The Beginnings of the R. L. Moore School of Topology

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Abstract

The roots of the R. L. Moore school of point set topology were formed around 1900 with Moore's most direct influences coming from his University of Texas teacher G. B. Halsted and from his graduate school teachers at the University of Chicago, mainly O. Veblen and E. H. Moore. It was recognized as a school by the 1930s as Moore and his students achieved recognition for their work and as this group interacted with the Polish school of topology. In addition to the mathematical subject, the other main factor that helped to identify this group as a school was the distinctive method of teaching Moore used to introduce the subject to students.

In 1938 when R. L. Moore was president of the American Mathematical Society, the official fiftieth-anniversary historian, R. C. Archibald, surmised that Moore "may almost be thought of as the founder of a 'school' in analysis situs" (1938, 241). In their classic 1935 compendium *Topologie*, Alexandroff and Hopf, in describing the study of locally-connected continua, stated that "In den Händen der polnischen (Mazurkiewicz, Sierpinski, Kuratowski, Zarankiewicz u.a.) sowie der amerikanischen Schule von R. L. Moore (Moore, Kline, Ayres, Gehman, Whyburn, Wilder u. a.) hat sich eine umfangreiche Theorie entwickelt" (In the hands of the Polish school (Mazurkiewicz, Sierpinski, Kuratowski, Zarankiewicz, et al.) as well as of the American school of R. L. Moore (Moore, Kline, Ayres, Gehman, Whyburn, Wilder, et al.) it has developed into an extensive theory.) (Alexandroff and Hopf, 1935 19). The designation "school," though it is not freely applied by mathematicians or historians, nevertheless may not be possible to define very precisely. This paper may contribute to an historiographical understanding of the notion of a mathematical school in general but it tries to determine why mathematicians, who likely did not have a well-formulated definition of the term, nevertheless readily applied it in this particular case. Looking back we can try to delineate the origins and describe something of the nature of those factors which at least came to characterize Moore's school if not to instigate it. It is my hypothesis that the key formative factors are Moore's axiomatic approach to point set topology and his method of teaching. These factors will be a pervasive but underlying theme in the following account. An overview of Moore's mathematical development is followed by a look at how the school could be defined from an internal point of view (its subject and membership) and from an external viewpoint.

1 Stages of development

1.1 Texas, 1898–1901

As a teenager Moore (1882-1974) attended a school of high quality for its time and place in 1890s Dallas, Texas.[1] Though he was far from self-taught he did develop a high degree of skill at independent learning, especially in mathematics. In his only autobiographical account, a film about his teaching method, Moore described how, before entering the University of Texas at Austin in 1898, he worked through a calculus textbook by covering up the proofs or worked examples and only revealing them line by line if he could not work them out himself (Moore, R. L., 1965).

Mathematics at the university was under the charge of George Bruce Halsted (1853–1922). Most of Moore's mathematics courses were taught by Halsted but, according to grade reports, a couple of his classes, advanced calculus (elliptic integrals, gamma functions, Fourier series) and group theory, were taught by an instructor who had been a Halsted student, L. E. Dickson. Halsted made the first translations into English of the non-Euclidean geometries of Bolyai and Lobachevskii, and was later to produce an edition of Saccheri. He received his undergraduate training at Princeton and went on to work with J. J. Sylvester at Johns Hopkins University. On the Austin campus he was the most colorful and memorable character according to later student memoirs. His colleagues, especially those who felt he had a knack for being too forthright in his criticism of them and of the university governing board, were probably not surprised when he was finally dismissed from the university in 1902. No one, however, seems to have doubted his expertise in his area of mathematics or his effectiveness as a teacher (Lewis, 1976).

In the last months before he left Texas for a succession of other universities elsewhere, Halsted achieved what was probably his single most satisfying teaching accomplishment that gave R. L. Moore an impressive start on his own career. Soon after Hilbert's *Grundlagen der Geometrie* appeared in 1899 Halsted, though he did not work on the research frontier that Hilbert's work represented, began to introduce it into his teaching. In particular it prompted him to propose a question to Moore regarding the possible dependence of one of Hilbert's axioms of order (or betweenness as they were called). Halsted did not disclose to Moore that he, Halsted, had learned of this axiomatic refinement from a recent paper by the renowned Chicago mathematician E. H. Moore. R. L. Moore succeeded in proving the dependence and in a sufficiently original way for Halsted to publish it in the *American Mathematical Monthly*, with full credit to his student, and for E. H. Moore to acknowledge its improvement over his own method of proof. The latter was, however, not so pleased by what appeared to be Halsted's lack of public acknowledgement of the Chicago Moore's role in this and the apparent failure of Halsted to inform the Texas Moore of the earlier published result.[2]

R. L. Moore always counted this as his first publication, but, at the same time, because it appeared under Halsted's name, he seems to have found it a slight embarrassment at the beginning of his career to have to explain that the result was indeed wholly his. The entire experience, even the negative one of not having the publication in his name, presages three main features of

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[1] Descriptions of this school and of Moore's teacher there can be found in the most detailed biography to date (Traylor, 1972). A new biography, drawing on archival sources that were unavailable in 1972, is in preparation by John Parker.

[2] To help set the record straight, E. H. Moore gave a summary of the tripartite nature of this episode, citing all of the relevant papers, in (Moore, 1902).

the Moore method of teaching: a focus on the axiomatic aspect of a geometrical subject, an encouragement of students to discover results on their own even if the results are already known and in the literature, and an emphasis on independent work at all levels that can be unambiguously credited to one individual.

From a purely mathematical research point of view there are two significant gaps in Moore's early career that bound his graduate school years at the University of Chicago. The first occurred as he made plans to attend the university: in spite of his early connection with Chicago and E. H. Moore, it seems that the lack of an available fellowship combined with miscommunication or bad timing resulted in at least a year's delay. During this time Moore taught at a high school in Marshall, Texas. In a way this could hardly be called a substantial setback considering that in 1901 when he graduated from Texas he was nineteen years old, a year or so younger than L. E. Dickson and O. Veblen, for example, when they had earlier arrived at Chicago. Also, there is no indication that he ever said anything negative about this teaching experience however unwanted it may have been.

1.2 Chicago, 1903–1905

When Moore arrived at Chicago in 1903 he was in the ideal place for building upon the sort of topic Halsted had initiated with him. E. H. Moore, the chair of the department, probably had high expectations of R. L., not only because of the preview he had through the latter's work on the betweenness axioms, but also because up to that point his most thoroughly prepared student at Chicago had been L. E. Dickson who had come from Texas in 1894 and Halsted declared Moore to be at least the equal of Dickson.

At Chicago he benefited from the best that the United States had to offer in the way of a mathematical research environment. It was an environment that compared favorably to that offered in Europe to such an extent that it was no longer considered necessary for its graduates to spend a year studying abroad as those in earlier generations—like Halsted, after graduating from Johns Hopkins University—had done (Parshall and Rowe, 1994 Ch. 9). R. L. Moore, in fact, seems *never* to have traveled out of the United States. Though the direction of Moore's early work followed that of Veblen in geometry more than anyone else's at Chicago, the most immediately important new lessons for him were probably in analysis since this was an essential background needed for appreciating some of the just-emerging topics relating to geometry. It was primarily from the study of functional continuity that fundamental geometrical issues were being raised concerning the meaning of dimension and the nature of the continuum. From this new area arose Moore's particular branch of topology, the study of continua. The subject was beyond Halsted's domain but there was one further public interaction between Moore and his mentor on a not unrelated topic which helps to mark the transition from Texas to Chicago and from geometry to topology.

Halsted often made a point of dispensing with assumptions of continuity in any form in his geometrical work—as if this were desirable to do from a pedagogical point of view at least. His elementary textbook of 1904, *Rational Geometry,* tried to present much of Hilbert's *Foundations* in textbook form with a view to presenting the theory of proportion without assuming continuity—hence the "rational" in the title serves a double purpose. Halsted had been in touch with Hilbert and the latter had given his polite blessing to the book which Halsted proceeded to dedicate to him. The author was thus taken aback when, after sending the book to Hilbert, he apparently passed it along to his former assistant, Max Dehn, to handle and the latter gave it a negative review (Dehn, 1904).[3] The twenty-six year old Dehn had by this time achieved fame in the area of the geometrical uses of continuity by his solving of Hilbert's Third Problem. Having received generally favorable reviews otherwise, Halsted called upon his former student to help meet Dehn's "onslaught" by assisting in a revised second edition (Halsted, 1907) and by telling Halsted what he thought of Dehn's points. "Why is the angle-sum-excess in a circle-arc-triangle not proportional to the area?," Halsted asked, "Explain all this to me." (RLM, 12 Dec. 1904) They made changes that at least blunted, if they did not entirely satisfy, Dehn's objections. Halsted also removed the dedication to Hilbert for good measure. (In 1911 this second edition was translated into French and Japanese.) If revenge was sought by Halsted he may have received some satisfaction through Moore's second publication[4] which tacked a rather surprising result onto Dehn's dissertation and where Moore prominently stated "Reference will be made to Halsted's *Rational Geometry* (R. G.) in case of theorems for which demonstrations without use of Hilbert's [axiom-group] III are therein indicated" (Moore, R. L., 1907 370).

R. L. Wilder provided a masterful analysis of the earliest influences on Moore's work (Wilder, 1982). He singled out Moore's axiomatic approach as a distinguishing feature throughout his corpus and pointed to a non-geometrical work that was finished in 1906 while Moore was at Princeton, though probably started while he was at Chicago, and that was devoted to founding a theory of positive integers based on two operations and the undefined term "integer." Moore apparently submitted this to the *Annals of Mathematics,* possibly through Veblen, and also showed it to E. H. Moore. These two, as well as E. V. Huntington, editor of the *Annals,* in effect advised R. L. to learn more logic. Since Moore was proposing to dispense with the usual notion of order in his system, it seemed advisable for him to be up on the recent literature concerning the axiom of choice and the continuum hypothesis. Wilder conjectured that we can see here already Moore's "dislike for such investigations" concerning foundations (Wilder, 1982 75-76), investigations that Wilder himself seems to have enjoyed following. Wilder did not digress to note that the hand of Halsted is once again evident: he had long been planning a textbook in "rational arithmetic" to match his *Rational Geometry,* and had enlisted the aid of his closest student. In this book, a chapter entitled "Arithmetic As Formal Calculus" is described as "essentially a contribution from Dr. R. L. Moore, of the University of Pennsylvania" (Halsted, 1912 101). The indefinite "essentially" is probably sufficient reason why Moore never included this as one of his publications.

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[3] Halsted wrote to Dehn on 12 December 1904 asking several pointed questions in an effort to understand the latter's criticism, and sent a copy of the letter to Hilbert (Cod. Ms. Hil., 129 Beil. 1). Dehn wrote to Hilbert summarizing his problems with Halsted's work and remarking: "Wenn ich mich nicht täusche, haben Sie mal gesagt, dass gerade für die Grundl. d. Geom. sich häufig Leute besonders interessieren, die zu ihrer Erfindung und Durchforschung nicht durchaus geeignet sind" (If I am not mistaken, you once said that frequently it was precisely the *Grundlagen der Geometrie* that especially interested those people who are not thoroughly qualified to deal with its discovery and investigation.) (Cod. Ms. Hil., 67, 26 Dec. 1904). It is interesting to note that Dehn took another geometry book to task the following year over the same general issue of misunderstandings of the application of continuity in geometry. His very negative review, of Theodore Vahlen's *Abstrakte Geometrie: Untersuchungen über die Grundlagen der Euklidischen und Nicht-Euklidischen Geometrie,* probably played a role in Dehn's expulsion from his teaching position when Vahlen came to have substantial influence in such professional matters in 1930s National Socialist Germany (Siegmund-Schultze, 1984).

[4] His second publication not counting problem solutions in the *American Mathematical Monthly* **8**(1901), 196 and **11**(1904), 45.

1.3 Pennsylvania and return to Texas, 1920–1969

After graduating from Chicago Moore held a succession of university positions before settling at Texas: Tennessee, Princeton, Northwestern, and Pennsylvania. In spite of the great promise that his teachers saw in him, there was a period of several years immediately after Chicago which appear to match the pre-Chicago hiatus in his development when he taught high school. It was not until the University of Pennsylvania, where he was from 1911 to 1920, that the signs of what was to come are evident. As we shall see, it was probably at Pennsylvania that Moore constructed the first set of that system of axioms that was to characterize his future work and also where he began to implement his method of teaching.

Moore had long been hoping to return to his home state of Texas and in 1920 he was able to do so (Lewis, 1989). Pennsylvania could well have had the same sort of environment that was to prove so effective for Moore at Texas for most of the next half-century, but Texas was still home for him. With the advantage of hindsight we can say that perhaps it was only an institution like Texas or Pennsylvania where Moore could have thrived as he did: clearly reputable or up and coming places but without nationally renowned senior mathematicians or, at that point, any younger people comparable to Moore with whom he might have had to compete for students. Furthermore, what would prove to be vitally important for his teaching method, he needed to establish himself in short order to the extent needed to assure a high degree of control over the selection of students for his classes and over the way those classes were conducted. There may not have been many institutions in the United States in the 1920s satisfying these conditions.

Since there are some aspects of the Moore method of teaching that are often misunderstood it may be useful to emphasize a couple of points here. It is not clear that Moore necessarily always had to give prior approval for students to enter any of his classes. His main concern in any case was that students coming into his advanced courses not know too much about the subject matter and thereby spoil the class for others. Throughout his career at Texas he regularly taught undergraduate calculus and for many years used a textbook. In this course it seemed to be his goal to exhibit the problem-solving power of the calculus as a way of interesting students in mathematics. At the same time he was also scouting for potential students who might benefit from going on to take his topology course. It was often, if not usually, in the latter that a student would first encounter the full-blown axiomatic, theorem-proving regimen where they were expected to prove the theorems by working on them independently and presenting the results in class. These latter characteristics have come to be commonly thought of as the “Moore method” but in fact they form only a part of how Moore actually taught; a more complete understanding needs to take into account his sequence of courses.[5]

2 The School

Though the notion of a Moore school has become even better established than in the 1930s, its very success, in a way, leads to a fuzziness of definition. As a first approximation we can say that the school at a given time consists of mathematicians who devote themselves to the branch of topology that deals with continua and, in particular, within the framework of the set-theoretic topology established by R.L. Moore. However, as is apt to happen with any attempt to be precise here, there is plenty of room for ambiguity. Does this mean that people are members only for a certain period of time, as long as they work in this area? The “framework” established by Moore was changing even during his lifetime, is it still identifiable today? Even if the subject

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[5] A study of one type of Moore’s calculus class at Texas, including transcriptions of Moore's own description of some of his practices, is given in (Eyles, 1998).

can be delineated, can one still become a member without having some direct lineage from Moore or Moore's students? Whatever the ambiguities of the boundary are, at a given time there appears to be a core of mathematics and mathematicians that other mathematicians can identify. In the formative years of the Moore school the subject and the members were tied together through the Moore method of teaching. The method of teaching, however, has to do with the way the subject developed and is not a characteristic of the school per se. Though all of Moore's doctoral students experienced this method in his courses and some went on to practice it, or some variant of it, in their own teaching, using it was neither a necessary nor sufficient condition for being a member of the Moore school.

2.1 Subject

R. L. Wilder classified Moore's publications into geometry, analysis, point set theory, continuous curves, structure of continua, and positional properties (Wilder, 1982). Point set theory is the defining subject of the school and (Moore, R. L., 1916), though it has obvious links to his earlier work, marks the beginning of its development. It was his first presentation of a sequence of axiom systems for the topological (non-metrical) characterization of the plane. Fifty-two theorems based on these axioms are proven. Ben Fitzpatrick, Jr. (1932-2000), one of the most active researchers of the later generation in the Moore school, in (Fitzpatrick, 1997) provided an overview of the development of the key initial axioms from 1916 to the first edition of Moore's magnum opus, (Moore, R. L., 1932). F. Burton Jones (1910-1999), of an earlier generation, also looked at the seminal nature of the 1916 paper (Jones, 1997).[6] Jones points out that proving these theorems, with subsequent modifications made over the years, was what Moore, from that point on, set his students to prove in his topology course (which went under various names such as "Theory of Point Sets"). Of course, the paper itself, as with any other literature on the subject, was out of bounds for the student. By all accounts, Theorem 15 was the pons asinorum:

> If A and B are distinct points of a domain M, there exists a simple continuous arc from A to B that lies wholly in M. (Moore, R. L.,1916 136)

The subject seems ideally suited to getting students rather quickly to a research level. A calculus course can be sufficient lead-in, but all of the starting material needed for proving the theorems can be provided at the blackboard as Moore did. Any new concepts needed might be left up to the student to discover. The levels of systems of axioms enable a step-by-step progression through the theorems. The fact that the object being investigated—ultimately the Euclidean plane—connects to the physical world where spatial intuition may help, must surely also count as an attraction from a student point of view. In other words, the subject that Moore developed has the potential of capturing the attention of certain minds in much the same way that Euclid's geometry did for centuries.

This pedagogical advantage supplements what must be the most important ingredient for a subject to be the basis of a school, namely its value as mathematics. Since this value judgment is ultimately made by the larger community of mathematicians, it is also treated below as part of the view of the school from outside. There is prima facie evidence that the subject continues even today to be a thriving branch: since 1959 there has been an MSC classification number for Moore spaces and there were 253 *Mathematical Reviews* items as of 13 December 2002 that have this as a primary or secondary classification. Of these about 46% were published after 1981 and 22% after 1991. These bare figures indicate that the area is still active though possibly declining, but a

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[6] Though these last two works appear in the same compendium, and the authors knew of each other, they were evidently prepared independently.

more careful analysis and comparison with other fields would be needed to confirm any such conclusion. Furthermore, "Moore spaces" does not capture all that ought to be included as the subject of the school.

One way in which the systematic aspect of Moore's work got mathematicians' attention was by demonstrating how theorems obtained by others fit into the scheme he developed. In the 1916 paper it was probably more a matter of demonstrating the relevance of his project to ongoing work. Thus Moore made a point of showing how certain theorems of N. J. Lennes could be proven from certain of Moore's axioms (Moore, R. L., 1916 139). Though Moore was very much a part of the growing current of topology that stemmed from work in Europe by M. Fréchet, A. Schoenflies, and F. Hausdorff, just to name those Europeans he cited in this paper, a reference to Lennes was important for Moore to make. A fellow student at Chicago, even if he seems to have been overlooked for a considerable period outside of the United States, Lennes was recognized by American researchers as being on the cutting edge of the subject (Wilder, 1978) and his work was followed closely by Moore.

Moore's (1916) evolved into the American Mathematical Society Colloquium Publication, *Foundations of Point Set Theory* (Moore, R. L., 1932), which also became a record of accomplishments of a number of his students and of others by Moore's either fitting their work into the text or making reference to their publications. This practice is especially noticeable in the second, revised edition of 1962.

2.2 Members

Moore credits "Mr. J. R. Kline, one of my students," with the original version of one of the proofs in (Moore, R. L., 1916 150). Thus it seems probable that Moore had been putting these theorems before his classes as he was working on them himself. Nevertheless, Kline (1891–1955) and George H. Hallett, Jr. (1895–1985), his first and second doctoral students respectively, both wrote dissertations on a topic that resembles more Halsted's work than Moore's new direction (Kline, 1916; Hallett, 1920). Though Veblen's axiomatization of Euclidean geometry plays a more prominent role, Halsted's presentation of two-dimensional double-elliptic geometry in his *Rational Geometry* is a starting point for both Kline and Hallett. (Indeed this latter topic was continued as late as in the dissertation of Donald A. Flanders, a student of Kline (Flanders, 1927).) Moore's third student, Anna Mullikin (1893–1975), was the first to do a dissertation within the framework of (Moore, R. L., 1916). These first students may have appeared at the time to be no more likely to be the beginning of a school of mathematics than the first three doctoral students of any other mathematics professor.

To what extent can we regard these first three students as members of the Moore school? Hallett had a distinguished career in political science and Mullikin devoted herself to teaching high school mathematics. The two had no mathematics publications beyond their dissertations. In contrast, Kline went on to have a distinguished career in university mathematics, staying on at Pennsylvania and working in the same vein as Moore himself—in fact the only joint paper to Moore's name was with Kline. Kline was also secretary of the American Mathematical Society from 1941 to 1950. He and Moore kept in close touch until Kline's death in 1955, sharing information about their students and recommending potential students to each other. Appendix II lists Kline's doctoral students. Many of them and their students (listed with their publications through several generations in (Traylor, 1972)) form the first branch of the Moore-school tree.

To return to the other two students, Anna Mullikin, in her one publication, arrived at an important result though it later turned out to have been published years earlier by Janiszewski in a Polish mathematical physics journal, as acknowledged in (Moore, R. L., 1924). Her work, nevertheless, entitled her to a permanent place in the school and it is cited in (Moore, R. L., 1932 463).

In addition, in the course of her long career as a high school teacher, one of her students, Mary-Elizabeth Hamstrom, became a doctoral student of Moore. The second Moore student, Hallett, though his dissertation was essentially out of the school and he did not continue in the mathematics profession, did publish (Hallett, 1919) while at Pennsylvania that was in the spirit of (Moore, R. L., 1916) and that was cited in (Moore, R. L., 1932) as well as by Gehman, a later student of Kline, and Wilder. Traylor interviewed Hallett in 1971 to determine what he thought were the benefits of being a Moore student:

> Dr. Moore's method of teaching brought out what appeared to me to be the two most important faculties in mathematical research—one which would surprise most people, I'd regard as imagination and the second, the ability to critical analysis in applying logic to what you think of to try out. And this same criteria [sic], of course, apply to almost everything else. It is a method of thinking. And I think such success as I've had in the work I've done in the field of government probably has a good deal to do with that - because they don't catch me up very often in theories of logic in bills, or different parts of bills, that don't hang together. (Traylor, 1972 86)

Kline, Mullikin, and Hallett exemplify the three possible base directions that one would expect a mathematics graduate to take: mathematics research, mathematics teaching, or another profession not centered on mathematics. If this sample of students is any indication, it might be possible to claim for many of Moore's doctoral students that they brought a "Moore school" attribute to whichever direction they took, even if, strictly speaking, the school is made up exclusively of research mathematicians.[7]

Thus the trajectory for the beginning of the school appears to have been set by the time Moore came to Texas in 1920. His first three students there were R. L. Wilder (1896–1982), R. G. Lubben (1898–1980), and G. T. Whyburn (1904–1969). All three remained academic research mathematicians. Moore did not encourage his students to remain at Texas and the only one who did was Lubben. Lubben's situation at Texas has remained something of a mystery: Why did he have no doctoral students? Why did he not further flourish along the research lines he seemed to have begun rather promisingly? Some have conjectured that it was because of Moore's dominance in the department but it may have been more because of personal reasons; Lubben retired due to ill health in 1959. Wilder has maintained that it was Moore's policy not to let anyone else teach the courses in his field at Texas—a policy, Wilder observed, that was probably held by other professors with respect to their departments elsewhere during the early twentieth century (Wilder, 1989 197).

Wilder continued in his own direction in topology (topology of manifolds), developed an interest in foundations of mathematics, and was also an historian of mathematics who took an anthropological approach to the subject. He had twenty-two doctoral students (not counting shared supervision) at the University of Michigan and was elected president of both the American Mathematical Society (1955–1956) and the Mathematical Association of America (1965–1966). Whyburn also achieved distinction by developing in his own branch of topology (topological analysis) and was the sole supervisor for twenty-five doctoral students, most at the University of Virginia. He was president of the American Mathematical Society (1953–1954). Though Wilder and Whyburn were very much in the Moore school tradition in their early work—something also

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[7] A list of Professor Moore's doctoral students is given in Appendix I. Some published lists may also include students whose dissertations he co-signed but for whom he was not the principal supervisor.

reflected in the publications of their early students—the fact that each evolved into new directions in topology justifies saying that they did not stay in that school. Perhaps a closer study would reveal a definite point where each "left" the Moore school but there was certainly no personal rejection of Moore—they always remained in touch and friendly—and no repudiation of their earlier work. The boundary, once again, may not be perfectly clear and, in an historical sense, just as with a family, once a member always a member.

Both Wilder and Whyburn were elected to the National Academy of Sciences, in reverse order to their Moore-school seniority, in 1963 and 1951 respectively. Moore had been a member since 1931. Since election to the Academy is made by members in the same field, this raises the possibility of a "school," once it obtains representation in this elite group, electing further members from within its ranks. Voting records are confidential, but there does not appear to have been any question that these deserved membership. In 1965 R H Bing (1914–1986) became the third Moore student to become a member.

This account focuses on the origins of the school and for that purpose Moore's first six doctoral students, along with their students, suffice already to explain the nature of the core membership by the 1930s. It would, however, be a mistake to leave the impression that the school's growth since then has been drawn solely from Moore's doctoral students and successive generations of their academic descendants. At Texas Moore became closely allied with certain colleagues in the department whose own research and teaching methods complemented Moore's. The two closest were H. J. Ettlinger (1889–1986), a student of G. D. Birkhoff at Harvard who joined the department before Moore, and H. S. Wall (1902–1971), a graduate of the University of Wisconsin at Madison who came to Texas in 1946. The three shared information about students and sometimes a student would have Ettlinger or Wall as doctoral supervisor and Moore as the principal influence on his or her work—examples in the other direction have not been found. For example, Fitzpatrick, mentioned above, was a doctoral student of Ettlinger but also took courses with Moore, devoted his career to Moore's subject, and utilized Moore's teaching method in his own classes. The cooperation between the three professors was probably a key reason for Moore's success at Texas. In fact, if it were not for Moore's undoubted domination within the group, the term "Texas school," which indeed is sometimes used, would be a more appropriate name than "Moore school".

Before leaving this look at membership, note should be made of the personal role of Moore himself. The film (Moore, R. L., 1965) includes a few scenes from outside the classroom that show Moore enjoying chatting with his students. On occasion Moore and his wife would have groups of students at their home, but the classroom could also be a venue for non-mathematical discussions, even if one-sidedly from Moore. This could happen especially when no student had a proof to present. Moore's political and social views were evident at times, even if only indirectly revealed in the way he might pose a question about a current event. Nevertheless some of his last doctoral students were unaware at the time of his strong segregationist stance, for example. Moore's views on segregation were apt to have been even less visible to earlier students at Texas since legally sanctioned segregation did not entirely disappear from the state until about 1950. The university did not admit African Americans until that year and the number admitted was small and slow to grow (De León and Calvert, 2002). There were relatively very few African American mathematics students during the remainder of Moore's tenure at Texas and consequently the fact that none were ever in his classes may not have attracted much overt attention. The Moore school, however, was another matter and thanks to Kline there were African Americans in it from an early time, namely Kline's students D. W. Woodard and W. W. S. Claytor.[8] Those who knew him typically describe Moore as a powerful personality, and it was a personality that did not attract some students, but to those who could benefit from him the experience in itself became a part of the Moore-school bond. This strong personal aspect does not seem to have bred a school of imitators, however. On the contrary, from what we know of the most publicly prominent of Moore's students, beginning with Kline, if they were influenced by his personal example to any degree, it was in the direction of thinking and acting independently—precisely what the Moore method is purported to encourage.[9]

3 Views from Outside

The Moore school got its designation as a "school" primarily from mathematicians outside of the group. In an interview about the history of the mathematics department at Princeton University, N. Jacobson and E. J. McShane revealed some of the ways the Moore group was discussed during the time of the topologist Lefschetz who was at Princeton from 1924 to 1953.

> Jacobson: There's another thing which might be worth mentioning, something of historical interest. The topology that was done at Princeton was what became algebraic topology. At Texas was the school of R. L. Moore, the school of point set topology. I don't know if they felt the same way about Lefschetz. Eventually what happened was that Raymond Wilder saw that the two things could be put together to become one subject. It was a great achievement of his, but before that there were always jokes being made about the Texas and Polish school. About, for example, simple aspicular cactoids, a bizarre configuration that R.L. Moore's school came up with. That was one of the things that added to the gaiety of Princeton.
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> McShane: The school you're talking about was referred to by Lefschetz as the "concerning school", since the papers were "concerning" this and "concerning" that. Lefschetz commented, "To write a book about topology and confine oneself to this subject matter, is like writing a book on zoology and confining oneself to the rhinoceros." (Duren et al.)

Playful as these remarks are they hinge on the fact that a mathematical school may—and perhaps must by its nature—come to an end. Its very success may lead to a merger with the larger

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[8] Kline in his many letters to Moore would often give the latest news of what his Penn students were doing in mathematics, including Woodard and Claytor. In a letter of 24 October 1933 Kline describes Claytor's "very fine thesis … perhaps the best that I have ever had done under my direction" (RLM, box 20, folder 5). Unfortunately we do not have Moore's letters to Kline, beyond whatever copies Moore sometimes made for himself, and it is not known if he ever commented on the work of any of Kline's students. We should note that Moore did make use of both Woodard's and Claytor's work in the table presented here as Appendix III.

[9] Further details on Moore's students can be found in (Fitzpatrick, 2000). Largely through Fitzpatrick's efforts and the support of the Educational Advancement Foundation over a hundred interviews and talks by or about Moore's students, undergraduate as well as graduate, have been recorded and transcribed for deposit in the Archives of American Mathematics at the University of Texas at Austin. The Foundation also supports the Legacy of R. L. Moore Project whose main goal is to make the Moore method, as a form of enquiry-based learning, more widely known and assist those wishing to use it or adapt it to other subjects at any level of education.

stream of mathematics where its identity as a school is lost. On the other hand, its subject matter may run dry of what other mathematicians regard as fruitful results. During Lefschetz's time, at least, there was little risk of the latter happening to the Moore school.

3.1 The Polish School

One of the things that helps to delineate a school is having another with which to compare and contrast. The Polish school came essentially to the same topological topic independently of the Moore school. This is another example of those near simultaneous happenings in the history of mathematics, like the discovery of non-Euclidean geometries, that are striking even if they can be explained away by arguing that the subject was ripe for discovery.

Shortly before 1920 W. Sierpinski (1882–1959) began to bring together a critical mass of Polish mathematicians to concentrate on one area and thereby support each other to an extent that had not occurred before in Poland. He recruited to the cause three rising stars only slightly younger than himself and this group of four in Lwów formed the growth point of the school which later centered at Warsaw. Z. Janiszewski (1888-1920) received his doctorate under Henri Lebesgue in Paris in 1911. S. Mazurkiewicz (1888–1945) and S. Ruziewicz (1889–1941) received their doctoral degrees under Sierpinski (Duda, 1996). With respect to the choice of topology, Ciesielski and Pogoda have summarized the origins of the Polish school in terms that have parallels with the American group:

> Why did Warsaw mathematicians select just topology for the realization of the program of Sierpinski and Janiszewski? ... First it was a logical consequence of their earlier interests: Sierpinski was interested in set theory, Janiszewski in the properties of continua, and Mazurkiewicz in curves. Second, Polish mathematicians probably anticipated that in the future not "pure" set theory but its applications would predominate. Also, young people are not afraid to attack difficult and original problems. When one undertakes such a task, the chance of final success is better in a new, developing branch of science. Topology was a very good candidate for this. (Ciesielski and Pogoda ,1996 35)

In 1920 *Fundamenta Mathematicae,* the journal of the Polish school, was founded and the arrival of its first issue in the United States appears to have been the first that the Moore group knew of the Polish group. As soon as the two groups came to learn about each other an interesting mixture of competition and cooperation arose. Articles by Moore and Kline in volume 3 of *Fundamenta* in 1923 were the first articles by non-Europeans in the journal and mark the beginning of a long relationship between the schools. In the R. L. Moore archives the earliest correspondence between Moore and a member of the Polish school is represented by a draft letter from Moore dated 1925 thanking Sierpinski for volume 7 of *Fundamenta.* Lubben and Kline visited the Warsaw group in 1926. In 1930 G. T. Whyburn and C. Kuratowski published a joint paper in the journal. Whyburn and his wife, Lucille (one of the few M.A. students of Moore), visited during their Guggenheim-sponsored European trip of 1929-30 (Whyburn, L., 1977).

The Americans must have been initially taken aback by the fact that some of what they had assumed were new results established by them, such as Mullikin's mentioned above, had appeared years earlier in journals now being cited in *Fundamenta*. Whereas up to this point there could be a rather planned development of work under the control of Moore and his students, now there was an enhanced concern over establishing priority and sharing information. Nevertheless the relationship seems to have benefited all concerned. To this day there are descendants of the Polish school who meet with the Texas descendants at topology conferences even as the subject they share has changed over the twentieth century. The most frequent meetings are those in the

Spring Topology and Dynamics Conference series, which has met since 1967 and whose proceedings are published as *Topology Proceedings* (Auburn University).

In the decades after 1920 the two schools exhibited quite different styles of presentation. As one might expect, the Moore school used a fairly consistent terminology throughout and tended to emulate Moore's style, even to the point of echoing "concerning …" in their paper titles, as Lefschetz pointed out. The Poles, presumably because they did not have anything equivalent to (Moore, R. L., 1916) as a starting point, seem to have had a much more ad hoc approach to definition and theorem building. Moore was not one for using special symbols if something could be stated clearly and fairly succinctly in words and the Polish group was regarded as profligate in their notational invention. G. T. Whyburn in his review of Hausdorff's *Mengenlehre* took the occasion to give something of a lecture on the need to curtail unnecessary symbolism pointing out that it did not help if one had to learn new symbols with each new paper (Whyburn, G. T., 1928). Survey books and textbooks are probably the main influences in establishing, or at least helping to confirm, a standard notation and in this field the big books were being done by others than the Texas school; there is nothing comparable on their side to Kuratowski's *Topologie* (1933), for example. The Moore group seems to have been a minority voice in the evolution of notation.

3.2 Moore's Citation Report

There are several typescript pages in the Moore archival collection without title or date that evidently made up a report by Moore of his work up to about 1936. It consists of two parts: first a narrative account from his first paper on Hilbert's betweenness axioms through his 1932 book and somewhat beyond, and then a compilation of references by others to his works. The first, narrative part, includes quotations by others that point to original and important results by Moore such as this from a 1926 paper by T.H. Hildebrandt: "The problem of determining conditions under which the any-to-finite Borel Theorem is valid in a space $\mathscr{L}$ remained unsolved for some time. … The first solution of the problem was given by R.L. Moore." The narrative concludes with these remarks:

> Three of my former students, J.R. Kline, R. L. Wilder and G. T. Whyburn have made invited addresses at meetings of the Society. [Moore gives references for these.] For further comments on my work I refer you to these reports. (RLW, box 45, folder 3, p. 6)

The table in Appendix III summarizes Moore's compilation by grouping the references to his work by decades. A photocopy of these pages is in the R. L. Wilder archival collection and is the source in this account. Wilder evidently found it useful in preparing his account of Moore's work. At the top of the compilation of citations he wrote "Compiled by Moore. [Q. Why is not my name cited here? I made many and copious references to Moore. R.L.W.]". Wilder seems not to have noticed that *no* students of Moore are included; students of students are, however. Thus this compilation may be intended to document precisely Moore's influence beyond his students. In addition, whether or not by design, it could help to make the argument that the school had not been built upon the basis of members simply referring to each other, as in a kind of pyramid scheme. There is certainly the potential of the sort of criticism that Moore may have been addressing: namely, that the importance of his work could be artificially created, that it was of little real relevance to others, and that with sufficient self-referencing a group such as his could build up a reputation as mathematically relevant on a large scale. Such a so-called "school" would seem destined for a short life if the members could relate only to each other, but Moore may not have thought that this was obvious to administrators or colleagues in other disciplines at Texas.

At this stage in his career it was understandable that Moore would focus more on the importance of the mathematics than on the teaching aspects of his career. The only other substantial account of his work that he prepared was on his classroom methods and for that he chose the medium of film when the opportunity arose (Moore, R. L., 1965).

4 Concluding Remarks

Depending on its context, the term “school” can be used with quite different connotations: it can be used as a form of positive recognition and even praise, or it can imply marginalization. The quotations at the beginning of this paper, from the historian Archibald and the mathematicians Alexandroff and Hopf, I take to be positive examples. It may be difficult to determine in some circumstances, however, on which side of this sometimes fine line the term is being used, as illustrated in the McShane and Jacobson discussion quoted above. Presumably at some point the usefulness of the term “Moore school” will have altogether run its course and the term will serve only a purely historical function. Clearly the complete story of the school has yet to be written. There are Moore students still very active today, as well as academic descendants at least some of whom count themselves as members of the school. It is apt to be a while before we can treat this group with the same kind of historical perspective that we have in looking, for example, at the late-nineteenth century school associated with K. Weierstrass.

If we could plot the whole of the changing universe of mathematicians and their works over time, then a school might be discernible as a particular type of constellation that emerges out of its surroundings, affects those surroundings for a period of time, and eventually merges into other patterns, smaller or larger. In their account of the Polish school, Ciesielski and Pogoda point to the decreased influence of Poland in global developments in topology after the 1930s. Certainly the Second World War had an effect but they also speculate that certain choices were made about the direction of research that may have contributed to the country’s weaker showing in topology relative to earlier years. “But is this the fault of the creators of the Warsaw School? Is it anybody's fault?” they ask, and conclude that it is probably too early to judge (Ciesielski and Pogoda, 1996 39). The proximate causes for the decline of a school may eventually be determined, but decline may ultimately be a property of schools as such. In any case, for both the Polish and Moore schools it will probably prove easier to describe their beginnings than it will be their endings.

Appendices

I R. L. Moore's Doctoral Students

John Kline	1916	John Slye	1953
George Hallett, Jr.	1918	John Mohat	1955
Anna Mullikin	1922	Bennie Pearson	1955
Raymond Wilder	1923	Steve Armentrout	1956
Renke Lubben	1925	William Mahavier	1957
Gordon Whyburn	1927	L. Bruce Treybig	1958
John Roberts	1929	James Younglove	1958
Clark Cleveland	1930	George Henderson	1959
Joe Dorroh	1930	John Worrell Jr.	1961
Charles Vickery	1932	James Cornette	1962
Edmund Klipple	1932	Howard Cook	1962
Robert Basye	1933	Dennis Reed	1965
F. Burton Jones	1935	Harvey Baker Jr.	1965
Robert Swain	1941	Blanche Baker	1965
Robert Sorgenfrey	1941	Roy Davis, Jr.	1966
Harlan Miller	1941	Jack Rogers Jr.	1966
Gail Young, Jr.	1942	Martin Secker	1966
R H Bing	1945	David Cook	1967
Edwin Moise	1947	John Hinrichsen	1967
Richard Anderson	1948	Joel O'Connor	1967
Mary Ellen Rudin	1949	John Green	1968
Cecil Burgess	1951	Michael Proffitt	1968
B. J. Ball	1952	Jesse Purifoy	1969
S. Eldon Dyer	1952	Robert Jackson	1969
Mary-Elizabeth Hamstrom	1952	Nell Stevenson	1969

II J. R. Kline's Doctoral Students

Harry Gehman	1925	Clarence Lovell	1933
William Ayres	1927	W. W. Shieffelin Claytor	1934
Donald Flanders	1927	Adam Smith	1934
Dudley Woodard	1928	Arthur Milgram	1937
Thomas Benton	1929	Ebon Betz	1939
Norman Rutt	1929	Erik Hemmingsen	1946
Leo Zippin	1929	Edward Knobelauch	1946
Virgil Adkisson	1930	Athanasios Papoulis	1950
Joseph Kusner	1931	Lida Barrett	1954

III Moore's Citation Compilation

If the author citing Moore was a student of a Moore student the name of the author's professor is given in square brackets. The numbers in the table are a count of the referencing papers. In his typescript, referred to above, Moore gave the full references to the papers and the number of references within each paper.

Authors Citing Moore	**1902-10**	**1911-20**	**1921-30**	**1931-36**
Veblen, O.	2	1	1	
Halsted, G.B.		1		
Chittenden, E.W.		1	3	
Kuratowski, C.			12	2
Urysohn, P.			2	
Woodard, D.W. [Kline]			1	
Nikodym, S.			1	
Szymanski, P.			1	
Miller, E.W. [Wilder]			1	
Itard, H.G.			1	
Benton, T.C. [Kline]			1	
Alexandroff and Urysohn			1	
Cohen, L.W. [Wilder]			1	
Mazurkiewicz, S.			1	
Swingle, P.M. [Wilder]			2	
Wilson, W.A.			2	

Knaster, B.			3	
Vietoris, L.			2	
Forder, H.G.			1	
Hill, L.S.			1	
Menger, K.			2	1
Zarankiewicz			3	1
Ettlinger, H.J.			3	
Hildebrandt, T.H.			1	
Tietze, H.			1	
Fréchet, M.			2	
Birkhoff, G.D.			1	
Aitchison, B. [Whyburn]				1
Whitney, H.				2
Aronszajn, N.				2
Rosenthal, A.				1
Cech, E.				1
Wilkosz, W.				1
Steenrod, N.E.				1
Dancer, W. [Wilder]				1
Claytor, S. [Kline]				1
Eilenberg, S.				3
Hurewiecz, W.				1
Borsuk, K.				1
Morrey, C.B.				1
Amman, G.				1
Chojnacki, Ch.				1

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RLM R. L. Moore Papers. Archives of American Mathematics, Center for American History, University of Texas at Austin.

RLW R. L. Wilder Papers. Archives of American Mathematics, Center for American History, University of Texas at Austin.

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Albert C. Lewis obtained his Ph. D. in history of mathematics at the University of Texas at Austin in 1975. His B.A. and M.A. degrees in mathematics were also obtained at Texas during the time that Professor R. L. Moore was teaching but he did not take courses with Moore whom he met only after the latter’s retirement. Lewis’s interest in the Moore school grew out of an earlier historical interest in Moore’s teacher at Texas, G. B. Halsted.