Axiomatics Between Hilbert and the New Math: Diverging Views on Mathematical Research and their Consequences on Education

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Abstract

David Hilbert is widely acknowledged as the father of the modern axiomatic approach in mathematics. The methodology and point of view put forward in his epoch-making Foundations of Geometry (1899) had lasting influences on research and education throughout the twentieth century. Nevertheless, his own conception of the role of axiomatic thinking in mathematics and in science in general was significantly different from the way in which it came to be understood and practiced by mathematicians of the following generations, including some who believed they were developing Hilbert's original line of thought.

The topologist Robert L. Moore was prominent among those who put at the center of their research an approach derived from Hilbert's recently introduced axiomatic methodology. Moreover, he actively put forward a view according to which the axiomatic method would serve as a most useful teaching device in both graduate and undergraduate teaching in mathematics and as a tool for identifying and developing creative mathematical talent.

Some of the basic tenets of the Moore Method for teaching mathematics to prospective research mathematicians were adopted by the promoters of the New Math movement.

1 Introduction

The flow of ideas between current developments in advanced mathematical research, graduate and undergraduate student training, and high-school and primary teaching, involves rather complex processes that are seldom accorded the kind of attention they deserve. A deeper historical understanding of such processes may prove rewarding to anyone involved in the development, promotion and evaluation of reforms in the teaching of mathematics.

The case of the New Math is especially interesting in this regard, because of the scope and depth of the changes it introduced and the intense debates it aroused. A full history of this interesting process is yet to be written. In this article I indicate some central topics that in my opinion should be taken into account in any prospective historical analysis of the New Math movement, its origins and development. In particular, I suggest that some seminal mathematical ideas of David Hilbert concerning the role of axiomatic thinking in mathematics were modified by mathematicians of the following generations, and that this modified version of Hilbert's ideas provided a background for key ideas behind the movement. The modifications undergone along the way touched not only on how ideas related to contemporary, advanced mathematical research might be used in the classroom (contrary to Hilbert's point of view), but also on the way in which these ideas were relevant to research itself. I will focus on the so-called Moore Method as a connecting link between Hilbert's axiomatic approach and the rise of the New Math.

2 Hilbert's Axiomatic Method

In 1899 the Göttingen mathematician David Hilbert (1862–1943) published his groundbreaking book *Grundlagen der Geometrie*. This book represented the culmination of a complex process that spanned the nineteenth century, whereby the most basic conceptions about the foundations, scope and structure of the discipline of geometry were totally reconceived and reformulated. Where Euclid had built the discipline more than two thousand years earlier on the basis of basic definitions and five postulates about the properties of shapes and figures in space, Hilbert came forward with a complex deductive structure based on five groups of axioms, namely, eight axioms of incidence, four of order, five of congruence, two of continuity and one of parallels. According to Hilbert's approach the basic concepts of geometry still comprise points, lines and planes, but, contrary to the Euclidean tradition, such concepts are never explicitly defined. Rather, they are implicitly defined by the axioms: points, lines and planes are any family of mathematical objects that satisfy the given axioms of geometry.

It is well known that Hilbert once explained his newly introduced approach by saying that in his system one might write "chairs", "tables" and "beer mugs", instead of "points", "lines" and "planes", and this would not affect the structure and the validity of the theory presented. Seen retrospectively, this explanation and the many times it was quoted were largely behind a widespread, fundamental misconception about the essence of Hilbert's approach to geometry. A second main reason for this confusion was that twenty years later Hilbert was the main promoter of a program intended to provide solid foundations to arithmetic based on purely "finitist" methods. The "formalist" program, as it became known, together with a retrospective reading of his work of 1900, gave rise to a view of Hilbert as the champion of a formalist approach to mathematics as a whole. This reading has sometimes been expressed in terms of a metaphor typically associated with Hilbert, namely, the "chess metaphor", which implies that 'mathematics is not about truths but about following correctly a set of stipulated rules'. For example, the leading French mathematician and founding Bourbaki member, Jean Dieudonné (1906–1992), who saw himself as a follower of what he thought was Hilbert's approach to mathematics said that, with Hilbert, "mathematics becomes a game, whose pieces are graphical signs that are distinguished from one another by their form" [Dieudonné 1962, 551].

For lack of space, I cannot explain here in detail why this conception is historically wrong, why Hilbert's axiomatic approach was in no sense tantamount to axiomatic formalism, and why his approach to geometry was empiricist rather than formalist.[1] I will just bring in two quotations that summarize much of the essence of his conceptions and help give a more correct understanding of them. The first quotation is taken from a lecture delivered in 1919, where Hilbert clearly stated that:

> We are not speaking here of arbitrariness in any sense. Mathematics is not like a game whose tasks are determined by arbitrarily stipulated rules. Rather, it is a conceptual system possessing internal necessity that can only be so and by no means otherwise. (Quoted in [Corry 2006, 138])

The second quotation is taken from a course taught in 1905 at Göttingen, where Hilbert presented systematically the way that his method should be applied to geometry, arithmetic and physics. He thus said:

> The edifice of science is not raised like a dwelling, in which the foundations are first firmly laid and only then one proceeds to construct and to enlarge the

[1] For a detailed accounts of the background and development of Hilbert's axiomatic approach see [Corry 2004]. See also [Corry 2006].

> rooms. Science prefers to secure as soon as possible comfortable spaces to wander around and only subsequently, when signs appear here and there that the loose foundations are not able to sustain the expansion of the rooms, it sets about supporting and fortifying them. This is not a weakness, but rather the right and healthy path of development. (Quoted in [Corry 2004, 127])

This latter quotation is of particular importance for the purposes of the present article, since it suggest that in Hilbert's view the axiomatic approach should never be taken as the starting point for the development of a mathematical or scientific theory. Likewise, Hilbert never saw axiomatics as a possible starting point to be used for didactical purposes. Rather, it should be applied only to existing, well-elaborated disciplines, as a useful tool for clarification purposes and for allowing its further development.

Hilbert applied his new axiomatic method to geometry in the first place not because geometry had some special status separating it from other mathematical enterprises, but only because its historical development had brought it to a stage in which fundamental logical and substantive issues were in need of clarification. As Hilbert explained very clearly, geometry had achieved a much more advanced stage of development than any other similar discipline. Thus, the edifice of geometry was well in place and as in Hilbert's metaphor quoted above, there were now some problems in the foundations that required fortification and the axiomatic method was the tool ideally suited to do so. Specifically, the logical interdependence of its basic axioms and theorems (especially in the case of projective geometry) appeared now as somewhat blurred and in need of clarification. This clarification, for Hilbert, consisted in defining an axiomatic system that lays at the basis of the theory and verifying that this system satisfied three main properties: independence, consistency, and completeness. Hilbert thought, moreover, that just as in geometry this kind of analysis should be applied to other fields of knowledge, and in particular to physical theories. When studying any system of axioms under his perspective, however, the focus of interest remained always on the disciplines themselves rather than on the axioms. The latter were just a means to improve our understanding of the former, and never a way to turn mathematics into a formally axiomatized game. In the case of geometry, the groups of axioms were selected in a way that reflected what Hilbert considered to be the basic manifestations of our intuition of space.

In 1900, moreover, "completeness" meant for Hilbert something very different to what the term came to signify after 1930, in the wake of the work of Gödel. All it meant at this point was that the known theorems of the discipline being investigated axiomatically would be derivable from the proposed system of axioms. Of course, Hilbert did not suggest any formal tool to verify this property. Consistency was naturally a main requirement, but Hilbert did not initially think that proofs of consistency would become a major mathematical task. Initially, the main question Hilbert intended to deal with in the *Grundlagen*, and elsewhere, was independence. Indeed, he developed some technical tools specifically intended to prove the independence of axioms in a system, tools which became quite standard in decades to come. Still as we will see now, the significance and scope of these tools was transformed by some of those who used them, while following directions of research not originally envisaged or intended by Hilbert.

3 Postulational Analysis in the USA

Postulational Analysis was a research trend that developed in the first decade of the twentieth century in the USA, particularly at the University of Chicago under the leadership of Eliakim Hastings Moore (1862–1932). Moore was one of the first mathematicians to give close attention to Hilbert's *Grundlagen* and to teach it systematically. In the fall of 1901 he conducted in Chicago a seminar based on the book, where special attention was devoted to the possibility of revising Hilbert's proofs of independence. Indeed, Moore proved that Hilbert's system contained a redundancy involving one axiom of incidence and one of order (see [Parshall & Rowe 1991, 372–392]). For Hilbert, the real focus of interest lay in the interrelation among the various groups of axioms — in which he saw the isolable facts of our spatial intuition — rather than among the individual axioms across groups. Moore's was one of several, minor corrections of this kind to the *Grundlagen* that were proposed over the coming years. Hilbert eventually incorporated some of these in forthcoming editions of his book, but he did not see in them a matter of deep concern with respect to his presentation and to the meaning of the achievement implied in his axiomatization endeavour.

Edward Huntington (1847–1952) was a Harvard mathematician that took another step in applying Hilbert's tool in a direction not previously intended by Hilbert. In an article of 1902, Huntington analyzed two systems of postulates used to define abstract groups. This was followed by a similar analysis by Moore for two other systems of postulates for groups. Several other American mathematicians soon followed suit. E. H. Moore's first doctoral student and later colleague at Chicago, Leonard Eugene Dickson (1874–1954), himself a distinguished group-theorist, published his own contributions on the postulates defining fields, linear associative algebras, and groups. Oswald Veblen (1880–1960), another Moore student, completed his dissertation in Chicago in 1903. He presented in it a new system of axioms for geometry, using as basic notions point and order, rather than point and line. Yet another one of Moore's student to pursue this trend was Robert Lee Moore (1882–1974), to whom I want to devote closer attention below.[2]

Works of this kind were at the heart of postulational analysis. Unlike Hilbert in the case of geometry, in undertaking their analyses these mathematicians were not mainly concerned with the specific problems in the disciplines whose systems of axioms they analyzed (e.g., those of the system of complex numbers, the continuum, or the abstract theory of groups). Rather they turned the systems of postulates themselves into mathematical objects of intrinsic interest, and to these they devoted their consideration. They proved no new theorems about, say, groups, nor did they restructured the logical edifice of the theory of groups. They simply refined existing axiomatic definitions and provided postulate systems containing no logical redundancies. As a matter of fact, these systems were not always adopted since, in spite of being logically cleaner, they were less suggestive than those more commonly used. Thus for instance, in defining a group, one typically requires the existence of a neutral element e, such that for any element a of the group, one has

$$a * e = e * a = a. \tag{1}$$

Postulational analysts showed that if one assumes associativity, and also that $e * a = a$, then the left hand side of (1) also follows. And yet, textbook in algebra continued to introduce the concept of groups by referring to conditions (1). In this sense, the efforts of the postulational analysts deviated from Hilbert's original point of view. Neither Hilbert nor any one of his collaborators ever paid significant attention or performed any research of their own in this direction.

4 The Moore Method of Mathematical Education

Still as a graduate student in Austin, Texas, R. L. Moore was able in 1902 to display his talents working along the lines of postulational analysis when he achieved a redundancy result related to Hilbert's *Grundlagen*, very similar to E. H. Moore's result mentioned above. He was invited to Chicago for doctoral studies which he completed in 1905 with a dissertation

[2] For details on the American School of Postulational Analysis, see [Corry 1996 (2004), 172–182].

on "Sets of Metrical Hypotheses for Geometry". Moore went on to become a distinguished topologist and above all the founder of a very productive and influential school of researchers and institution-builders in the USA. Postulate analysis and the outlook embodied in it became central to both Moore's research and teaching. It was to the latter activity, however, rather than the former, that Moore directed most of his energies throughout his unusually long career. Moore directed 50 Ph.D students who can claim now about 1,678 doctoral descendants. Many of them continued to practice teaching with a devotion similar to that of the master, and applying methods similar to his [Parker 2005, 150–159].

To be sure, a precise definition of the Moore Method is not a straightforward matter. In fact, given the quantity and quality of mathematicians who came under Moore's direct and indirect influence, one must presume that many of them developed their own versions of this teaching method. Still, many of his students consistently mentioned the training they received from Moore as the single most decisive factor in the consolidation of their own mathematical outlooks and scientific personalities. One such distinguished pupil, F. Burton Jones (1910–1999), offered this vivid account of his former teacher's methodology [Jones 1977, 274–275]:

> Moore would begin his graduate course in topology by carefully selecting the members of the class. If a student had already studied topology elsewhere or had read too much, he would exclude him (in some cases he would run a separate class for such students). The idea was to have a class as homogeneously ignorant (topologically) as possible. Plainly he wanted the competition to be as fair as possible, for competition was one of the driving forces. ... Having selected the class he would tell them briefly his view of the axiomatic method: there were certain undefined terms (e.g, "point" and "region") which had meaning restricted (or controlled) by the axioms (e.g., a region is a point set). He would then state the axioms that the class was to start with. ... An example or two of situations where the axioms could be said to apply (e.g., the plane or Hilbert space) would be given. He would sometimes give a different definition of region for a familiar space (e.g. Euclidean 3-space) to give some intuitive feeling for the meaning of an "undefined term" in the axiomatic system. ... After stating the axioms and giving motivating examples to illustrate their meaning he would then state some definitions and theorems. He simply read them from his book as the students copied them down. He would then instruct the class to find proofs of their own and to construct examples to show that the hypotheses of the theorems could not be weakened, omitted, or partially omitted.
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> When the class returned for the next meeting he would call on some student to prove Theorem 1. After he became familiar with the abilities of the class members, he would call on them in reverse order and in this way give the more unsuccessful students first chance when they *did* get a proof. Then the other students ... would make sure that the proof presented was correct and convincing.

The axiomatic method, then, was applied by Moore to teaching in a way that was essentially the same as that he followed in research. In both cases, axiomatic analysis was given a centrality that was foreign to Hilbert's original approach. Some of the main ideas behind Moore's method can be summarized as follows:

- Strict selection of students best suited to learn according to the method
- Prohibition of the use of textbooks as part of the learning process
- Prohibition of collaboration among students as part of the learning process

- Almost total elimination of frontal lectures in class
- Fully axiomatic presentation of the mathematical ideas, with very little external motivation

Actually, Moore himself summed up the essence of his didactical approach in just eleven words: "That student is taught the best who is told the least."[3]

In order to avoid misunderstandings, I would like to stress that Moore devised this method as a way to turn out successful, productive research mathematicians. Independently of the question how successful the method was in reaching this aim, Moore never claimed that it should be used for other kinds of mathematical training such as that, for example, of engineers or physicist. Nor did he ever promote its use as a convenient approach for high-school or primary instruction. At any rate, one would not be surprised to realize that even for graduate-level training of pure research mathematicians, not everyone shared his enthusiasm for this method. Indeed, Moore was roundly criticized by students as well as established mathematicians from the very time he began to conceive of and promote it. One interesting testimony of this critical attitude comes from another distinguished Moore student, Mary Ellen Rudin (*1924). On the one hand, she praised Moore as a teacher who knew how to infuse self-confidence in those students who could bear with him. Thus she said:[4]

> He built your confidence so that you could do anything. No matter what mathematical problem you were faced with, you could do it. I have that total confidence to this day. ... He somehow built up your ego and your competitiveness. He was tremendously successful at that, partly because he selected people who naturally had those qualities he valued.

Her main criticism, though, concerned the breadth of mathematical education she received as a graduate student taught under this method:

> I felt cheated because, although I had a Ph.D. I had never really been to graduate school. I hadn't learned any of the things that people ordinarily learn when they go to graduate school [algebra, topology, analysis]. I didn't even know what an analytic function was.

And curiously, anticipating the eventuality that these ideas might be applied to school education, she warned:

> I would never allow my children to study in a school that followed Moore's methods. I think that he was destructive to anyone who would not exactly fit his way.

The point that I want to stress in this brief description of Moore — both as a researcher (within the trend of postulational analysis) and as teacher (along the lines of his method) — is how his conceptions derived directly from Hilbert's ideas but at the same time took a peculiar turn that led to practices deviating from Hilbert's in essential ways.

5 From Moore to the New Math

The Soviet launching of the Sputnik on October 4, 1957, is usually taken as a turning point in the status of public debates in the USA and Western Europe about the need for deep reforms in scientific and mathematical education. Such debates had already been underway since

[3] Quoted in [Parker 2005, vii].

[4] The next three quotations are take from [Albers and Reid 1988].

1951 in the context of the School Mathematics Study Group (SMSG), under the initiative of Max Beberman (1925–1971). But it was the impact of this dramatic event that turned a hitherto rather marginal debate into a matter of widespread public interest. In 1958 Ed Begle (1914–1978) was appointed director of the SMSG. Under his very active leadership, an accelerated process was initiated that culminated in the teaching revolution usually known as the New Math movement [Raimi 2005, Usiskin 1999].

For reasons of space even a brief account of the sources and development of the New Math reform program and its impact cannot be given here. I must limit myself to present in a rather telegraphic way its main guidelines and principles:

- An attempt to bridge the gap with current university-level mathematics
- Primacy of "principles" over "calculation"
- Emphasis on structures, sets, patterns
- "Autonomous experimentation" over "statements by the teacher" and "learning by heart"

The point I want to suggest here is that some of these principles and guidelines were inspired, at least partially, by the widespread, perceived success of the Moore Method in many American institutions of higher learning. To be sure, Moore never expressed any opinions on SMSG or about the New Math, and, moreover, he deliberately did not want to be regarded as a pedagogue [Anderson & Fitzpatrick 2000]. And yet, the pervasiveness of ideas originating in his didactical practice are easily recognizable in the spirit of New Math. In fact, although Begle completed his Ph.D degree in Princeton under Solomon Lefschetz (1884–1972), the deepest influence on his career came from Raymond A. Wilder (1896–1982), with whom in Michigan he had studied topology, the field in which he built his own reputation as a distinguished researcher [Pettis, 1969]. Wilder, in turn, was a Moore student, and perhaps the one that contributed more than anyone else to spread the gospel of the Moore method [Wilder 1959]. It does not seem too farfetched, then, to presume that the ideas underlying the Moore Method, via Begle, greatly influenced the rise of New Math.

This kind of influence can also be assessed by looking at it from the side of the critics. As it is well known, the New Math was the target of strong criticisms of many kinds. It is interesting to see in this criticism how the program is identified with central trends in twentieth century mathematics supposedly derived from Hilbert. In such critical assessment, Hilbert's conception of mathematics is typically associated (wrongly so) with some kind of axiomatic formalism as explained above. One remarkable example of this appears in an address delivered in 1966 by Peter Lax at a conference held in Moscow, on axiomatics in mathematics education. These are some excerpts of his talk:

> [T]he current trend in new texts in the United States is to introduce operations with fractions and negative numbers solely as algebraic processes. The motto is: Preserve the Structure of the Number System. I find this a very poor educational device: how can one expect students to look upon the structure of the number system as an ultimate good of society? ... The remedy is to stick to problems which arise naturally; to find a sufficient supply of these, covering a wide range, on the appropriate level is one of the most challenging problems for curriculum reformers. My view of structure is this: it is far better to relegate the structure of the number system to the humbler but more appropriate role of a device for economizing on the number of facts which have to be remembered. ... What motivates textbook writers not to motivate? Some, those with narrow mathematical

> experiences, no doubt believe those who, in their exuberance and justified pride in recent beautiful achievements in very abstract parts of mathematics, declare that in the future most problems of mathematics will be generated internally. Taking such a program seriously would be disastrous for mathematics itself, as Von Neumann points out in an article on the nature of mathematics ... it would eventually lead to rococo mathematics. ... As philosophy it is repulsive, since it degrades mathematics to a mere game. And as guiding principle to education it will produce pedantics, pompous texts, dry as dust, exasperating to those involved in teaching the sciences. If pushed to the extreme it may even cause the disappearance of mathematics from the high school curriculum along with Latin and the buffalo.

Hilbert is not mentioned here by name, but here as in other places, the putative reduction of mathematics to a "mere game", is a sure sign of a negative reference to what many considered to be his mathematical legacy.

6 Concluding Remarks

In the foregoing pages I provided an outline of a line of development that led us from Hilbert's introduction of the new axiomatic approach at the turn of the twentieth century to the rise of the New Math in the USA in the early 1960s. The connecting link was Robert Lee Moore and the way in which he adopted the axiomatic approach in both research and teaching. Although for reasons of space I will not be able to develop this claim in detail here, I want to suggest in this closing remarks that a parallel development can also be traced in the European context, and especially in the French one. Here, the connecting link was provided by the influential group of mathematicians that worked beginning in the late 1930s under the common pseudonym of Nicolas Bourbaki. Like Moore, Bourbaki also came up with a modified version of Hilbert's mathematical conceptions, including the use of the axiomatic method [Corry 1998]. Bourbaki's views became highly influential in training of research mathematicians all over the world, especially via their famous series of textbooks Éléments de Mathématique [Corry 2007]. This influence transpired also in various ways into the realm of French school teaching with reforms introduced in the late 1960s, especially through the work of the "Commission Lichnerowicz", with the added influence of the ideas of Jean Piaget, that were considered at the time as mutually complementary with those of Bourbaki, via the connecting link of the notion of "structure" that arose in both mathematics and developmental psychology [Charlot 1984]. As a matter of fact, Bourbaki's influence was also felt in the American context, especially through the figure of Marshall Stone (1903–1989). A detailed account of this interesting and complex trend of ideas will have to be left for a future opportunity.

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