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ON (SOME) PRESUPPOSITIONS IN MATHEMATICS¹

Key words: Computer-assisted proofs, apriority of mathematics, informal proofs

1. The formal and the informal discourse in mathematics. 2. The problem of apriority in mathematics. 3. Final remarks.

This paper is based on the talk *Presuppositions in Mathematics* which was given at the *Science versus Utopia. Limits of Scientific Cognition* conference, UKSW, Warsaw, 23-24.11.2011. During the talk, I mentioned three groups of problems, which, I think, deserve special attention in this context:

1. The relationship between the formal and informal discourse in mathematics.

2. The question of *ignorabimus* in mathematics.

3. The problem of the apriority of mathematics.

It is impossible to discuss all these problems here, so I will concentrate on the first and the last only. A detailed analysis of these matters is beyond the scope of a single article, but I hope to indicate some points for further discussion.

1. THE FORMAL AND THE INFORMAL DISCOURSE IN MATHEMATICS

To simplify matters, let us consider two radically different points of view, concerning the nature of mathematical proof (which we might label as the “semantic” and the “formalistic” point of view):

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(1) In the semantic tradition (which can be traced back e.g. to Descartes), mathematical proof is a sequence of intuitively acceptable propositions. The “formal layer” of proof is not the crucial one. What is important is the fact, that competent mathematicians understand the proofs and accept them; they recognize the assumptions as obvious, and the steps in the proofs as legitimate. Proving theorems is, from this point of view, a sequence of intellectual acts of the mathematicians’ mind. So ultimately, doing mathematics is possible, because we are in the possession of a kind of *intellektuelle Anschauung* of the subject matter.

(2) From the formalist point of view, mathematical proof is a formal construct. The crucial fact is, that some purely formal rules of manipulating strings of symbols are given, and the proof conforms to these rules. Intuitive understanding of proof by mathematicians is neither a necessary, nor a sufficient condition for the correctness of the proof. Proving theorems is therefore rather considered to be manipulating strings of symbols than performing intellectual acts.

It is a remarkable fact, that mathematical proofs we encounter in everyday practice are precise and rigorous, but they are not formalized in the sense of proof theory. Real mathematical proof is in fact written in a “natural mathematical language”, i.e. a kind of mixture of natural and symbolic language (usually English, with some hundreds of additional symbols). No mathematician ever writes proofs in the language of, for example, ZFC (or any other formal theory used to formalize the underlying informal discourse). In fact, formalizing proofs from, for example differential topology would not lead to any cognitive gain. It is quite probable, that the authors of the proofs would not usually recognize their formalized versions. An expert in, for example complex analysis would not consider the question, whether a certain proof has been formalized, to be really important. Moreover, I think that the expert would not be willing to invest his energy in formalizing proofs he considers to be perfectly understandable and convincing. He would rather concentrate on solving open problems, creating new ideas and inventing new concepts. The knowledge of the formal counterparts (i.e. the formalized versions) of the proofs is not of primary, or indeed any importance to the development of the given area of mathematics.

On the other hand, it is widely believed, that mathematical proofs **can in principle** be formalized, so a kind of “formalizability postulate” is held to be true. There must be a connection between the (informal) proofs we know from seminars and textbooks on, for example, differential geometry or probability theory, and certain formal strings of symbols in, say, ZFC. But why do we believe, that there is such a connection? And if the criterion for being acceptable proof is having a formal counterpart, why do we accept mathematical proofs without really producing these counterparts? The problem of the relationship between the formal and the informal elements in mathematical proofs, of “Hilbert’s bridge” between informal proof and its formal counterpart, therefore requires attention.

Of course, there is no doubt that there are purely formal elements in many real mathematical proofs; for example when a series of algebraic operations is performed. On the other hand, real proofs are far from being formalized and the experts accept the given proof when they feel convinced, and not after being presented the formalized version. These formalized versions never really occur in mathematical practice. The key point is the moment of grasping the general idea, the *Leitmotiv* of the proof in an intellectual act (or usually in a series of intellectual acts). The question arises, what is the rationale behind the mathematicians’ decisions of accepting mathematical proofs – and what are the connections of their decisions with the (potential) existence of the formalized version of the proof?²

We know from everyday practice that mathematical proofs are not formalized. On the other hand, we assume that such proofs could, in principle, be formalized, if necessary. But why should formalizing proofs be necessary? Moreover, isn’t formalizing proofs just a waste of time, if mathematicians are perfectly happy with their informal, but convincing proofs? Does providing formalized versions of proof have anything to do with the real cognitive processes, which take place in mathematics, and does it provide more understanding of the given subject?

² This fact can be seen in the context of discovery: the source of a proof is a general idea, which sometimes comes as kind of “illumination”. The (often cumbersome) details of the proof are completed much later.

In the process of proving theorems, we proceed from the acceptance of a certain sentence α to the acceptance of another sentence β .³ But what is the nature of these transitions? In practice we never decompose them into elementary formal operation, and such a process very rarely has a linear structure: we usually take into account not only one single preceding formula (two formulae, when applying the modus ponens rule), but we usually make use of a substantial fragment of our background knowledge. What is important in accepting β is a whole „bunch” of the mathematicians’ convictions concerning the given subject.

From a purely formal point of view, the description of this background knowledge is quite clear: it consists of previously accepted axioms and already proven formulas. However, this of course does not describe the real cognitive processes, which take place while proving new theorems (in general: enriching our mathematical knowledge). From the intuitive, semantic point of view, proving theorems amounts to performing a series of intellectual acts, which give insight into the truth of mathematical statements, and these insights legitimise the transitional steps in our argumentation. A natural question arises: what is the fundamental level of these operations? And what is the warrant for the legitimacy of these fundamental, “atomic” acts?

One of the reasons for looking for formalized versions of proofs is the need for clear criteria of mathematical truth, free of subjective elements. From this point of view, the formalizability postulate is a kind of methodological constraint concerning the legitimacy of proofs. But we can view this postulate from two very different angles:

(1) We can consider it to be a discovery about mathematical proofs, which stems from an insight into their deep nature. From this point of view, the background of this postulate is the discovery of what mathematical proof really is, namely, a formal derivation in a certain system. From this point of view, informal proof is, in a sense, just an abbreviation of the real proof.⁴ We could also say, that mathematicians,

³ Some of the most important places in proofs are „labeled” by „hence”, „therefore” *etc.*

⁴ In [Azzouni 2004] the author explicitly states the thesis, that what makes a real proof convincing is a formal derivation behind it, formulated in a certain algorithm.

while formulating informal proof, only produce some “hand-waving” arguments, which have to be justified by providing the formal versions of the proofs (or at least: providing an argument, that such a version exists). We might therefore say, that not all mathematicians really know what proof is; many of them are not aware of the real (i.e. the formal) nature of mathematical proof.

(2) But we might also claim that the formalizability postulate is not a discovery, but quite an arbitrary stipulation concerning acceptable mathematical arguments. From this quite different point of view, the formalizability postulate amounts really to a radical redefinition of the notion of “being mathematical” or “being an acceptable mathematical argument” – and this redefinition is motivated by some philosophical needs, not by the needs of mathematical practice. From this point of view, acceptable proof is that proof, which mathematicians accept as legitimate, and this intuitive judgment is the ultimate criterion. The question as to whether proof can be reconstructed in a highly artificial symbolic system (from the point of view of mathematical practice) is not really important for the problem of the legitimacy of proof. The adherents to this point of view indicate the fact, that the formalizability postulate is quite a recent development in the history of mathematics, and that virtually no mathematician is really interested in the formal counterpart of proofs.⁵ No mathematician thinks of mathematical

mic system. So what mathematicians really do is finding some abbreviations of these proofs. From Azzouni “derivation-indicator” point of view, the truth-makers are the formal derivations, and our proofs are simply some indicators of them.

⁵ In [Barwise 1989] we find the following observation: “The idea that reasoning could somehow be reduced to syntactic form in a formal, artificially constructed language is a relatively recent idea in the history of mathematics. It arose from Hilbert’s formalist program. There were proofs for thousands of years before logicians came along with the mathematizations of the notion. But these ‘formal proofs’ are themselves certain kinds of mathematical objects: sequences of sentences in a formally specified artificial language, sequences satisfying certain syntactic constraints on their members. They certainly aren’t what mathematicians since the time of the ancient Greeks were constructing, for one thing. For another, no particular system can claim to be the real notion of proof, since there are endless variations, as is evident from the fact that there are as many different deductive systems as there are textbooks in logic. They can’t all be the real notion of proof. Rather, they provide somewhat different

proofs as meaningless strings of formulae – mathematical proofs are rather meant to convey new ideas and provide understanding of matters. “Proofs are the way to display the mathematical machinery (...) and to justify that a proposed solution to a problem is indeed a solution” (Rav 1999, 13). The proofs, we encounter in practice are not formalized – and mathematical knowledge is conveyed by these proofs. We do not bother about the formalized versions of Wiles or Perelman’s proofs of the Fermat and Poincare conjectures. Even if number theorists of differential geometers would study them, this would not increase their mathematical knowledge.

If we consider the formalized versions of the proof to be the epistemic warrants of the theorems, another quite cumbersome problem arises. Some of these formal counterparts are not feasible in any reasonable sense. The example given in [Boolos 1987] is quite illuminating in this context. The author examines a certain reasoning formalized in first-order logic. The extralogical symbols are: a constant “1”, a one-argument function symbol “s”, one two-argument function symbol “f”, and one predicate “D”.

The assumptions:

$$\forall n f(n, 1) = s1;$$

$$\forall x f(1, sx) = ssf(1, x);$$

$$\forall n \forall x f(sn, sx) = f(n, f(sn, x));$$

$$D(1);$$

$$\forall x (D(x) \rightarrow D(sx)).$$

Corollary: $D(f(ssss1, ssss1))$.

Intuitively, this reasoning concerns natural numbers, where s is the successor, f is a function defined on pairs of natural numbers, D is

models of that notion. (...) I think it is clear that current formal models of proof are severely impoverished since there are many perfectly good proofs that are not modeled in any current deductive system. (...) Moreover, identifying proofs with formal proofs leads to what may be an even more serious mistake. (...) While writing things out in complete logical notations can sometimes result in added clarity, all too often it merely obscures things, which is why practicing mathematicians almost never use such a language. And, it is not uncommon for an error to enter the picture in the translation from the English description to the formal specification.” Quoted after: [Rav 2007].

a property of natural numbers. The conclusion states that the number $f(5,5)$ has the property D.

The proof of the conclusion formulated in second-order formalism is very simple and convincing. But the version formalized in first-order logic is far from feasible. The function $f(x,y)$ grows very quickly, in the style of the Ackermann function, and the proof of $D(f(5,5))$ is of extraordinary length, out of our reach (and even of the reach of any imaginable, super-fast computer). The situation is quite concerning for adherents to the “the real proofs are the formal derivations” view. The formal proof of $D(f(ssss1,ssss1))$ exists (this fact can be proved), but is too long to be of any practical cognitive importance.⁶ So the adherent to the formalizability postulate is in some trouble here: how is it possible, that the epistemic warrants are the formal proofs, not the informal ones, if nobody is ever able to have even the faintest idea of what the formal proof looks like – but everyone can understand the informal proof perfectly well, and there is no doubt, that the proof works? Boolos claims that his example shows that the first-order formalism is not a proper idealization of our reasoning processes [Boolos 1987]. In particular, even our ability to recognize some first-order sentences as corollaries of certain reasonings involves applying cognitive resources exceeding first-order logic, and making use of some *par excellence* semantic notions.⁷

Boolos’ example shows, that there are proofs, which simply are not feasible in first-order logic: we can prove, that they are simply too long. So what is their ontological and epistemological status? Is it reasonable to maintain, that the truth-makers for some mathemati-

⁶ It is a familiar fact [cf. e.g. Gödel 1936], that some proofs can be very long in one formal system, but very simple in another one – but a price has to be paid for this simplicity, e.g. by accepting some very strong assumptions (concerning e.g. second order logic). An example is a version of Kruskal’s theorem, which is provable in ZFC, but unprovable in PA: only particular instances of this theorem are provable in PA, but the proofs are of extraordinary length [cf. eg. Simpson 1987].

⁷ An interesting thesis is formulated by Isaacson [Isaacson 1987]. He claims, that the known sentences independent of PA (like the Paris-Harrington or Parisa-Kirby sentences) involve some second order notions – and that our informal judgment of their truth also must involve such notions.

cal statements are formal derivations, which could never be performed within our universe? And – still worse – is it reasonable to maintain, that these derivations are the epistemic warrants for our knowledge, even if we will never have a chance to learn them? This seems to be a quite strange idea.

So we are confronted with two radically different points of view:

(1) The real mathematical proofs are the informal ones. The formal versions of proofs are not really important from the cognitive point of view, because they are artifacts of a (artificial – from the point of view of mathematical practice) formal system. This system is merely a tool for imitating mathematics.⁸

(2) The real proofs are the formal ones. The informal proofs are only a kind of abbreviations (indicators) for the formal proofs. We accept the informal proofs only because we know that they can be formalized, i.e. their real nature can be revealed. The fact, that mathematicians are content with informal proofs is just a psychological phenomenon, but the true nature of mathematical proofs is their formal nature.

This choice between these two standpoints is far from simple. It seems to me, that many philosophers of mathematics accept the formalizability postulate, because it gives a clear criterion of mathematical truth (and provides solutions to other philosophical problems⁹). But it certainly isn't obvious that this criterion is the proper one.

2. THE PROBLEM OF APRIORICITY IN MATHEMATICS

According to the received view, proving mathematical theorems is a purely intellectual, rational activity: we start with some axioms, some basic truths and *via* a sequence of logical steps proceed to the conclusions. This process, of course, involves an understanding of the mathematical concepts we use, and the acceptance of the consecutive steps of the proofs. Some understanding of at least the formal rules is in-

⁸ And – we might add – this imitation is motivated by the needs of the philosophers (to provide an elegant, simple framework) not the needs of real mathematicians, who are perfectly happy without this reconstruction.

⁹ E.g. accepting the thesis, that mathematics is really reducible to set theory provides a simple solution to many ontological problems.

volved here. We might say, that a necessary condition for performing mathematics is a kind of insight into the world of mathematical concepts.¹⁰ Proving theorems amount to grasping inferential connections, which exist between the premises of the mathematical argument and its conclusions, and mathematical proofs reveal the interplay of mathematical ideas. From this point of view, mathematics is surely an *a priori* enterprise. Could some empirical elements be involved here? In a more concrete (and provocative) formulation: could there be some genuine knowledge about, for example, natural numbers justified by empirical methods?

As it stands, the question seems almost meaningless, as it is not clear what is meant by “empirical”. To discuss the issue, we have to make the notion of empirical method more precise. Let us first consider simple examples, where some empirical elements are present in mathematical proofs. The most obvious case is the use of paper and pen while proving theorems, but it is also obvious, that this is not an interesting example of the use of an empirical device. It is not essential in any reasonable sense. The same applies to an abacus or any simple mechanical calculating device. But there are cases, where the presence of empirical ingredients deserves more attention. I think in particular, that computer-aided proofs lead to intriguing philosophical problems, which are of quite a different kind than the (uninteresting) problem of the status of the sheet of paper as an auxiliary empirical device.

The most famous example of computer-aided proof (CAP) is the proof of the four-color theorem (4CT). It states, that 4 colors suffice to color any map in such a way, that adjacent countries have different colors.¹¹ The hypothesis was formulated in 1852 by Francis Guthrie, and during the next 124 years many partial results have been obtained [see e.g. Kainen, Saaty 1986; Wilson 2004]. But the general solu-

¹⁰ According to Gödel: “Despite their [i.e. set theoretic objects] remoteness from sense experience, we do have something like a perception (...) as is seen from the fact that axioms force themselves upon us as being true, I don’t see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception” [Gödel 1947/64, 271].

¹¹ Of course, we have to make some natural assumptions concerning the countries – e.g. that they are in one piece etc, but the details are not important here.

tion was not found until 1976, when Appel, Haken and Koch produced a proof of the general case [Appel, Haken 1977; Appel, Haken, Koch 1977].¹² Their proof involved the computer in an essential way, so a conceptual and philosophical problem concerning the status of the proof emerged.¹³

The main philosophical issue concerning 4CT can be formulated in very a simple way, as the question of whether the 4CT has **really** been proven by the computer? Is 4CT genuine **mathematical** knowledge concerning graphs, or is it just a practical (physical) knowledge concerning the outcome of a certain empirical process (the computation of the computer)? Is 4CT now part of mathematical knowledge – like the Hahn-Banach or Stokes' theorems – or is its status somehow different? There has been a lot of discussion concerning 4CT¹⁴, which has not been settled in a definite way.

The prevailing view amongst mathematicians is (I think) a kind of a reluctant acceptance of the CAP's. Some mathematicians claim that there is no real problem here, because we know perfectly well how the computer works, but there are also skeptical opinions. One of the sources of this scepticism is the fact that we usually expect proofs not only to prove theorems, but to also explain why the mathematical facts exist. This is only partially true in the case of 4CT.¹⁵ Such a skeptical opinion is expressed e.g. by Rota: "Mathematicians are on a lookout for an argument, that will make all computer programs obsolete, an argument that will uncover the still hidden reasons for the truth of the conjecture" [Rota 1997, 186]. According to Rota (who is also refer-

¹² Their proof of Appel, Haken and Koch has been improved [e.g. Allaire 1977; Robertson et al. 1997] but these improvements do not alter the status of 4CT.

¹³ The proof of 4CT is probably the most famous example of a CAP. Another example is the proof of Kepler's conjecture concerning dense packing of spheres [Hales 2005].

¹⁴ Cf. [Tymoczko 1979], followed e.g. by [Detlefsen, Luker 1980], [Krakowski 1980], [Swart 1980], [Teller 1980], [Levin 1981].

¹⁵ The proof does not consist of purely formal calculations – there is a lot of ingenious mathematics in it. But ultimately, an indispensable part of the proof is done by the computer – and the mathematician has to rely on the outcome of the computing process.

ring to the opinion of an expert in the field), the CAP proof has a non-explanatory character, and does not reveal the true reasons for the truth of 4CT.¹⁶ Partially, this follows from the fact, that the proof of 4CT is not surveyable: we can survey it locally, but of course it is not possible to survey the whole proof [cf. e.g. Bassler 2006 for discussion of the question of surveyability].

So one of the problems is the non-explanatory character of CAPs, which leaves us with an uneasy feeling. However, even if we neglect the problem of explanation in mathematics¹⁷, there is another problem, connected with the presence of an empirical element in CAPs. We have to rely on the laws of physics in a very essential way, which is qualitatively different to the reliance on the laws of physics while using paper and pencil. Does this mean that the notions of mathematical proof and of mathematical knowledge should be modified? Should we perhaps accept a quasi-empirical account of mathematical knowledge (perhaps in the manner of Quine?¹⁸). What is the ultimate epistemological warrant for 4CT: our trust in the axioms and rules of inference, or rather our trust in the laws of physics and the robustness of the electronic equipment we use while producing new mathematical knowledge? We could think of a hypothetical supercomputer, which works 2^{100} times faster than ordinary computers. We could use such a computer in a very simple way: it would just generate formal proofs in ZFC of increasing length. As it works very quickly, we can be quite sure of the fact, that our supercomputer sooner or later produces a formal proof of

¹⁶ Dawson claims, that formal proofs, even if “provide verification that a result follows logically from given premises, they may fail to convey understanding of why it does” [Dawson 2006, 271].

¹⁷ For a discussion of the problem of explanation cf. e.g. [Mancosu 2001; Mancosu 2008].

¹⁸ According to Quine’s holistic doctrine, our knowledge forms a kind of a seamless web of beliefs, answerable only to sensory stimulation: “our statements about the external world face the tribunal of sense experience not individually but only as a corporate body” [Quine 1953]. In particular, our mathematical knowledge constitutes a part of this web, and its justification relies on its role in our overall theory of the world. Mathematical claims are not **justified** by intuitive access, but by the analysis of the relationships between mathematics and science.

a non-trivial theorem.¹⁹ What if it produces a formal solution of a certain open problem in mathematics? And what if it prints out new results (say number-theoretic results) – should we claim, that our **mathematical** knowledge about natural numbers increases? And what if we didn't use a digital computer, but another physical (perhaps analogue) problem-solving device?²⁰ Is the knowledge obtained a different kind of knowledge? This idea seems quite strange, but if we used such a device, we surely could not claim that the knowledge was obtained via purely intellectual acts and, moreover, it would not be clear if it **could** be obtained in this way.

3. FINAL REMARKS

Two main issues were discussed in the article:

(1) The relationship between the formal and informal discourse in mathematics.

(2) The problem of the *a priori* character of mathematics.

In both cases, we are confronted with certain presuppositions about mathematics. It is unclear if they are justified and they deserve a thorough discussion. It is impossible to discuss matters in detail in an article as short as this, but I hope that these remarks will at least contribute to a better understanding of the issues.

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¹⁹ Most of the results generated – more or less – “at random” would be not interesting, but even the success rate of 0.001% is sufficient to obtain some interesting results.

²⁰ We might think of quantum computers (which are equivalent to Turing machines, but quicker due to some peculiar quantum-mechanical phenomena) or about hypercomputing devices.

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PRESUPOZYCJE W MATEMATYCE

Streszczenie

W artykule rozważa się problem presupozycji w matematyce i w filozofii matematyki. Zarówno matematycy jak i filozofowie matematyki przyjmują pewne założenia dotyczące matematyki, np. założenie o możliwości jej sformalizowania, założenie o istnieniu w matematyce problemów nierozstrzygalnych (lub – przeciwnie – że wszystkie problemy mogą być rozstrzygnięte), czy założenie, że dowody matematyczne nie mają treści empirycznej. W tym kontekście w artykule dyskutuje się trzy grupy problemów, które wydają się szczególnie interesujące:

1. relacja między formalnymi i pozaformalnymi dyskursami w matematyce,
2. kwestia „ignorabimus” w matematyce,
2. status aprioryczny matematyki.

Słowa kluczowe: dowody wspomagane komputerowo, aprioryczność matematyki, nieformalne dowody