark University for an associate professorship in his new Mathematics Department at Chicago. Ostensibly this should have been the perfect opportunity for one who had come to America to find permanency and who had yet to find a permanent job. Furthermore, Harper was giving him the chance to help mould the department right from the beginning.

Bolza first came to the United States in 1888 with his parents' blessings and financial support. Thirty-five years old by this time, Bolza had had a difficult academic career in Germany.[19] In 1875 he went to Berlin to study at the university. However, in accordance with his father's wishes he was simulataneously enrolled in the vocational academy there, a most unusual situation for a German student at this time. His father's plan was to secure a classical university education for his son while preparing him to take over the family-owned factory. After two semesters of this divided loyalty, Bolza left the technical school and decided to study physics. By 1878, however, he realized that he would never make an experimental physicist, and so he opted for pure mathematics. After two years of study and not much progress toward his Ph.D., Bolza turned to preparation for the State qualifying examination for *Gymnasium* teachers. Unfortunately, it took him three years to pass the test. After an additional year of practice teaching, Bolza finally had a profession in 1884, almost ten years after entering the university.

Thoughts of a Ph.D. in mathematics still with him, he worked on his research while teaching in Freiburg, and Klein accepted his results as a dissertation in 1886. As a result of his work, Bolza left Freiburg for Göttingen for the 1886–87 academic year, where he and his younger friend Maschke participated in a private mathematical seminar with Klein. The experience of working in such close conjuction with the intense and insightful Klein shattered Bolza's self-confidence. He doubted his ability to gain a university position (his age also being against him), and his post in the *Gymnasium* proved too draining on him physically.

Following the lead of one of his friends, Bolza left Germany for America in 1888. There, for the winter term of 1888–89, he secured a readership on Klein's recommendation at Johns Hopkins, which he followed up with a position at Clark the following academic year. Indeed, the outlook seemed brighter in America, but a permanent position still eluded him. Moreover, halfway through his second year at Clark, his father died in Germany after a long illness. In the summer of 1891 he returned home to Freiburg to comfort his mother and to coax her into staying with him in Worcester for the following year. His initial plan was to remain at Clark for one more year and then to return with his mother to Germany. Harper's arrival in Worcester and his subsequent offer at first only raised doubts in Bolza's mind about his decision to leave America. Nevertheless, he refused the call to Chicago and proceeded with his travel plans. Just ten days before he was to leave, another plea inveigling him into coming for at least one year arrived from Harper. Bolza's colleague, Henry Taber (1860–1936), urged him to accept the offer and finally convinced him to go to Chicago and talk to Harper in

[18] Not to be confused with the rank of associate professor, 'associate' or 'tutor' was one of the various grades of temporary, as opposed to permanent faculty.

[19] The following biographical account of Bolza's life has been condensed from his very readable and highly interesting autobiography, *Aus Meinem Leben* (Munich: Verlag Ernst Reinhardt, 1936). See particularly pp. 18–25 for his account of his early experiences in America. G. A. Bliss used this same source in his article, "Oskar Bolza: In Memoriam", *Bulletin of the American Mathematical Society*, 50 (1944), 478–89, especially pp. 478–80.

person. This one side trip gained not only Bolza's acceptance, but also a position for his longtime friend, Heinrich Maschke.

Bolza and Maschke had first met in 1875 while both were students at Berlin.[20] Maschke received his Ph.D under Felix Klein at Göttingen in 1880, six years before Bolza finished, and immediately took a teaching position at the *Luisenstädtische Gymnasium* in Berlin. Like Bolza he felt the physical burden of the teaching load at the *Gymnasium* and longed for a university position. However, the competition for such positions was intense, and it was difficult to change from the station of *Gymnasium* teacher to that of university professor.[21] At his friend's urging, he finally came to the United States in 1891. Before leaving, he retrained himself as an electrician in order to insure his livelihood. As Bolza later explained, 'he did not wish to rely entirely upon the doubtful chances of finding a university position'.[22] This proved to be a wise move for he quickly found a job as an electrician at the Weston Electric Instrument Company in Newark. Although this met Maschke's financial needs, it did not satisfy the intellectual needs of a man who yearned for the chance to prove himself as a mathematician. Harper's solicitation of Bolza provided him with just such a chance.

During the course of his negotiations with Harper in Chicago, Bolza agreed to take the job but only on the condition that Harper also hire Maschke. At first this seemed impossible due to a lack of funds, but at the last moment a new gift came through.[23] Harper offered Maschke an assistant professorship, and the staff was complete. All that remained was for these three men to combine their efforts and form a real, working department.

In the beginning both Maschke and Bolza had their doubts about the success of their new association. As Bolza so candidly expressed it:

> Of the greatest importance now was the question of how the relations in the mathematics department would turn out. As already mentioned, E. H. Moore, formerly an associate professor at Northwestern University in Evanston, Ill., had been called to head the Mathematics Department. According to the '*Führerprinzip*' which reigned at the University, Moore answered only to the President, whose complete trust he possessed, and otherwise was theoretically the absolute ruler of the department. He was almost five years younger than I, even more than eight years younger than Maschke and was at that time little known. In addition to that, Maschke and I were foreigners who for many years had been close friends and who had lived in the absolute freedom of the German university. All of these were factors which could have risked the inner peace of the department.
>
> But on the one hand, Maschke and I recognized early on Moore's towering ability.... And on the other hand, Moore was extremely tactful, considerate, and unselfish and consulted us as equals on all important questions which concerned the department. Thus, it was not hard for us to accept him willingly as a leader whom we gladly and cheerfully served, even more so since he also manifested

[20] This information on Maschke's life may be found in Oskar Bolza, "Heinrich Maschke: His Life Work", *Bulletin of the American Mathematical Society*, 15 (1908), 85–95 (p. 86).

[21] For a clear picture of the educational hierarchy in Germany at this time, see Lewis Pyenson, *Neohumanism and the Persistence of Pure Mathematics in Wilhelmian Germany* (Philadelphia: American Philosophical Society, 1983), especially chapter 3, pp. 16–26.

[22] Ibid.

[23] Bolza (footnote 19), p. 25.

extraordinary organizational abilities and a forward-charging, selfless enthusiasm in the interest of the department.[24]

By all accounts, then, the members of this mathematical triumvirate perfectly complemented each other as mathematicians, as teachers, and as administrators. In the words of one who knew and studied under them all, 'Moore was brilliant and aggressive in his scholarship, Bolza rapid and thorough, and Maschke more deliberate but sagacious'.[25] With these character traits pooled into one corporate entity, it is little wonder that good, carefully thought out ideas ensued and that they were efficiently and effectively implemented.

3. E. H. Moore's organizational years 1892–1902

The first major undertaking of this young department involved the World's Columbian Exposition held in Chicago in 1893 to commemorate the four-hundredth anniversary of the discovery of America. It resulted in Chicago becoming 'overnight *the* leading centre of American [mathematical] research, with Moore as its inspired chief'.[26]

The organizers of the Exposition conceived, in addition to the displays, amusements, and cultural activities associated with a world's fair, of a series of congresses which would represent the current intellectual activities of the world and bring together the leading experts in many diverse fields.[27] The success of such an endeavour would insure that the 1893 Columbian Exposition in Chicago surpassed the landmark Paris Exposition of 1889 in at least one way since the idea of holding congresses had been implemented in Paris on only a limited scale. Furthermore, by emphasizing the intellectual aspects of world culture, the organizers would somewhat play down the nationalistic and materialistic sides of the world fair.[28] These high ideals served the mathematics department at the University of Chicago well. Of the 214 local organizing committees charged by the World Congress Organization to contact the world's leaders in the various fields,[29] the Chicago mathematics department of Moore, Bolza, and Maschke together with Henry S. White (1861–1943) of Northwestern formed the committee on mathematics. Thus, the fledgling department at Chicago was almost instantly thrust into the limelight in its unofficial role as host to the mathematicians of the world.

The congress proved quite successful, especially as an impetus to Moore and his colleagues at Chicago. In all, forty-five people representing nineteen states as well as Austria, Germany, and Italy actually attended the meeting, and mathematicians of France, Russia, and Switzerland were well-represented by their submitted papers.[30]

[24] Ibid, p. 26. The translation is mine.

[25] Bliss, "E. H. Moore" (footnote 3) p. 833.

[26] Garrett Birkhoff (footnote 2), p. 31. Birkhoff's emphasis.

[27] Reid Badger, *The Great American Fair: The World's Columbian Exposition and American Culture* (Chicago, 1979), pp. 77–8. I would like to thank Professor David Rowe for this reference and for his helpful remarks concerning Felix Klein's participation in the mathematical congress and in the Evanston colloquium.

[28] Ibid., p. 78.

[29] Ibid.

[30] Of the forty-five people actually in attendance, forty-one were American, one was Austrian, two were German, and one was Italian. Thus, the American contingent to this 'international' congress definitely outnumbered the participants from all other countries combined by a margin of ten to one. Of the thirty-nine papers read, however, only thirteen were delivered by Americans. Of the remaining papers sixteen were German, three Italian, one Swiss, three French, two Austrian, and one Russian. Most notable among these foreign contributors were Hermite, Hilbert, Minkowski, Netto, Max Noether, Study, and Weber. Although populated primarily by Americans, the meeting truly was international in content.

Furthermore, as part of its contingent to the fair, the Prussian Ministry of Culture sent Felix Klein to the congress as its official representative.[31] He brought with him recent articles by over a dozen of his countrymen in an effort to disseminate German mathematics more fully and in order to pave the way for a meaningful exchange of ideas between mathematicians working in Germany and those working elsewhere in the world. Of all the foreign nations actively participating in some aspect of the fair, only the Prussian government made such a gesture to the mathematical congress. Considering the fact that two of the members of the local committee, Bolza and Maschke, were native Germans who had obtained their Ph.D.'s under Klein, and that the other two members, Moore and White,[32] had studied in Germany, this gesture served to underscore the enormous debt that American mathematics owed to Germany. The American participation at all levels at the congress proved, however, that mathematics was finally established in this country.

At the beginning of September, immediately after the close of the congress, Moore and his colleagues continued their mathematical activity by attending a two-week-long colloquium given by Felix Klein at Northwestern University.[33] Klein had been trying to work out plans for such a series of talks in the United States for some time. Thus, when Moore, Bolza, Maschke, and White approached him about the congress, he offered to give the follow-up lectures free of charge. This colloquium served a dual purpose: first, it allowed the twenty-four people in attendance to hear more from Klein and to talk to him one-on-one, and second, it brought home the point to Moore and White that regular and frequent colloquia involving the mathematics departments at Chicago and Northwestern should definitely be held in the future.[34]

After Klein returned to Germany, the two departments did set up a schedule of meetings to discuss mathematics and to present papers. By 1896, however, the idea of organizing something bigger had occurred to Moore and his friends. In December of that year, Moore took the initiative and mailed a circular to professors and students of mathematics who lived and worked within a reasonable train ride of Chicago. As its title proclaimed, the letter represented 'a call to a conference in Chicago' on 31 December, 1896, and 1 January, 1897, in Ryerson Laboratory at the University of Chicago. At the meeting, Moore proposed to discuss the feasibility of organizing a Chicago section of what had since become the American Mathematical Society.

In April 1894, with the papers read at the international congress amassed and organized, E. H. Moore approached the New York Mathematical Society for money toward the publication of the proceedings, thereby performing the Head Professor's prescribed task of editing 'any appropriate papers or journals'. The Society in conjunction with the publisher, Macmillan and Company, worked out an equitable

[31] Eduard Study (1862–1930), professor extraordinarius at the University of Marburg in Germany, also accompanied Klein.

[32] White also obtained his Ph.D. under Klein at Göttingen.

[33] These lectures were given in English and ranged widely over the areas of geometry and algebra. Meticulous notes, virtually transcripts, were taken during each of the twelve lectures by Alexander Ziwet (1853–1928) who was then an assistant professor at the University of Michigan. After Klein's careful revisions, Ziwet saw the lectures through the press. See Felix Klein, *The Evanston Colloquium: Lectures on Mathematics* (New York, 1894). This volume, although not affiliated with the New York (as of 1 July 1894, American) Mathematical Society, served as the impetus and the archetype for the series of AMS sponsored colloquia which began in Buffalo, New York in 1896.

[34] Archibald, ed. (footnote 2), I, 74–5.

agreement for financing the venture, and the volume appeared later that year.[35] As Raymond C. Archibald (1875–1955) explained, 'this major publication enterprise, transcending local considerations and sentiment quickened the desire of the Society for a name indicative of its national or continental character'.[36] On 1 July, 1894, largely as a result of E. H. Moore's initiative and insight, this desire became reality, and the *American* Mathematical Society met for the first time.

It did not take Moore and his Midwestern colleagues long to realize, however, that a national name did not necessarily imply a national organization. After 1894 as before, the monthly meetings of the Society took place in New York City, which effectively prevented all but the Northeastern mathematicians from attending and participating regularly. Moore's idea of a formally sanctioned Chicago section, then, would serve to involve the Midwesterners officially in the Society's activities. Such a group would provide necessary mathematical interaction for those located in the Midwest who could not pack up and go to New York each year, much less each month. Moore stressed the supposed national character of the Society in his letter and strongly hinted at its obligations toward the non-Eastern members. As he put it:

> Our Society represents the organized mathematical interests of this country. Its function is to promote those interests in all possible ways.
>
> Do we not need most of all frequent meetings? Those who have attended the summer meetings know the keen stimulus and inspiration resulting from personal contact—inside and outside the stated meeting—with colleagues from other institutions. The regular monthly meetings of the Society afford similar opportunities to those who live in the vicinity of New York.
>
> By the organization of *sections* of the Society can similar advantages be secured for other parts of the country? Shall, for instance, a Chicago section be organized?[37]

In short, the creation of a section of the Society of Chicago would have served to shift some of the mathematical power and activity from New York to Chicago. Under the shrewd leadership of Moore and his associates, then, the Department of Mathematics at the University of Chicago could have achieved preeminence. This was precisely what happened.

The Chicago section, which was formally sanctioned in the By-laws of the American Mathematical Society in 1897, had its first official meeting on 24 April, 1897. At that time Moore was elected chairman of the group, a post he held until 1902, and Thomas F. Holgate (1859–1945) of Northwestern was elected secretary. The energy of these two men, as well as of the other Midwestern mathematicians, reflected itself in the statistics based on the section's first three years. From April, 1897, to April, 1900, a total of between eleven and twenty-one actual members of the Society attended each meeting in addition to numerous non-members, and a total of 106 papers were delivered by forty-one different speakers.[38] Furthermore, the research presented was for the most part of a

[35] Such a volume was in fact published. See *Mathematical Papers Read at the International Mathematical Congress Held in Connection with the World's Columbian Exposition: Chicago 1893* edited by Eliakim H. Moore, Oskar Bolza, Heinrich Maschke, and Henry S. White, (New York, 1896). The facts and figures which follow are based on information in the introduciton of the book, pp. vii–xvi.

[36] Archibald, ed. (footnote 2), I, 7.

[37] Ibid, p. 75.

[38] Ibid, p. 77. A statistical study of the first twenty-five years of the Chicago Section may be found in Arnold Dresden, "A Report on the Scientific Work of the Chicago Section, 1897–1922, "*Bulletin of the American Mathematical Society*, 28 (1922), 303–7.

very high quality. Those in attendance were privy not only to the latest work of Moore, Bolza, and Maschke but also to the thesis research of such rising stars as Dickson, Veblen, George Birkhoff, and Robert L. Moore (1882–1974) by the end of the first decade of the twentieth century.[39]

This fundamental research developed in an environment characterized not only by the activity of the Chicago section of the Society, but also by Moore's continued enthusiasm and industriousness. After the formation of the local group and after its settling in period, Moore had the opportunity to devote his thoughts to other projects which would benefit his department, the Society, and American mathematics as a whole. Once again, in keeping with the list of duties of the Head Professor, Moore became involved in the movement to found a new mathematics journal which would provide a much needed forum for American mathematicians.

It may be surprising that mathematicians at this time felt the need for another journal considering that there were already three major American journals dedicated solely to the publication of mathematical research: the *American Journal of Mathematics* founded by James Joseph Sylvester and William E. Story (1850–1930) at Johns Hopkins in 1878; the *Annals of Mathematics* begun by Ormond Stone at the University of Virginia in 1884; the *Bulletin of the American Mathematical Society* first issued in 1891 under the direction of Thomas S. Fiske (1865–1944).

In the eyes of Moore and others, each of these journals had shortcomings. The avowedly *American Journal*, for example, published an inordinate number of papers by foreign mathematicians, thereby depriving American contributors of precious space. During its first ten years of operation, fully one-third of the ninety papers which appeared came from foreign sources.[40] The editorship, first under J. J. Sylvester (until 1883) and then under Simon Newcomb (1835–1909), distinctly favoured and valued European over American mathematics, or so it was believed. Whereas the *American Journal* published advanced research from foreign sectors, the *Annals* printed American 'papers of intermediate difficulty and more popular character'.[41] This was perfectly in keeping with Stone's original plan, however. He felt the need for a journal that catered more to the mathematical enthusiast than to the high-powered mathematician. Finally, the *Bulletin* proclaimed its mission as an 'Historical and Critical Review of Mathematical Science' right on its title page, so full-length research articles were not appropriate there either. The United States sorely needed a journal under editors who

[39] Joseph H. W. Wedderburn (1882–1948) also first presented his beautiful theorem on finite division algebras before the Chicago Section in April of 1905 while he was a visiting fellow at the University of Chicago. For a discussion of this theorem and the environment in which it was proved, see Karen Hunger Parshall, 'In Pursuit of the Finite Division Algebra Theorem and Beyond, *Archives internationales d'Histoire des Sciences*, 33 (1983), 274–99.

[40] Thomas S. Fiske, "Mathematical Progress in America, "*Bulletin of the American Mathematical Society*, 11 (1905), 238–46 (p. 239). Furthermore, another third of the articles came from people associated in one way or another with Johns Hopkins.

[41] J. J. Lick, "Ormond Stone—1847–1933", *Bulletin of the American Mathematical Society*, 39 (1933), 318–9 (p. 318). In the first twelve volumes, which appeared while the journal was still housed at the University of Virginia, only three foreigners contributed (if we exclude a letter from Cayley which was published there but which he did not actually submit), making the *Annals* decidely American. The popular nature of the journal was characterized by a section of problems and solutions in each issue, and the articles which came out during the first few years of operation were definitely at an intermediate level. This gradually changed after 1890, however, when people like Bôcher, Moore, Dickson, Osgood, and Maschke began sending articles there. In fact, Dickson's Ph.D. thesis appeared in volume twelve. See L. E. Dickson, "The Analytic Representations of Substitutions on a Power of a Prime Number of Letters with a Discussion of the Linear Group", *Annals of Mathematics*, 12 (1897–1898), 65–120, 161–83.

recognized and emphasized the merits of Americans working at an advanced level in the field of mathematics.

In the spring of 1898, Thomas S. Fiske of Columbia University, the secretary of the Americam Mathematical Society, proposed to the governing council that the Society approach Newcomb with the suggestion of making the *American Journal* a joint venture between Johns Hopkins and the Society. A committee consisting of Fiske, Moore, Newcomb, Maxime Bôcher of Harvard, and James Pierpont (1886–1938) of Yale was appointed to draw up a proposal to submit to Johns Hopkins. With Newcomb on the drafting committee (he was also president of the Society), acceptance of the proposal by the authorities at Hopkins should have been immediate. Apparently though, Newcomb's strongest allegiance lay with his journal and not with his Society. In the last stages of the bargaining, he and the University refused to accept the committee's first and third recommendations, namely,

> (1) That the *American Journal of Mathematics* shall bear upon its title page the inscription Founded by the Johns Hopkins University. Published under the auspices of the Johns Hopkins University and the American Mathematical Society... (3) That the *Journal* shall have a board of seven editors, of whom two shall be selected by The Johns Hopkins University, and five by the Council of the American Mathematical Society...[42]

They wanted no mention of the Society on the title page and complete control over the editorial board.

This sort of bickering did not appeal to Moore and to many of the other members. Therefore, when asked later by Newcomb and Fiske if he would consider serving on the editorial board of the *Journal*, Moore refused. So many other people approached for the position also refused that the plan was abandoned altogether. With this option no longer viable, the problem became one of financing a totally new journal without further jeopardizing relations with Johns Hopkins University and the editorial board of the *American Journal*. William F. Osgood recalled the mixed feelings at the lively New York meeting which followed the collapse of the *American Journal* plans in this way:

> A number of the younger men spoke informally and all were agreed on the desirability of a journal, the one difficulty being the financial one. But a few of the older men had been contributors to the *American Journal*. They had followed with enthusiasm the development of the Johns Hopkins and it was natural that they should feel a certain loyalty to its publications. These views had not, however, been expressed at the meeting. Few of the younger men knew that they existed, and little would they have cared when the interests of mathematics were at stake. Young men are impetuous, and when they are sure they are right, proceed directly to reach their ends. Not till later do they learn the importance of listening to a minority which is wrong, but sincere and of winning it over if possible, without sacrificing their main objects.[43]

This difference of opinion and divided loyalty dissolved completely away when Maxime Bôcher hit upon the idea of referring to the new journal not as a 'journal' but as the 'transactions' of the Society. Certainly a publication under that name would present no direct competition to any other extant or future mathematical journal. Thus, the

[42] Archibald, ed. (footnote 2) I, 56. A detailed account of the negotiations with Newcomb appears on pp. 56–8.

[43] Ibid, p. 58.

Society founded its *Transactions* on 25 February, 1899, and Moore, Fiske, and Ernest W. Brown (1866–1938) of Yale were appointed editors with Moore as editor-in-chief.[44]

Brown vividly conveyed the sense of adventure and uncertainty associated with the early years of the journal as well as Moore's matchless contributions when he wrote:

> I think that none of us knew much if anything about running a mathematical journal—I certainly didn't. But we were young and could still learn. I like to think of the immense amount of trouble we all took—and especially Moore—to get the best information, the best printing, the best editing and the best papers before the first number appeared [January, 1900]. And the work did not stop there. We wrestled with our younger contributors to try to get them to put their ideas into good form. The refereeing was a very serious business.... Most of it in those days was, I believe, done by Moore himself though he sought outside assistance whenever possible.[45]

Right from the start this editorial staff maintained only the highest standards of style and mathematical quality. In the first issue, for example, there were papers by Moore, Bolza, and Maschke, as well as work by Bôcher, Osgood, Dickson, and the foreign mathematicians Paul Gordan (1837–1912) and Eduoard Goursat (1858–1936).[46] Although contributions from abroad were encouraged, they were not included unless they had previously been presented at one of the Society's meetings. In this way the editors remained faithful to the transactional nature of their publication and avoided all possible accusations of an anti-American bias.[47] They also invited European mathematicians to take an interest in the American Mathematical Society and its proceedings. The willingness of such prominent Europeans to accept this invitation signalled a coming of age of American mathematics, and once again we find E. H. Moore at the forefront of such developments.

With virtually complete control over the Society's official standards for publishable research, Moore wielded great power over his fellow mathematicians, forcing them to strive for the very best intellectually. He also enjoyed growing influence within the political sphere of American mathematics. When the first number of the *Transactions* appeared in 1900, Moore had already completed a two-year tenure as vice-president of the Society. Apparently his political acumen was as sharp as his mathematical ability, for in December of 1900 he was elected president of the A.M.S. His colleagues had awarded him their highest honour.[48]

[44] Ibid, pp. 58–9.

[45] Ibid, p. 60.

[46] This list became only more impressive as the Moore years (1900–1908) went by, including names like Hilbert, Eisenhart, Hadamard, Fréchet, Veblen, Wedderburn, Poincaré, George D. Birkhoff, and R. L. Moore.

[47] Archibald, ed. (footnote 2), I, 59. Whereas one third of the *American Journal's* papers during its first ten years came from abroad, under Moore each volume of the *Transactions* was only 12·5 per cent European on the average. Foreign contributors numbered 4 in 27, 4 in 21, 2 in 28, 4 in 29, 4 in 28, 4 in 29, 2 in 31, 3 in 28 in the first eight volumes, respectively.

[48] Interestingly enough, in 1903 Moore was ranked the number one mathematician in the United States by a group of ten mathematicians selected by James McKeen Cattell for *American Men of Science.* The rankings, which remained secret until they were published thirty years later in the fifth edition of this directory, were: 1) Moore, 2) George W. Hill of the Nautical Almanac Office, 3) William F. Osgood of Harvard, 4) Maxime Bôcher also of Harvard, 5) Oskar Bolza of Chicago and Simon Newcomb of Johns Hopkins, 6) Frank Morley also of Hopkins, 7) Ernest W. Brown of Haverford, later of Yale, 8) Henry S. White of Northwestern, 9) L. E. Dickson of Chicago, and George A. Miller of Stanford in 1903, later of the University of Illinois. Since Cattell gave no indication as to the criteria used in selecting the ranking committee, this list is of somewhat questionable value. See *American Men of Science: A Biographical Dictionary*, 5th ed edited by James McKeen Cattell and Jaques Cattell (New York, 1933), pp. 1261–78 (p. 1269).

Moore's two-year term as president marked several interesting changes in the Society. The sixth president,[49] he was the first Midwesterner. His efforts over the previous eight years had finally won national respectability for his region of the country, and his single-mindedness of purpose had resulted in a clear shift of power. Moore was also the first pure mathematician to accede to the presidency. Although not as widely renowned as the astronomer–presidents, George W. Hill (1838–1914) and Simon Newcomb, Moore had gained international recognition as a mathematician, receiving an honourary degree from the University of Göttingen in 1899 for his achievements in pure research. Finally, Moore was by far the youngest man to have served as the Society's president, assuming that position at the age of thirty-eight. John McClintock (1840–1916) and Robert S. Woodward (1849–1924), the next youngest of the six, were both fifty at the time of their inaugurations. Thus, the American mathematical community learned that not only could its leader hail from the Midwest, but he could also be a younger man at the peak of his creative career.

Compared with the preceding eight years, Moore's tenure as president of the A.M.S. was a relatively calm period. By the time he came to power, the Society boasted a steadily increasing membership, financial solvency, and two important periodicals. As we have seen, Moore had already done much in an unofficial capacity to assure this success. Not content to glide through on past achievements, however, Moore adopted a very important cause to promote during his presidency, namely, the advancement of mathematics education in America.

As a university level educator, Moore became painfully aware of the vast differences in mathematical preparation among the students entering the university at the turn of the twentieth century. He also recognized that college or university training in mathematics varied greatly from institution to institution. As a solution to these problems of non-uniformity, Moore advocated drawing up national mathematics requirements for admission into colleges and technological schools as well as setting standards for degrees in mathematics nationwide. Since he viewed the American Mathematical Society as the logical vehicle for instituting, supporting, and maintaining such standards, he set up various committees during his term of office to study and make recommendations on this issue.

As a teacher of mathematics, E. H. Moore was also deeply concerned about the pedagogy of mathematics. A follower of his Chicago colleague John Dewey (1859–1952), Moore renounced the standard lecture method of teaching mathematics in favour of a laboratory method in the Deweyan sense. For Dewey, learning must always be motivated be experience. In presenting new subject matter to a child, for example, 'the lack of any organic connection with what the child has already seen and felt and loved makes the material purely formal and symbolic...any fact, whether of arithmetic, or geography, or grammar, which is not led up to and into out of something which has previously occupied a significant position in the child's life, for its own sake, is forced into [the] position'[50] of being a 'base or mere symbol',[51] that is, one which has no real meaning to the child. Thus, in order to teach elementary mathematical concepts, the instructor must fundamentally relate the new ideas to the child by means

[49] In chronological order, the other presidents were John H. VanAmringe (1835–1915), John McClintock (1840–1916), George W. Hill (1838–1914), Simon Newcomb (1835–1909), and Robert S. Woodward (1849–1924). For biographical sketches of these men, see Archibald, ed. (footnote 2) I, 110–44.

[50] John Dewey, *The Middle Works, 1899–1924*, edited by Jo Ann Boydston, vol. 2, (*1902–1903: The Child and the Curriculum* (Carbondale: Southern Illinois University Press, 1976), pp. 286–7.

[51] Ibid.

of some hands-on type of experience or, more broadly, through so-called manual training. For instance, a conception of number might come through measurement of baking powder for a cake and a conception of number applied to chemistry might come from baking a cake with too much or too little of this essential ingredient. At the Laboratory School affiliated with Dewey's Department of Pedagogy at the University of Chicago, the teachers implemented precisely this experiential philosophy of education in teaching children at the elementary level. Dewey himself admitted, however, that '... the first person who succeeds in working out the real correlation of mathematics with science and advanced form of manual training, will have done more to simplify the problems of *secondary* education than any other one thing that I can think of.'[52]

E. H. Moore attempted to meet this challenge with his adaptation of the laboratory method to secondary and university level instruction in mathematics and physics. Moore sought to relate mathematical and physical concepts to the student's realm of experience by actively engaging the student in experimentation of an intrinsically interesting nature. Thus, 'in the physics laboratory it is undesirable to introduce experiments which teach the use of calipers or of the vernier or of the slide rule. Instead of such uninteresting experiments of limited purpose, the students should be directed to extremely interesting problems which involve the use of these instruments, and thus be led to learn the instruments as a matter of course, and not as a matter of difficulty. Just so the smaller elements of mathematical routine can be made to attach themselves to laboratory problems, arousing and retaining the interest of the students.'[53] As for the more difficult problem of presenting theorems and their proofs, experience must again prevail. The instructor must first convince the student of a theorem's truth at an intuitive level whether by means of experimentation or computation or graphic depiction. Then, 'in most cases, much of the proof should be secured by the research work of the students themselves.'[54]

Moore pushed for the widespread adoption of the laboratory method in teaching elementary, secondary, and university level mathematics.[55] As vice-president and then president of the American Mathematical Society, Moore saw this body as his primary vehicle for instituting this change. American mathematics education needed help according to Moore, and he wanted to guarantee its reform from the bottom up. He underscored his commitment to these ideals by reserving his final official forum as the Society's president, his retiring address, for a final plea for change.[56]

[52] John Dewey, *Lectures in the Philosophy of Education: 1899*, edited by Reginald D. Archimbault (New York: Random House, 1966), p. 295. My emphasis.

[53] E. H. Moore, "On the Foundations of Mathematics", *Science*, new series, 17 (1903), 401–16 (p. 412). See also John Dewey, *The Early Works, 1882–1898*, edited by Jo Ann Boydston, vol. 5, (*1895–1898*): "A Pedagogical Experiment" (Carbondale: Southern Illinois University Press, 1972), pp. 244–6 (p. 246).

[54] E. H. Moore, (footnote 53) pp. 412–13. This also serves as one of the fundamental ideas in Robert L. Moore's celebrated method of teaching mathematics. See the end of Section 4 below.

[55] As implemented by E. H. Moore and his colleagues, the laboratory method emphasized the "... fundamentals and their graphical interpretations. The courses were so-called laboratory courses, meeting two hours each day, and requiring no outside work from the students. It might be added parenthetically that, as with many such new plans, the amount of work required of the instructor was exceedingly great." (Bliss, footnote 3, p. 834).

[56] See note 53 above.

After stepping down from his national post, he set an example by implementing these ideas in his own department at the University of Chicago. Along with Bolza and Maschke, Moore built up a major teaching institution at Chicago which turned out some of the foremost American mathematicians of the first half of the twentieth century. In order to understand fully Moore's role in the founding of an American mathematical community, we must finally turn to the mathematical children he fathered, the second generation, and to the intellectual environment in which they matured.

4. The Mathematics Department at Chicago and Moore's early students

As we have already said, President Harper as well as Moore and his colleagues tried to stimulate a lively intellectual atmosphere at the University. In the Department of Mathematics, part of this effort was reflected in the vast number of journals which Moore, in his capacity as chairman, had ordered through the library. He clearly realized the importance of keeping abreast of the literature. Primarily through subscriptions to the greatest possible number of journals would his faculty and students have the opportunity to study trends in research, to learn of new, open problems, and to remain on top of the current developments in their respective areas.

In 1903, Harper's decennial report listed the journals received by the mathematics library. They numbered an incredible thirty-eight.[57] A mathematician looking for a reference could consult five American journals, three from England, nine from France, ten in German, one from Holland, nine from Italy, one in Portuguese, and one from Sweden. Fifteen years earlier Florian Cajori (1859–1930) surveyed the major colleges and universities and inquired as to the number of mathematics journals they took.[58] Of the 168 which responded, only Johns Hopkins, the U.S. Naval Academy, and Columbia claimed to subscribe to almost all mathematics journals currently in print (perhaps twenty-one or so in 1888). The vast majority of the rest (117) took none at all. Harvard, Yale, and Princeton were conspicuously absent from the list of schools which answered Cajori's survey, and of the 117 institutions which took no journals virtually all were small liberal arts colleges. Nonetheless, these figures suggest that by the standards of 1903 the University of Chicago possessed vast mathematical resources in relation to most other schools.

It was also extremely fortunate to have the faculty resource of Moore, Bolza, and Maschke. Following President Harper's wishes, these three men founded the Mathematical Club in 1892 as soon as the University opened.[59] They conceived of it as a workshop where research papers in preliminary versions were presented, ripped apart, and put together again. Gilbert A. Bliss (1876–1961) described the atmosphere at the club meetings well when he wrote:

> Those of us who were students in those early years remember well the tensely alert interest of these three men in the papers which they themselves and others read before the Club. They were enthusiasts devoted to the study of mathematics, and aggressively acquainted with the activities of the mathematicians in a wide variety

[57] *The President's Report*, pp. 247–63. These pages contain an alphabetical listing of all of the periodicals to which the University of Chicago library subscribed. The mathematics journals may be gleaned from this list.

[58] Florian Cajori, *The Teaching and History of Mathematics in the United States* (Washington, D. C.: Government Printing Office, 1890), pp. 296–302. The numbers which follow are based on Cajori's data.

[59] Department of Mathematics, "The Junior Mathematical Club of the University of Chicago", with an Historical Sketch by Herbert E. Slaught, Chicago, 1906–41, p. 1. (Typewritten.)

of domains. The speaker before the Club knew well that the excellence of his paper would be fully appreciated, but also that its weaknesses would be discovered and thoroughly discussed. Mathematics, as accurate as our powers of logic permit us to make it, came first in the minds of these leaders in the youthful department at Chicago, but it was accompanied by a friendship for others having serious mathematical interests which many who experienced their encouragement will never forget.[60]

The experience of presenting a paper or an idea before such a dynamic and inquiring group of minds must have been rewarding indeed, not to mention frustrating. After one of these sessions of questioning and probing, though, results emerged more polished, papers more succinct, ideas more concrete. The renown of the second generation of standard-bearers which emerged from this environment testified to the success of Moore's efforts in building both his department and a national mathematical community.

In all Moore supervised the dissertations of thirty Ph.D. candidates during his forty years at the University of Chicago, and although the names of many of his later students are not well-known, a list of his students from 1896 to 1907 contains some of the brightest stars of twentieth century mathematics.[61] The algebraist Leonard E. Dickson, the geometer Oswald Veblen, the mathematical physicist George D. Birkoff, and the topologist Robert L. Moore[62] each grew up on E. H. Moore's brand of mathematical thinking and matured into independent-minded mathematicians who made seminal contributions to their respective fields. Together these four mathematicians published thirty books and over six hundred papers in addition to directing the research of almost two hundred Ph.D.'s[63]. Furthermore, each of them edited major journals, served as president of the A.M.S., and won election to the National Academy of Sciences. Like their mathematical father, they also built or maintained exceedingly strong departments at their respective institutions.

Dickson, who received the University of Chicago's first doctorate in mathematics in 1896, became an assistant professor there in 1900 and remained at Chicago until his retirement in 1939.[64] During this time he sustained the research momentum of the original triumvirate after Maschke's death in 1908 precipitated Bolza's return to Germany two years later. He filled this large gap with his prodigious output of papers, books, and students[65] and engendered an entire school of ring theorists which eventually scattered all over the United States.

Veblen came along a bit later, receiving his Ph.D. in 1903. After staying on at Chicago for two more years as an associate, he joined the ranks of Woodrow Wilson's

[60] Ibid.(footnote 3), p. 833.

[61] For a complete list of Moore's students, see Bliss (footnote 3), p. 834.

[62] Although R. L. Moore's name was listed in the reference cited in note 52, it did not appear in Archibald's sketch of Moore's life. See Archibald, editor (footnote 2) I, 144–50 (p. 146). As R. L. Wilder noted in his obituary of R. L. Moore, the records needed to clear up this matter are incomplete. See R. L. Wilder, "Robert Lee Moore, 1882–1974", *Bulletin of the American Mathematical Society*, 82 (1976), 417–27 (p. 419).

[63] These numbers breakdown in the following way. Of the papers, Dickson had over 280, Veblen had 69, Birkhoff had 203, and R. L. Moore had 68. As for the books, Dickson published a phenomenal eighteen, Velben six, Birkhoff five, and R. L. Moore one. Finally Dickson directed 64 students, Veblen 14, Birkhoff 45, and R. L. Moore 50.

[64] For further biographical information on Dickson, see Archibald, ed. (footnote 2) I, 183–94, and A. A. Albert, "Leonard Eugene Dickson: 1874–1954", *Bulletin of the American Mathematical Society*, 61 (1955), 331–45.

[65] See note 63 above.

(1856–1924) preceptors[66] at Princeton. A full professor by 1910, Veblen left Princeton University in 1932 to become professor at the newly-created Institute for Advanced Study. As one of the early organizers, he put together the Institute's world famous School of Mathematics which boasted such luminaries as James W. Alexander (1888–1971), Albert Einstein (1879–1955), John von Neumann (1903–57), Hermann Weyl (1885–1955), and Veblen himself on its initial faculty. A haven of research for research's sake, the Institute, as Veblen conceived of it, was a breeding ground for new results where younger but gifted scholars could go as temporary members to work alone, together, or with the members of the permanent faculty in a perfectly conducive research environment.[67]

Like Veblen, Birkhoff also taught as one of Princeton's 'preceptor guys,'[68] but in 1912 he accepted an assistant professorship at his undergraduate alma mater, Harvard. Although Birkhoff wrote his dissertation under Moore and looked to him for advice in his earlier years, his research was far afield of Moore's interests. Inspired by the writings of Henri Poincaré (1854–1912), Birkhoff ardently pursued questions involving dynamical systems such as the three body problem and made good advances in these and other areas.[69] At Harvard his abilities were recognized by the large number of students[70] who worked under his direction and by the administration which appointed him Dean of the Faculty of Arts and Sciences in 1936. Birkhoff's international influence was reflected in the many awards and honours he received from universities and societies around the world. Perhaps the greatest testimony to this respect was his appointment to the presidency of the International Congress of Mathematicians which was to have been held in Cambridge, Massachesetts in 1940.[71]

Less well known worldwide, Robert L. Moore secured national renown both for his research in point-set topology and for his famous 'Moore method' of teaching. A true Texan, 'he was proud, steadfast, and ever ready to defend his ideas, but appreciative of (often delighted with) an opponent who openly opposed him'.[72] His uncompromising dedication to his teaching and to his method resulted in an extremely large school of University-of-Texas-bred topologists all of whom bore the R. L. Moore stamp. The method, which bore a distinct resemblance to E. H. Moore's laboratory method of teaching mathematics, involved discovery. Students, rather than being spoonfed theorems and their proofs, were skillfully led to 'discover' the mathematics for themselves.[73] In this way R. L. Moore believed that the student's ability to produce

[66] The preceptorial system was the brainchild of Woodrow Wilson who was President of Princeton from 1902 to 1910. It amounted to a sort of small group system where a young and energetic faculty member read on a given subject with a small number of undergraduates in order to stimulate intellectual activity. There were no lectures, necessarily, just discussions among thoughtful people. For a more detailed description of the system as conceived by Wilson, see Woodrow Wilson, "The Preceptorial System at Princeton", *Educational Review*, 39, (1910) 385–90.

[67] For further biographical information on Veblen, see Archibald, ed. (footnote 2) I, 206–11, and Saunders Mac Lane, "Oswald Veblen: 1880–1960," *Biographical Memoirs of the National Academy of Sciences*, 37 (1964), 325–41.

[68] H. S. Taylor, "Joseph Henry Maclagen Wedderburn: 1882–1948, "*Obituary Notices of Fellows of the Royal Society*, 6 (1948–49), 619–25 (p. 620). Wedderburn was another of the first preceptors probably owing to Veblen's recommendation.

[69] For more on George Birkhoff's life, see Archibald, ed. (footnote 2) I, 212–18, and Marston Morse, "George David Birkhoff and His Mathematical Work", *Bulletin of the American Mathematical Society*, 52 (1946), 357–91. Morse also goes into great detail on the various aspects of Birkhoff's work.

[70] See note 63 above.

[71] This Congress, which was postponed due to the war in Europe, was eventually held in Cambridge in 1950. Since G. D. Birkhoff had died in the interim, the presidency went to Oswald Veblen. See Mac Lane (footnote 67) p. 334.

[72] Wilder (footnote 62) p. 418.

totally original results later on was enhanced. As one of his more famous students admitted though, 'it was a unique method employed by a unique man in a unique situation'.[74] Like the laboratory method applied to mathematics, few could implement this 'Moore method' with the success of the original innovator.[75]

The accomplishments outlined above reflect the successes and hard work not of E. H. Moore but of his gifted students. Nevertheless, these four careers suggest an impressive genealogical tree. Rooted in the European and especially in the German mathematical tradition, this tree has Moore as its trunk and his four distinguished students as its main branches. At its crown hundreds of other branches, many strong and some weak, represent the subsequent mathematical generations. A flourishing heritage, it symbolizes the firmly-grounded mathematical community which has grown during the twentieth century in America due in large part to the energy and enthusiasm of E. H. Moore.

This account of Moore's career from 1892 when he assumed his professorship at the University of Chicago to roughly 1902 when he completed his term as president of the American Mathematical Society details not only the most productive period of his professional life but also the most formative years of American mathematics. Moore came along at a time when mathematics in the United States was struggling to leave its infancy and childhood behind and to develop into a mature and productive adult.

5. Conclusion

In the 1870s and 1880s, mathematics had been born on the North American continent through the efforts of men like J. Willard Gibbs of Yale, Benjamin Peirce of Harvard, and James J. Sylvester of Johns Hopkins, but rapid growth during that period had been an impossibility. Since the American educational system had only begun to offer more advanced training in mathematics, the level of sophistication of the students had not reached sufficient heights.

By the turn of the twentieth century, however, this situation had begun to change largely as a result of the efforts of Eliakim Hastings Moore. Based at the University of Chicago, Moore was both a dynamic administrator and a first-rate mathematician. His administrative duties began in 1892 when William Rainey Harper chose him as chairman of the mathematics department of the new University of Chicago. In this post Moore worked diligently for mathematics at a local, national, and international level. From his organization of the international mathematics congress at the World's Columbian Exposition to his founding of the Chicago Section of the American Mathematical Society to his presidency of this same body, Moore's activities bore the clear stamp of his well-thought-out ideas and persistence.

[73] Compare E. H. Moore (footnote 53) pp. 412–13.

[74] Wilder (footnote 62) p. 418.

[75] E. H. Moore had one other student of importance during the period from 1896 to 1907 who deserves mention. Although never particularly productive as a research mathematician, Herbert E. Slaught (1861–1937) carried on Moore's pedagogical and organizational traditions. Slaught became an editor of the *American Mathematical Monthly* in 1907, and sought to have this publication taken over by the A.M.S. Because of its more popular and less research-oriented nature, the Society refused. Believing firmly in the need for an organization devoted to teachers of mathematics and college students, Slaught organized the Mathematical Association of America in 1915 and became its president in 1919. The *Monthly* became the official publication of this new association. For more on Slaught and his contributions to mathematics teaching in America, see W. D. Cairns, "Herbert Ellsworth Slaught—Editor and Organizer", *American Mathematical Monthly*, 45 (1938), 1–4, and Gilbert A. Bliss, "Herbert Ellsworth Slaught—Teacher and Friend", *American Mathematical Monthly*, 45 (1938), 5–10.

As a professor of mathematics, his deep concern for his subject and for his students always characterized his career. His students Leonard E. Dickson, Oswald Veblen, George D. Birkhoff, and Robert L. Moore, nurtured on Moore's brand of abstract thinking, also went on to make seminal contributions to mathematics and to its organization. E. H. Moore limited neither his interests nor his efforts to his doctoral students, however. He also pushed throughout his early career for the pedagogical reform of mathematics at especially the secondary and college levels. L. E. Dickson's closing words in his obituary of Moore summed up the man's achievements quite succinctly: 'Moore's work easily places him among the world's great mathematicians. In America, his various accomplishments made him the leader. But he was a leader who was universally loved, and this was because he was at the same time a prince of a man'.[76]

[76] Dickson (footnote 3) p. 80.

GRADUATE STUDENT AT CHICAGO IN THE TWENTIES

W. L. DUREN, JR.*

As an undergraduate at Tulane in New Orleans, 1922–'26, I was programmed to go to the University of Chicago and study celestial mechanics with F. R. Moulton. My teacher, H. E. Buchanan, had been a student of Moulton. That was an example of the great strength of the University of Chicago. Its PhD graduates made up a large part of the faculties of universities throughout the Mississippi Valley, Midwest and Southwest. So they sent their good students back to Chicago for graduate work. I went there first in the summer of 1926 and came to stay in 1928. In the interim I studied Moulton's *Celestial Mechanics* and some of his papers in orbit theory. I met Moulton at a sectional meeting of the MAA where he was the invited speaker. He was a man of great charm and energy and was most encouraging to me. But by the time I got to Chicago in 1928 Moulton had resigned. I was told that he felt it was an ethical requirement, since he and his wife were getting a divorce. On the advice of T. F. Cope, another former student of Buchanan, who was working with Bliss, I turned to Bliss as an advisor in the calculus of variations.

It was a down cycle for mathematics at Chicago. All the great schools have their downs as well as ups, partly because great men retire, partly because their lines of investigation dry up. At Chicago at that time a young student could see the holdovers of the great period, 1892–1920, in Eliakim Hastings Moore, officially retired, Leonard E. Dickson, rounding out his work in algebra, Gilbert A. Bliss, busy with administration and planning for the projected Eckhart Hall. Also there was Herbert E. Slaught, teacher and doer, one of the original organizers of the Mathematical Association of America and its MONTHLY, even if he played only a supporting role in mathematics itself. He had an extrovert, friendly personality that reached out and got hold of you, whether he was organizing a department social or the Mathematical Association of America. He was the teacher of teachers and key figure in Chicago's hold on education in the midwest and south. Every graduate department needs a man like Slaught if it is fortunate enough to find one. He was being succeeded by Ralph G. Sanger, a student of Bliss, an outstanding undergraduate teacher, though not the organizer Slaught was.

The University of Chicago was founded in 1892 with substantial financial support from John D. Rockefeller. William Rainey Harper, the first president, had bold educational ideas, one of which was that the United States was ready for a primarily graduate university, not just a college with graduate school attached. Harper brought E. H. Moore from Yale to establish his department of mathematics. Moore's graduate teaching was done in a research laboratory setting. That is, students read and presented papers from journals, usually German, and tried to develop new theorems based on them. The general subject of these seminars was a pre-Banach form of geometric analysis that Moore called "general analysis." It was itself not altogether successful. But even if general analysis did not succeed, Moore's seminars on it generated a surprising number of new results in general topology, among them the Moore theorem on iterated limits and Moore-Smith convergence. Moore's seminars also produced some outstanding mathematicians. His earlier students had included G. D. Birkhoff, Oswald Veblen, T. H. Hildebrant and R. L. Moore, who took off in different mathematical directions. R. L. Moore developed the teaching method into an intensive research training regimen of his own, which was very successful in producing research mathematicians at the University of Texas.

I studied general analysis with other members of the faculty including R. W. Barnard, whom

* With the help of Antoinette Killen Huston, who earned her way as graduate student by serving as secretary to Mr. Bliss. She was Adrian Albert's first student, receiving her PhD degree in 1934.

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Moore had designated as his successor and whose notes record the second form of the theory, [Am. Philosophical Soc., *Memoirs*, v. 1, Philadelphia, 1935]. Instead of taking the general analysis courses, my old friend E. J. McShane, from New Orleans, worked in Moore's small seminar in the foundations of mathematics. Although he was officially a student of Bliss, I think he was in a sense Moore's last student.

Moore himself was meticulous in manners and dress. He would stop you in the hall, gently remove a pen from an outside pocket and suggest that you keep it in the inside pocket of your jacket. Nobody thought of not wearing a jacket. But Moore was less gentle if you used your left hand as an eraser, and he displayed towering anger at intellectual dishonesty. To understand him and his times one must read his retiring address as President of the Society [*Science*, March 1903]. In those days the Society accepted responsibility for teaching mathematics and Moore's address was largely devoted to the organization of teaching, the curriculum, and the ideas of some of the great teachers of the time, Boltzman, Klein, Poincaré, and, in this country, J. W. A. Young and John Dewey, whose ideas Moore supported by proposing a mathematics laboratory. This address was adopted as a sort of charter by the National Council of Teachers of Mathematics and republished in its first Yearbook (1925). By the time I got to Chicago the Association had been formed to relieve the Society of concern for college education, and NCTM to relieve it of responsibility for the school curriculum and training teachers. In the top universities only research brought prestige, even if a few, like Slaught, upheld the importance of teaching.

L. E. Dickson's students tended to identify themselves strongly as number theorists or algebraists. I felt this particularly in Adrian A. Albert, Gordon Pall and Arnold Ross. All his life Albert strongly identified himself, first as an algebraist, later with mathematics as an institution and certainly with the University of Chicago. I remember him as an advanced graduate student walking into Dickson's class in number theory that he was visiting, smiling and self confident. He knew where he was going. Dickson was teaching from the galley sheets of his new *Introduction to the Theory of Numbers* [University of Chicago Press, 1929] with its novel emphasis on the representation of integers by quadratic forms. I think he requested Albert to sit in for his comments on this aspect. He was tremendously proud of Albert. I remember A^3 too with his beautiful young wife, Frieda, at the perennial department bridge parties. He had superb mental powers; he could read a page at a glance. One could see even then that as heir apparent to Dickson he would do his own mathematics rather than a continuation of Dickson's, however much he admired Dickson.

In the conventional sense Dickson was not much of a teacher. I think his students learned from him by emulating him as a research mathematician more than being taught by him. Moreover, he took them to the frontier of research, for the subject matter of his courses was usually new mathematics in the making. As Antoinette Huston said, "He made you want to be with him intellectually. When you are young, reaching for the stars, that is what it is all about." He was good to his students, kept his promises to them and backed them up. Yet he could be a terror. He would sometimes fly into a rage at the department bridge games, which he appeared to take seriously. And he was relentless when he smelled blood in the oral examination of some hapless, cringing victim. He was an indefatigable worker and in public a great showman, with the flair of a rough and ready Texan. An enduring bit in the legend is his blurt: "Thank God that number theory is unsullied by any application." He liked to repeat it himself as well as his account of his and his wife's honeymoon, which he said was a success, except that he got only two papers written.

The theme of beauty for its own sake was expressed more surprisingly by another Texan who worked in mechanics and potential theory, W. D. MacMillan. According to the story he had come to Chicago as a mature man, without a college education, to sell his cattle. Having sold them, he went to

Chicago's Yerkes Observatory to see the Texas stars through the telescope. He was so fascinated that he stayed on to get his degrees in rapid succession, all *summa cum laude*. Then he remained as a member of the faculty. One day in his course on potential theory he wrote some important partial differential equations on the board with obvious pleasure, drawing the partial derivative signs with a flourish. Standing back to admire these equations, he said: "That is just beautiful. People who ask, 'What's it good for?', they make me tired! Like when you show a man the Grand Canyon for the first time and you stand there as you do, saying nothing for a while." And we could see that old Mac was really looking at the Grand Canyon. "Then he turns to you and asks, 'What's it good for?' What would you do? Why, you would kick him off the cliff!" And old Mac kicked a chair halfway across the room. He was a prodigy, a good lecturer, an absolutely fascinating personality with a twinkling wit. Some of his work was outstanding, yet he had few doctoral students.

Celestial mechanics was being carried on by the young Walter Bartky, who was, I think, Moulton's last student. But celestial mechanics had gone into a barren period and Bartky with his superb talents turned to other applications of differential equations, to statistics and to administration.

Lawrence M. Graves was the principal hope of the department for carrying on the calculus of variations, which he did in the spirit of functional analysis. He was my favorite professor because he knew a lot of mathematics, knew it well, and in an unassuming way was glad to share it with you. Although he taught Moore's general analysis, he pointed out the difficulties in it to me. His own brand of functional analysis was more oriented towards the use of the Frêchet differential in Banach space.

Research in geometry at Chicago was a continuation of Wylczinski's projective differential geometry. There was no topology, though we heard that Veblen's students studied something called *analysis situs* at Princeton. I knew so little about the subject that years later when I wanted to prepare for Morse theory I spent months studying Kuratowski's point set topology before it dawned on me that what I wanted was algebraic topology. E. P. Lane and his students carried on the study of projective differential geometry using rather crude analytical methods, that is, expansions in which one neglected higher order terms. We who were not Lane's students tended to look on it with disdain as being non-rigorous. But the structure of the theory was beautiful, I thought. Lane was honest about the shortcomings of the methods, though he did not know how to overcome them.

Lane was a very fine man. I had come to Chicago in 1926 to run the high hurdles in the National Intercollegiate Track and Field Meet at Soldiers Field. I placed in the finals and some members of the U. S. Olympic Committee urged me to keep working for the 1928 Olympics. So I worked on the Stagg Field track until an accident set off a series of leg infections. I was very sick in Billings Hospital in the days before antibiotics and it was Lane who came to the hospital to see me and make sure that I got the best available care. The only way I was ever able to express my thanks to him was to do a similar service to some of my own students in later years. I guess that is the only way we ever thank our teachers.

Bliss was an outstanding master of the lecture-discussion. He could come into a class in calculus of variations obviously unprepared, because of the demands of his chairmanship, and still deliver an elegant lecture, drawing the students into each deduction or calculation, as he looked at us quizzically and waited for us to tell him what to write. His students learned their calculus of variations very thoroughly. Yet we did not work together, except in so far as we presented class assignments. Each research student reported to Bliss by appointment. The subject itself had come to be too narrowly defined as the study of local, interior minimum points for certain prescribed functionals given by integrals of a special form. Generalization came only at the cost of excessive notational and analytic complications. It was like defining the ordinary calculus to consist exclusively of the chapter on maxima and minima. A sure sign of the decadence of the subject was Bliss's project to produce a history of it, like Dickson's *History of the Theory of Numbers.* The history reached publication only in the form of certain theses imbedded in *Contributions to the Calculus of Variations,* 4 vols, 1930–1944, University of Chicago Press.

It is perhaps surprising that this narrowly prescribed regimen turned out men who did important work in entirely different areas as, for example, A. S. Householder did in biomathematics and numerical analysis, and Herman Goldstine did in computer theory. Among all of us Magnus Hestenes has been most faithful to the spirit of Bliss's teaching in carrying on research in the calculus of variations. Yet when Pontryagin's Optimal control papers revived interest in the subject many years later, students of Bliss were easily able to get into it. Optimal control theory really contained relatively little that was correct and not in the calculus of variations. In fact, optimal control was anticipated by the thesis of Carl H. Denbow, *loc. cit.*

Quantum mechanics was breaking wide open in the twenties. Bliss himself got into it with his students by studying Max Born's elegant canonical variable treatment of the Bohr theory. While that was going on, Sommerfeld's *Wellenmechanische Ergänzungsband* to his *Atombau und Spektrallinien* [Vieweg, Braunschweig, 1929] came out. It was the first connected treatment of the new wave mechanics formulation of quantum mechanics due to de Broglie and Schrödinger. We dropped everything to study wave mechanics. Bliss was a remarkably knowledgeable mathematical physicist and quite expert in the boundary value problems of partial differential equations. That was not so remarkable in a mathematician of his generation. The narrowing of the definition of a mathematician and withdrawal into abstract specializations was just beginning. In fact Bliss had been chief of mathematical ballistics for the U.S. Government in World War I, and later was commissioned to do a mathematical study of proportionate representation for purposes of reassigning Congressional districts. Bliss did not follow up his move into quantum mechanics but returned to the classical calculus of variations.

There were always more students in summers with all the teachers who came. Visiting professors like Warren Weaver, E. T. Bell, C. C. Mac Duffee and Dunham Jackson came to teach. And there was the memorable visit of G. H. Hardy which was supposed to provide a uniting of Hardy's analytic approach to Waring's theorem with Dickson's algebraic approach. Even with this infusion of talent, the offerings of the department were rather narrow. Besides having no topology as such, more surprisingly, there was little in complex function theory. And I do not recall being in a seminar, either a research or journal seminar. Essentially all teaching was done in lectures. Yet the only one of the abler students who I remember taking the initiative to go elsewhere was Saunders MacLane, when he did not find at Chicago what he was looking for.

I once asked Edwin B. Wilson, a famed universalist among mathematicians, how he came to switch from analysis to statistics at Yale. With a humorous twinkle he said: "An immutable law of academia is that the course must go on, no matter if all of the substance and spirit has gone out of it with the passing of the original teacher. So when (Josiah Willard) Gibbs retired, his courses had to go on. And the department said: 'Wilson, you are it'." A graduate student at Chicago in the late twenties could see this immutable academic law in effect. In each line of study of the, then passing, old Chicago department, a younger Chicago PhD had been designated to carry on the work. If, in one's immaturity, this was not apparent, the point was made loud and clear in a blast from Dickson during a colloquium with graduate students present. Dickson charged the chairman with permitting the department to slide into second rate status. It was true that the spirit of original investigation had given way to diligent exposition in some of these fields. In some cases the fields themselves had gone sterile.

It was the lot of Bliss to preside over this ebb cycle of the department. He did an impressive best possible with what he had, with high mathematical standards, firmly, kindly and quietly. Most of the difficulties he had inherited. Bliss was able to appoint some outstanding young men but, if he had asked for the massive financial outlay to bring in established leading mathematicians to make a new start like the original one under President Harper, the support would not have been forthcoming, even with a mathematician, Max Mason, as president and certainly not with the young Robert M. Hutchins, bent primarily on establishing his new college. It took the Manhattan Project, the first nuclear pile under the Stagg Field bleachers and Enrico Fermi to convince Hutchins of the importance of physical science and mathematics and to throw massive resources into the reorganization of the department near the end of World War II. Such reorganizations are necessary from time to time in every graduate department. They can be effective only when the time is right. It is the mark of a great university to recognize the necessity to break the immutable law of academia, and the opportunity, and to do it when the time is right. However, there were deep hurts, symbolized by Bliss's refusal ever again to set foot in Eckhart Hall to his death. But this is really getting ahead of my story.

It was no ebb cycle for the University of Chicago as a whole in the twenties. There was intellectual excitement in many places in the university. I attended the physics colloquia where the great innovators of the day came to talk. With Mr. Bliss's grudging consent, I took Arthur Compton's course in X-rays. He already had the Nobel Prize for his work on the phenomena of X-rays colliding with electrons. Yet he seemed so naïvely simple minded to me, far less expert and mentally profound than other physicists in the department. Somewhere in here Einstein came for a brief visit. He permitted himself to be escorted by the physics graduate students for a tour of their experiments. To one he offered a suggestion. The brash young man explained immediately why it could not work. Einstein shook his head sadly. "My ideas are never good," he said.

Michelson, another Noble Prizeman, was around, though retired. So was the great geologist, Chamberlin, with his planetesimal hypothesis in cosmology. In biology and biochemistry the great breakthroughs on the chemical nature of the steroid hormones and their effects on growth and development were excitingly unfolding. Young Sewall Wright was attracting students to his mathematical genetics. Economics promised a real breakthrough, though as it turned out, it was slow in coming. Linguistics was burgeoning. Anthropology and archeology were still actively following up the results of digs in Egypt, Turkey and Mesopotamia. The great debates over the truth of theories of relativity and quantum mechanics were raging. What was later to be planet Pluto had been observed as "Planet X" but heated arguments persisted on what it really was. On Sundays the University Chapel produced a succession of the leading Christian and Jewish spokesmen of the day. The textbook, *The Nature of the World and of Man*, H. H. Newman ed., University of Chicago Press, 1926, by illustrious Chicago faculty members was the best survey of physical and biological knowledge for college students that I have ever seen, though now dated, of course.

And outside the university the dangerous and ugly city of Chicago nevertheless had its charms, cultural and otherwise, that could take up all the time (and money) of a country boy. One could hear Mary Garden or Rosa Raisa at the Chicago Opera by getting a job as usher or super, or attend a fiesta in honor of the patron saint of some Halstead Street community that maintained its identity with the home village in the old country. One could drink wine at Alexander's clandestine speakeasy. For recall that it was Prohibition and the height of the bootlegging days of Al Capone and rival gangs. The famous Valentine Day massacre was just one of the lurid stories in the Chicago Tribune. We students formed an informal protective association to promulgate rules to optimize safety for oneself and date. One old boy from Georgia, a graduate student in history, was so impressed by our admonition never to approach a car asking him to get in, that, when a police car challenged him with order to stop, he just took off in a blaze of speed. Caught later, out of breath, his one phone call brought some of us to police court to testify to his character. The officer who had made the arrest moved to dismiss the charges on the condition that "the defendant appear at Soldiers Field next Saturday and run for our company in the policemen's track meet." But it was grim business. Police, armed with machine guns, in such a car once arrested me on suspicion of rape on the Midway (not guilty!). Other students were mugged, raped, robbed and even killed.

Like today it was a time of inflation and most of us were poor. I had a full fellowship of $410, of which $210 had to be returned in tuition for three quarters. A dormitory room cost $135 out of what was left. We could get cheap meals at the Commons, and on Sundays one could go to the Merit Cafeteria and splurge on a plate-sized slab of roast beef. It cost 28¢ but it was worth it. We all looked forward to a teaching job, I think. Those jobs required 15 hours of teaching for about $2700. Soon the depression hit and, if we were lucky, we kept our jobs with salary cut to $2400. Some beginning salaries for Chicago PhD's were as low as $1800 in the early thirties.

Before closing these recollections I must write something about women as graduate students in those times, not long after the victory of women's suffrage. Only years later did I learn that it was considered unladylike to study mathematics. Many of the graduate students in mathematics were women. In fact there were 26 women PhD's in mathematics at Chicago between 1920 and 1935. I shall mention only a few by name. Mayme I. Logsdon (1921) was in the faculty of the department. Mina Rees (1931) was already showing the kind of ability that led her to a distinguished administrative career at Hunter College and CUNY. She did more than any other person to gain federal support for mathematics through her position as chief, Mathematics Branch ONR, when the National Science Foundation was established. Others included Abba Newton (1933), chairman at Vassar, and Frances Baker (1934) also of Vassar, Julia Wells Bower (1933), chairman at Connecticut College, Marie Litzinger (1934), chairman, Mt. Holyoke, Lois Griffiths (1927) Northwestern, Beatrice Hagen (1930) Penn State, and Gweneth Humphreys (1935) Randolph Macon. Graduate students married graduate students, though of necessity only after the man had his degree. In the department Virginia Haun married E. J. McShane. Emily Chandler, student of Dickson, married Henry Pixley and continued her publishing and teaching career at the University of Detroit. Antoinette Killen married Ralph Huston. They both later taught at Rensselaer Polytech. Aline Huke married a non-Chicago mathematician, Orrin Frink, and continued her teaching at Penn State. Jewel Hughes Bushey was in the department of Hunter College. These, and a number of others, were able to continue their professional work in spite of family obligations. Even intermarriage between departments was permitted! My wife to be, Mary Hardesty, was in zoology. We got our PhD degrees in the same commencement.

Looking back on those days, I wonder if the current women's liberation has even yet succeeded in pushing the professional status of women to the level already reached in the twenties. Maybe this time women can hold their gains in universities.

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Reminiscences of Mathematics at Chicago [1]

MARSHALL H. STONE

In 1946 I moved to the University of Chicago. An important reason for this move was the opportunity to participate in the rehabilitation of a 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. Be that as it may, the University resolved at the close of the war to rebuild the department.

The decision may have been influenced by the 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, and had succeeded in making a series of brilliant appointments in physics, chemistry, and related fields. Something similar clearly needed to be done when the University started filling the vacancies that had accumulated in mathematics. Professors Dickson, Bliss, and Logsdon had all retired fairly recently, and Professors W. T. Reid and Sanger had resigned to take positions elsewhere. The five vacancies that had resulted offered a splendid challenge to anyone mindful of Chicago's great contribution in the past and desirous of ensuring its continuation in the future.

When the University of Chicago was founded under the presidency of William Rainey Harper at the end of the 19th century, mathematics was encouraged and vigorously supported. Under the leadership of Eliakim Hastings Moore, Bolza and Maschke it quickly became a brilliant center of mathematical study and research. Among its early students were such mathematicians as Leonard Dickson, Oswald Veblen, George Birkhoff, and R. L. Moore, destined to future positions of leadership in research and teaching. Some of

Marshall H. Stone

these students remained at Chicago as members of the faculty. Messrs. Dickson, Bliss, Lane, 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 had brought a much increased emphasis on the calculus of variations and a certain tendency to 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 was also ripe for a fundamental revision of graduate and undergraduate mathematical education.

The invitation to Chicago confronted me with a very difficult question: "Could the elaboration of a modernized curriculum be carried out more successfully at Harvard or at Chicago?"

When President Hutchins invited me to visit the University in the summer of 1945, it was with the purpose of interviewing me 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' 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 which I sought the answer to the question with which it confronted me. It soon became clear that the situation at Harvard was not ripe 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 there already had in mind. Nothing could be farther from the truth. In fact, my negotiations were directed towards 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 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 experiences had taught me that administrative promises of wholehearted 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, as the department had to be consulted about the matter, and responded by voting unanimously that Professor Lane should be retained in the office. As I was unwilling to move merely on the basis of a promise to appoint me to the chairmanship at some later time, the administration was brought around to arranging the appointment, and I to accept 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 up-hill fight all the way. The University was not about to implement the plans I had proposed in our negotiations without resisting and raising objections at every step. 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 like to think that I am not by nature an autocrat, and that the later years of my chairmanship provided evidence of this belief. At the beginning, however, I took a strong line in what I was doing in order to make the department a truly great one.

The first recommendation sent up to the administration was to offer an appointment to Hassler Whitney. The suggestion was promptly rejected by Mr. Hutchins' 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 Andre Weil. He was a somewhat controversial personality, and I found a good deal of hesitation, if not reluctance, on the part of the administration to accept my recommendation.

In fact, while the recommendation eventually received favorable treatment in principle, the administration made its offer with a substantial reduction in the salary that had been proposed; and 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 take the matter up at the highest level. Though I had to go to an 8 a.m. appointment suffering from a fairly high fever, in order to discuss the appointment with Mr. Hutchins I was rewarded by his willngness to renew the offer on the terms I had originally proposed. 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.

HAND-TO-MOUTH BUDGETING

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 one evening 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, we could turn our attention to a thorough study of the curriculum and the requirements for higher degrees in mathematics. The group that was about to undertake the task of redesigning the department's work was magnificently equipped for what it had to do. It included, in alphabetical order, Adrian Albert, R. W. Barnard, Lawrence Graves, Paul Halmos, Magnus Hestenes, Irving Kapansky, J. L. Kelley, E. P. Lane, Saunders Mac Lane, Otto Schilling, Irving Segal, M. H. Stone, Andre Weil, and Antoni Zygmund. Among them were great mathematicians, and great teachers, and leading specialists in almost every branch of pure mathematics. Some were new to the University, others familiar with its history and traditions. We were all resolved to make Chicago the leading center in mathematical research and education it had always aspired to be. We had to bring great patience and open minds to the time-consuming discussion 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 beside the new graduate program.

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.

This streamlined program of studies, the unusual distinction of the mathematics faculty, and a rich offering of courses and seminars have attracted many very promising young mathematicians to the University of Chicago ever since the late '40s. The 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. As most members of the department lived near the University and generally spent their days in Eckhart, close contact between faculty and students was easily established and maintained. (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 create conditions so favorable for such mathematical activity, Professor Bliss earned the eternal gratitude of his University and his department. Anyone who reads the roster of Chicago doctorates since the later '40s 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. If this is done, the program must be considered as a highly effective one.

As I have described it, the Chicago program made one conspicuous omission — it provided no place for applied mathematics. During my correspondence of '45–'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 had also hoped that it would be possible 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 to increase its offerings in applied mathematics. It had no engineering school, and rather recently had even 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 had already worked out an entirely new program in physics and were in no mood to modify it in the light of subsequent changes that might take place 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 for the creation of a Department of Statistics, exerted particularly by the economists of the Cowles Foundation. A committee was appointed to make recommendations to the administration for the future of statistics with Professor Allen Wallis, Professor Tjalling Koopmans, and myself 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 and developed informal ties with the Department of Mathematics.

At a somewhat later time a similar committee was set up to bring the instruction in applied mathematics into focus by coordinating the courses offered in several different 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 then 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?"*

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 more than 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.

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*He was soon to become a permanent member of the Institute of Advanced Study.

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.

The Stone Age of Mathematics on the Midway [1]

FELIX E. BROWDER

After its foundation as a distinguished department by E. H. Moore in the 1890s, the single most decisive event in the history of the Department of Mathematics at the University was the assumption of its chairmanship by Marshall Harvey Stone in 1946. Stone arrived in Chicago from a professorship at Harvard as the newly appointed Andrew MacLeish distinguished service professor of mathematics as well as chairman of the department. Within a year or two, he had transformed a department of dwindling prestige and vitality once more into the strongest mathematics department in the U.S. (and at that point probably in the world). This remarkable transformation, which endowed the department with a continuing vitality during the trials of the following decades is unparalleled, to the writer's knowledge, in modern academic history for its speed and dramatic effect. This was no easy victory on the basis of great infusions of outside money for bringing in men and building research facilities. It was completely a triumph for Stone's sureness of judgment in men and his determination and strength of character in getting done what he knew had to be done.

Stone's account of the transformation which he formed and led is unparalleled for its candor and its objectivity (despite the strong flavor of Stone's personality) and for its remarkably open presentation of the process by which academic decisions are reached and leadership exerted. The central problem of academic life for institutions which aspire to excellence and to greatness is precisely the achievement of that excellence and that greatness.

Within every academic institution, policy leadership falls into two patterns. The most common pattern, which is the basis of the ongoing routine of the institution's existence, falls within the rational-bureaucratic mold (to use the classical terminology of Max Weber) in terms of rationalized general policies and procedures to be applied uniformly to an array of cases in the context of a balance of special interests and influences. The other pattern, which is less common, is that of charismatic leadership in which the individual judgment and personal qualities of the administrator play a fundamental role in both the choice and nature of the policy decisions which are made, and in their acceptance by those who are affected by them. Stone's account gives us a picture of the most highly developed form of charismatic leadership, one which turned out to be enormously successful. What is most interesting about it is the question it raises about the role of charismatic leadership in the search for academic excellence.

To my knowledge, there is no case in which academic excellence in any reasonably high degree has been achieved and maintained without an infusion of charismatic leadership, either public or behind the scenes. Yet to an ever greater degree, it has become increasingly incompatible with the growing pressures and struggles of interests that tend to dominate the organized life of our universities.

When Marshall Harvey Stone arrived at the University in 1946 to play such a distinctive role, he was a relatively young man (43) and a mathematician of great distinction and great reputation. He had spent most of his academic life at Harvard, getting his Ph.D. degree there in the late '20s with the dominant personality of the Harvard department, George David Birkhoff who had himself been a student of E. H. Moore at Chicago. Stone had done fundamental work in a number of widely-known directions, in particular on the spectral theory of unbounded self-adjoint operators in Hilbert space and on the applications of the algebraic properties of Boolean algebras in the study of rings of continuous functions. He was an inner member of the country's mathematical establishment, having obtained a full professorship at Harvard as well as such honors as election to the National Academy of Sciences. He was profoundly involved in the growing trend toward putting mathematics research and education on an abstract or axiomatic foundation, and was sharply influenced by the efforts of the Bourbaki school in France in this direction, which achieved a major impact in the years after the end of World War II.

Most important of all, Stone was a man of forceful character and unquestioned integrity, with a strong insight into the mathematical quality of others.

Stone's fundamental achievement at Chicago was to bring together a faculty group of unprecedented quality. In the senior faculty he appointed four very diverse men with widely different personal styles and mathematical tastes. The most important of these was undoubtedly Andre Weil, the dominant figure of the Bourbaki group, who was, then and now, one of the decisive taste-makers of the mathematical world, as well as a brilliant research mathematician in his own work.

S. S. Chern, who was to be the central figure of differential geometry in the world, was brought from his haven at the Princeton Institute after his departure from China.

Antoni Zygmund, who became the central figure of the American school of classical Fourier analysis, which he was to build up single-handed, came from the University of Pennsylvania.

Saunders Mac Lane, who had been Stone's colleague and sympathizer in the abstract program as applied to algebra, came from a professorship at Harvard.

Together with Adrian Albert, who had been Dickson's prize student at Chicago and a longtime member of the Chicago department, these men formed the central group of the new Stone department at the University.

To do full justice to the kind of revolution that Stone brought about in Chicago mathematics, one needs to perform the unedifying task of acknowledging the decay of the department in the late '30s and early '40s. The great prestige and intellectual vitality that had been created under the long reign of E. H. Moore as chairman had not been maintained after his retirement from the chairmanship at the end of the '20s. His successors, G. A. Bliss and E. P. Lane, were not Moore's equals in either mathematical insight or standards. Especially under Bliss' regime, a strong tendency to inbreeding was in action, and as the great elder figures of the department died or retired, they were not replaced by younger mathematicians of equal caliber. Some of the most promising of those who came into the deparetment soon left. There was one principal exception: Adrian Albert. But despite his distinction as an algebraist in the Dickson tradition, Albert at that time had neither the influence nor the vision to bring about the kind of radical transformation that the department needed, and that Stone brought about.

The insights that Stone provides in his first-hand account of his great achievements and of how they were brought about provide us once more with a dramatic vindication of the decisive importance of the special qualities of significant individuals as the major agents of the development of academic institutions. In academic terms, Marshall Stone served as a great revolutionary and a great traditionalist. The revolution he made is the only kind which has a permanent significance – a revolution that founds or renovates an intense and vital tradition.