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 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.

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 that animated 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, 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.

Hilbert's Axiomatic Method

In 1899 the Göttingen mathematician David Hilbert (1862–1943) published his ground-breaking 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, starting with 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 so that postulates comprising their basic properties might be defined on them. Rather, they are introduced as undefined, basic terms and then 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 a main reason for a widespread,

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* Editor's note: In this issue of the Journal, we are publishing an interview with Henry Pollak, which also includes a discussion of the history of New Math. The Journal will be happy to publish other articles on the history of New Math. Broad discussion is welcome. It goes without saying that the views of the editors do not necessarily coincide with the views of the authors of the articles published in the journal.

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's putative conception of mathematics, 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, p. 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 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, p. 127)

This latter quotation is of particular importance for the purposes of the present article, since it suggests 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, there is no evidence that Hilbert ever saw axiomatics as a possible starting point to be used for didactical purposes, and certainly not in elementary and high-school education. Rather, it should be applied only to existing, well-elaborated disciplines, as a useful tool for clarification purposes and for allowing the further development of such theories.

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. Moreover, Hilbert thought 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 fully formal, 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 from what the term came to signify after the 1930s, 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 the decades that followed. 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.

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 a seminar in Chicago based on the book, where special attention was devoted to the possibility of revising Hilbert's proofs of independence. At his time, Moore realized that Hilbert's system contained a redundancy involving one axiom of incidence and one of order (see Parshall & Rowe, 1991, 372–392). Hilbert took notice of this mistake found in his analysis of the axioms, as well a handful of additional similar ones found over the next few years, and subsequent editions of the *Grundlagen* were corrected accordingly. But it is important to stress that these were minor, technical errors that Hilbert did not see as enormously affecting the main thrust of his presentation of geometry. For him, the real focus of interest lay in the interrelation among the various groups of axioms, rather than among the individual axioms across groups. For him, the groups corresponded to the isolable basis of our spatial intuition and the main task of his axiomatic approach was to show the way in which they logically interacted to create the body of geometric knowledge. Thus, the *Grundlagen* is a book on geometry, not on axiomatics, and the latter was just a means to enhance our understanding of the former rather than an end in itself.

But in the case of E. H. Moore, his students at Chicago, and some other contemporary USA mathematicians, their study of the *Grundlagen* led to development of a point of view that diverged from Hilbert's in this significant yet subtle matter: they turned the analysis of systems of axioms into a field of intrinsic mathematical interest in which the requirements introduced by Hilbert oriented the research questions and afforded the main technical tools to deal with them. Thus for instance, in an article of 1902, the Harvard mathematician Edward Huntington (1847–1952) 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. 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 a trend that became known as "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). They proved no new theorems about, say, groups, nor did they restructure 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, none of these systems was typically adopted in subsequent research in its respective discipline 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

$$a * e = e * a = a. \qquad (+)$$

Postulational analysts showed that if one assumes associativity, and also that $e * a = a$, then the left hand side of (+) also follows. And yet, textbooks in algebra continued to introduce the concept of groups by referring to condition (+). 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 to articles published in the USA as part of this trend or pursued anything similar to it.[3]

The Moore Method of Mathematical Education

In 1902, while still a graduate student in Austin, Texas, R. L. Moore was able 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 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. He developed and consistently followed a unique approach to teaching that became variously known as the "Moore Method," the "Texas Approach," or the "Discovery Method." He actively looked for talented students and trained them following this approach. In this way, Moore directed 50 Ph.D students who can claim now more than 1,678 doctoral descendants. Many of them continued to teach with a devotion similar to that of the master, and applying methods similar to his (Parker, 2005, 150–159). Many became prominent members of the USA mathematical community.

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:

> 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. (Jones, 1977, 274–275)

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 schematically 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 lectures in class
- ∞ Fully axiomatic presentation of the mathematical ideas, with very little external motivation

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."[4]

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 of 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 physicists. Nor did he ever promote its use as a convenient approach for high-school or primary instruction. But even at the level for which he intended it, it is important to stress that not everyone shared his enthusiasm for this method. Indeed, Moore was roundly criticized by students as well as by established mathematicians from the early stages when he began to conceive and promote it. Thus, for instance, Moore himself reported that in the early twenties, during a summer visit to Chicago, he discussed effective methods of teaching mathematics with E. H. Moore and Dickson. R. L. Moore explained the approach he had then started developing as a young teacher at the University of Pennsylvania: posing questions or theorems for students and insisting that they settle them on their own. Assistance of any sort, including conversations with fellow students and searching in books, were strictly forbidden. Students should rely on their own capabilities. Dickson, from what we know, "tended to quickly deride that approach, but E. H. Moore, as was his wont, said little" (Traylor 1972, p.92). In fact, Dickson and R. L. Moore represented in many senses different conceptions of mathematical research and teaching. Also, while mathematical scholarship was for Dickson a leading value in his approach to research and teaching, for R. L. Moore it played a minor role, and he considered it unnecessary and perhaps even damaging in relation with the training of aspiring research mathematicians.[5]

More revealing and significant are the criticisms voiced by those students that never found Moore's method and personal approach to be acceptable to begin with.[6] In fact, similar criticisms can be found in the words of some who can be counted among the success stories of the Moore Texas school. There is for instance Mary Ellen Rudin, who ranks high in the list of his distinguished students. 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:[7]

> 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, however, 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.

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.

To summarize this brief account of Moore and his method, I would like to stress how his influential and at times controversial conceptions and approach—both as a researcher (within the trend of postulational analysis) and as teacher (along the lines of his method)— derived directly from Hilbert's ideas but at the same time took a peculiar turn that led to practices deviating from Hilbert's own in essential ways. As will be seen in the next section, a similar deviation can be found at the basis of some leading ideas of the New Math movement.

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 at least since 1951 in the context of the University of Illinois Committee on School Mathematics (UICSM), 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 School Mathematics Study Group (SMSG), recently established at Yale. Under his very active leadership, an accelerated process was initiated that culminated in the teaching revolution usually known as the "New Math."[8]

In a short article it is impossible to make full justice to the complexity of the New Math movement, its origins, evolution, principles and impact, or the ways in which the concept applies differently to primary and secondary education at various times (to mention just one important nuance). For the purposes of my discussion, however, it will suffice to describe it roughly as a phenomenon that took place in the period 1955 to 1975 (I will be considering here only the American case). It attempted to introduce into textbooks and classrooms the language of set theory and logic, and to present algebra and geometry along the lines of recent, abstract trends, while putting a strong emphasis on the axiomatic approach as then conceived. At the same time, it also promoted a stronger emphasis on topics that were less pursued theretofore in secondary schools, such as statistics, probabilities, and combinatorics. Initially embraced with great enthusiasm, it later came under strong criticism and retrospectively was considered a failure.

Among the guidelines and principles that often were mentioned and discussed in relation with the New Math movement, I would like to stress the following, fundamental four:

- 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"

It is not difficult to see the similarity between these principles and some leading ideas of Moore's Method. It does not seem too risky or farfetched to surmise that the former were inspired, at least partially, by the widespread, perceived success of the latter 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 expressed his desire not to be regarded as a pedagogue (Anderson & Fitzpatrick, 2000). Nor I am claiming that Moore's personal influence was instrumental in directing the activities of SMSG. Rather, as I said in the introduction, my aim here is to call attention to certain ideas arising with Moore didactical practice, which became central to mathematical discourse at the time in the USA and that permeated the background of the discussions that led to, and accompanied, the development of the New Math movement. Whether encouraged by mathematicians at various USA universities or promoted by high-school teachers directly involved in the preparation and implementation of study programs in mathematics, the pervasiveness of such ideas are easily recognizable therein and should be paid attention to when trying to make sense of the historical development of the movement. In particular, although I do not claim to be able to establish at this point a direct, causal connection with his activities at SMSG, one cannot overlook the fact that the deepest mathematical influence on Begle's career came from Raymond A. Wilder (1896–1982). Indeed, before completing his Ph.D degree at Princeton under Solomon Lefschetz (1884–1972), Begle studied topology with Wilder in Michigan, and topology was 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). Thus, Begle provides an obvious, possible link in the genealogy of ideas that connects the Moore Method and the rise of New Math.

A glance at some prominent documents related to the New Math movement and the debates around it provide interesting evidence about the issues that concerns us here. Thus, for instance, in 1955 a Commission of Mathematics was appointed by the College Entrance Examination Board (CEEB). This latter institution had been established in 1900 by a consortium of private high schools and colleges in order to attempt to establish uniform entrance requirements for universities. In 1959 the Commission, chaired by Princeton mathematician Albert W. Tucker (1905–1995), published a report under the title of "Program for College Preparatory Mathematics" (CEEB, 1959). This report was highly influential in the eventual development of the New Math (Fey, 1978) and it clearly echoes many central motives of the Moore Method (though not exclusive of it) among the principles that should be followed in establishing a new model curriculum for "college-capable" high school students.[9] Thus, for instance, in suggesting the need for an "increased emphasis upon algebra and for instruction oriented toward a more contemporary point of view," the report stated that:

> One way to foster an emphasis upon understanding and meaning in the teaching of algebra is through the introduction of instruction in deductive reasoning. The Commission is firmly of the opinion that deductive reasoning should be taught in all courses in school mathematics and not in geometry courses alone.

The preferred way to reach the aims of the suggested program was through an emphasis on independent student's work and discovery. Still, the proponents paid due lip service to the possibility of accommodating other approaches:

> Members of the Commission would decry an authoritarian approach to method and practice, but a teacher who believes that such an approach is most effective may present this material in the same way that he has, presumably, taught the traditional content. Most if not all of the Commission members would prefer to see a developmental approach, which would encourage the student to discover as much of the mathematical subject matter for himself as his ability and the time available (for this is a time-consuming method) will permit.

Additional, illuminating evidence for the present discussion can be gathered from the symposium report *The Role of Axiomatics and Problem Solving in Mathematics*, published by the Conference Board of the Mathematical Sciences in 1966.[10] The report was intended for submission to the International Commission on Mathematical Education held as part the Moscow International Congress of Mathematicians. The coordinator of the volume was E. G. Begle, and among the editors was Moore's student Burton Jones. Ideas derived from the Moore Method or from the way it was perceived are clearly manifest in many of the texts comprising this collection. For lack of space I bring here only a few examples.

Frank Allen was a high school teacher who participated in SMSG, and was President of the National Council of Teachers of Mathematics in the years 1962–

1964. His contribution to this collection displayed the most extreme form of promotion of axiomatics as a main principle of mathematical school education that would help pupils "acquire a deeper understanding of elementary mathematics." He believed that the adequate use of the axiomatic method would develop the student's intuition and ability for mathematical discovery, and in arguing for this, he used a rhetoric reminiscent of Burton Jones' description of the Moore Method as quoted above. He thus said (Allen, 1996, 11-12):

> Those who believe that teachers should encourage the development of intuition and the construction of plausible arguments should have no quarrel with this axiomatic method. Every formal proof is preceded by many introductory exercises, experiments, and conjectures. Many plausible arguments are presented by both teacher and pupil.
>
> Those who carry the banner for 'discovery' and for 'multiple attack' on problems should be particularly enthusiastic about the axiomatic method. As noted earlier the multicontrapositive concept 10 … suggests as many as n+1 different attacks on the proof of a theorem the hypothesis of which is a conjunctive statement having n clauses. Some of these may be very easy to prove while others are difficult or even impossible. Students are intrigued by the problem of selecting the one that is easiest to prove and by the fact that one proof will suffice to establish n+1 mutually equivalent statements. After a student has verbalized all of the n (partial) contrapositives of a theorem having n clauses in the hypothesis, he begins to understand what the theorem says.

Allen's extreme enthusiasm was shared by few, and it was criticized especially by some of the mathematicians that contributed to the collection. Thus, for instance, R. Creighton Buck (1920–1998), who was chairman of the mathematics department at the University of Wisconsin at Madison. While approvingly referring to the CEEB Commission report of 1959 call for paying "more attention to the deductive structure of algebra and for a greater reliance upon general principles rather than upon special tricks," Buck also voiced a strong criticism about possible excesses related to this approach (Buck, 1966, 20):

> However, in the hands of some who perhaps do not understand the role of axiomatics in mathematics, these points have been exaggerated and carried to extremes that are certainly unwise and probably harmful. Unfortunately, we as mathematicians are at fault in that we have not communicated our attitudes toward our subject to the general community. Too often, we have allowed others to speak in our behalf, and in so doing have allowed a distorted picture of the nature of modern mathematics to be widespread. A concern for axiomatics represents only a small portion of the activity of a professional mathematician, and even less for the professional scientists for whom mathematics is a tool.

Interestingly, Buck's criticism of this extreme views stemmed from a conception of the essence and value of the axiomatic method that was very close to Hilbert's original ones, but it is evident that he was critical of what he thought to be Hilbert's negative influence. Thus, in Euclidean geometry as Buck conceived it, axioms were not arbitrary rules in a formal game with signs devoid of actual content, but rather a way to summarizing experience or experiment. One formulates these axioms in a rigorous fashion just in order to be able to deduce from them with full confidence things that are not so easily experienced directly, or that are too many to memorize as facts. At the same time, he criticized "modern logicians" (among whom I surmise that he was including Hilbert) for using axioms as if they were no different in principle than the description of the legal moves of a bishop in chess. While such an approach might be necessary and valuable for the study of matters like consistency, the investigation of whether some of the axioms of Euclidean geometry are deducible from others would be, for children in school, an arid study. Still in accordance to Hilbert's views, Buck stated that time and effort spent "proving the obvious" are a hindrance to progress, and he added: "The course in geometry should be a study of geometry, not abstract axiomatics for its own sake. Go as quickly as possible to the theorems on concurrence. Prove that the process for constructing a pentagon works..."

Another mathematician contributing to the volume was a distinguished Moore student, Gail S. Young (1915–1999). Like Buck, he believed that teachers retrained along the principles of "rigorous mathematics" did not master its essentials, and were led to overemphasizing axiomatic rigor in their teaching and distorting its real value. "The real spirit of contemporary mathematics," he thought, "is that of creative understanding, of abstraction for greater clarity of thought and ease of proof, or experimental study of the relatively concrete. It is not rigorous deduction of theorems from fixed postulate sets. That is a tool, not the goal."

As a last example I would like to quote from the article by Peter Lax, who was among the most vocal critics of the New Math.[11] It is interesting to see in his criticism how he identifies this educational program 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. These are some excerpts of his contribution to the volume (Lax 1996, 113–116):

> [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 pedantic, 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 explicitly mentioned by Lax as the originator of the views that he was disapprovingly describing, but as in Buck's article and in many 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 Hilbert's mathematical legacy.

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, in a version of the new approach to axiomatics that diverged from Hilbert's. Among the important sources of ideas that inspired the New Math was a certain perception of the Moore Method and the attempt to apply to school mathematics what this method had considered to be of high value in the training of research mathematicians.

Educational reforms in the spirit of New Math were implemented at roughly the same time in Europe. For reasons of space I will not be able to develop here in detail a similar, parallel argument for Europe as I did for the USA. Nevertheless, I would like at least to outline it, especially for the French case. 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, 2008). 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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Notes

1. For a detailed account of the background and development of Hilbert's axiomatic approach see (Corry, 2004; 2006).
2. For details on the American School of Postulational Analysis, see (Corry, 1996 (2004), 172–182).
3. Of the very few original contributions to postulational analysis in Germany, the first was by Abraham Halevy Fraenkel (1891–1965) in 1912, when he was still a young graduate student. This led to the abstract definition of ring. See (Corry, 2000).
4. Quoted in (Parker, 2005, vii).
5. On this point see (Corry, 2007).
6. Moore had a rather conflict-prone personality that won him many enemies. See (Corry, 2007).
7. The next three quotations are taken from (Albers and Reid, 1988).
8. My main source of information and quotations concerning New Math comes from the Website of Ralph Raimi, at the University of Rochester. Raimi has posted an elaborate draft version of an unpublished book on the history of New Math. See (Raimi, 2005). On the origins of New Math and the work of Beberman, see also (Usiskin, 1999).
9. For information on the report, and the passages quoted here, see http://www.math.rochester.edu/people/faculty/rarm/ceeb_59.html#_ftn3.
10. (Begle et al (eds.), 1966). The quotations here are taken from http://www.math.rochester.edu/people/faculty/rarm/axiomatics.html.
11. Of course, the most vocal critic was Morris Kline (1908–1992). See, e.g., (Kline, 1958), (Kline et al, 1962). Kline also contributed to the 1966 collection. For a reply to Kline's early criticism, see (Meder, 1958).