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THE AXIOMATIC METHOD IN THEORY AND IN PRACTICE

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Abstract
In this contribution, views are confronted and critically evaluated concerning the place and cogency of an axiomatic method, seen as a framework for mathematical theories, versus its actual use in mathematicians' ordinary proof-practices.

It is a fairly common view that mathematicians derive theorems from axioms by using valid rules of logic[1], thus conferring validity upon the propositions embodied in theorems. On the surface of it, this view might seem rather reasonable, judging from the customary style of mathematical texts in the 'theorem/proof' format. And what could be counted as validly proving, by the very meaning of the word 'proof', unless it consists of following step-by-step rules or axioms of logic?

The trouble with this view is that lacks in textual evidence from the day-to-day proof practice of mathematicians, for nor rules or axioms of logic are customarily cited in the course of a proof[2], and most mathematicians who are not logicians could hardly name any rule or axiom of logic and relate them to their actual proof practices. Wouldn't it be more reasonable (and historically correct) to hold that the rules formulated by logicians only encode what mathematicians have always been doing in validly proving theorems, and not the other way around that mathematicians proceed by following step by step the rules and axioms of logic?[3]

[1] Thus, the mathematical physicist David Ruelle (2007, p. 3) writes: "If you define precisely a set of axioms and rules of logical deduction, you have all you need to do mathematics."

[2] The only exception being perhaps an occasional reference to the axiom of choice (AC), but I do not consider AC as an axiom of logic.

[3] Here is (hopefully) an amusing analogy: Physicists and astronomers have set up differential equations to describe the motions of planets. But planets do not, so to speak, consult these equations in order to know where to go!

Granted, however, that deduction plays a key role in mathematical practice — though it does not describe it exclusively, we are led to the following considerations. In the systematization of knowledge by a network of propositions, in which a given proposition is proven on the strength of previously proven propositions and /or accepted definitions, to avoid infinite regress, there ought to be undemonstrated propositions and undefined terms upon which the network is seated. Aristotle's *Posterior Analytics* can be considered as the first manual of epistemology with a detailed elaboration of the structure of deductive reasoning, with examples drawn from mathematics. And yet, as (Hintikka 1980) writes:

> ... there is a very real sense in which Aristotle's conception of an axiomatically built science is foreign to real mathematical methods of argumentation. Aristotle believed that the tools by means of which all deductions needed in an axiomatic science are effected is his... syllogistic logic. This assumption colors Aristotle's whole philosophical theory of the structure of an axiomatically constructed theory (as a science) as it is presented in his *Posterior Analytics*. It alienated him from mathematical practice, and led him to ideas quite foreign to what we are likely to find in mathematical axiomatics. (p. 137)

A mathematician who looks nowadays at Aristotle's syllogistics is particularly struck by the absence in it of a treatment of *conditionals*, i.e., reasonings of the form 'if... then... ', the typical mode of mathematical arguments. On this score, (Kneale and Kneale 1971) write:

> If, as we have suggested, Aristotle was interested primarily in reasoning such as we find in geometry, it would be natural for him to concentrate his attention on general propositions and definitions. Nothing in his favorite examples of demonstration would force him to consider the meaning of 'if... then'. (p. 128)

Turning to modern considerations of the axiomatic method, Robert Blanché in his little booklet entitled 'Axiomatics' (Blanché 1962) writes:

> It is now common practice for a deductive theory to be presented in the form of an axiomatized system (sometimes know as an axiomatized theory)... It is a system in which the undefined terms and the undemonstrated propositions are made completely explicit, the latter being put forward simply as hypotheses on the basis of which

> all propositions of the system can be constructed according to fixed and completely explicit rules of logic. (pp. 2–3)

But what — perhaps — can be done *in theory* does not imply that it is actually done in mathematicians' proof *practice* by explicitly referring to rules of logic to substantiate their arguments.

In an article published in the proceedings of the 2004 Rome conference entitled 'Mathematical Reasoning and Heuristics', Cellucci (2005) nailed down the issue in the following trenchant words:

> One of the most uninformative statements one can could possibly make about mathematics is that the axiomatic method expresses the real nature of mathematics, i.e., that mathematics consists in the deduction of conclusions from given axioms. (p. 137)

And a few pages later, Cellucci cites Hintikka's statement that "contrary to the oversimplified picture that most philosophers have of mathematical practice, much of what a mathematician actually does is not to derive theorems from axioms"[4]. In a similar vein, the eminent logician Solomon Feferman writes: "... mathematicians hardly mention axioms at all in their proofs in their daily practice, and some go through their entire career without appealing once to an axiom of *any* kind". (Feferman 2006, p. 143).

In a dissenting voice to that which was affirmed by Cellucci, Gianluigi Oliveri read a paper entitled 'Do we really need axioms in mathematics?' at the very same 2004 Rome conference and concluded his lecture emphatically with the statement "Yes, indeed, we do need axioms in mathematics".[5]

Now what are we to make of these various affirmations? As a matter of fact, I happen to agree — in spite of the apparent incompatibility — with what *each* of the just cited authors has maintained. Specifically, there is *no* contradiction in what Cellucci and Oliveri have argued for. It just happened that there is none, nor has there ever been a unique conception what axioms are; or stated more precisely, what class of principles counts as axioms and what is taken to be their specific epistemic function in mathematics. Thus, for instance, the term 'axiom[a]' does not occur in Euclid; however, Proclus refers to Euclid's 'common notions' — the *koinai ennoiai* — by the name of axiomata[6]. Reciprocally, what Archimedes calls 'axiomata' in Book I of

[4] cf. Hintikka (1996, p.95).

[5] *Vide* Oliveri (2005).

[6] See the discussion by the philologist and historian of mathematics Szabó (1969, pp. 378–389; 412–416).

'On the sphere and cylinder' was translated into Latin by Heiberg as *definitiones*. In short, the dividing line between axioms and definitions has never been sharply maintained, and Poincaré, in discussing non-Euclidean geometries, considers axioms to be just *disguised definitions*[7]. There are, moreover, types of axioms that are not even 'disguised' definitions, they are definitions *tout court*: namely, those that define various algebraic structures, such as groups, rings, vector spaces, Boolean algebras, and the like. This is characteristic of a process starting from the middle of the 19th, known as the axiomatization of abstract structures. Feferman refers to these axioms as *structural axioms* (to be distinguished from *foundational axioms*). In his words:

> When the working mathematician speaks of axioms, he or she usually means those of some particular part of mathematics such as groups, rings, vector space, topological spaces, Hilbert spaces, and so on. These kinds of axioms have nothing to do with self-evident propositions, nor are they arbitrary starting points. They are simply definitions of kinds of structures which have been recognized to occur in various mathematical situations. I take it that the value of these kinds of *structural axioms* for the organization of mathematical work is now indisputable.
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> In contrast to the working mathematician's structural axioms, when the logician speaks of axioms, he or she means, first of all, laws of valid reasoning that are supposed to apply to *all* parts of mathematics, and, secondly, axioms for such fundamental concepts as number, set and function that underlie *all* mathematical concepts; these are called *foundational axioms*.
>
> The foundational axioms correspond to such basic parts of our subject that they hardly need any mention at all in daily practice, and many mathematicians can carry on without calling on them even once. Some mathematicians even question whether mathematics needs any axioms *at all* of this type; for them, so to speak, mathematics is as mathematics does. According to this view, mathematics is self-justifying, and any foundational issues are local and resolved according to mathematical need, rather than global and resolved according to possibly dubious logical or philosophical doctrines.
> (Feferman et al. (2000), p. 403; italics in original).

[7] In his words: "... les axioms de la géometrie ... ne sont que des définitions déguisées", cf. Poincaré (1891). Reprinted in Poincaré (1902, pp. 63–76).

Essentially, the *structural axioms* are definitional axioms, or that might equally called *frame axioms*, for they fix the frame of what is 'inside', but they do not serve as a basis for the purely *logical machinery* in establishing the theorems of the theory that they enclose. For instance, group theory started with the study of permutations and led to the concept of permutation groups under the operation of 'multiplying' permutations. When the general abstract concept of group was formulated toward the end of the 19^{th} century in terms of the 3 or 4 axioms what we find now in textbooks on group theory, these axioms just *define* what is meant by a group[8]. One of the first theorems in this process stated that every finite group — as abstractly defined by the group axioms — is isomorphic to a group of permutations. This justified the 'axiomatization' as being correct in the sense of capturing abstractly the familiar structure of a permutation group. The ensuing abstract concept of a group, extended now to include infinite groups, enabled mathematicians to study group structures in a wide variety of contexts. The large number of significant theorems mathematicians have proved and continue to prove in group theory are not deduced from the definitional axioms that only fix the concept of a group, and as such, are only stated in the first few pages of elementary textbooks. To take one of numerous examples, in the ordinary proofs of the well-known Sylow theorems[9], no mention is ever made of the group axioms; the proofs proceed through astute algebraic constructions and arguments to justify that the steps accomplish what is claimed for them[10]. It certainly took more than just some astute algebraic constructions, as in the proof of the Sylow theorems, when it came to the monumental work of scores of group theorists in the classification of all finite groups. In the classification project, extensive novel conceptual machinery had to be developed; concepts and techniques from other branches of mathematics had to be used or transformed — as it was the case with the proof of the Fermat conjecture by Andrew Wiles and his collaborators. But in neither case had any axioms to be introduced, nor did the proofs proceed *textually* by deduction from the axioms of, say, ZFC set theory. Surely, in many branches of mathematics

[8] See (Wussing 1984, Part III).

[9] *Vide*, for example, (Lederman 1957, pp. 126–134).

[10] By the way, this is the typical mathematical method we find ordinarily in proofs, a far cry from the popular view in some philosophical circles that proofs consist of deriving theorems from axioms according to well-defined rules of formal logic. Here comes in the distinction between ordinary, oft called 'informal proofs' in the day-to-day proof practice of mathematicians, and formal derivations in the specified symbolism of a formal logical calculus that resemble a computer program. Such formal derivations are never published in research journals, but can serve didactic purposes. See Rav (2007) for a detailed discussion with examples.

(though not in all) mathematicians nowadays use the *language* of set theory or that of category theory or in combined way (and managed quite well before either one was invented). But other than in formalized derivations or in foundational work, the axioms of a set theory codify for actual practice the 'grammar' of that set-theoretical language, in particular, the *constructions* it allows for when studying abstract structures. To continue with the analogy, just as grammarians and linguists study and compare grammars, set-theorists prove theorems *about* axiomatized set theories, study the relative strength of various axiom systems and the like; here, in this technical work, axioms are indispensable as (Oliveri 2005) has stressed. But, in the course of their proofs, set-theorists proceed just as mathematicians do in other specialties; they do not derive explicitly their proofs *from* the axioms of ZFC.

Unlike the indispensable place of axioms in foundational studies and mathematical logic in general, when one looks at other branches of mathematics, oft referred to as mainstream mathematics, it is striking what subsidiary role, if any, is played by axioms other than in geometry (to be discussed later) or in the introduction of structural axioms that define the subject matter of a theory. Indeed, few mathematical theories have ever been axiomatized. Think about the remarkable growth of mathematical knowledge from the Renaissance on, such as the invention of coordinate geometry in the 17$^{\text{th}}$ century, to be followed by the creation of the infinitesimal calculus, its numerous subsequent offshoots like the study of ordinary and partial differential equations, and up to all that goes now under the heading of real and complex analysis. Typically, from its start, analysis stands out as an example of a non-axiomatized edifice, whether seen as a unity or as a conglomeration of its unaxiomatized subtheories. In objecting to my claim that analysis has never been axiomatized as a deductive theory, the referee has maintained that "Dedekind's essay on continuity and irrational numbers (Dedekind 1872) was intended as an axiomatization of analysis". The issue deserves closer scrutiny, for it is instructive to distinguish between founding the infinitesimal calculus (as it was called) on a *rigorous basis*, as it took place progressively throughout the 19$^{\text{th}}$ century, and, on the other hand, what would be developing real and complex analysis on an *axiomatic basis*. The rigorization of the calculus just required giving precise definitions of its key concepts, such as function, limit, continuity, derivative, integral, and the like; and then using these definitions in an actual *analytic proof* rather than basing proofs on geometric evidence and intuition. An axiomatization of analysis, however, would consists of giving analysis a *closed form* by fixing an explicit set of axioms from which the theorems of analysis could be logically derived. My position is that this has never been done; moreover, I'll give further below an argument why an axiomatization of analysis or any other major branch of mainstream mathematics, for that matter, would practically not be feasible.

The term ‘analytic proof’ that I have used above was borrowed from the title of Bolzano’s (1817) pamphlet *Rein analytischer Beweis. . .* , in translation by (Russ 1980), “Purely analytic proof of the theorem, that between each of two roots which guarantee an opposite result [in sign], at least one real root of the equation lies”. This states what is now called the Intermediate Value Theorem for continuous functions.

> Bolzano argued — writes Edwards (1979, p. 308), that the intuitive geometric proof — a continuous curve must somewhere cross any straight line that separates its endpoints — is based on an inadequate conception of continuity. [Here Bolzano gives what is now the standard definition of a function being continuous at a point].
>
> As a crucial lemma, Bolzano asserted that, if M is a property of real numbers that does not hold for all x, and there is a number u such that all numbers x<u have property M, then there exists a *largest* U such that all numbers x<U have property M. In his attempted proof by the now-familiar bisection method, he produced a “Cauchy sequence” $\{u_n\}$ intended to converge to the desired U. Although he (and later Cauchy) stated correctly what is now called the “Cauchy convergence criterion” [$\{u_n\}$ converges if and only if, given $\epsilon > 0$, $\mid u_m - u_n \mid < \epsilon$ for m and n sufficiently large], he could not (nor could Cauchy) supply a complete proof for lack of a completeness property of the real number system.

Indeed, what was lacking both in Bolzano and in Cauchy’s work was an appropriate definition or construction of the real number system, and, on the basis of such a definition, prove the completeness property of the real numbers, i.e., that every Cauchy sequence of real numbers has a real number as its limit. The first rigorous construction of the system of real numbers was given by (Dedekind 1872) in his essay “Continuity and Irrational Numbers”. There, he presented his theory of irrational numbers as defined by cuts — now referred to as *Dedekind cuts* — that enabled him to *prove* the completeness property of the real numbers (called by him the principle of continuity). Dedekind than shows in closing that this principle is equivalent to “one of most important theorems” of infinitesimal analysis, stating that “if a magnitude x grows continually but not beyond all limits, it approaches a limiting value”, and further proves it to be equivalent to Cauchy’s convergence criterion, thus completing the gap left out in the work of Bolzano[11] and Cauchy. The last sentence of Dedekind’s 1872 reads as follows: “These examples

[11] Dedekind later stated that he did not know at that time of the work of Bolzano.

may suffice to bring out the connection between the principle of continuity and infinitesimal analysis." An axiomatic foundation of analysis? Hardly! The term *axiom* appears only once in Dedekind's 1872 essay[12], and that in reference to the *continuity of the geometric straight line* that served as the intuitive background for his theory of cuts. Nothing more to be said on that score.

The year 1872 also saw the appearance of other publications dealing with constructions of the real number system, notably (Kossack 1872), (Heine 1872), (Méray 1872), and the now better known theory of (Cantor 1872) with a definition of the real numbers in terms of Cauchy sequences of rational numbers, called "fundamental sequences" by Cantor. The details are well known and need not detain us here[13]. Just one comment. Cantor, in the manner of Dedekind, speaks here only once about an *axiom*, namely, that to every 'Zahlengröße' i.e., real number, in his terminology, there corresponds a unique point on the geometric straight line. (The converse holds only for the *equivalence classes* of fundamental sequences (modulo null sequences), a concept Cantor did not have). Thus, one speaks nowadays of the *Dedekind-Cantor Axiom*, stating that there is a bi-unique correspondence between the points of a straight line and the totality of all real numbers. The Dedekind-Cantor axiom has obviously no bearing on actual proof practices in real analysis but serves as an intuitive guide of considerable pedagogical value. Technically, the crux of the constructions by Dedekind and Cantor resides in their respective proofs that the real number system R is *complete*, a characteristic shown by Dedekind to be equivalent to the least upper bound property of R. From there on numerous important theorems of real analysis can be deduced.

In the spirit of the now prevailing abstract structuralist approach, the respective constructions of the real number system, starting with the rational numbers as building blocks, can be streamlined as follows. Starting with the definition of an ordered field and proving that every such field contains a subfield isomorphic to the field of rational numbers, an ordered field F is then said to be *complete* if and only if every non-empty subset S of F that contains an upper bound has a least upper bound. Does one now have to assume as a postulate or axiom that there exists a complete ordered field? No, for Dedekind's theory of cuts, for instance, can then be used to prove that

[12] On p. 12 in the 1963 Dover reprint.

[13] But it is worth mentioning, nonetheless, that a particularly simple modern descendant of Cantor's theory of the real numbers is given in the constructive analysis of (Bishop 1967, p. 15). In Bishop's theory, a real number is just a *regular sequence* of rational numbers, where such a sequence $\{x_n\}$ is said to be regular if $\mid x_m - x_n \mid \leq m^{-1} + n^{-1}$ (m, n natural numbers).

there exists a complete ordered field; one then proves that any two complete ordered fields are isomorphic. The unique (up to isomorphism) ordered field thus obtained is then 'baptized' as the field of real numbers R[14]. Notice the logical order in this procedure.

A different approach to defining the real numbers system was undertaken by Hilbert, the champion of axiomatization. A preliminary set of axioms for the arithmetic of the reals, running in parallel, in a sense, to the axioms of geometry, was first given by Hilbert in the Foundations of Geometry of 1899 (§13), and further elaborated in (Hilbert 1900). I follow here freely the translation of the latter as given in (Kline 1972, pp. 990–992). Hilbert starts with the undefined term 'number', denoted by a, b, c, $\ldots$, and states four groups of axioms: I. Axioms of connection. II. Axioms of calculation. III. Axioms of order. IV. Axioms of continuity, with subparts IV_1 the Axiom of Archimedes, and IV_2 the Axiom of completeness, that states the following: "It is not possible to adjoin to the system of numbers any collection of things so that in the combined collection the preceding axioms are satisfied; that is, briefly put, the numbers form a system of objects which cannot be enlarged with the preceding axioms continuing to hold".

> Hilbert points out — writes Kline (op. cit., 991–992) — that these axioms are not independent; some can be deduced from the others. He then affirms that the objections against the existence of infinite sets are not valid for the above conception of real numbers. For, he says, we do not have to think about the collection of all possible laws in accordance with which the elements of a fundamental sequence (Cantor's sequences of rational numbers) are formed. We have but to consider a closed system of axioms and conclusions that can be drawn from them by a finite number of logical steps. He does point out that it is necessary to prove the consistency of his set of axioms, but when this is done, the objects defined by it, the real numbers, exist in the mathematical sense. Hilbert was not aware at this time of the difficulty in proving the consistency of axioms for real numbers.

To Hilbert's claim that his axiomatic method is superior to the genetic method[15], Bertrand Russell replied that the former has the advantage of theft over

[14] See (Birkhoff and MacLane 1941), chapters II and III for details, or any standard textbook on modern algebra.

[15] i.e., constructing the various number systems from the bottom up, starting with the natural numbers.

honest toil. Methodologically speaking, indeed, constructivists of all shades and logicists of old (and perhaps even of new) have a definite preference for the genetic method, for more information is yielded by constructive methods, though at the price of more toil; in other words, more is put into proofs and less into (oft unprovable) postulates. Thus, Dedekind's theory of cuts — a construction — enabled him to prove the continuity principle and the latter in turn, proven equivalent to the completeness property of the real numbers and the least upper bound principle. In Hilbert's axiomatic approach, however, the latter property of R would have to be proven in a different set-up by other methods and perhaps by appealing to some further axioms.

In a masterful account, (Landau 1930) has developed by the genetic method the hierarchy of number systems, starting with the Peano axioms for the natural numbers and leading all the way up to the complex numbers. No further axioms than those of Peano are used in this work, the arithmetic and order properties of each system are then developed by strict definitions and rigorous proofs, with a minimum of verbal explanations, a style for which Landau is well known. Landau treats the real numbers following the method of Dedekind cuts, culminating in a proof of the completeness property of the real number system in Theorem 205 (p. 113) that Landau justifiably calls Dedekind's Main Theorem.

Let us take stock. Whatsoever road one takes for laying the foundations solely for the real and complex number systems, be it even through an axiomatization of R in the manner of Hilbert, this can not be counted as an axiomatization of real (and complex) analysis; the latter would require a systematic development of analysis as a closed system based on an explicit statement of a set of axioms from which its theorems can be derived by purely logical deductions. And as I maintained at the outset of this section, mathematicians have never done this[16]. Indeed, a strict axiomatization of analysis, or any field of mainstream mathematics, for that matter — certain geometries excepted — would be counter-productive, and essentially not feasible. The reason is simple, stemming from what can be called the *transfer of technologies*. The issue is this. Ideas and concepts that were developed independently in a particular area of mathematics are frequently used and applied in a different context, much to the enrichment of the latter. Encapsulating a major branch of mathematics, such as analysis, for example,

[16] And may I just add, though my main argument does not depend on this, that deriving theorems from axioms would conflict with the basic conception of constructive analysis in the spirit of (Bishop 1967) and his followers. As Bishop puts it: "Our program is simple: to give *numerical meaning* to as much as possible of classical abstract analysis" (From the preface; emphasis added)

within a rigid axiomatic framework, would bloc beforehand such unforeseeable developments, including re-proving theorems by different methods, as mathematicians often do[17].

Let me illustrate the interplay of ideas and methods from diverse branches of mathematics in the development of real and complex analysis as exemplified in the textbook of (Rudin 1974)[18]. Thus, throughout the book one finds significant employment of concepts and methods coming from such diverse fields of mathematics as general topology, linear algebra, homotopy theory, group theory (in order to define modular functions), and of course, standard set-theoretical constructions, including an appeal to Hausdorff's Maximality Principle in two proofs[19]. In an appendix, Rudin cites the Axiom of Choice (AC) and proves that it implies the Hausdorff Maximality Principle (and mentions that it is equivalent to it). This is the only place where an axiom is cited in Rudin's book. But, here is the crux: AC is not an axiom of real and complex analysis! Such a use of set-theoretical tools is a royal example of a "transfer of technologies", typical of mathematicians' proof techniques.

Number theory, as a major branch of mainstream mathematics, is another example of a non-axiomatized theory worth mentioning[20]. Here, as in the previous example of real and complex analysis, the use in proofs of a multitude of concepts and methods that were developed in other areas of mathematics block beforehand any attempt of axiomatization. (And what purpose could that even serve even if it were feasible?) Elsewhere, with a tinge of humor, the following makes the point: "As a matter of fact, the Queen of mathematics — as Gauss called number theory — is rather promiscuous, opening her arms to algebraic, analytic, topological, geometrical, proof-theoretical, numerical, model-theoretic, combinatorial, and come as they may types of

[17] See the general discussion in (Dawson 2006).

[18] The book covers the material aiming at a one-year course on an advanced senor or first-year graduate level.

[19] The Hausdorff Maximality Principle asserts that every nonempty partially ordered set P contains a maximal totally ordered subset. Rudin uses it to prove that every orthonormal set B in a Hilbert space H is contained in a maximal orthonormal set in H (Theorem 4.22, p. 92). Also, the Principle is used to prove the Hahn-Banach Theorem (p. 111) that says the following: If M is a subspace of a normed linear space X and if f is a bounded linear functional on M, then f can be extended to a bounded linear functional F on M so that $\| F \| = \| f \|$.

[20] It is important to distinguish number theory, as the term is understood by the general mathematical community, from Peano Arithmetic (PA) that is a topic of study in mathematical logic and is discussed, for example, in the chapter on undecidability by (Enderton 2001)

proofs and methodologies. Categorically, the Queen disdains being degraded to the rank of a recursively axiomatisable theory. *Noblesse oblige!* ”[21].

So far my arguments for seeing where axioms do *not* come in. Now let’s see where axioms do come in. First, there are types of axioms that have been called ‘structural axioms’ or, by my preferred terms, ‘definitional axioms’, or ‘frame axioms’, for they define and fix the frame of a concept or of a theory. Typical examples of such axioms are those that tell us what is meant by terms like ‘vector space’, ‘metric space’, ‘topological space’, ‘group’, ‘partially ordered set’, ‘Hilbert space’, ‘Banach space’, ‘functional’, ‘normed linear space’, and so on. Thus, for example, in the book by Rudin cited above, where these terms and similar ones are frequently used, they are introduced and defined by sets of axioms. For the sake of a concrete illustration, the axioms for a topological space are recalled in the following footnote[22]; they tell us what a topological space is.

Let us turn now to the classical conception of axioms that has it origin in the Hellenic culture. It is generally conceded by historians of mathematics that we owe to the Greeks the organization of mathematical knowledge on a deductive basis, having its roots in the teachings of Plato (c. 428–347 B.C.E.). Thus, (Kline 1972) writes:

> Plato did affirm the desirability of a deductive organization of knowledge. The task of science was to discover the structure of (ideal) knowledge and to give it an articulation in a deductive system. *Plato was the first to systematize the rules of rigorous demonstrations and his followers are supposed to have arranged theorems in logical order...* Whether or not mathematics was already organized on the basis of explicit axioms by the Platonists, there is no question that deductive proof from some accepted principles was required from a least Plato’s time onward. (p. 45, emphasis added).

The organization of essentially all mathematical knowledge accumulated by the Greeks down to the third century B.C.E. received its classical organization in the *Elements* of Euclid. “It is quite certain — writes Kline (op. cit.) — that Euclid lived in Alexandria about 300 B.C. and trained students there,

[21] Citation from (Rav 1999, pp. 16–17), where other examples of non-axiomatized theories are discussed.

[22] A topological space is a set X and a family of subsets Ω called the open sets of the space such that the following axioms are satisfied:
1. $\varnothing \in \Omega$ and $X \in \Omega$.
2. If $\mathrm{O}_1 \in \Omega$ and $\mathrm{O}_2 \in \Omega$, then $\mathrm{O}_1 \cap \mathrm{O}_2 \in \Omega$.
3. If $\mathrm{O}_i \in \Omega$ for every $i \in I$, then $\bigcup\{\mathrm{O}_i : i \in I\} \in \Omega$.

though his education was probably acquired in Plato's Academy... The particular choice of axioms, the arrangement of the theorems, and some proofs are his as are the polish and the rigor of demonstrations. The form of presentation of proof has already been noted in Autolycus and was pretty surely used by others who preceded Euclid. Despite all he may have taken from earlier texts and other sources, Euclid was unquestionably a great mathematician". (pp. 56–57).

Be that as it may, what are we to make of the oft-repeated claim that *Euclid's Elements* have always served as the perfect paradigm of a mathematical theory based on explicitly stated axioms and definitions from which all theorems are deductively established in accordance with accepted canons of logic?

Some aspects of *Euclid's Elements* were already criticized in antiquity. But perhaps the most severe criticism in modern days comes from the pen of the algebraic geometer and historian of mathematics Abraham Seidenberg, who in his 1975 article with the challenging title 'Did Euclid's Elements, Book I, develop geometry axiomatically?' wrote:

> Historians are fond of repeating that Euclid developed geometry on an axiomatic basis, but the wonder is that any mathematician who has looked at *The Elements* would agree with this. Anyone who looks at *The Elements* with modern hindsight sees that something is wrong; but we have all been told in our childhood that Euclid had the axiomatic method, so the usual reaction is to speak of 'gaps'. This word is hardly right, though, if there is nothing there in the first place.
>
> Could it be that, by insisting on the axiomatic method, we are viewing *The Elements* from a false perspective and seeing its accomplishments in a bad light? This is precisely what I intends to prove.
>
> The Greeks of Euclid's time had the axiomatic method; Aristotle's description of it can be considered a close approximation to our own. Or better yet, one may consider Eudoxus' theory of magnitude as presented in Book V of *The Elements*: the procedure there disclosed is pretty much in accordance with our view of what an axiomatic development should be. It is known, however, that *The Elements* is a compilation of uneven quality, so that it is unwarranted to assume that Book I is written from the same point of view as Book V. (pp. 263–4)

The key point of Seidenberg's critique is that Book I is *not* an axiomatic basis for the *theorems* in Euclid but for a theory of geometric constructions.

In the summary (p. 294), Seidenberg recapitulates this point: "The construction postulates are *bona fide* and axioms in a sense: they serve to control the straightedge and compass constructions. But they are not axioms for the development of geometry and indeed, tell us nothing about space, except incidentally that there is a line on any two points".

The question related to the conceivable independence of Euclid's fifth postulate (now referred to as the parallel axiom) has lead to the invention of non-Euclidean geometries, resulting in a renewed concern with the axiomatic method in geometry and the emergence of a novel and far reaching view in the work of Hilbert. But let us look first at one of Hilbert's forerunners conception of the axiomatic founding of geometry, namely that of Pasch.

Seidenberg, in the article cited above, writes:

> The axiomatic method as understood today was initiated by Moritz Pasch[23]. This method consists of isolating from a given study certain notions that are left undefined and are expressly declared to be such, (the so-called *Kernbegriffe* in Pasch's terminology of 1926), and certain theorems that are accepted without proof (the so-called *Kernsätze*, i.e., axioms); from this initial fund of notions and theorems, the other notions are to be defined and theorems proved using only logical relations, without appeal to experience or intuitions. The resulting theory takes the form of purely logical relations between undefined concepts[24]. (p. 292)

However, Pasch says nothing further as to what ought to be understood by 'logical relations'.

The foundations of geometry occupied too the Italian school, with notable contributions by Peano (1888) and, more significantly by Peano (1889),

[23] cf. Pasch (1882/1926).

[24] In relation to the quoted passage, the referee has observed that "Pasch was very explicit that his 'points' and 'lines' are not undefined terms, but terms that are grounded in experience (1882, Introduction). While Pasch does talk about deductions being valid regardless of the meanings of the terms, he clearly has an empiricist view of geometry".

where Peano has put systematically to use the symbolism of the logical language that he had invented[25]. Other notable contributions came from the pen of Veronese (1891) and Peano's pupil Mario Pieri (1894).

Let us turn now to Hilbert. Soon after Hilbert completed his famous systematization of algebraic number theory, published as 'Die Theorie der algebraischen Zahlkörper' in 1897, known as the 'Zahlbericht', he gave at Göttingen a series of lectures on projective geometry and the foundations of Euclidean geometry. These lectures resulted in the publication of the little booklet entitled 'Die Grundlagen der Geometrie' (Hilbert 1899) that was translated into English by E.J. Townsend as 'The Foundations of Geometry' (Hilbert 1902).[26]

One can hardly sufficiently stress the novelty and impact of Hilbert's methodological conception as set forth in the 'Grundlagen' of 1899, a work that went subsequently through numerous editions. Among the key innovations in Hilbert's axiomatization of geometry in the 'Grundlagen' are the following:

- Points, straight lines, and planes are not defined *materially*, as in Euclid and as Frege would have wanted it to be as claimed by him in the exchange of letters with Hilbert[27].
- Nor is it quite correct, as some have asserted, that Hilbert's axioms are an *implicit* definition à la Gergonne[28] of the relevant geometric objects, since for Hilbert it's their mutual relations that matter and not the definition of individual geometric terms.

[25] Kennedy (1972) has claimed, with considerable justification, that Peano's 1889 axiomatization of geometry and his well-known axiomatization of arithmetic, published in the same year, establishes Peano as the father of modern axiomatics. However, the novelty of his symbolic notation was an obstacle at that time to a due recognition of Peano's axiomatizations. Thus, Poincaré referred mockingly to Peano's symbolic notation as 'peanese'. The important place of Peano as one of the fathers of the *logicist foundation of arithmetic* is now recognized in our speaking of Peano's axioms of arithmetic as *Peano arithmetic*.

[26] For the various stages and lectures given by Hilbert, leading to the 'Grundlagen' of 1899, see Toepell (1986). Hilbert's lectures on projective and Euclidean geometry prior to 1899 were published by Hallett and Major (2004). For an overall historical and philosophical perspective, cf. Cavaillès (1938; 1981)).

[27] cf. Hilbert (1971; 1899).
For a defense of Frege's views in the Frege-Hilbert controversy, see Blanchette (2007).

[28] See Gergonne (1818/19).

- Eudoxus' theory of magnitudes is reformulated as an axiomatization of the real numbers, including an explicit formulation of the axiom of Archimedes, thus leading to a distinction between Archimedean and non-Archimedean number systems.
- Metamathematical considerations are explicitly addressed, such as a proof of the consistency of the various axioms *relative* to coordinate geometry, and proof of their mutual independence.

There is an extensive discussion by Bernays in (Edwards 1967, vol. 3, pp. 497 –498) of Hilbert's axiomatization of geometry, part of his general article about Hilbert. I'll just quote here a short extract from it:

> A main feature of Hilbert's axiomatization of geometry is that the axiomatic method is presented and practiced in the spirit of the abstract conception of mathematics that arose at the end of the nineteenth century and which has been generally adopted in modern mathematics. It consists in abstracting from the intuitive meaning of the terms for the kinds of primitive objects (individuals) and for the fundamental relations, and in understanding the assertions (theorems) of the axiomatized theory in a hypothetical sense, that is, as holding *for any interpretation or determination of the kinds of individuals and for the fundamental relations for which the axioms are satisfied.* Thus, an axiom system is not regarded as a system of statements about a subject matter but as a *system of conditions for what might be called a relational structure. Such a relational structure is taken as the immediate object of an axiomatic theory.* (Emphasis added)

Hilbert's relational conception, or as we would say now, model-theoretic view, as embodied in his axiomatization of geometry, was strikingly expressed in a discussion he had with Schoenflies and Kötter at a railroad waiting room in Berlin, when Hilbert said, as related by Blumenthal (1935, p. 403): "One should always be able to say, instead of 'points, lines, and planes', 'tables, chairs, and beer mugs'"[29]. Lenhard and Otte (2002) have discussed from a wide-ranging historical and philosophical perspective the underpinning of Hilbert's *model-theoretic* interpretation of geometric axioms, as compared to the *logical-analytic* interpretation by Pasch.

[29] Original formulation: "Man muß jederzeit an Stelle von 'Punkte, Geraden, und Ebenen' 'Tische, Stühle, Bierseidel' sagen können".
Further interesting points in the comparison of Pasch's versus Hilbert's axiomatization of geometry are brought out by Contro (1975/76).

It was quite a natural step for Hilbert, after his foundational work on geometry, to go one step further and formulate a foundational view on all of mathematics based on the axiomatic method, with particular attention to the compatibility — read: consistency — of the axioms. Thus, at his famous lecture entitled 'Mathematical Problems' delivered before the International Congress of Mathematicians at Paris in 1900, Hilbert said:

> When we are engaged in investigating the *foundations of a science*, we must set up a system of axioms which contains an exact and complete description of the relations subsisting between the elementary ideas of that science. *The axioms so set up are at the same time the definitions of those elementary ideas* ; and no statement within the realm of the science *whose foundations we are testing* is held to be correct unless it can be derived from those axioms by means of a finite number of logical steps. (Italics added).

Hilbert then raises the question of the independence of the axioms and then says further:

> But above all I wish to designate the following as the must important among the numerous questions which can be asked with respect to the axioms: *To prove that they are not contradictory, that is, that a finite number of logical steps based upon them can never leas to contradictory results* (emphasis in original; citations from Hilbert (1976).

Notice in the above passages the various ingredients in Hilbert's view about the function of axioms: Their primary function is *foundational*, that is, they describe the *relations* that are the core of a given (mathematical) theory. And then in a subsidiary way, axioms have a definitional role and equally, serve a logical-deductive function. At this stage in 1900, Hilbert only speaks in generality about the need of using "a finite number of logical steps" without saying anything directly about the logical steps *per se*. But four years later, in his Heidelberg lecture (see Hilbert 1904)), a key point is stressed in the following statement:

> Arithmetic is often considered to be part of logic, and the traditional fundamental logical notions are usually presupposed when it is a question of establishing a foundation of arithmetic. If we observe attentively, however, we realize that in the traditional exposition of the laws of logic certain fundamental arithmetic notions are already used, for example, the notion of set, and to some extent, also that

> of number. Thus we find ourselves turning in a circle, and that is why *a partly simultaneous development of the laws of logic and of arithmetic is required if paradoxes are to be avoided.* (Cited from the translation in (Hilbert 1967), p. 131; emphasis added)

When we come to Hilbert's lecture at Zurich in 1917, later published as 'Axiomatisches Denken' (Hilbert 1918) ('Axiomatic thinking'), the foundational program — with its ultimate aim to prove the consistency of arithmetic — is now spelled out with its core elements. Notably among them is *the demand to axiomatize, and, within the axiomatic framework, treating mathematical proofs as objects for mathematical investigations.* Here we find the first sketch of the method of metamathematics, to be subsequently fully elaborated. The article closes with what can be called a Gloria to the axiomatic method:

> All that can ever be an object of scientific thinking, as soon as it is ripe for the formation of a theory, falls within the purview of the axiomatic method, and thereby indirectly within the domain of mathematics. In the symbols of the axiomatic method, mathematics is called upon to a leading role in science. (p. 415; my translation).

Curiously, in view of the crucial role that Hilbert attributes here to the axiomatic method, the gulf between Hilbert's later meta-theoretical conception and his own actual earlier proof practices is rather striking. For nowhere, for instance, in his number-theoretical works that are reprinted in vol. I of his collected works did Hilbert derive his theorems from any *explicitly* stated axioms and with reference to rules of logical deduction. However, in view of the so-called foundation crisis with the discovery of paradoxes in intuitive set theory, not to speak of the challenge by Brouwer of the validity of classical analysis, Hilbert's main occupation eventually turned completely to foundational questions, with the ultimate aim to put the foundational crisis to rest once and for all by his ensuing meta-mathematical method. Here, the axiomatic method is indispensable. For once mathematical theories are the very objects of metamathematical studies, one can only investigate axiomatized theories as to their consistency, relative strength, and the like. Nor could model theory have indeed a subject matter unless one studies models of axiomatized theories. But be it noticed that when logicians study axiomatized theories, the theorems they prove are not derived as such from axioms. Logicians prove theorems in the same manner as, for example, analysts develop methods in their study of differential equations. The emphasis here, as elsewhere in mathematical research, is on solving problems, to develop methods and concepts, and to argue 'logically' — informally, in the sense of Aberdein (2007) — in order to establish that the methods introduced do indeed accomplish that which was required to obtain.[30]

To conclude, as Cellucci has maintained, the axiomatic method is not *the* mathematical method, *pace* Hilbert. But, as Oliveri has argued, we do need axioms; it only depends on what type of axioms and where and when do they come in. Certainly, in the study of mathematical theories, when the theories themselves are the object of an investigation, unless such theories are axiomatized, these studies cannot even get off the ground. However, in the day-to-day actual proof practices of mathematicians, axioms are hardly ever evoked or even mentioned.

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REFERENCES

Aberdein, A. (2007). "The informal logic of mathematical proof", in B. Van Kerkhove and J.P. Van Bendegem (2007): 135-151.

Birkhoff, G. and S. MacLane (1941). *A Survey of Modern Algebra.* New York, N.Y., Macmillan.

Bishop, E. (1967). *Foundations of constructive analysis.* New York, N.Y., McGraw-Hill.

Blanché, R. (1962). *Axiomatics.* London, Routledge & Kegan Paul.

Blanchette, P.A. (2007). "Frege on consistency and conceptual analysis." *Philosophia Mathematica* (3) 15: 321–346.

Blumenthal, O. (1935). "Lebensgeschichte", in D. Hilbert (1935 III): 388–424.

Bolzano, B. (1817). "Rein analytischer Beweis des Lehrsatzes, dass zwischen zwei Werten, die ein entgegengesetztes Resultat gewähren, wenigstens eine reelle Wurzel der Gleichung liege." (Pamphlet). Prague. English translation in Russ (1980). Reprinted in Russ (2004).

Cavaillès, J. (1938/1981). *Méthode axiomatique et formalisme.* Paris, Hermann.

Cantor, G. (1872). "Über die Ausdehnung eines Satzes aus der Theorie der trigonometrischen Reihen." *Math. Annalen* 5: 123–132. Reprinted in Cantor (1932): 92–101.

[30] For a detailed discussion of these points, see my (1999) article.

Cantor, G. (1932). *Gesammelte Abhandlungen mathematischen und philosophischen Inhalts.* Berlin, Springer. Reprint (1966), Hildesheim, Olms.

Cellucci, C. (2005). Mathematical discourse vs. mathematical intuition, in *Mathematical Reasoning and Heuristics*, C. Cellucci and D. Gillies, Eds., London, Kings College: 137–165.

Contro, W.S. (1975/76). "Von Pasch zu Hilbert." *Arch. History Exact Sci.* 15(3): 283–295.

Dawson, J.W., Jr. (2006). "Why do mathematicians re-prove theorems?" *Philosophia Mathematica* (3) 14: 269–286.

Dedekind, R. (1872). *Stetigkeit und irrationale Zahlen.* Braunschweig, Vieweg. Reprinted in Dedekind (1930–1932), III: 315–334. English translation in Dedekind (1901; 1963). Revised translation in W. Ewald (1996): 765–779.

Dedekind, R. (1930–1932). *Gesammelte mathematische Werke.* Braunschweig, Vieweg; 3 volumes, R. Fricke, E. Noether, and Ö. Ore, Eds.

Dedekind, R. (1901; 1963). *Essays on the Theory of Numbers: I. Continuity and irrational numbers. II. The nature and meaning of numbers.* Wooster Woodruff Beman, translator. Chicago, IL, Open Court (1901). Reprint: New York, Dover (1963).

Edwards, C.H., Jr. (1979). *The Historical Development of the Calculus.* New York-Heidelberg-Berlin, Springer.

Edwards, P., Ed. (1967). *The Encyclopedia of Philosophy.* New York, Macmillan & Free Press.

Enderton, H.B. (2001). *A Mathematical Introduction to Logic.* Second edition, Burlington, MA, Harcourt/Academic Press.

Ewald, W. (1996). *From Kant to Hilbert: A Source Book in Mathematics.* Oxford, Clarendon Press.

Feferman, S. (2006). "Are there absolutely unsolvable problems? Gödel's dichotomy." *Philosophia Mathematica* (3) 14(2): 134–152.

Feferman, S., H.M. Friedman, M. Maddy, and J.R. Steel (2000). "Does mathematics need new axioms?" *Bull. Symbolic Logic* 6(4): 401–446.

Gergonne, J.D. (1818/19). "Essai sur la théorie des définitions." *Annales de Mathématiques pures et appliquées* 9: 1–35.

Hallett, M. and U. Majer, eds. (2004). *Hilbert's Lectures on the Foundations of Geometry 1891–1902.* Berlin-Heidelberg-New York, Springer.

Heine, E. (1872). "Die Elemente der Funktionenlehre", *Journal für die Reine und Angewandte Mathematik* 74: 172–188.

Hilbert, D. (1897). "Die Theorie der algebraischen Zahlkörper", *Jahresbericht der Deutschen Mathematiker Vereinigung* (JDMV) 4: 175–546. Reprinted in Hilbert (1932 I): 63–482.

Hilbert, D. (1899). *Grundlagen der Geometrie.* Leipzig, Teubner.

Hilbert, D. (1902). *Foundations of Geometry.* La Salle, IL, Open Court.

Hilbert, D. (1900). "Über den Zahlbegriff", *Jahresbericht der Deutschen Mathematiker Vereinigung* (JDMV) 8: 180–194. Reprinted as Anhang VI in *Grundlagen der Geometrie*, 3rd ed., (1909), Leipzig, Teubner.

Hilbert, D. (1904). "Über die Grundlagen der Logik und der Arithmetik", in *Verhandlungen des dritten Mathematiker-Kongresses in Heidelberg, August 1904.* Leipzig, Teubner (1905): 174–185. English translation in Hilbert (1967).

Hilbert, D. (1918). "Axiomatisches Denken." *Mathematische Annalen* 78: 405–415.

Hilbert, D. (1932, I; 1933, II; 1935, III). *Gesammelte Abhandlungen* (Collected Works, 3 volumes). Berlin, Springer. Reprint (1965), Bronx, N.Y., Chelsea.

Hilbert, D. (1967). On the foundations of logic and arithmetic, in *From Frege to Gödel.* J. van Heijenoort, Ed., Cambridge, MA, Harvard University Press: 130–138.

Hilbert, D. (1971). Letter to Frege, dated December 29, 1899, in G. Frege: *On the Foundation of Geometry and Formal Theories of Arithmetic*, E.H. Kluge, translator. New Haven, Conn., Yale University Press.

Hilbert, D. (1976). "Mathematical Problems", in *Mathematical developments arising from Hilbert problems.* Proceedings of Symposia in Pure Mathematics, vol. XXVIII, Part 1, F.E. Browder, Ed., Providence, RI, American Mathematical Society: 1–34.

Hintikka, J. (1980). "Aristotelian axiomatics and geometric axiomatics". *Proceedings of the 1978 Pisa Conference*, J. Hintikka et al. (Eds.), Dordrecht, D. Reidel: 133–144.

Hintikka, J. (1996). *The principles of mathematics revisited.* Cambridge, Cambridge University Press.

Kennedy, H.C. (1972). "The origins of modern axiomatics: Pasch to Peano." *American Mathematical Monthly* 79: 133–136.

Kline, M. (1972). *Mathematical thought from ancient to modern times.* New York, N.Y., Oxford University Press.

Kneale, W. & M. (1971). *The development of logic.* London, Oxford University Press.

Kossack, E. (1872). *Die Elemente der Arithmetik.* Berlin.

Landau, E. (1930/1965). *Grundlagen der Analysis.* First edition, Leipzig, Teubner 1930. Reprinted from the 4th edition 1965, New York, N.Y., Chelsea. English: *Foundations of analysis* (1951), F. Steinhardt, translator, New York, N.Y., Chelsea.

Lederman, W. (1957). *Introduction to the theory of finite groups.* Edinburgh, Oliver & Boyd.

Lenhard, J. and M. Otte (2002). “Analyse und Synthese – Von Leibniz und Kant zum Axiomatischen Denken.” *Philosophia Naturalis* 39(2): 259–292.

Méray, C. (1872). *Nouveau Précis d’analyse infinitésimale.* Paris.

Oliveri, G. (2005). “Do we really need axioms in mathematics?”, in *Mathematical Reasoning and Heuristics.* C. Cellucci and D. Gillies, Eds., London, Kings College: 119–135.

Pasch, M. (1882/1926). *Vorlesungen über neuere Geometrie.* Leipzig, Teubner 1882. Second edition 1926, with an appendix by Max Dehn, Berlin, Springer.

Peano, G. (1888). *Calcolo geometrico, secondo l’*Ausdehnungslehre *di H. Grassmann, preceduto dalle operazioni della logica deduttiva.* Torino.

Peano, G. (1889). I principi di geometria, logicamente espositi. *Opere scelte* 2, U. Cassino Ed., Roma, Editioni Cremonese (1958): 56–91.

Pieri, M. (1894), in *Rivista di Matematica* 4: 51–90.

Poincaré, H. (1891). “Les Géometries non-euclidiennes.” *Revue générale des Sciences pures et appliquées* 2(15 déc. 1891): 769–774. Reprinted in Poincaré (1902): 63–76.

Poincaré, H. (1902). *La Science et l’Hypothèse.* Paris, Flammarion.

Rav, Y. (1999). “Why do we prove theorems?” *Philosophia Mathematica* (3) 7: 5–41.

Rav, Y. (2007). “A critique of a formalist-mechanist version of the justification of arguments in mathematicians’ proof practices.” *Philosophia Mathematica* (3) 15: 291–320.

Rudin, W. (1974). *Real and complex analysis.* (2nd edition). New York, N.Y., McGraw-Hill.

Ruelle, D. (2007). *The Mathematician’s Brain*: A personal tour through the essentials of mathematics and some of the great minds behind them. Princeton, N.J., Princeton University Press.

Russ, S. (1980). “A translation of Bolzano’s paper on the Intermediate Value Theorem.” *Historia Mathematica* 7 (2): 156–185.

Russ, S. (2004). *The Mathematical Works of Bernard Bolzano.* Oxford, Oxford University Press.

Seidenberg, A. (1975). “Did Euclid’s *Elements*, Book I, develop geometry axiomatically?” *Archive for History of Exact Sciences* 14(4): 263–295.

Szabó, Á. (1969). *Die Anfänge der griechischen Mathematik.* München, R. Oldenbourg. English: *The beginning of Greek mathematics*, A.M. Ungar, translator. Dordrecht, D. Reidel (1978).

Toepell, M.-M. (1986). “On the origins of David Hilbert’s ‘Grundlagen der Geometrie’.” *Archive for History of Exact Sciences* 35: 329–344.

Van Kerkhove, B. and J.P. Van Bendegem, eds. (2007). *Perspectives on Mathematical Practices (PMP 2002)*. Dordrecht, Springer.

Veronese, G. (1891). *Fondamenti di geometria.* Padova.
Wussing, H. (1984). *The genesis of the abstract group concept.* Cambridge, MA, MIT Press. Translation of *Die Genesis des abstrakten Gruppenbegriffes.* Berlin, VEB (1969).