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Historia Mathematica 31 (2004) 263–278
HISTORIA MATHEMATICA
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Defining a mathematical research school: the case of algebra at the University of Chicago, 1892–1945

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Available online 23 September 2003

Abstract

Historians of science have long considered the concept of the "research school" as a potent analytical construct for understanding the development of the laboratory sciences. Unfortunately, their definitions fall short in the case of mathematics. Here, a definition of "*mathematical* research school" is proposed in the context of a case study of algebraic work associated with the University of Chicago's Department of Mathematics from the University's founding in 1892 through 1945.

Sommario

Gli storici della scienza si sono serviti per molto tempo del concetto di "scuola di ricerca" come strumento analitico nel contesto delle scienze sperimentali. Sfortunatamente, le loro definizioni non sono in gran parte applicabili nel caso della matematica. In questo lavoro si propone una definizione di "scuola di ricerca *matematica*," la quale viene poi esaminata nel contesto di uno studio dei lavori algebrici prodotti dal Dipartimento di Matematica dell'Università di Chicago tra il 1892 e il 1945.

MSC: 01A55; 01A60; 01A72; 01A73; 17-03

Keywords: Mathematical research schools; University of Chicago; Algebra; E.H. Moore; Leonard E. Dickson; Joseph H.M. Wedderburn; Nathan Jacobson; A. Adrian Albert

E.H. Moore's early work in algebra

The University of Chicago opened in 1892 as an institution of higher education devoted to undergraduate and graduate education for young men and women as well as to the production of original research and the training of future researchers. Reflective of changes in American higher education,

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0315-0860/$ – see front matter
doi:10.1016/S0315-0860(03)00048-X

especially in the closing quarter of the 19th century, the new, adequately and privately endowed university sought, from its founding, a faculty capable of realizing these institutional goals. In mathematics, the first faculty—what we might call members of the first generation of research mathematicians on American shores—consisted of one American—Eliakim Hastings Moore—and two Germans—Oskar Bolza and Heinrich Maschke [Parshall and Rowe, 1994, 279–294].

The American, Eliakim Hastings Moore, had earned a doctoral degree at Yale College for an original, if ultimately unexciting, thesis on n-dimensional geometry in which he extended some theorems of the English mathematicians William Kingdon Clifford and Arthur Cayley. Moore had then journeyed first to Göttingen for a summer of language training and then to Berlin for a year of exposure to the mathematics of giants such as Karl Weierstraß and Leopold Kronecker. When the University of Chicago was putting together its faculty, Moore was teaching at nearby Northwestern University and eagerly accepted the call to a professorship and acting headship of Chicago's new Department of Mathematics. Almost immediately, his research interests shifted from geometry to algebra, a move spurred most likely by Chicago's evolving, algebraically oriented, mathematical environment.

In Chicago's first Winter Term of operation in 1893, Bolza taught a graduate-level course on the theory of permutation groups based on the classic work of Joseph Serret, Camille Jordan, and especially Eugen Netto, while Maschke continued his research in the theory of finite linear groups [Parshall and Rowe, 1994, 372–375]. Moreover, the Department's Mathematical Club, its weekly series of research-oriented workshops, had a decidedly algebraic focus in the University's first year [Parshall and Rowe, 1994, Table 9.1]. In this algebraic atmosphere and in light of what the emergent American mathematical community would soon recognize as Moore's uncanny ability to capitalize on hot research topics, Moore, too, moved into the theory of finite groups and immediately began proving new results. Perhaps his most notable early result and the work that may be said to mark the beginning of a tradition in algebra at Chicago was his contribution to the Chicago Mathematical Congress held in conjunction with the World's Columbian Exposition in August of 1893. Entitled "A Doubly-Infinite System of Simple Groups," Moore's Congress paper reflected the abstract point of view then increasingly characteristic of trendsetting German mathematics, namely, the methodology of identifying and classifying mathematical objects. In Moore's case, the objects were finite simple groups, and he discovered an entirely new class [Moore, 1896].

At the time of Moore's discovery, there were four known classes of finite simple groups in addition to the cyclic groups of prime order p and the alternating groups A_m for $m > 4$. One of these four was the class of groups now denoted $PSL_m(p)$ of order

$$\frac{(p^m-1)p^{m-1}(p^{m-1}-1)p^{m-2}\cdots(p^2-1)p}{\delta},$$

where $(p,m) \neq (2,2), (3,2)$ and $\delta = \gcd(p-1,m)$. In his 1870 book, *Traité des substitutions*, Jordan had done quite a bit of work on this class of finite groups of substitutions over the prime field $\mathbb{Z}/p\mathbb{Z}$, and Moore picked up on that work, focusing on the special case of $m = 2$.[1]

One problem Moore encountered, however, was that a group of order 360 that he had discovered in 1892 [Moore, 1892], as well as a group of order 504 discovered in the spring of 1893 by his countryman, Frank Nelson Cole [1893], failed to fit into any of the six known categories. Moore soon recognized that these groups fit into a new class of what he termed *doubly-infinite* or two-parameter

[1] Compare the discussion of this work in Parshall and Rowe [1994, 324–325, 377–378].

groups of order $p^n(p^{2n}-1)/\delta$, for $(p,n) \neq (2,1), (3,1)$, and he showed that, in fact, all of these new groups—what would today be denoted $PSL_2(p^n)$—are simple.[2] Before establishing this main result, however, Moore needed to come to terms with the underlying fields with p^n elements. This led him to an unexpected field-theoretic theorem, namely, "[e]very existent field $F[s]$ is the abstract form of a Galois field, $GF[p^n]$, where $s = p^n$" [Moore, 1896, 211].[3] Thus, in his efforts to identify and classify finite simple groups, Moore also characterized finite fields in a new and provocative way. What might be called Moore's structural approach to the algebraic questions raised in his Congress paper became even more pronounced after 1901 when he also embraced the axiomatic point of view he encountered in David Hilbert's ground-breaking *Grundlagen der Geometrie* [Hilbert, 1899].[4] An abstract and structural approach came to characterize much of the algebraic work that issued from the University of Chicago over the course of the first five decades of the 20th century.

Moore's first student, Leonard E. Dickson

The first student to be influenced by Moore's new algebraic ideas was Leonard Eugene Dickson. Dickson had come to the University of Chicago in 1894 to pursue graduate studies under Moore and in 1896 earned one of the two mathematics Ph.D.'s awarded that year, the program's first doctorates.[5] Dickson's thesis, entitled "The Analytic Representation of Substitutions on a Power of a Prime Number of Letters with a Discussion of the Linear Group," followed directly on the work Moore had done in his Congress paper in 1893 [Dickson, 1897]. Dickson focused structurally on the finite fields $F = GF[p^n]$, for p a prime and $n \in \mathbb{Z}^+$, that Moore had worked with. Dickson considered a polynomial $\phi(X)$ of degree $k \leqslant p^n$ (with coefficients in F) and defined an associated mapping $\phi: F \to F$, $\xi \mapsto \phi(\xi)$ to be a *substitution quantic* $SQ[k;\, p^n]$ of degree k on p^n letters, provided it was bijective. The first part of his dissertation then aimed at a "complete determination of all quantics up to as high a degree as practicable which are suitable to represent substitutions on p^n letters" [Dickson, 1897, 66 or 652], although complete results were given only for degrees $k < 7$ with partial results given for degrees 7 and 11 [Parshall and Rowe, 1994, 379].

The second part of the thesis took up the general linear group $GL_m(F)$, where, as in part one, $F = GF[p^n]$. Jordan had already studied these groups for the finite fields $F = GF[p]$ and m arbitrary in his *Traité*, but, in the spirit of Moore's move from singly to doubly infinite finite simple groups, Dickson sought to generalize Jordan's structural work to fields with p^n elements [Dickson, 1897, 67 or 653].[6] Dickson established that his $GL_m(F)$ was a group, calculated its order, and explored its composition se-

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[2] Note that Moore's group of order 360 is the doubly infinite group with $p = 3$, $n = 2$, and $\delta = 2$, while Cole's group of order 504 is the doubly infinite group with $p = 2$, $n = 3$, and $\delta = 1$.

[3] Here, Moore used the traditional definition of a Galois field: given an indeterminate X, take an irreducible monic polynomial $f(X) \in \mathbb{Z}_p[X]$ of degree n over the prime field $\mathbb{Z}_p = \mathbb{Z}/p\mathbb{Z}$. Then the Galois field $GF[p^n]$ is the collection of p^n equivalence classes of $\mathbb{Z}_p[X]/(f(X))$. Compare Parshall and Rowe [1994, 378].

[4] On the foundational work that issued from Chicago, in particular, and from the United States, in general, see Parshall [2003].

[5] The other Ph.D. that year went to Bolza's student, John Irwin Hutchinson, for a thesis in elliptic function theory. Hutchinson followed his Chicago Ph.D. with a job at Cornell where he remained until his death in 1935. On Dickson's early work at Chicago, compare Parshall and Rowe [1994, 379–381].

[6] Moore had dealt with the case n arbitrary but finite and $m = 2$.

ries. This led him to one of the main results in his dissertation, namely, if Z denotes the center of $SL_m(F)$, then $SL_m(F)/Z$ is simple provided $(m, n, p) \neq (2, 1, 2)$ or $(2, 1, 3)$. "Dickson's theorem thus generalized his adviser's research of 1893 to *triply-infinite* systems of simple groups (in the three parameters m, n, and p)," at the same time that it exploited Moore's structural approach to algebraic questions [Parshall and Rowe, 1994, 380–381]. Moreover, Dickson's analysis also uncovered a previously unknown class of finite simple groups, the groups $SL_m(F)/Z$ for $m \geqslant 3$ and $n > 1$ [Dickson, 1897, 128–138 or 714–724].

Dickson followed his doctorate with a year-long foreign study tour that took him first to Leipzig, where Sophus Lie was lecturing on his formidable theory of transformation groups, and then to Paris and the grand master, Camille Jordan. Following teaching positions at the University of California, Berkeley and at the University of Texas, Dickson returned to Chicago as an Assistant Professor in 1900. The following year, an expanded version of his doctoral dissertation appeared as the book, *Linear Groups with an Exposition of the Galois Field Theory*, under the imprint of the distinguished German publishing house of B.G. Teubner Verlag [Dickson, 1901],[7] and Dickson saw his first student successfully through to the Ph.D.[8] In all, Dickson supervised the doctoral work of some 67 students during his 39-year career at Chicago, and this process began just as Moore's active research interests were shifting from algebra to questions of a more foundational nature. In a very real sense, the algebraic mantle at Chicago passed in the early years of the 20th century from Moore to his student and now colleague, Dickson. Moreover, the approach as well as the kinds of mathematical objects Moore had pursued continued to characterize the algebraic work coming out of Chicago.

Algebra at the University of Chicago in the opening decades of the 20th century

The imprint of an emergent Chicago style of algebra may be detected in that first decade of the 20th century not only in Dickson's work but also in the work of at least one notable visitor to the Chicago department. During the 1904–1905 academic year, the young Scot mathematician-in-training, Joseph Henry Maclagan Wedderburn, brought his Carnegie fellowship to the University of Chicago to pursue his algebraic studies. Interestingly, by 1904, and as a result undoubtedly of Moore's successes and of Dickson's auspicious entry onto the mathematical scene with his book on linear groups, Wedderburn chose to follow his study trip to Germany and the Universities of Leipzig and Berlin with a year-long stay in Chicago. In Leipzig, he encountered the work of Friedrich Engel, in Berlin that of Georg Frobenius, and in Chicago that of Moore and Dickson [Parshall, 1983; 1985]. Wedderburn's choices suggest that the algebraic research coming out of Chicago was viewed as state-of-the-art.

The 1904–1905 academic year was another very active one in Chicago. Moore was heavily involved in his new interest in foundational questions and had been working on the problem of determining a suitable set of axioms for a group. Dickson had also been seduced by this foundational work, focusing on sets of axioms for fields and for linear associative algebras, while he continued his researches on linear groups. Wedderburn came into direct contact with both of these mathematicians, and their approach fundamentally influenced his own subsequent mathematical choices and direction [Parshall, 2003].

[7] On this work and its publication history, see Parshall [1991].

[8] Dickson's first student, Thomas Putnam, earned the degree for a dissertation "Concerning the Linear Fractional Group on Three Variables with Coefficients in the Galois Field of Order p^n." He went on to positions at the University of California, Berkeley.

By January of 1905, Wedderburn and Dickson were in a friendly but intense competition to answer the question "is every finite division algebra a field?" Reminiscent of Moore's abstract characterization of finite fields as Galois fields in his 1893 Chicago Congress paper, the question was at the same time structural and concerned with abstract algebraic objects. Although Dickson's work suggests that he thought the answer to the question was "no," Wedderburn had proved the theorem in the affirmative by March of 1905 [Wedderburn, 1905] and, in so doing, had unwittingly embarked on a career devoted to understanding the structure of linear associative algebras and related algebraic objects [Parshall, 1983].

Wedderburn returned to the University of Edinburgh in the summer of 1905 to take up a position as Lecturer in Mathematics and to continue work toward his doctoral degree. The main piece of research that he submitted for that credential in 1908 was his ground-breaking paper of 1907, "On Hypercomplex Numbers" [Wedderburn, 1907]. Wedderburn had completed an early draft of this work while in Chicago in 1904–1905 [Parshall, 1985, 313–314], and it bore the clear imprint of the emerging Chicago style of algebra. Wedderburn's predecessors—mathematicians such as Theodor Molien, Georg Frobenius, and Élie Cartan—had worked with linear associative algebras over algebraically closed fields of characteristic zero, and their techniques had hinged on properties of that underlying field. Wedderburn, however, took a more general approach, working primarily over arbitrary fields, and developed new, substantially field-independent techniques. Moreover, he sought the underlying structure of linear associative algebras, how they were put together, so to speak. Exploiting the elegant technique of decomposing his algebras using idempotent elements, Wedderburn proved, among other results, that if A is a finite-dimensional algebra over a field F, then:

- if A is simple, it can be expressed as the tensor product of a division algebra and a full matrix algebra over F [Wedderburn, 1907, 99];
- if A is semisimple, then it is the direct sum of simple algebras [Wedderburn, 1907, 99]; and
- the so-called Wedderburn Principal Theorem, namely, if N is the maximal nilpotent ideal (or *radical*) of A, then A contains a subalgebra B isomorphic to A/N, provided F is a field of characteristic zero [Wedderburn, 1907, 105].[9]

As he acknowledged in the printed version of his 1907 paper, "the greater part of Sections 1, 2, 4–6 was read in the Mathematical Seminar of the University of Chicago early in 1905, and owe much to Professor Moore's helpful criticism" [Wedderburn, 1907, 78].

Meanwhile, back in Chicago, Wedderburn's contemporary, Dickson, continued to produce voluminously on the theory of both linear groups and algebras. By 1914, he had paused briefly to take stock of what had been the rapid development of the theory of algebras, publishing a terse, 73-page book entitled *Linear Algebras* [Dickson, 1914]. There, he aimed to bring together some of the key results in the field, but, as one of his biographers noted, it was more than somewhat ironic that Dickson "presented the Cartan theory of linear associative algebras rather than the Wedderburn theory," although he "stated the results of the latter theory in his closing chapter without proofs" [Albert, 1955, 333]. The very messy Cartan theory of the late 1890s concerned itself with the structure of the entire algebra—including its nilpotent part—whereas the very elegant theory Wedderburn had developed hinged on factoring out that

[9] Wedderburn did not give the Principal Theorem in this generality in Wedderburn [1907]. In particular, he considered only the special case where A/N is a division algebra, and he made no explicit restrictions on the characteristic of the underlying field F.

aberrant nilpotent part and concentrating on the well-behaved semisimple part [Parshall, 1985, 335]. Why Dickson made this choice is unclear, but he would rectify this tactical error some nine years later. In the meantime, the 1910s found him consumed by number theory, and he published his massive three-volume *History of the Theory of Numbers* between 1919 and 1923 [Dickson, 1919–1923]. In some sense, his interests in algebras and in number theory coalesced in another of his ground-breaking works, the 1923 book on the *Arithmetics of Algebras* [Dickson, 1923], which came out in a greatly expanded German-language edition in 1927 [Dickson, 1927]. This treatise—unlike the 1914 tract on linear algebras—not only highlighted the Wedderburn structure theory but also solidified Dickson's international reputation [Fenster, 1998].

The third generation of Chicago-connected algebraists

As Della Fenster has shown in her extensive studies both of Dickson's mathematics and of his mathematical persona, Dickson was a highly effective, if idiosyncratic, role model for budding mathematicians [Fenster, 1997]. He imparted to them his sense not only of what areas merited attention—primarily the theory of algebras and later the related theories of rings and division rings—but also of the kinds of questions that should be asked—primarily structural questions aimed at understanding the objects' internal organization and construction. Moreover, he presented to them, through his own personal example, an image of the driven researcher guided by the highest possible standards.

Among Dickson's students in algebra (as opposed to number theory), Olive C. Hazlett earned her doctorate in 1915 for a classification of all (not necessarily associative) nilpotent algebras with four or fewer basis elements over $\mathbb{C}$ that drew directly from Wedderburn's 1907 work [Fenster, 1994, 174]. Hazlett, who eventually secured a position at the University of Illinois in 1925, represented an interesting feature of Dickson's training of future researchers, namely, the encouragement of women in research-level mathematics. In fact, 18 of his 67 Ph.D. students were women, making Dickson personally responsible for slightly more than 8% of all women Ph.D.s in mathematics in the United States between 1900 and 1940 [Fenster, 1994, 166]. Although Hazlett did not find herself in a position conducive to the training of future researchers, she did continue successfully with her own research, lecturing on her new results on the arithmetic of a general associative algebra at the International Congress of Mathematicians in Toronto in 1924 [Hazlett, 1928] and publishing some 17 papers despite a career plagued by mental breakdowns [Fenster, 1994, 179].

Another Dickson student, C.C. MacDuffee, finished his Ph.D. in 1921 and immediately went to Princeton, where Wedderburn was continuing his work on the theory of algebras and engaging in research and teaching on the theory of matrices. By 1924, MacDuffee had moved on to the Ohio State University and nine years later had authored the widely read textbook, *The Theory of Matrices* [MacDuffee, 1933], which presented that theory as it had developed in the hands of both the Americans and the Europeans. Wedderburn himself followed one year later with his own *Lectures on Matrices* [Wedderburn, 1934]. MacDuffee continued his efforts at codifying the "new" algebra after his move to the University of Wisconsin in 1935. In 1940, he published *An Introduction to Abstract Algebra*, aimed at beginning graduate students in American classrooms. In his preface, he explicitly linked his undertaking to the philosophy he had imbued at Chicago. "The phenomenal development in algebra which has occurred in recent years," he wrote, "has been largely the result of a changed point of view toward the subject, the displacement of formalism by generalization and abstraction. The maxim so often emphasized by the late E.H. Moore that the existence of parallel theories indicates an underlying unifying theory has been thoroughly vindicated in modern algebra. Number theory, group theory, and formal algebra have been unified and abstracted to produce what is now known as abstract algebra" [MacDuffee, 1940, v]. In presenting that abstract algebra to his American audience, MacDuffee moved from finite groups to rings and fields to matrices before bringing his book to a triumphal close with one of the main achievements of his mentors, Dickson and Wedderburn, the theory of linear associative algebras [MacDuffee, 1940, 251–296].

The most famous third-generation student directed by Dickson, however, was A. Adrian Albert. Albert earned his master's degree in 1927 for a thesis in which he showed that any central division algebra of dimension 16 over its base field (of characteristic zero) is a crossed product algebra.[10] He went on the next year to earn his Ph.D. for more work on division algebras. As Della Fenster noted in her doctoral thesis, Albert's early research "had its origins in the work of both Dickson and Wedderburn" [Fenster, 1994, 185]—in the case of Dickson, work presented in the 1906 paper in which he defined the concept of a cyclic algebra [Dickson, 1913], and, in the case of Wedderburn, the 1907 paper "On Hypercomplex Numbers" [Wedderburn, 1907], as well as later work in 1921 on cyclic algebras per se [Wedderburn, 1921]. This intellectual lineage—as well as the fact that Albert immediately followed his doctoral work with a year-long stay in Princeton "attracted by that great master of associative algebra theory," Wedderburn [Jacobson, 1974, 1076]—further exemplifies the mathematical and intellectual continuity of Chicago's program in algebra.

The questions Albert examined were timely. In the structure theorems he presented in his 1907 paper, Wedderburn had effectively shown that the study of finite-dimensional semisimple algebras reduces to that of division algebras. Thus, the search for division algebras and, in general, the classification of them became a focal point of the new theory of algebras. As early as 1905 in his competition with Wedderburn over the finite division algebra theorem, Dickson had been interested in division algebras, and this interest only intensified in light of Wedderburn's revolutionary structural results. In 1906, Dickson had defined a new class of algebras, so-called *cyclic* algebras, which have dimension n^2 over the base field F [Dickson, 1914]. These contain a maximal subfield that is cyclic over F; that is, the maximal subfield is a Galois field with cyclic Galois group G of order n. Moreover, Dickson noted that the class of cyclic algebras contained division algebras. In 1914, Wedderburn established a critical sufficient condition for a cyclic algebra to be a division algebra [Wedderburn, 1914],[11] and by 1921, he had extended this work to *central* division algebras, that is, division algebras with center equal to the base field [Wedderburn, 1921]. Wedderburn showed that every central division algebra of dimension 9 over the base field is cyclic, and he proved that Dickson's cyclic algebras were actually special cases of what would come to be called Abelian crossed products. Dickson then showed in 1926 that Abelian crossed products could be generalized even further to crossed products based on any (that is, not necessarily Abelian) Galois field extension and, in so doing, generated yet another new class of division algebras [Dickson, 1926]. Albert's result thus extended Wedderburn's 1921 theorem to the next case, dimension 16 [Jacobson, 1974, 1078–1079].

[10] See [Albert, 1929]. Albert later refined this result. In Albert [1932], he admitted that his original proof was unnecessarily complicated and gave a simpler proof of the result. Finally, in Albert [1934], he proved the result in its full generality, noting first that the result actually holds for any infinite field (characteristic $\neq 2$) and then handling the characteristic 2 case.

[11] MacDuffee included an exposition of some of this work on cyclic algebras in MacDuffee [1940, 273–277], again taking the opportunity to highlight the work of his mentors, Dickson and Wedderburn.

Just like Dickson's student, Albert, one of Wedderburn's students, Nathan Jacobson, also became caught up in the quest to understand cyclic and crossed product algebras. In 1909, Wedderburn had left his native Scotland to spend the rest of his academic career at Princeton University, where, as noted above, he continued the work on the theory of algebras he had begun at Chicago and where he embarked on related work in matrix theory. Although circumstances ultimately resulted in his having only three doctoral students, one of those, Jacobson, not only continued in his adviser's mathematical footsteps but also completed, in a very real sense, the structure theory that Wedderburn had been so instrumental in establishing [Parshall, 1992].

Jacobson earned his Princeton Ph.D. in 1934, six years after Albert, for a thesis on "Non-commutative Polynomials and Cyclic Algebras" [Jacobson, 1934]. Wedderburn had suggested the topic to him, motivated by the question "Do there exist non-crossed product central division algebras?" [Jacobson, 1989a, 1:2]. Although his research did not yield an answer to this original question, he did come up with some new results on cyclic algebras.

It was also during the course of his doctoral studies at Princeton that Jacobson became aware of Wedderburn's 1924 paper on "Algebras Which Do Not Possess a Finite Basis" [Wedderburn, 1924], a paper which, according to Jacobson, "was one of those that inspired my later work on the structure theory of rings" [Jacobson, 1989b, 2]. That later work was also informed generally by Emmy Noether's ring-theoretic researches of the 1920s and 1930s and more particularly by Emil Artin's extension in 1927 of Wedderburn's structure theory to rings satisfying the descending chain condition for right ideals [Artin, 1927]. In a series of papers in 1945, Jacobson succeeded in taking this further by laying the groundwork for a structure theory of rings without finiteness conditions [Jacobson, 1945a, 1945b, 1945c, 1945d]. (Recall that in his 1907 paper, Wedderburn always worked with algebras that were finite-dimensional over their base field.) In particular, Jacobson defined what came to be called the Jacobson radical of a ring, a structure in an arbitrary ring in some sense analogous to the maximal nilpotent ideal Wedderburn had worked with in a finite-dimensional algebra. I.N. Herstein, a later Chicago ring theorist fundamentally influenced by Wedderburn's research and his approach to the theory of algebras, described the import of the Jacobson radical with characteristic clarity. "In order to study a general ring," he wrote, "we want to slice out of the ring a certain piece—the so-called radical—in such a way that we do not slice out too much, so that the piece being cut away is capable of description yet at the same time we do not want to cut out too little, so that the object resulting after the excision is also capable of description" [Herstein, 1968, 9]. Jacobson's analysis of his radical thus did for the new structure theory of rings what Wedderburn's isolation of the maximal nilpotent ideal did for the structure theory of algebras [Jacobson, 1945b]. Jacobson provided a complete exposition of these and other results in 1956 in his highly influential book, *Structure of Rings* [Jacobson, 1956].

Jacobson ultimately transplanted to Yale University the brand of algebraic inquiry that his adviser, Wedderburn, had imbued at Chicago, although, as noted, in a form enhanced further by the ideas of Emmy Noether. Many of his students, the fourth generation, worked on the theories of various kinds of algebras—both associative and nonassociative—and in ring theory and went on to do influential work.[12]

As for Albert, he followed his first postdoctoral year with Wedderburn in Princeton by two years as an Instructor at Columbia. He then returned to a position at Chicago in 1931 and remained there for the rest of his life. His work continued to center on the theory of algebras. In 1931, he came within a

[12] See Jacobson [1989a, 1:xi] for the complete list of Jacobson's students and their dissertation topics.

hair's breadth of winning the biggest prize in the field [Albert, 1931], namely, determining the complete classification of all rational division algebras, that is, division algebras D such that the center F of D is a finite (Galois) extension of $\mathbb{Q}$. Yet another classification theorem, it had also captured the interest of the German mathematicians, Richard Brauer, Helmut Hasse, and Emmy Noether, and they just edged Albert out of the result.[13] Although stung by this incident, Albert went on to publish his influential treatise, *Structure of Algebras* [Albert, 1939], before his interests—like those of his contemporary, Jacobson—moved from associative to nonassociative algebras. Still, Albert remained in some sense obsessed for the rest of his career with the crossed product algebras he had studied in his earliest work, convinced that, in fact, *every* central division algebra is a crossed product algebra. He was ultimately unable to prove this, and in 1972, just months before his death, the Israeli mathematician, Shimshon Amitsur, found a counterexample [Amitsur, 1972].

Like Jacobson at Yale, Albert conveyed, during a forty-year career at Chicago, his brand of algebra to a number of students who perpetuated and developed it.[14] Spread from coast to coast, these fourth-generation students from the 1940s and early 1950s continued their research primarily in ring theory and in the theory of nonassociative algebras, wrote textbooks, and trained their own students well into the 1990s.

Defining a *mathematical* research school

This overview of Chicago-connected algebraic results now raises the question, was there something that could properly be called a Chicago school of algebra? Mathematicians tend to use the term school loosely. One often hears the set of Ph.D. students of mathematician X who happens to be located at institution Y referred to as "X's school" or the "Y school," but exactly what analytic value does this highly informal notion of a school have in trying to assess meaningfully the real intellectual and social connections between mathematicians or the complex development of mathematical theories?

That the word "school," which has often been invoked in the history of mathematics, has been understood in a loose sense is indicated by the pervasive usage of the word in quotation marks. For example, Uta Merzbach referred to "[t]he 'Noether school"' in Merzbach [1983, 168] and understood this to mean "those who collaborated with [Noether] in attempting to make algebra the tool and foundation of all of mathematics ... during the last decade of her life." Michael Scanlon used "standards of work and approaches ... developed with the American community of research mathematicians" to "identify an American 'school' of foundational studies in at least the period 1900–1930" [Scanlon, 1991, 982]. Other instances of schools—in an ill-, un-, or underdefined, intuitive sense—in the historical literature on mathematics include the Peano school [Kennedy, 1980, 84–89 and 187], the Warsaw school [Duda, 1996], and numerous examples throughout [Grattan-Guinness, 1994, 2; 1791–1792], where the term is used to indicate everything from simply "the students of a mentor" to "the students of a mentor who shared a common approach" to "those limited geographically who came to share a common approach" to "those who work within a certain tradition" to .... Grattan-Guinness did write briefly but explicitly

[13] See Albert/Hasse [1932] for an account of how the result followed quickly from work Albert had communicated to Hasse. Unfortunately, Albert's letter initially went astray, and Brauer, Hasse, and Noether published their independent work [Brauer/Hasse/Noether, 1932], thus making no mention of Albert's results.

[14] See Block et al. [1993, 1:xxxvii–xxxviii] for the full list of Albert's students and their dissertation topics.

about schools in the mathematical context in Grattan-Guinness [1997, 755–757]. There, he argued that "only occasionally can one point to a *school* in the strict sense, with a leader (not necessarily beloved) and geographical centre, a specified programme of work (maybe not only in mathematics), settled means of diffusing or even publishing, and a strong sense of bonding among its members" [Grattan-Guinness, 1997, 755–756]. This variety of "understood" meanings suggests that some attempt at an actual definition that would not only fit what historians of mathematics think of as a school but also serve to analyze new historical contexts might be warranted.[15]

Historians of science also use the word school, but, for them, a school is almost exclusively something associated with the laboratory sciences, so mathematics falls outside their purview. Still, historians of science—largely unlike writers on the history of mathematics—have at least tried to provide a definition of school as an analytical construct for evaluating and understanding the past. A consideration of some of their definitions sheds light on how these definitions might be adapted to the mathematical context.

In what has become a classic study in the history of science, the British historian of science, J.B. Morrell, considered the notion of *research schools* in his 1972 analysis of the 19th-century chemists Justus Liebig and Thomas Thomson [Morrell, 1972]. In Morrell's words, the concept of a research school "centred on laboratories in which ambitious disciples devotedly served an apprenticeship and afterwards produced knowledge under the aegis of a revered master of research" [Morrell, 1972, 1]. This conception was thus clearly shaped by the image of the crowded laboratory in which students and professor worked shoulder-to-shoulder on some experiment or program of experiments conceived of and orchestrated by the seemingly all-knowing director. Morrell then proceeded to lay out seven criteria for a research school. First, there had to be a leader who guided a program that was too big for him to deal with alone. Second, there had to be manpower "for the creation, maintenance, and expansion of a research group"; that is, "there had to be a regular supply of motivated students who were keen to apprentice themselves to a recognized or emerging master of his subject" [Morrell, 1972, 4]. Third, the area of inquiry needed to be such that "a set of relatively simple, fast, and reliable experimental techniques could be steadily applied by both brilliant and ordinary students to the solution of significant problems" and in so doing generate a body of knowledge that in some sense became the "property" of the group [Morrell, 1972, 5]. Fourth, there had to be publications "to convert private work into public knowledge and fame" [Morrell, 1972, 5]. Fifth, the leader had to have sufficient institutional power to ensure that his research goals could be realized. Sixth, the leader also needed to be charismatic in order to attract sufficient numbers of disciples. And finally, seventh, the leader required sufficient institutional support to assure that the laboratory could run from day to day and year to year [Morrell, 1972, 6–7]. Clearly geared toward types of science that *require* significant space, relatively large numbers of collaborators, material infrastructure other than blackboards, chalk, and books, and a fixed physical location, Morrell's seven criteria for a research school do not apply particularly well to the case of mathematics.

Another historian of science, Gerald Geison, drew from Morrell's work at the same time that he was guided by his own research on the 19th-century Cambridge physiologist, Michael Foster, in coming up with another definition of research school. In his 1981 article entitled "Scientific Change, Emerging Specialties, and Research Schools," Geison defined a research school as "a small group of mature scientists pursuing a reasonably coherent programme of research side-by-side with advanced students in the same institutional context and engaging in direct, continuous social and intellectual interaction"

[15] This, of course, is a different issue from understanding how historical actors, who actually employed the word school in a particular historical context, conceived of the term. Compare Albert Lewis's article in the present issue of *Historia Mathematica*.

[Geison, 1981, 23]. For Geison, then, even more so perhaps than for Morrell, the interaction of individuals in close physical proximity was critical to the existence of a research school. He did offer at least one caveat to his conception of research school as a unit of historical analysis, however. He noted that "it might be necessary to acknowledge the existence of spatially dispersed research schools, or at least to recognize and take account of the extent to which the members of a research school may extend its geographic scope by moving elsewhere" [Geison, 1981, 35]. The precise definition aside, Geison argued that as an analytical tool in the history of science, the notion of a research school has the potential to "enrich our understanding of emerging specialties" and to "refine our efforts to specify the conditions under which innovative science is most likely to be done" [Geison, 1981, 36].

While historians of science have continued to debate and refine the concept of the research school,[16] very little, if any, of this discussion and debate has focused on examples presented by mathematics. This then raises the question, what might an appropriate definition for a *mathematical* research school look like?

First of all, while mathematics has a critical sociological component, it lends itself much more naturally and easily than do the experimental sciences to the individual investigator or to small groups of two or three investigators in collaboration.[17] It is not done in the context of the expensive infrastructure of the laboratory; it does not require the interaction of individuals *in close physical proximity*, central to Geison's definition of a research school; it is linked less by geography and more by the interaction of individuals through ideas.

Still, mathematics does share certain characteristics with the laboratory sciences. This interaction of individuals through ideas centers on means of communication, which are key to the experimental sciences as well. Mathematicians, like experimentalists, communicate informally at the blackboard, by letter, through attendance at meetings, and, in the modern era, by telephone, fax, e-mail, and the Internet. They also communicate with each other formally through publications, which establish not only priority but also reputation. Journals and books serve as the permanent record of the ever-evolving body of mathematical knowledge, while textbooks, in particular, establish priority of place for knowledge that should be common knowledge.

Mathematics also resembles the experimental sciences in that it is learned through a kind of apprenticeship; the graduate student, working in association with an adviser both inside and outside the graduate classroom, is generally guided into a mathematical area and toward a particular problem or set of problems. Through this apprenticeship, the student learns not only an explicit body of knowledge from the adviser in a particular pedagogic context but also a set of values and other intangibles that go on to guide the student's choices of mathematical problems and areas as well as to shape the student's sense of mathematical taste. The absorption of these sorts of intangibles—"tacit knowledge," to use Michael Polanyi's phrase [Polanyi, 1958]—represents just as key an aspect of the mathematical apprenticeship as it does of the laboratory apprenticeship with its acquired sense, for example, of bench craftsmanship.[18] Unlike in the laboratory context, however, the problem or set of problems tackled by the student may not

[16] See, for example, the essays in Geison and Holmes [1993]. John Servos opened this volume with an introduction to "Research Schools and Their Histories" [Servos, 1993]. The notes in his chapter, as well as in the others in the volume, provide a good overview of the literature on the concept of the research school. As Servos remarks, Fruton [1990] also has a valuable bibliography containing literature on the topic.

[17] A notable example of a large and concerted group effort in mathematics, however, was (and is) the classification of the finite simple groups. This, however, seems to be the exception rather than the rule in mathematics.

have any direct bearing on some immediate and focused research problem of the adviser. In mathematics, perhaps more so than in the experimental sciences at least as Morrell characterized them, the student is less a skilled helper and more an evolving, independent researcher. The student most often leaves the adviser's institutional context to pursue research elsewhere and, in so doing, may transplant mathematical ideas and values.

Putting together these characteristics of mathematics and its practice thus suggests the following components of at least a first approximation of a meaningful definition of a *mathematical* research school. First, a mathematical research school initially requires a leader, who actively pursues research in a particular area of mathematics. That leader may be charismatic, that is, s/he may have a personal magic that arouses loyalty or enthusiasm like Morrell's laboratory directors, but this would not seem to be a necessary condition for a leader of a mathematical research school. Rather, the "magic" of the latter leader's mathematical work and ideas may be the more critical factor. Second, that leader advocates a fundamental idea or approach to some set of inherently related research interests or research interests that become related by virtue of the idea or approach. Third, the leader trains students and, in so doing, imbues them with a sense not only of the validity and fruitfulness of the approach but also of the "right" way to go about asking and answering questions; explicit and tacit knowledge are conveyed through the education process. Those students then go forth and pursue research according to that approach so that the ideas and approach may naturally extend beyond the leader's original institutional setting. In this process, the original leader may pass from the scene but may be replaced by another like-minded leader or leaders who train students appreciative of and actively engaged in research informed by the approach, and so on. The passing from the scene of the original leader and/or the multiplication of geographical loci of instruction may mark, moreover, the transition from a mathematical research school to a mathematical specialty or subdiscipline.[19] Fourth, the publication of the research not only represents recognition of the research done but also comes to reflect the external validation of the approach. This external validation may result in the extension of the ideas and approach by other researchers nationally and internationally. According to this definition, the mathematical research school is thus a vehicle for the formation of new research specialties and, hence, is an analytical tool for understanding at least one way that mathematics develops over time.

There are a few things to note about this proposed definition, however. First, it differs from the naïve notion of "X's school" as "the Ph.D. students of X" by requiring four specific criteria to be met. In other words, what might be considered the space of mathematical research schools is four-dimensional.[20] There must be a leader and students (as in the naïve notion), but the leader and the students also need actively to embrace and extend a common method or approach. In the naïve sense of school, simply being a student of X does not necessarily mean having a sense of any common approach that should be pushed; X may never have had this sense to impart; and "students" of X may not have actually been Ph.D. students of X. Without the sense of common approach, moreover, the criterion of external validation of an approach

18 Compare Olesko [1993] on the role of pedagogy in imparting both tacit and explict knowledge and thus in the process of school formation.

19 The present definition concerns primarily the becoming and the being of a mathematical research school. Interesting questions for further thought and consideration are: What constitutes the end of a mathematical research school? Given a particular mathematical research school how does, or does, it evolve into a mathematical specialty or subdiscipline?

20 This definition is intended as a first approximation. Subsequent scholars may feel the need to add to or further refine the criteria specified here.

via publication also fails to be met.[21] Second, by proposing a definition for *mathematical research school* here, the intention is not merely to enlarge or narrow the size of the space of "mathematical research schools" relative to previous conceptions—whether explicit or implicit—of the phrase, but rather to specify what seem to be four natural and critical analytical dimensions in the hope of establishing a basis for common historical and historiographical interpretation. With this understanding of the definition of a mathematical research school, consider now the case of algebra at the University of Chicago from 1892 to roughly 1945.

A Chicago school of algebra?

Chicago had a recognized leader in E.H. Moore, an energetic although not particularly charismatic person, who was interested in the 1890s in what soon became hot, algebraic ideas stemming from the theory of finite simple groups and who approached his research from an enticing, structural—and after 1901, axiomatic—point of view. Moore supervised the doctoral research of Leonard Dickson, who took up not only Moore's general area of research but also his sense of what questions to ask. Moore and Dickson both fundamentally influenced the mathematical approach and the area of interest of the visiting Scot graduate student, Joseph H.M. Wedderburn. Wedderburn at Princeton and Dickson at Chicago continued to pursue research questions about the structure of algebras and later of related objects, division algebras, while Moore moved out of algebra and into function theory. Although neither the "hard-bitten" Dickson[22] nor the withdrawn and solitary Wedderburn was particularly charismatic, both did research recognized as exciting and seminal. They disseminated this work widely in journals such as the *Annals of Mathematics* and the *Transactions of the American Mathematical Society* in addition to foreign journals such as the *Journal of the London Mathematical Society* and Crelle's *Journal für die reine und angewandte Mathematik*, and gained for their ideas recognition both at home and abroad.

In Germany, for example, Helmut Hasse explicitly recognized these concerted American efforts in the theory of algebras. Writing in English in the *Transactions of the American Mathematical Society* in a paper dated in 1931, he acknowledged that "[t]he theory of linear algebras has been greatly extended through the work of American mathematicians" [Hasse, 1932, 171]. He went on to note, however, that "[o]f late, German mathematicians have become active in this theory" and suggested that the German "results do not seem to be as well known in America as they should be on account of their importance" [Hasse, 1932, 171].[23] From his vantage point in Germany, then, Hasse saw in the United States of the late 1920s and early 1930s a research dynamic involving a group of mathematicians actively exchanging and building results in a particular area of interest, and many of these mathematicians, such as Dickson,

[21] Consider, for example, the "Noether school" mentioned above. It *is* a school by the definition proposed here, but not by the naïve definition. People in the Noether school were part of Noether's circle rather than those who earned their Ph.D.'s under her, but Noether was a leader with convinced followers. Together, they pursued a common approach, and through their publications, others came to recognize theirs as a new and valuable approach. (It was this latter point that went underanalyzed in Merzbach's characterization.) The "Noether school" is also not a school by Morrell's definition, since Noether never had institutional power and had only marginal institutional support. Relative to mathematics, then, the naïve definition is underdefined, whereas Morrell's definition is overdefined.

[22] Saunders Mac Lane characterized Dickson in this way in an interview with Della Fenster on 5–6 March, 1992. See Fenster [1994, 154].

[23] These passages are from the introduction to Hasse's paper, which is quoted at greater length in Curtis [1999, 232].

Wedderburn, and Albert (to whose work Hasse's paper was largely addressed), had deep connections to Chicago.[24]

American mathematicians-to-be, students desirous of advanced training in algebra, recognized this as well. They were thus attracted to the classrooms of Dickson, Wedderburn, and, eventually, Albert and Jacobson and were brought to the research level in the theory of algebras—both associative and nonassociative—as well as in the theory of division algebras and in ring theory. Nor were these Americans and their students deaf to Hasse's cautions against insularity, cautions reflective of a sense of the existence of an overly intercommunicating mathematical research school. They freely incorporated into their approach the latest ideas of mathematicians such as Emmy Noether and Emil Artin and passed that evolving approach on to their students, who dispersed throughout the country. The result was new and recognized research specializations in ring theory and the theory of nonassociative algebras characterized by the quest for the objects' underlying structure.

At this point, three things seem clear. First, the definitions of "research school" to be found in the literature on the history of science are inadequate for mathematics. Second, it seems possible to define the concept in the mathematical context in such a way to provide a useful analytical tool for understanding at least one type of historical development within mathematics, the development of new research specialties. And, third, at least by the definition proposed here, there *was* a Chicago school of algebra between 1892 and 1945.

Acknowledgments

The notion presented here of a "mathematical research school" was largely developed in the History of Mathematics Seminar at the University of Virginia during the course of our discussions and readings in the spring of 2002. I thank my students, Laura Martini and Deborah Kent, for their insights and criticisms. These ideas were first presented publicly at the joint meeting of the Italian Mathematical Union and the American Mathematical Society in Pisa, Italy in June 2002, then again at a conference at the Open University in Milton Keynes, England in September 2002, and, finally, at the Workshop in the History of Nineteenth- and Twentieth-Century Algebra held at the Mathematical Sciences Research Institute in Berkeley in April 2003. This paper has benefited particularly from the extended and lively discussions at the latter meeting and from private conversations with Ivor Grattan-Guinness and Albert Lewis.

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[24] The Italians also recognized and responded to the work coming from these and other Americans. In particular, in 1921, the Pisa-trained Gaetano Scorza published his massive treatise, *Corpi numerici e algebre*, in direct response to Dickson's 1914 tract on linear algebras [Scorza, 1921]. Scorza recognized Dickson's tactical error in presenting in 1914 the old-fashioned work of Cartan in detail rather than the trendsetting research of Wedderburn. Other Italian mathematicians, such as Franceso Cecioni, engaged in precisely the same algebraic questions as, for example, Dickson and Albert. See Cecioni [1923] where Cecioni independently hit upon the class of Abelian crossed product algebras. This resulted in extended research contact between the Italians and the Americans. Laura Martini explores the contexts of the algebraic work of Scorza, Cecioni, and others in her article in the present issue of *Historia Mathematica*.

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