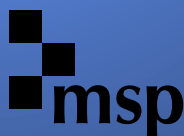# CELEBRATIO MATHEMATICA

SAUNDERS MAC LANE

MATHEMATICS AT THE UNIVERSITY OF CHICAGO: A BRIEF HISTORY

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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 (1851–1936), 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 Chicago. G. A. Bliss [1934] (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 [Dickson 1923a] and in number theory [1919; 1920; 1923b]. He and five others in this group of 39 Ph.D.s subsequently held the office of president of the American Mathematical Society (A.M.S.). 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).

Figure 1. E. H. Moore (University of Chicago Archives)

- 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 [1934] 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."

Figure 2. Oskar Bolza (University of Chicago Archives)

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 famous second proof [1908] 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, 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)
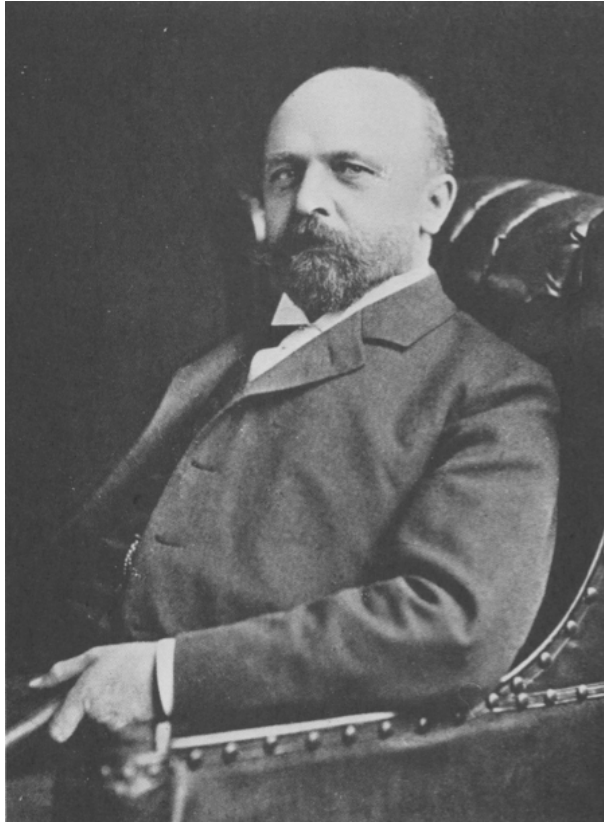 - – Linear Differential Equations (Maschke)

Figure 3. Heinrich Maschke (University of Chicago Archives)

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 Holder'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 [1919; 1920; 1923b], while his monograph *Algebras and Their Arithmetics* was translated into German
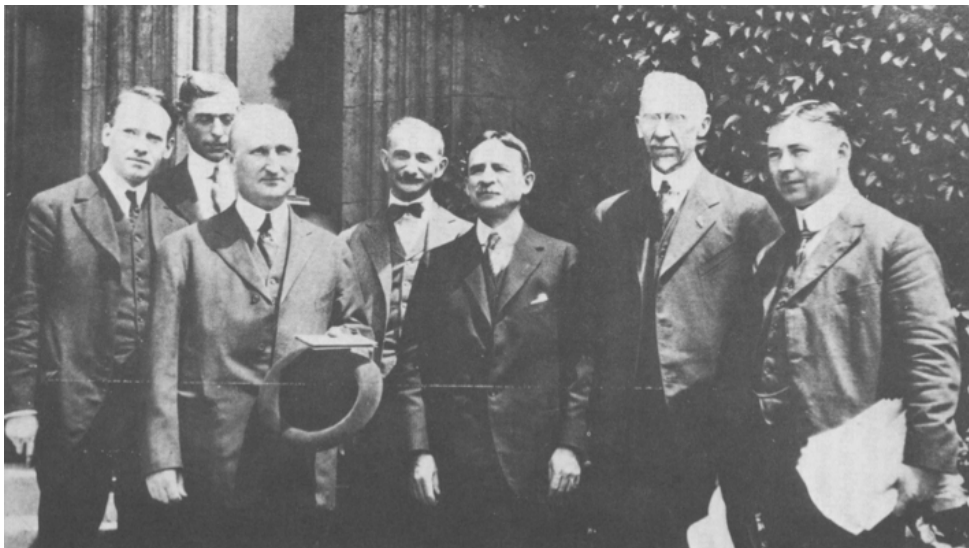
Figure 4. Left to right: Dickson, Wilczynski, Lunn, MacMillan, Moore, Slaught and Moulton (Courtesy of Saunders Mac Lane)

and had a major influence on the German school of abstract algebra [Dickson 1923a]. 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 I.I.T.; 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.

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 A.M.S. 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 [1935; 1939].

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 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 A.M.S. 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 (M.A.A.).
  - 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.

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 A.M.S.: 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 U.C.L.A., 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 N.S.F.; 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 U.C.L.A. 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 1954].

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 analysis, 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.

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 U.C.L.A..
- W. T. Reid (1907–1977), Ph.D. Texas 1927; instructor Chicago (1931) to 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 M.I.T.) 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). Thus 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 Bartkey in 1926. Perhaps because of his activity in military research during W.W. II, Bartkey'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.

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 W.W. 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 A.M.S. 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.

The Stone department

Robert Maynard Hutchins, president of the University of Chicago (1929–1951), had brought the Manhattan project to the university during W.W. 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 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 U.C.L.A., 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 1954]).

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 M.I.T. 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.

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 M.I.T. 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.

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.

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 [1976] and the autobiographical notes of Bliss [1952] 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 W.W. 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 [1952] 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 tenure 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 [Bliss 1946], 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 [Hestenes 1966].

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 endured 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 [Mac Lane 1981].

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.

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