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Anti-foundationalist Philosophy of Mathematics and Mathematical Proofs

Stanisław Krajewski

University of Warsaw
Krakowskie Przedmieście 3 Street
00-927 Warszawa, Poland

e-mail: stankrajewski@uw.edu.pl

Abstract:
The Euclidean ideal of mathematics as well as all the foundational schools in the philosophy of mathematics have been contested by the new approach, called the "maverick" trend in the philosophy of mathematics. Several points made by its main representatives are mentioned – from the revisability of actual proofs to the stress on real mathematical practice as opposed to its idealized reconstruction. Main features of real proofs are then mentioned; for example, whether they are convincing, understandable, and/or explanatory. Therefore, the new approach questions Hilbert's Thesis, according to which a correct mathematical proof is in principle reducible to a formal proof, based on explicit axioms and logic.
Keywords: mathematical proof, axiomatic proof, formal proof, philosophy of mathematics, foundations of mathematics, mathematical practice, explanatory proof, analytic proof, Hilbert's thesis.

1. Historical Background: from Euclid to Hilbert

For centuries mathematical proofs have been seen as special, different from any other kind of argument. Mathematicians and all educated Westerners could point to their exceptional traits: proofs in mathematics seem more precise, more elaborate, more compelling, more certain, more logical than any other proof-like discourse – so much more that they can be seen as absolute. A crucial evidence has been provided by the Euclidean axiomatic system of geometry. This book was taught to all who were able to follow mathematics and served as a paradigm of mathematical argument. Euclid's system was seen as complete, all geometrical theorems were supposedly reducible to the initial general "common notions" and specific postulates. As late as the 19th century, it turned out that some implicit assumptions were used and that a more complete treatment was needed in order to achieve the goal of having the system of geometry that is purely logical and does not depend on intuitive visualization. This was possible due to the work of Moritz Pasch and

David Hilbert. In addition, the development of non-Euclidean geometries showed the limitations of the intuitive methods and the need for rigor. All of these developments did not diminish the influence of the Euclidean ideal of axiomatic mathematics. Rather, they seemed to confirm the view that mathematics consists, at least ideally, of axiomatic theories that can be presented in a very rigorous way, making explicit all assumptions.

One element of the contemporary version of the axiomatic method has been different from the approach of Euclid: rather than defining directly the objects of the theory (for example, points and lines) the objects were indirectly defined by the axioms that expressed the main properties of the objects and, even more important, basic relations between the objects. Nothing more was assumed than what was stated by the axioms. Hence Hilbert's famous remark that the objects of his system of geometry can be anything, for instance "tables, chairs, and beer mugs," as long as they satisfy all the axioms. This approach made possible a new variant of the axiomatic method; it slowly emerged in the 19th century. Namely, arbitrary axioms can be proposed and their realizations studied. Hence the notion of a group and other structures studied in abstract algebra. How they can be applied to the world is another matter. Pure mathematicians may disregard it. In practice, however, axioms were never completely arbitrary; rather, they conveniently codified regularities observed in the world of mathematical objects. Yet the idea that axiomatic theories can have multiple realizations became a new norm. In the 20th century the theory of models emerged, or a study of possible theories and their various interpretations.

In order to have a strict mathematical theory of models it was necessary to have a full description of the logical machinery utilized to prove theorems form axioms. This was possible due to the work of Frege and later proponents of logicism. Hilbert was happy that as if in result of "a preestablished harmony" logic itself was axiomatized: the so-called first order logic was identified as basic.

In addition, due mainly to Georg Cantor, actually infinite sets were introduced as an object of study in mathematics. The general concept of a set was also necessary in order to develop systems of higher order logics that reflected methods naturally used by mathematicians. To make clear what properties of sets may be used so that we can avoid antinomies that were plaguing the early research dealing with infinite sets, Zermelo axiomatized set theory. Since then, in the early 20th century, it was developed by Fraenkel and others so that the ZF (or ZFC, that is, ZF with the axiom of choice added) system emerged that has been seen as an adequate basis for abstract mathematics. Interestingly, the axiomatization of set theory was made in the spirit of Euclid: the principal properties of the intuitive concept of a set were listed so that all other properties of "pure sets" could be logically derived.

As a result of all those well-known developments, some hundred years ago it became widely agreed that the axiomatic method could be seen as normative. Its strengthening, namely the notion of a formalized theory, became the ideal of mathematical theory, especially for those who assumed that the right approach to mathematics must be grounded in logic. A formalized theory is axiomatic, the axioms are expressed in a perfectly defined language, its underlying logic is axiomatized, and the meanings are assumed to be grasped by all these axioms together with formal rules of derivation of formulas from other formulas. This picture of the axiomatic approach and its refinement, the notion of formal theories, has been highly successful and extremely influential among philosophers. For some analytic philosophers this picture became a model of scientific and even philosophical analysis.

The notion of axiomatic mathematics involved an understanding of mathematical proof. Its essence was seen in Hilbert's concept of formal proof: it is a sequence of formulas of the underlying formal language, each of the terms of the sequence being either an axiom or the result of an application of one of the explicitly listed formal rules of inference to previous terms of the sequence. There are variants of these notions, for example the sequent calculus, and extensions, for example rules with infinitely many premises, but the general idea remains: proofs are essentially derivations, very much like calculations. While everybody knows that real proofs are very different from this ideal the supposition was that they are humanly available indications of ideal proofs. The underlying assumption, then, called sometimes Hilbert's Thesis or the Frege-Hilbert Thesis, is as follows:

> Every real mathematical proof can be converted into a formal proof in the appropriate axiomatic theory.

This attractive hypothesis has been, however, rejected by more and more philosophers of mathematics since at least the 1960s.

2. Movement Against the Euclidean Notion of Proof

Probably most mathematicians do not really care whether real proofs can be converted to formal proofs or not. They may believe those colleagues who say that this is the case, but they know well that this has nothing to do with their practice of proving mathematical results. Many would probably express doubts as to whether the formal proof is really always possible, even in principle. It is hard for me to say how many would, since I have not heard about representative studies on the issue conducted among professional mathematicians.

Whatever the opinions regarding Hilbert's Thesis among those who produce proofs, an increasing number of philosophers of mathematics and mathematicians reflecting upon their profession have begun to analyze mathematical proofs as they really are. This is a part of a more general turn in the philosophy of mathematics. The change began with the analysis of proofs of Euler's formula for polyhedral, V-E+F=2, made brilliantly by Imre Lakatos in the 1960s. Among others who contributed to the new trend let me mention Philip Kitcher, Reuben Hersh, Paolo Mancosu, Yehuda Rav, Carlo Cellucci, Brendan Larvor, David Corfield, and Brian Rotman. Their positions on many issues in the philosophy of mathematics differ, but all tend to deny the possibility of, and the need for, foundations of mathematics, that is, the idea of reducing the whole of mathematics to one theory, treated as its foundation. This new attitude is sometimes called, after Aspray and Kitcher [1, p. 17], "the maverick" tradition. It is opposed to the traditional philosophical schools of the foundations of mathematics: logicism, formalism, constructivism (including intuitionism). Some representatives of the new approach are playing down the role of logic. Many want to understand mathematics as a part of human culture. Most of them doubt, to varying degrees, the adequacy of realism in the philosophy of mathematics. All want to begin with genuine mathematical practice.

It will be useful to mention briefly some of the main points made in their works, especially those that are relevant to the analysis of proofs. I will summarize some views of a few of the above-mentioned authors, those who according to me have been most innovative. Actually, there is something paradoxical in looking for novelty in this new approach to mathematics, as the point of the new trend was to observe closely what real mathematicians actually do rather than to invent something new about them. A tension is, however, inevitable between experiencing, in this case experiencing mathematics, and describing the experience. We always need to indicate what strikes us as most important and name it, and this often requires invention: we try to detect relations, which may be hidden; we attempt to form a picture of the mechanism underlying the experience; and it may happen that we become aware of the realities that are so obviously present as to be missed in earlier descriptions. (See below, in this section, examples of each of these three categories: (i) hidden relations, (ii) underlying mechanisms, (iii) obvious features that are easily ignored.) More generally, we never provide a completely neutral account of an experience or a historical process, even if we do our best to remain neutral. Rather, we present a reconstruction taking advantage of our understanding of the situation. In the case of mathematics this can be far from obvious.

Thus, Lakatos in his celebrated book [25], based on papers written in the 1960s, presented the theory of the dialectical process of the development of mathematics from proof to refutation to improved proof to another refutation, etc. This means that proofs can be mistaken or at least imperfect even if they are recognized as flawless. The refutation comes from the (intuitive) mathematical background that provides potential falsifiers. By the way, Lakatos provided an insightful rational reconstruction of the historical process of proving, so this is an example of (ii), the underlying mechanism of the mathematical experience, namely the process of proofs and refutations. Also, he indicated the relation of proofs to the environment in which they live, and which can provide counterexamples. Lakatos introduced the term "quasi-empiricism" (see his [26]) together with the claim that the methods used to establish results in mathematics are not as (qualitatively) different form natural sciences as had been assumed in the received tradition in the philosophy of mathematics. (The term "quasi-empirical" was also used by Putnam [30].)

Reuben Hersh, generally known for a beautiful popularization of mathematics – the real one, not the logicians' picture of it – in the book [7], co-authored with Philip Davis, is another forefather of the maverick tradition. In [16] he introduced the distinction between the front and the back of mathematics. This distinction, borrowed from sociological and cultural studies is, by the way, a good example of (iii), an obvious feature that was ignored by philosophers of mathematics. Namely, it is clear to every mathematician that official mathematics, presented in publications and formal lectures, is radically different from the tentative efforts, guesses, trials, hypotheses and mistakes present in the mathematical kitchen. Hersh also advocated, on many occasions, the idea that mathematical entities are cultural creations having an intersubjective reality. This cultural approach was initiated by Raymond Wilder [43] (see also [44]), but Hersh was emphasizing much more strongly the inflexibility and objectivity of mathematical creations, another point obvious to any working mathematician.

Let me mention that to represent both aspects, createdness and objectivity of mathematical entities, and keep them as equally important I have introduced the concept of "suprasubjective existence" in [24]. Suprasubjective is defined as intersubjective and, at the same time, "objective without objects."

Rav [32] argues that many mathematical theories have not been axiomatized and it seems that they will never be: any attempt to do this would require far reaching changes in the theory. Even group theory, defined by axioms of the group, uses higher order methods that have little to do with axiomatic theories. And actually there has never "been a unique conception what axioms are" [33, p. 125]. Independently of this, Rav [31] proposed an interesting solution to the age old problem of whether what we do in mathematics can be characterized as invention or discovery. According to his proposal, concepts are invented and theorems are discovered. In relation to our main topic, he emphasized the crucial role of proofs in mathematics. They are the heart of the matter. Theorems are only convenient expressions of what has been or can be proved. Proofs are like bus routes and theorems like bus stops that are established in a rather arbitrary way.

Cellucci, in several publications, for example in [4] and in [6], has been advocating the concept of analytic proof that he traces back to Plato, while the concept of axiomatic proof, used by Euclid, was recommended by Aristotle. Cellucci reminds us that a mathematical work begins not with axioms but rather with a problem. To produce an analytic proof one has to find a suitable hypothesis that makes it possible to solve the problem. This hypothesis must be plausible and sufficient for a derivation of the theorem. The derivation may be deductive, but this is not necessary. Thus the crux of the proof is to find the suitable hypothesis. It may be a construction, a concept, a theorem, a picture, a theory, or a conjecture. The search for a right hypothesis is certainly pervasive in research and this, by the way, provides an example of (i), a hidden relationship between elements of mathematical experience. Cellucci claims that everything in mathematics is hypothetical: concepts, objects, theorems. He also claims that the nature of proof in mathematics is not essentially different from the method of other sciences and methods of arguing in other situations. In [6] a comprehensive theory of knowledge is presented encompassing mathematics.

Many of the points made by the above authors are made because of the emphasis put on the practice of mathematicians, and in particular their experiences. Talking about mathematical experience rather than mathematical reality one wants to emphasize the human aspect of mathematics. The same emphasis also applies to the analysis of proofs. One does not need to reject the presence of objective, mind-independent aspects of mathematics to claim that needs, peculiarities, and limitations of human beings are indispensable for any account of mathematical proofs. They must explain the matter, so some sort of psychologism seems to be inevitable. (See Krajewski [23].) Incidentally, this is another example of an obvious property that is often ignored by those who look for completely objective description, relations between essences, etc.

A much stronger claim to the effect that mathematics is a human activity and nothing more has been made by Rotman. He is close to the view of mathematics as consisting of social constructions (David Bloor initiated the whole school of sociological account of mathematics; see Ernest [11]). He is, however, watching the behavior of mathematicians in a very penetrating way. In [34] Rotman introduced "a semiotic of mathematics" and pursued the issue further in [35] and [36]. What mathematicians do is described as "thinking and scribbling" performed in order to address other mathematicians. Each mathematician is analyzed into three levels: a mathematical disembodied Subject manipulating signs, above it the real Person with a body and history, telling a metanarrative, and below it a skeletal Agent doing calculations and constructions, also infinite ones, in an imaginary world. A proof is seen as a thought-experiment, and mathematical assertions become predictions about the Subject's encounters with signs.

Let me also mention some other works important for the new philosophical approach. George Polya and his work [29] on non-deductive arguments in mathematics was as an important source, Thomas Tymoczko's influential anthology [40] has served as a reference, Reuben Hersh's anthology [19] gathered together many non-standard approaches to mathematics. In another vein, the book by Stanislas Dehaene [8] on our in-born protomathematical abilities added the neuronal aspect, and the book Lakoff and Núñez [27] emphasized further the fact that our mind is embodied and all the time we use metaphors relating to the physical world.

All varieties of the new, maverick, approach to the philosophy of mathematics share several points. First, the rejection of the Euclidean myth, according to which mathematics is fully objective, completely universal, and absolutely certain. Secondly, a most concentrated attack has been on the idea of the unification of mathematics within one theory, especially on any form of foundationalism, in particular the dominant proposal to have a version of ZF set theory as the foundation. Thirdly and more generally, any imposition of philosophically motivated standards on mathematical activity is rejected. The genuine practice of research mathematicians is declared to be the starting point. This can be expressed, using the term of Penelope Maddy (who, however, wrote as a foundationalist rather than a "maverick"), as "mathematics first", against the traditional "philosophy first" (*philosophia prima*) and the modern "science first." Among the main ingredients of practice is the mathematician's proof.

3. Proofs as They Really Are

In real mathematics problems are proposed and solutions are sought. At the beginning of research for proofs there are problems, not axioms. The work of axiomatizing various domains is also an example of a problem: deciding if given axioms are sufficient for proving a statement is just one more possible math problem. Below, some major features of real life proofs are listed. The proofs must be convincing, understandable, explanatory. (Cf. Hersh [17]: "Proving is convincing and explaining.") Moreover, proofs are meant as valid, final, but at the same time they contain gaps and are revisable.

3.1. Convincing

Most often proofs refer to neither axioms nor other first principles. Instead – as emphasized by Lakatos, Hersh and others – they refer to established mathematics. Whatever is used must be acceptable to appropriate experts. Proofs are presented in the way that makes them understandable to experts. (Textbook proofs for students are often more detailed, but they are fundamentally similar, only a more limited expertise is assumed.) The aim of a proof in a research paper is to convince experts: this category varies according to the context – it can mean all professional mathematicians or, at the other end of the spectrum, a handful of colleagues involved in researching the same topic. In each case a broad corpus of established mathematical results is assumed as given, its validity is not questioned. Of course, mistakes happen. They are, however, sooner or later identified and eliminated. A subtler situation than a simple mistake can occur: sometimes a new understanding of concepts emerges and previous results are rejected or limited to special cases. This was well illustrated by Lakatos who used the Euler formula for polyhedra. Another well-known example, also considered by Lakatos, among many others, is provided by Cauchy's theorem on the continuity of the limit of a converging sequence of continuous functions. Now it is considered a mistake, because uniform convergence must be demanded rather than the weaker pointwise convergence. There exist, however, analyses indicating the correctness of Cauchy's theorem if instead of the current concept of convergence or of the continuum another one is assumed, presumably one closer to Cauchy's original understanding. A perfect example is provided by Robinson's nonstandard analysis: pointwise convergence on standard and nonstandard numbers is sufficient for Cauchy's theorem.

3.2. Understandable

Another psychological property is often assumed by mathematicians: a proof must be understandable. For a human mathematician (are there any other?) one of the most convincing methods of proof is by producing appropriate pictures. This usually enables immediate understanding. Sometimes the picture itself constitutes the proof. Many pictorial proofs of the Pythagorean theorem serve as examples. This sort of proof is possible for many finite configurations, claims Giaquinto [13]; and Brown [2] says that perhaps also for some infinite ones. More than that, often a picture accompanies the invention of a proof in the mathematical "kitchen", to use Hersh's term, even though it rarely finds its way to the official presentation. Even if the matter is not geometric some visual arrangements, mental pictures – imprecise, hazy, messy, often moving, difficult to describe – seem to be common. They help us understand the situation. They are presented to other workers in the kitchen, to help make the point, to convince and induce understanding.

Even when no picture is associated with the proof, to be understandable the proof must be surveyable. Its structure should be graspable. And, preferably, one must be able to tell what is its point. While there are proofs which are not understandable, for example consisting only of calculations, they are seen as less satisfying. And anyway, the discovery of such a proof is usually guided by some understanding. Using Rotman's terms, it is important to be able to have a metanarrative explaining the essence of the narrative that constitutes the proof. This leads us to the next point.

3.3. Explanatory

One of the main features of proof is that it must explain the concepts involved, relations between them, and show not just the truth of conclusion but also *why* the conclusion is true. Often proofs are not providing sufficient explanation, for instance, if the crucial part consists of a calculation and no picture or idea can be indicated as a clarification of the formal manipulations. In such cases a deeper understanding of the proof is sought or other proofs are welcomed so that explanation can emerge. And actually, very often new proofs are sought to explain the aspects of the situation that seem still hidden. Let me mention an example from my own practice. A long time ago I formulated a conjecture (to the effect that a recursively saturated model of Peano arithmetic admits a full satisfaction class – the strict meaning of the terms is not important here) that was soon demonstrated in collaboration with two colleagues and published in Kotlarski, Krajewski and Lachlan [22]. The proof was rather indirect, using a proof theoretic technique. Many years later, long after I had stopped working in this area, Enayat and Visser [9] formulated another proof, much more natural, since it uses only model theoretic constructions. And recently, in 2020, James Schmerl, in the yet unpublished paper "Kernels, Truth and Satisfaction," took the model theoretic proof, and showed that if "stripped to its essentials," it can be expressed as a special property (the existence of a kernel) of certain directed graphs. Thus the technical problem in the proof was reduced to graph theory. The specific logical notions of satisfaction, models, etc. were invoked only as an application of an abstract graph theorem.

Even this modest example illustrates a general point: it is accepted and common to look for a proof by taking advantage of other branches of mathematics than the one in which the problem is formulated. A famous example is provided by Fermat's Last Theorem. Also merging methods and concepts of various branches is seen as valuable, for example probabilistic methods are used in various ways even if probability was not mentioned in the initial problem. New branches were created when similarities of constructions in different parts of mathematics were noticed and properly defined. Or, as a well-known saying goes, good mathematicians perceive analogies, and the best see analogies between analogies. Category theory is a good example.

It is also important to remember that there exist tentative proofs or proofs produced by doubtful methods, for example by analogy. A famous example is provided by Euler's calculations of some infinite sums. He used infinite polynomials as if they had properties similar to the finite cases. In this way he calculated the sum of the series of the reciprocals of the squares of natural numbers as equal to $\pi^2/6$. (See Polya [29, p. 20], or, for example, Putnam [30].) Of course, Euler was aware that his proof was not certain, but when he calculated the initial segment of the series and found it coincide with the proposed number, up to some decimal position, he was convinced that the result was true and the proof fundamentally correct. Later he found a more standard proof.

All the above examples indicate how natural and desirable it is for mathematicians to use unanticipated methods. In other words, proofs can be very far from being pure. Rather, anything is accepted as long as it leads to the aim of deciding the problem one way or another. The idea advocated by logicians that there is an established framework, language, axioms, and proofs are supposed to be conducted within the framework, is simply not true in living mathematics. On the other hand, there is an attractive element to this idea, and actually finding a pure proof of a major theorem established by extrinsic methods is seen as a valuable achievement. To introduce some "purity" one can also formulate a comprehensive theory in which all the methods used to solve the problem are expressible. One can also try to reconstruct the whole proof in set-theoretical language. Such moves are, however, alien to an overwhelming majority of mathematicians. And even if the proof can be reconstructed, it can no more be as convincing, understandable, explanatory as the original argument. I believe that the explanatory power is felt as the single most important feature of proof.

3.4. Revisable

The above-mentioned two examples, Euler's formula for polyhedra and Cauchy's theorem on continuity of the limit of continuous functions, show that proofs are revisable. This is not something mathematicians usually accept. When is a proof seen as good, proper, correct, worth its name? To quote Epstein [10, p. 137] proofs "are meant to be valid." That is to say, it is impossible for the conclusion to be false if the assumptions are true. The proof is supposed to show that something is a fact. Yet new evidence may emerge and the finality of the proof might turn out to be illusory. This possibility is emphasized by all champions of the maverick philosophy of mathematics. How is this possible?

One reason for the collapse of a proof is due to the possibility of changes in our understanding of the concepts used in a proof (cf. the concept of polyhedron). Another reason is due to changes in the standards of rigor (cf. Euler's calculation of the sum of the series of the reciprocals of the squares of natural numbers). Yet another reason is due to the chance of errors that keep popping up. While, as mentioned above, it is generally believed that errors can be ultimately overcome, the more complex the arguments the more probable are either mistakes in proofs or omissions that can be threatening. Some important examples have appeared rather recently, for example enormously long proofs, like the classification of all finite groups that has been achieved by a long collective process involving many mathematicians. There were leaders of the effort, but it seems that nobody has checked the whole proof. (See, for example, Byers [3].) Still it is believed that the job has been done. It is not impossible, though, that something has been overlooked.

Another important kind of example emerged when computers began to be used in mathematics. There exist proofs partly executed by computers. The four color theorem is the best-known example. (See Tymoczko [39]; it was the first philosophical analysis of computer-assisted proofs.) The possibility of error contained in the hardware used is a new source of uncertainty. Yet, repeating the proof on other machines very significantly reduces the chance error. It is probable that the chance human proofs contain errors is higher.

In addition to computer-assisted proofs there are probabilistic proofs. Using it one can prove that a very large number is prime but the proof procedure uses several random moves and is so conceived that it gives the result (that the given number is prime) only with a very high probability. If the chance of error is less than $1/2^{100}$ we can be pretty sure that the result is correct. (See, for example, Rav [32] for more details and references to the papers, from the 1970s, by Michael Rabin and by Robert Solovay and Volken Strassen.)

This last example gives a proof that there are *bona fide* mathematical proofs that lead to conclusions that are not certain. The claim of "the mavericks" is that all proofs share this characteristic. This applies even to most formal ones. As indicated by Cellucci and also by Friend [12, p. 207] even formalized proofs can have "external gaps". These are gaps residing in the external context of proof, specifically in the justification for an axiom or rule of inference. We take it for granted because we assume a standard interpretation. Yet a non-standard interpretation can appear, even of a logical symbol or of a basic concept like that of a set. Then some of the obvious properties may no longer be true. Think of the law of excluded middle which is rejected by constructivists or of the concept of set as defined by a set theory other than ZFC.

4. Conclusion

There is a whole spectrum of the views on the nature of mathematical proofs. An extreme position was expressed by Hardy: there is no such thing as a proof, "we can, in the last analysis, do nothing but point," so there are only rhetorical "devices to stimulate the imagination of the pupils" [14, p. 18]. The other extreme is expressed by Hilbert's Thesis: real proofs are abbreviations and approximations of the ideal formal proofs. Hersh wrote that the belief in the Thesis "is an act of faith" [17, p. 391]. Logicians tend to believe it; their evidence is inductive: so much has been formalized that it seems that we can never encounter insurmountable obstacles if we try hard enough. The point illustrated by the considerations contained in this paper is that even if this is the case and in principle we can convert each proof into a formal one, this is not really significant. The most important features of real proofs – their being convincing, understandable, explanatory – are lost in the process. And the reasons for revisability are not present within the formal proof. The maverick philosophy of mathematics has succeeded in exhibiting the whole range of problems related to Hilbert's Thesis. The debate on the possibility and significance of formalizability of proofs continues.

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